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Geoscientific Model Development Discussions

Interactive comment on "A Factorial Snowpack Model (FSM 1.0)" *by* R. Essery

Anonymous Referee #2

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This paper presents a new model for computing the coupled energy and mass balances of the snowpack. This reviewer greatly appreciated the methodology used for presenting the model herein. It is a very clear and step by step approach. The only criticism I have is that the paper seems to need more details about some of the choices made in terms of parameterizations and default parameter values. As-is, I feel the paper lacks just a bit more information. What I am asking is essentially for a bit more documentation and explanation of some parameter and parameterization choices, thus the author should be able to address the issues raised here fairly quickly.

1 This is perhaps minor, but more for information : Can the author justify the choice of using a liquid water prognostic variable ? Indeed, some multi-layer schemes use a heat content or enthalpy type scheme wherein the liquid water content is implicit and can be diagnosed at each time step. Does carrying an explicit water content prognostic variable simplify the code, or make it more efficient, or is there some other reason for

this choice?

2 The notion of snow fraction is introduced, but this seems to cause several issues to arise. Indeed, the effect of including protruding grass, debris, etc.. on the albedo can be quite significant. But how does the ground albedo impact the surface energy budget of the snow ? The assumption is that protruding elements have the same albedo as the ground I assume ? This seems a bit inconsistent with the vertical nature of this model : it seems that this model has been formulated to only consider the snowpack. Also, the snow roughness is also modified in a similar manner. This is perhaps more easy to justify since the snowpack is likely to Âń feel Âż the effect of other surface roughness elements within the same grid cell : is this the case ? If so, a word or 2 about this is merited. Finally, if the snow is truly fractional, doesn't this imply that the underlying soil can heat up thus melt must also be included in the lowest snow layer ? In which case melt would have to appear in Eq.s 3, 4 and 7 for layer 3 ?

3 Can the author say more about the snow fraction formulation (Eq. 13) ? How was this particular formulation selected ? This is important since so many land surface models use rather diverse highly empirical formulations. This leads to another question : the model is almost certainly highly sensitive to this parameterization. For example, if one changes h_f over some range, results are likely to change considerably, perhaps on par with the other parameterizations highlighted through sensitivity studies herein. Finally, why was h_f corresponding to 76 % of the ground selected as the default value ? And this formulation corresponds to a certain spatial scale (a field/parcel) or a larger area (or perhaps it is intended to be scale independent) ?

4 Several of the key parameterizations are represented in Fig.3 over their respective ranges. This is a very informative figure. However, this related to the main comment raised at the beginning of this review : how were the values for albedo, snow density, etc.. determined or set ? For example, the albedo equations are presented in Eq.s 10-11, but where the values or equations came from or were inspired by are mentioned. Perhaps they are totally original ? If so, can the author give some justification for the

choice of the formulations ? Are the parameters for snow density, albedo, etc. meant to capture the first order response of more complex models while remaining as simple as possible ? This could be beyond the scope of the current study, but it seems the reader could be left asking : How does the model compare to a simple or complex scheme ? In this case, curves using default parameter values from SNTHERM, CROCUS etc...would be quite informative by comparison.

5 Can the author give some more details about the numerics of ground-snow interface flux ? Is the snow-soil flux semi-implicit or fully implicit (with the soil temperature solution) ? What happens as the snow becomes vanishingly thin ? These are important numerical aspects, and it would be nice to have them documented herein.

6 Last lines of section 2 : Âń Finally, the thicknesses of the layers are reset while conserving mass... Âż this is a bit vague. If the purpose of this paper is model documentation, then it would be nice to see the algorithm or equations used to perform this task (even in the form of an Appendix). It is a critical step and it is too bad to not see it expressed here. With this, all of the details would be presented making this a complete model description.

7 Just after Eq. 47 : if the temperature of the snowfall is not assumed to be the same as the snowpack (T_snow in layer 1), then a snowfall energy term must appear in Eq. 5 to maintain energy budget closure it seems.

8 In the references, a technical note by Boone is cited. It might be better to cite the corresponding peer-reviewed/published paper (Boone and Etchevers, 2001, J Hydrometeor.)

The last 3 comments are really just suggestions. They would require some extra work, but I feel could potentially add a lot to this paper.

9 It would be quite nice to see a statistical summary (in the form of a table for example) of the sensitivities shown in Fig.s 4 and 5. Then the reader could more readily have a

feel for which parameters are most critical and in combination with which parameters.

10 It would have been very instructive to see if the simulated envelop of variability induced by the different parameterizations is similar to say, the inter-model scatter seen in an exercise like SnowMIP for example (for the same site).

11 What about performances for a couple more contrasting sites ? For example, Col de Porte is a rather unique site since is rather warm and is subjected to very frequent melt events. How does this model perform for a colder mountain site ? A site in a grassy area ? In a forest (albeit, this latter item is perhaps beyond the scope of the current study: but then it seems the author should say if the model is adapted for forest sites or not)?

Interactive comment on Geosci. Model Dev. Discuss., 8, 6583, 2015.