

M. Lehning (Referee #1)

General:

The paper presents a worthwhile development of the snow model SURFEX/ISBACrocus by introducing the optical diameter (or SSA) as a primary microstructure parameter that develops in time. In fact, this reviewer has suggested exactly this exercise in an earlier review on Jacobi et al. (2010) with the sentences "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" (Lehning, 2009). The authors of the current paper may want to acknowledge this. In the current paper, the two parameters dendricity and grain size are replaced by the optical diameter and this new formulation (C13) is compared to the original CROCUS formulation (B92). In addition, two earlier parameterizations (T07 and F06) are implemented as well and compared with respect to predicting snow density and specific surface area (SSA). The paper is well written and a suitable contribution for TC because it nicely demonstrates our current understanding in measurements and modelling of SSA. In particular, the comparison between long-time simulations of Col de Porte and Summit are an asset of the paper. Given the recent interest in SSA of snow because of its significance for remote sensing and snow chemistry, the paper well presents our current knowledge on SSA development. The comparison with field measurements shows that significant trends in measured SSA are not yet captured by the model. But since the new implementation reduces some redundancy in the model microstructure parameters progress has been demonstrated. On the other hand, since the influence of the microstructure parameters on the mechanical properties of snow have only a minor influence in the model CROCUS, the success of the new formulation can only be partially judged. It would be good to additionally present validation results for more conventional grain types and show quantitative model comparisons using an objective comparison such as in Lehning et al. (2001). I think that this would add a lot of scientific value to the paper. In addition, some issues need to be resolved as detailed below, before publication can be recommended.

We thank M. Lehning for his positive evaluation and for taking the time to provide detailed and constructive comments of our paper. We have carefully considered each comment and our responses are provided below. We are however surprised by the request of the Reviewer to acknowledge a previous review comment, implying that he deserves partial credit for our research strategy. While we are happy to receive sensible and constructive comments, the Reviewer must be aware that we independently have been following for almost 20 years a long term research strategy to measure snow SSA and implement its calculation in our snow model Crocus. This long term effort first required the development of adequate methods to measure snow SSA. This started with the CH₄ adsorption method, first described in Chaix et al. (1996) [CRAS, vol 322, ser. IIa, pp. 609-616]. Results from this method led to the first empirical parameterizations of snow SSA by Domine et al. (2007) and Taillandier et al. (2007), and also to the first theoretical developments on snow SSA evolution in Legagneux and Domine (2004) and Legagneux et al. (2005). Those CH₄ adsorption results also allowed the calibration of the model by Flanner and Zender (2006). Subsequent steps of actually including SSA in Crocus, an obvious continuation and a long term goal of our work, included the paper by Jacobi et al. (2010), which clearly represented only a first stage in this process. The current paper pursues this goal, while not completely fulfilling it as sphericity remains a variable in Crocus, which ideally should be removed. Thus, while it is true that the Reviewer's suggestion was adequate, claiming partial credit for it sounds a bit surprising. Regardless of this specific comment, we are again grateful for the time spent by the Reviewer in providing many useful and constructive comments, and this has been acknowledged in the revised version.

Regarding the Reviewer's suggestion of additionally presenting validation results in terms of conventional grain types and showing quantitative model comparisons using the method described in Lehning et al. (2001), please see our response to comment #22.

Detailed Comments:

(1) p. 4445, l. 19 ff: As Schirmer et al. (2009) have shown, the grain type or snow layering carries much less weight in avalanche danger assessment than e.g. the amount of new snow. The authors should therefore rephrase their statement in less absolute terms.

We thank the Reviewer for this comment. The sentence has been modified in the revised manuscript as follows:

“This notion can be important if the aim is to predict an estimate of the avalanche risk.”

(2) p. 4450, l. 12 ff: While being an important feature of the model, the grid resizing must not be described in so much detail here. Reference to earlier publications can be given and it is sufficient to say that layers can be merged (since this is discussed later in the paper).

