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Geoscientific
Model Development
Discussions

Interactive comment on "ESCIMO.spread – a spreadsheet-based point snow surface energy balance model to calculate hourly snow water equivalent and melt rates for historical and changing climate conditions" *by* U. Strasser and T. Marke

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********** Review: Concerning the convenience of the application in the field, I share the opinion of referee #1.

Comment: The utility of ESCIMO.spread in the field has been explained in more detail in the updated manuscript.

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********** Review: In contrast, the applicability of the tool to problems on global change depends more on the availability of an appropriate set of input data than on the model itself. The demonstrated example, based on a nontransparent and limited modification of temperature and precipitation input, equals more a sensitivity analysis of the model than an investigation of the evolution of the snow cover under a warmer climate. Hence, I recommend altering the caption of chapter 5 to something like 'Sensitivity analysis of model results to modification of input data'.

Comment: Thank you very much for this suggestion. The transparency of the climate change parameters in ESCIMO.spread has been improved by adding more detailed descriptions to the manuscript. Of course, the parameters used to define climatic changes (changes in temperature and precipitation) are as well suitable for carrying out a comprehensive sensitivity analysis, we have added this information to the manuscript. As the trends in temperature and precipitation used in the current study are not chosen arbitrarily, but represent best knowledge of currently expected changes in the region considered (e.g. IPCC, 2007), the authors would like to keep the caption 'Application in climate change conditions'.

********** Review: Overall the paper is generally understandable and the presented documentations of examples are comprehensible. The description of the model theory in respect to energy balance is profound, but the description of the accumulation processes and the mass balance are superficial and need additional information.

Comment: Thank you very much, we have added additional information on the calculation of the mass balance to the manuscript.

********** Review: The demonstrated errors in the input data series shows, that the model is obviously insensitive to such errors, and this should be commented in chapter 3.

Comment: Thanks, the manuscript has been updated accordingly.

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********** Review: If observational data of the SWE is available, the performance of the model runs can be rated or validated using three statistical criteria (efficiency criteria), which are more or less familiar. But although the authors show all calculation formulas, they fail to point out the difference and significance of the offered methods. In particular there are no references and explanations about the index of agreement. The mentioned section of the text should be expanded.

Comment: Thank you very much. To more clearly relate each efficiency criterion to a single reference and to provide additional information on the criteria applied, the respective section in the manuscript has been edited.

********** Review: Sophisticated expansions of the model require powerful programming techniques. The options of a spreadsheet are limited and cumbersome.

Comment: The authors share the opinion of reviewer #2 and do not consider ESCIMO.spread to be an ideal platform for the implementation of increasingly complex computing algorithms. The authors would like to emphasize the models utility when applied in the framework of student courses or directly in the field. Both of these model applications strongly limit model complexity.

********** Review: P628, line 3: better 'At the snow surface' instead of 'of the snow surface'.

Comment: Thank you very much, we have modified the manuscript accordingly.

********** Review: P 628, line 5: 'The model makes use of . . . incoming short and long wave radiation', because outgoing components will be calculated by the model

Comment: Thanks again, we have modified the manuscript accordingly.

********** Review: P630, line 14: In a strict sense Equation (1) is formulated for the snow surface (following the idea, that a surface is incapable to

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store energy. . .)

Comment: Thanks, you are totally right here. All calculations are carried out for the snow surface not considering the storage of energy within the snow pack.

********** Review: P630, line 19: A definition of the sign of the individual terms of equation (1) is missing: Here is a suggestion: energy flux densities directed to the surface are counted as positive and are expressed. . .

Comment: Thanks, we have added your suggestion to our manuscript.

********** Review: P630, line25: As mentioned in table 1 k is a constant. It depends not really on the value of air temperature, but it can be assigned to two different values k1 and k2, classified by positive or negative air temperature

Comment: Thank you very much, the manuscript has been modified accordingly.

********** Review: P631, line 5-9: The emitted long wave radiation is the sole exception of an explicit description of a component of the radiation balance. It would be helpful to declare the equation for the complete radiation balance to differentiate between the measured and calculated components.

Comment: Thank you very much for this suggestion, the manuscript has been modified accordingly.

********** Review: P632, line 14: From the start at the end of the section which explains the model, nothing is mentioned about the component B of equation (1). In contrast, a constant value of 2.0 Wm-2 is given for B in Table 2. This should be explained.

Comment: Thanks, the manuscript has been modified accordingly.

********** Review: P634, line 22: 'Are based on physical laws': Which ones?

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Comment: Thank you, we have modified the manuscript accordingly.

********** Review: P636, eq. 10 and Line 18: wrong calculation of R2?

Comment: Thank you for pointing out this flaw, we have corrected the calculation of the coefficient of determination in the manuscript.

********** Review: Table 1: Recession factor -> better use recession constant k1.

Comment: Thanks for your comment, but as k is used to multiply n (n = the number of days since the last considerable snowfall) the notation 'factor' seems to be adequate.

********** Review: Table 1: The Function of the parameter 'Threshold temperature for phase detection' is explained nowhere in the text!

Comment: Thanks, the respective section in the manuscript and also Table 1 has been modified accordingly.

********** Review: Table 1: As published in literature, the emissivity of a snow surface range from 0.96 to 0.99, depending on various properties, as like for instance surface roughness. Although the typical value may be above 0.98, a value of 1.0 is definitely too large. In this case snow is considered as an ideal black body emitter.

Comment: Thank you very much. The emissivity of the snow surface can easily be adjusted in ESCIMO.spread, we have corrected the value in the manuscript.

********** Review: What means the specific heat of snow? The given value equates to that of ice at 273.15K.

Comment:

Thanks, you are totally right. As snow consists of ice and air, we have used the specific

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heat of ice giving it the notation 'specific heat of snow' to avoid confusion.

********** Review: Check values for specific heat of water and melting heat of ice.

Comment: Thanks again, the respective values have been corrected in the manuscript.

********** Thank you very much for all your valuable comments and suggestions for further improving our manuscript! Your endeavours are highly appreciated!

Interactive comment on Geosci. Model Dev. Discuss., 3, 627, 2010.

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