Title: ESCIMO.spread (v2): Parameterization of a spreadsheet-based energy balance snow model for inside-canopy conditions

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This paper describes the extension of the existing “ESCIMO.spread point energy balance snow model” to an “ESCIMO.spread (v2)” model. In the revised model, the separation of liquid and solid precipitation has changed from a temperature based to a wet bulb temperature based approach, the model accounts for the storage of energy and liquid water in the snow pack including refreezing processes and a sub-model was included that allows to model inside canopy conditions with outside canopy measurements as an input. Despite these extensions the model can still be run with “standard” software and should be used for scientific and also educational purposes.

The paper is well written and understandable. I appreciate the simplicity of the model code and I see the benefit to have a flexible open source tool e.g. for student courses. Before publishing this manuscript and widely using the model I would like the authors to account for the following comments/issues.

General comments:

1. Calculating surface snow melt from the surface energy balance means that temperature does not directly control whether there is melt or not. However, in the general model description (p. 8160, lines 10-15) the authors state that air temperature has to be at the melting point temperature of ice. If the model should be an “energy balance snow model” this condition should be removed.

2. If I understood correct there is a problem in the model design as described in Section 2.3: From the description of the concept and equation 7 it seems to me that there could be cases where the cold content of the snow is “saturated”. If this is true, this would violate energy conservation in any energy balance model. More generally spoken, it seems that this conceptual parametrization cannot be implemented in an energy balance based model approach.

3. The implementation of available parametrizations is a good strategy to theoretically show the various impacts of trees on snow cover. I understand that the available measurements (one spot in the presumably very heterogenic canopy) do not allow evaluating the benefit of considering each of the processes individually. However, for the general model evaluation presented in the paper I have the following suggestions: All evaluation of inside canopy model results is based on quality criterions calculated between measurements and individual model results (like global radiation). In my opinion it would be necessary to calculate the increase in model skill when model results/measurements are adapted/not adapted for inside canopy conditions. For example, calculate the increase in skill when modeled inside canopy global radiation is used instead of outside canopy global radiation (similar for temperature, humidity, SWE, wind speed). This strategy would also avoid that high model skills (e.g. high R^2)

can (partly) be a result of pronounced daily cycles in both measured and modeled variables (e.g. true for global radiation, temperature etc.).

Specific comments:

1. p. 8156, line 4: “a concept for cold and liquid water storage consideration” should be replaced by “a concept for cold content and liquid water storage consideration”

2. p. 8156, line 14: “The validation results indicate a good overall model performance in and outside the forest canopy.” “good” could come along with objective quality criterions like RMSE (e.g. “The validation yields good/fair/… results with RMSE of ± xy RMSE [mm WE] / ± xy RMSE [mm WE] for outside / inside canopy conditions. Maybe the authors are also willing to consider this approach in Section 5.

3. In the introduction (p. 8158, lines 5-10) the authors state that the model only requires few input data. Hourly input data of temperature, wind speed, relative humidity, global and longwave radiation are quite expensive in my point of view as this requires a nearby automatic weather station (always limited to one point) or demanding downscaling approaches when using atmospheric model data.

4. In the introduction (p. 8158, lines 10-15) the authors state that the model “is even capable of simulating the evolution of a seasonal snow cover under climate change conditions” because temperature and/or precipitation trends can be applied. In my opinion this statement is very optimistic given the fact that (1) e.g. changes in precipitation very likely will also impact air humidity, radiation, temperature etc. and (2) the parametrizations for inside canopy conditions require many empirical parameters. Probably it would be more reliable to write something like “the model is able to calculate simple sensitivity tests for changed temperature/precipitation”.

5. P. 8159, line 7: “calculation of the beneath-canopy snow energy and mass balance”. If I understood correct (e.g. general comment 2), the model does not always calculate the snow energy balance as the energy balance is not closed in all cases.

6. p. 8159, lines 5-10 “the new version ESCIMO.spread (v2) reaches beyond the capabilities of most other freely available point-scale snow models”: I’m not sure if this is true as there are meanwhile very sophisticated energy balance models freely available (e.g. http://regine.github.io/meltmodel/). In my opinion the strength of ESCIMO.spread (v2) is that it is very simple/low cost to use and it has extensions to consider inside canopy effects.

7. In 2.2 wet bulb temperature is used to separate solid from liquid precipitation which is definitely a reliable approach. Nevertheless, it would be interesting to see the relative differences (%) in calculated snow fall amount for one winter when applying the dry bulb instead of the wet bulb temperature. Thereby it seems important that the relative humidity measurements are bias corrected (nearly 100% RH should be reachable in case of very wet conditions). Furthermore, it could be an idea to interpolate from 100% solid to 100% liquid precipitation for a given range of wet bulb temperature (e.g. 0±0.5 degree) to avoid jumps in the calculated snow fall amounts.

8. In Section 2.3 (besides issue 2 in the general comments) it was challenging for me to think of a cold content expressed in units [- mm w.e.]. From my point of view a cold content in an educational tool would be better related to negative temperatures

[degree C] of the snow pack (or certain layers) that can – together with the snow mass – be converted into energy content [J]. In a second step this would allow to calculate the energy [J] that is required to heat the snow pack up to the melting point temperature.

9. Section 4 and Fig. 8: It looks like there is an obvious bias in the RH measurements as the values never reach (nearly) 100%. Please also do a bias check for the outside canopy RH measurements.

10. In the results section the authors rarely comment on Fig. 10 (especially on the second pronounced snow pack in February 2013) which is essential as it shows the model capacity to reproduce outside canopy snow pack without any complications induced by the forest. If the model skill is higher inside the forest than outside (table 2) this could suggest that it is easier to model inside canopy conditions. However, I don't think that this is true but a result of (1) multiple error compensation effects (including errors in precipitation derived from a distant gauge) and (2) at least partly coincidence as there seems to be only 1 point measurement available in the canopy which does not represent the expectable strong spatial heterogeneity. The latter aspect is a serious issue for all the inside canopy evaluation and for future studies it would be desirable to do some small scale (e.g. within a couple of meters) cross section measurements of (at least) SWE inside the canopy.

11. p. 8172, lines 1-5: The lower R^2 for RH and wind speed might be a result of the weaker or missing daily cycles (see also general comment 3). Please also think again about potential offsets in the RH measurements (Fig. 8).

12. p. 8174, line 23: I think "trend" should be replaced by "patterns".

13. Fig. 2 and 3 could be moved to an appendix to better focus on the results (Fig. 4 and Figs. 6-11).

14. Fig. 2: "rain" and "snow" are a bit confusing in this plot. Maybe the authors could add a sentence from Section 2.2 ("Each of the displayed lines in Fig. 2 could be interpreted as a borderline to separate liquid and solid precipitation assuming a certain threshold wet-bulb temperature") in the legend instead. Please also consider again if it would make sense to implement a temperature range to gradually shift from 100% solid to 100% liquid precipitation.

15. Fig. 6: Maybe a scatter plot of the daytime values could better show the model skill. Currently, it is very hard to distinguish between the lines. Another idea could be to compare mean daily values.