We have adopted this suggestion and shortened the paragraph. However, we think that in the context of our study it is important to point out the fact that, on the one hand, the rules of aggregation in Crocus depend on the snow microstructural properties and, on the other hand, the characteristics of the merged layers are calculated in order to conserve the optical grain size of the former layers. Hence, the Sect. 2.2.3 has been rephrased as follows:

“Crocus can modify the discretization of the vertical grid, in order to keep the number of layers below a predetermined value (typically 50). When a new snowfall layer is added to an existing snowpack, the model first prescribes specific values accounting for its microstructure. Then, if the freshly fallen snow layer and the existing top layer have similar characteristics, they are merged. The similarity between both layers is determined from the value of the sum of their differences in terms of d , s and g_s , each weighted with an appropriate coefficient ranging from 0 to 200: 0 corresponds to the case in which the same grains are present in both layers and 200 to very different grains. In other words, merging is only possible for layers which are similar enough in terms of grain characteristics. If a new numerical snow layer is built from two older layers, its characteristics are calculated in order to conserve the averaged weighted optical grain size of the former layers. This ensures a strong consistency in the evolution of surface albedo. When, instead, merging is not possible, a new numerical layer is added to existing snowpack. A complete description of the grid resizing in Crocus can be found in Vionnet et al. (2012).”

(3) p. 4452, Eq. 4: I think this follows already from Eqs. 2-3 and thus this equation is redundant.

The Reviewer is correct, Eqs. 4a and 4b follow directly from Eq. 2. Yet, we feel that it is useful to keep these equations, as the separation between dendritic and non-dendritic regime is one of the most important features of B92 and C13 formulations. For instance, Eqs. 4a and 4b allow for understanding the range of validity of the rate equations reported in Table 2.

(4) p. 4453, l. 9: “...for wind drifting”

The revised manuscript has been corrected accordingly.

(5) p. 4453, Eq. 5: I cannot see how these rate equations will reduce to the ones in Table 3 for conditions of “no drifting”. Please explain this better and also define how τ depends on wind speed.

In the Crocus model, the parameter “ τ ”, which represents the time characteristic for snow grains change under wind transport, is given by the following expression (Eq. 12 of Vionnet et al., 2012)

$$\tau = \frac{\tau_0}{\Gamma_{drift}}$$

where τ_0 is an empirical constant set to 48 h and Γ_{drift} is defined as:

$$\Gamma_{drift} = \max \left[0, S_I \exp \left(-\frac{z}{0.1} \right) \right]$$

Therefore, through Γ_{drift} , compaction and fragmentation rates in a snow layer depend on the grain driftability index S_I and are propagated to the layers below with an exponential decay until it reaches a non-transportable layer ($S_I \leq 0$). S_I is computed as a function of the wind speed U and the modality index M_O , the latter depending, in turn, on the microstructural properties of snow (Eq. 11 of Vionnet et al., 2012):

$$S_I = -2.868 \exp(-0.085U) + 1 + M_O$$

The parameter τ is then directly related to the wind speed U through Γ_{drift} and S_I . Since all these variables and equations, mostly based on the work of Brun et al. (1997) and Guyomarc'h and Merindol (1998), have already been presented and discussed in Vionnet et al., 2012, in our manuscript we do not present them in detail but we just refer to these previous studies.

Regarding Eqs. 5a and 5b, they do not reduce to the ones in Table 2 for conditions of no drifting. In fact, Eqs. 5a and 5b (accounting for the effect of drifting on snow microstructure) and equations in Table 2 (accounting for the effect of metamorphism on snow microstructure) all apply at each time step. In case of no drifting, Γ_{drift} is equal to 0, τ is equal to ∞ and Eqs. 5a and 5b lead to unchanged values for d_{opt} ($d_{opt}/dt=0$). The same was true for the equations formulated in terms of dendricity and snow grain size, reported in Table 1 and Table 3 of Vionnet et al., 2012.

For more clarity, in the revised manuscript we have added the following sentences:

"[...] τ is computed from the driftability index (which depends on the wind speed and the microstructural properties of snow, see Sect. 2.2.4) [...]. In case of no drifting, τ tends to infinity and Eqs. (5) lead to unchanged values for d_{opt} ."

(6) p. 4454, l. 14: You should check this and then write that you have verified this.

We agree with this observation. In Fig. 3, for instance, it is possible to see that formulations C13 and B92 give the same results under low temperature gradient conditions and differ only when $G > 15 \text{ K}\cdot\text{m}^{-1}$. We have added a reference to this figure at the end of Sect. 2.3.1.

