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Interactive comment on “Implementation and evaluation of prognostic representations of the optical diameter of snow in the detailed snowpack model SURFEX/ISBA-Crocus” by C. M. Carmagnola et al.

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General:

The paper presents a worthwhile development of the snow model SURFEX/ISBA-Crocus 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 publi-

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cation 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.

Detailed Comments:

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

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snow. The authors should therefore rephrase their statement in less absolute terms.

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).

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

p. 4453, l. 9: "...for wind drifting"

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.

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

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.

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

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

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development is. This should be emphasized much more in the paper.

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

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.

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

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

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

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

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

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

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.

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

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.

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.

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

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grain types or also water content (at CDP) should be compared.

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.

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.

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.

References:

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