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The Cryosphere
Discussions

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.

M. Lehning (Referee)

lehning@slf.ch

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General: The paper takes a recent version of the French snow cover model CROCUS and introduces two published parameterizations to calculate the specific surface area of snow based on the CROCUS parameters density and grain type (which is then called a diagnostic SSA calculation) and temperature and snow age (which is then called a prognostic SSA calculation). 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. 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 quality 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.

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.

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.

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.

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).

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.

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.

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.

Minor comments:

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

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.

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

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

Interactive comment on The Cryosphere Discuss., 3, 681, 2009.