(7) p. 4454 – 4456: The presentation of the T07 scheme is still confused in my opinion. I already criticized this in my review on Jacobi et al. (2010), see Lehning (2009). If you use Eqs. (5) or (9) directly, you run into the problem that you describe in the text namely that you need to know the average temperature of the past development. Apart from the fact that using an average temperature instead of the full time history will certainly lead to errors in case of strongly varying temperatures, the use of the actual temperature is certainly a major problem. On the other hand, if you use Eq. (10) and calculate only changes in SSA for each model time step, you should not have this problem because then the full time history is already contained in the current SSA value. Thus, please be clear about how you have implemented T07.

We thank the Reviewer for this comment. As pointed out at the end of Sect. 2.3.2, the implementation in a snowpack model of the parametric equations proposed by Taillandier et al. (2007) is particularly cumbersome. In particular, the temperature appearing in these equations is the average temperature experienced by a snow layer during the period of interest. Since we do not know at each time step the full temperature history of the numerical layers, we decided to use their actual temperature. However, as stated by Lehning (2009), it is true that "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 problem was already encountered by Jacobi et al. (2010), but our approach for implementing the formulation T07 into Crocus is different from that followed in that study and already criticized by Lehning (2009). Indeed, we formulated a unified rate equation (see Eq. 10) which describes SSA changes as a function of the temperature gradient and the temperature. Using Eqs. 6 and 9, this rate equation can be written:

$$SSA(t + \Delta t) = SSA(t) - \left(D_{TG} \frac{B_{TG}}{t + e^{\frac{C_{TG}}{B_{TG}}}} + D_{ET} \frac{B_{ET}}{t + e^{\frac{C_{ET}}{B_{ET}}}} \right) \Delta t + \frac{1}{2} \left(D_{TG} \frac{B_{TG}}{\left(t + e^{\frac{C_{TG}}{B_{TG}}} \right)^2} + D_{ET} \frac{B_{ET}}{\left(t + e^{\frac{C_{ET}}{B_{ET}}} \right)^2} \right) \Delta t^2$$

The first term of the equation, $SSA(t)$, is the current SSA value and integrates the full time history of the snow layer. The other terms represent changes in SSA for each model time step and depend on the actual temperature of the layer. Hence, in this approach it makes sense to use the actual temperature to compute the SSA decrease at each time step (ΔSSA), since the full temperature history is already contained in $SSA(t)$.

In order to be clearer about how we have implemented the formulation T07 into Crocus, we have added the following sentence to Sect. 2.3.2 of the revised manuscript:

"This approach makes sense, since the actual temperature is used to compute changes in SSA at each model time step, whereas the full temperature history of the layers is already contained in the current SSA value (SSA(t) in Eq. 10)."

(8) p. 4457, Eq. 12: This is a very strange notation for the initial rate of change. Please use something like $(dr/dt)_0$.

We agree. The text has been modified accordingly.

(9) p. 4458, l. 8: Discontinuity in the derivative is not physical. The great discontinuity as also visible in Fig. 3 shows how incomplete our current understanding of SSA development is. This should be emphasized much more in the paper.

We agree with this good suggestion. In the revised manuscript, the following sentence has been added to Sect. 2.3.4:

"B92 and C13, for instance, display a discontinuous derivative when snow enters the non-dendritic state. This discontinuity, which has not physical meaning and comes from the empirical parameterization of the rate equations, is not present in T07 and F06."

(10) p. 4461, l. 22 ff: Please explain briefly how the instruments differ from each other.

The Reviewer makes a good suggestion. The following sentence has been added to Sect. 3.3 of the revised manuscript, as reported below:

"Its working principle is similar to that of DUFISSS (retrieval of SSA from infrared reflectance measurements), the main difference being that it allows the continuous acquisition of SSA profiles down to about 90 cm, with a vertical resolution of 1 cm."

(11) p. 4462, l. 5 ff: But this introduces a systematic error in the calculation of the absorbed radiation, since albedo is higher for low elevations, and you should at least say, how big this error is.

The Reviewer raises a very important question. We are fully aware of this problem, which cannot be detailed here and has been the subject of a separate publication (please see: Libois, Q., Picard, G., France, J., Arnaud, L., Dumont, M., Carmagnola, C. M., and King, M.: *Influence of grain shape on light penetration in snow*, The Cryosphere, 7, 1803–1818, doi:10.5194/tc-7-1803-2013, 2013).

In the current version of Crocus model, the albedo is calculated from the snow properties of the two upper numerical layers by splitting the solar radiation in three separate spectral bands. In the UV and visible range, albedo depends on the optical diameter and on the amount of light absorbing impurities, the latter being parameterized from the age of snow. In the infrared bands, albedo depends only on the optical diameter of snow (Vionnet et al., 2012). Hence, the solar zenith angle is not taken into account in this representation.

In order to overcome this problem and to improve the original radiative scheme of Crocus, a simplified radiative transfer model named TARTES (Two Stream Radiative Transfer in Snow, Libois et al., 2013) has been developed. This model allows for determining the vertical solar absorption profile and the spectral albedo of the snowpack. The snow physical properties prescribed as input to TARTES are the thickness, the density, the SSA, the grain type (the model explicitly takes into account the shape of the grains) and the impurity content of each layer. The solar spectral flux at the surface must also be prescribed in terms of solar zenith angle and intensity. TARTES has been recently implemented into Crocus and tests are underway to compare this new scheme with the old radiative representation. In particular, it will be possible to determine the impact of the solar zenith angle on the broadband albedo simulated by Crocus, which is now hard to estimate.

(12) p. 4463: These are standard metrics and the description can be shortened.

The Reviewer is right, both Δ_{SSA} and Δ_{dopt} are standard metrics. Their description has then been slightly shortened in the revised manuscript.

(13) p. 4464, l. 11: "along with a slight density...."

This error has been corrected in the revised manuscript.

(14) p. 4465: The problems here may have to do with "event-driven deposition" (Groot Zwaaftink et al., 2013), which is a common problem in polar regions.

This is an excellent remark. This possible explanation for the discrepancies (especially in terms of snow height) between our simulations and observations at Summit has been added to the revised manuscript. The suggested reference has also been indicated:

"This can be mostly explained by the spatial variability of the snowpack due to the effect of wind, and in particular to the event-driven deposition of snow, which is frequent in polar regions (Groot Zwaafink et al., 2013)."

(15) p. 4465: You should already mention here that the differences between model runs is significantly smaller than between any model and the observation.

We appreciate this suggestion. The following comment has been added to Sect. 4.2.1 of the revised manuscript as requested:

"In addition, it can be noticed that the differences between different simulations are significantly smaller than those between simulations and observations."

(16) p. 4466, l. 17 ff: Again you could discuss "event-driven deposition" in this context.

We agree. Please see our previous reply to comment #14.

(17) p. 4466, l. 22: Simulated SSA IS underestimating (it not only appears that)

The Reviewer is correct. The text has been modified accordingly:

"Moreover, it is clear that the simulated SSA values underestimate the observations."

(18) p. 4466, l. 27: The new model does NOT overcome this problem, it only slightly reduces the error. The discussion should clearly state that there is a quite limited understanding of the process given the model - measurement comparison.

The Reviewer makes a good point. The paragraph had been rephrased as follows:

"The new metamorphism formulations using the optical diameter as a prognostic variable allows for reducing the discrepancies between simulations and observations. [...] Even in this case, however, the observations are not perfectly reproduced, meaning that our understanding of the processes involved in the SSA decrease over time is not complete."

(19) p. 4467, l. 11: GR: In Fig. S3 the vertical profiles are presented...

The text has been corrected accordingly.

(20) p. 4468, l. 7: Please don't overstate your results. I don't think that the overall features are well captured. At least be specific and say which features you think are well captured.

We thank the Reviewer for pointing out this inconsistency. In fact, in this case, the measured SSA range of variations and the vertical layering are not well reproduced by simulations (see Fig. S5b). The text has been modified accordingly:

"For the SSA, the range of variations and the vertical layering are not well reproduced."

(21) p. 4470: Please mention that SNOWPACK has bond size as an additional parameter, which strongly influences both thermal and mechanical snow properties in the model.

We thank the Reviewer for this relevant input. This information has been added to the revised manuscript:

" [...] in addition, SNOWPACK also incorporates a representation of bonds between grains."

(22) p. 4471: As already pointed out in the "general" section above, it is very important to also compare other model results. Density alone is too insensitive here. Therefore grain types or also water content (at CDP) should be compared.

We appreciate the Reviewer's point of view and we agree that the success of the new formulation of metamorphism in terms of optical diameter can be judged more adequately by additionally presenting validation results for more conventional grain types and liquid water content. For this reason, we have added a new figure to

the supplementary material of the revised manuscript. These new results are discussed in Sect 4.2.2 of the revised manuscript, as reported below:

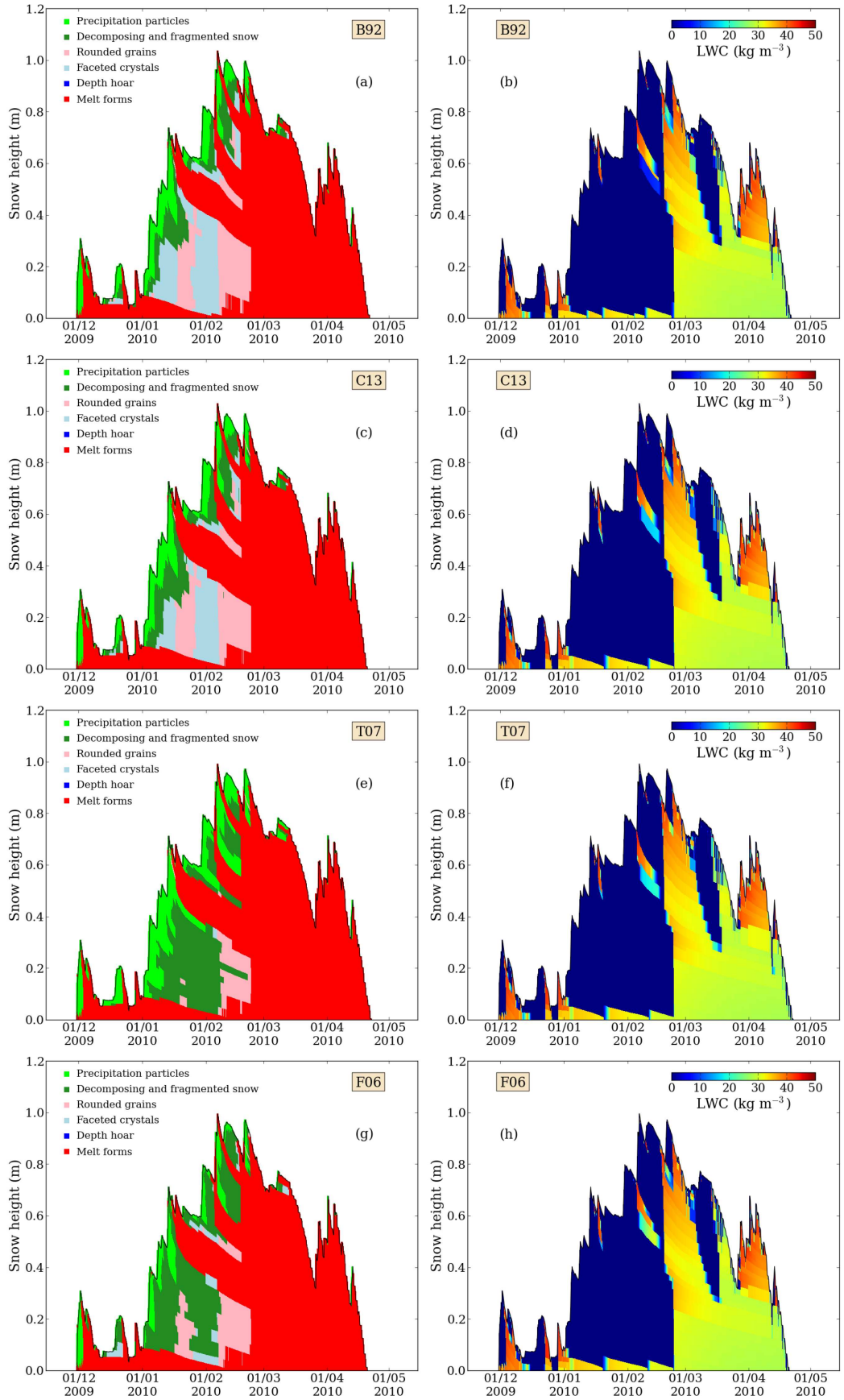


Fig. 6: Simulated snow types (left column) and liquid water content (right column) at Col de Porte during winter 2009/2010. Simulations were run using different metamorphism formulations.

"Figure S6 shows that all formulations perform similarly in terms of snow types and liquid water content. The only significant differences appear when the temperature gradient is between 5 and 15 K m⁻¹. In this case, B92 and C13 make the SSA decrease faster than T07 and F06 in a time period ranging from about 10 and 60 days since snowfall (see Fig. 3b). This is the main reason why B92 and C13 simulate the presence of faceted crystals between January and February, whereas T07 and F06 indicate, for the same period, the presence of decomposing and fragmented snow. Since visual observations between January and February revealed the coexistence, within deep layers, of both faceted and decomposing crystals, it is not easy to determine which formulation matches the observed snow profiles better in terms of snow types. In fact, since the very notion of grain types represents a discontinuous evolution of a continuous process, it is inevitable that thresholds between types vary slightly between formulations, and even different observers would place the limit between types differently. Other comparisons performed at Summit and at Col de Porte during 2011/2012, however, showed that F06 seems to reproduce more accurately the observed snow types. The differences during the melt period, as well as those in liquid water content, are negligible, since the laws of metamorphism are the same for all formulations in case of wet snow."

In the "general" section above, the Reviewer also suggests to use the method described in Lehning et al. (2001) in order to improve the comparison between model results and field measurements. In fact, a quantitative comparison of observed and simulated snowpack profile data is challenging, especially because of the unavoidable mismatch in heights of snow layers. This long-standing problem limits the number of quantitative assessments of model performance regarding internal snowpack properties (Morin et al., 2013). In this context, the mapping algorithm described in Lehning et al. (2001) is very useful, since it allows for pairing simulated and observed layering and for evaluating the goodness of fit between measured and simulated snow properties along a vertical profile. That being said, we believe that the application of this method to our study will not change our main results. For instance, by adjusting the total depth of the simulated snowpack to the observed snow profiles (the first step of the procedure described by Lehning et al., 2001), we found only a slight improvement in the agreement between measured and simulated SSA profiles in terms of RMSD for individual profiles. In other words, we think that applying the method of Lehning et al. (2001) will not have a significant impact on our findings, since even without using this method we found that all formulations of metamorphism lead to similar results in terms of simulated SSA, with a statistical agreement between measured and simulated SSA profiles that is rather satisfactory and comparable to previous studies.

(23) p. 4472, l. 22: I think Meirold and Lehning (2004) have first pointed out (and modelled) the influence of grain shape on light transmission. Please add this reference and you may even already mention it in the introduction.

Indeed, the effect of grain shape on light transmission was already studied by Meirold and Lehning (2004). This relevant work has then been mentioned both in the introduction and in the discussion of our revised manuscript.

(24) p. 4472, l. 28ff: I don't think that explicitly considering the ratio of vertical to the horizontal component of the thermal conductivity makes much sense in a one-dimensional model. The horizontal component would just be another (arbitrary) parameter since it has no physical meaning in a model that allows only vertical transport.

We agree with the Reviewer if the purpose is to consider heat fluxes. However, in this context, we are seeking a description of snow microstructural properties or adequate proxies. The anisotropy of the thermal conductivity (A_k), i.e. the ratio of the vertical to the horizontal component of the thermal conductivity, seems to be strongly correlated to the degree of snow grain faceting (Calonne et al., 2011). Therefore, it would be possible, in principle, to replace one of the old microstructural variables of Crocus (the sphericity) by A_k . Even if we appreciate the Reviewer's point of view, we think that the implementation of this new variable within Crocus may constitute a step forward towards a more physical description of snow microstructure. Rather, the main issue with the anisotropy of the thermal conductivity is that this variable does not characterize the grain shape directly (as the sphericity does), since it is also dependent on the presence of bonds and interconnections between grains. In addition, the link between A_k and other relevant properties related to grain shapes, such as the transmission of light through the snowpack, is still unclear.

Despite all these limitations, we feel that it is important to point out the fact that the anisotropy of the thermal conductivity is one of the possible candidates (even if not the most promising) to replace the sphericity in the future versions of Crocus, in much the same way as the SSA has already replaced the dendricity and the snow grain size.

(25) Fig. 4: In b) the temperature gradient is wrongly reported. Also, since F06 captures the data best, it should be discussed, why this cannot be achieved with the C13 model.

We thank the Reviewer for this suggestion. The end of Sect. 2.3.4 has been modified as follows in the revised manuscript:

“Results reveal that, albeit with some differences, all formulations perform similarly. The formulation F06 reproduces the experimental data slightly better than the other formulations, whereas B92 and C13 generally perform not quite as well, mostly because of their change of regime between dendritic and non-dendritic snow, which does not match the observed behaviour of SSA decrease. Analogous results are expected to be found when these different representations of the SSA rate of change are implemented directly into Crocus.”