

## ***Interactive comment on “Modeling the impact of wintertime rain events on the thermal regime of permafrost” by S. Westermann et al.***

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Received and published: 29 September 2011

Thank you very much for the helpful comments and detailed suggestions on our manuscript. We hope that we have been able to clarify the criticised points in the revised version of the manuscript. In the following we provide a detailed response to all issues raised in the review. All changes to our manuscript are given in italics, and while the review appears in bold font.

**The manuscript provides modeling results of the effects of rain on snow on the thermal regime of a permafrost soil in a maritime arctic climate. As far as the overall structure, the manuscript is well written and allows the reader to understand the research questions behind the problem. Furthermore, the**

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**results are deeply commented and discussed.**

**As far as the modeling scheme is concerned, two major comments are reported: 1. the modeling scheme is not accurately documented, which makes it difficult to be reproduced. Further details on the equations should be provided in the Appendix;**

In the revised version, we have extended the information on the employed modeling scheme given in the Appendix, and we state explicitly, which equations are necessary to fully reproduce the scheme.

**2. the paper does not cite the latest “cold-hydrology” models present in literature, e.g. SHAW (Flerchinger, 1991), COUP (Stahli et al., 1996) and GEOTop (Rigon et al., 2006; Endrizzi et al., 2011) that study the effects of coupled heat and water transfer in permafrost.**

In the revised version, we have acknowledged these studies in a new introductory paragraph to “3. Model setup”, which puts our model in perspective to this work:

*The employed model is a thermal snow and soil model supplemented by a “cold hydrology” scheme for percolation of rain water in snow. Unlike sophisticated snow schemes, such as SNOWPACK (Bartelt and Lehning, 2002; Lehning et al., 2002b,a), or fully coupled heat- and mass transfer models (Dall’Amico et al., 2011), such as COUP (Stähli et al., 1996; Gustafsson et al., 2004) or GEOTop (Zanotti et al., 2004; Rigon et al., 2006; Endrizzi et al., 2011), it does not include a comprehensive description of all natural processes. Instead, we only account for the processes that are most relevant for the formation of the thermal regime of the soil. As an example, water movement within the soil, which Weismüller et al. (2011) show to be of secondary importance for the thermal regime at the study site, is not included.*

**The paper may be published if the authors provide a more exhaustive explanation of the modeling equations and details (see Section “Comments on the**

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equations”). Below I have listed a number of other comments that should be addressed prior to publication.

#### Comments on the text

- pg. 1702 line 6:  $K(z, T)$  and  $c_{eff}$  : add measurement unit;  
done

- pg. 1702 line 12:  $\theta_\alpha$  and  $c_\alpha$ : add measurement unit;  
done

- pg. 1702 Eq. (1): provide reference;  
done

- pg. 1702 line 6: “effective heat capacity”: provide reference;  
done

- pg. 1703 lines 1-4: it is not clear if  $\theta_w^{max} = \theta_s$  or  $\theta_w^{max} < \theta_s$  where  $\theta_s$  is the soil porosity. Furthermore, I would like to know whether in the simulation the soil is considered always saturated or unsaturated;

The volumetric fractions of all soil constituents are given in Table 2, we have now added the volumetric air fraction, so that it is clear that  $\theta_w^{max} + \theta_o + \theta_m + \theta_a = 1$ . The porosity is hence  $\theta_s = 1 - \theta_o - \theta_m = 0.4 > \theta_w^{max}$ , corresponding to unsaturated soil. In the simulation, the water content of the soil remains constant in time and space (see pg. 1702, line 14-15, old manuscript).

- pg. 1704 line 5:  $\theta_o = 0.1$ : in Table 2 one realizes that  $\theta_o = 0.0$ : please explain;

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We are sorry for the poor choice of the symbols, which has led to confusion: p. 1704, l.5 is “theta zero” (see pg. 1702, line 13-14, old manuscript), while in Table 2 it is “theta small o”, as “o” for organic. In the revised version, we employ a different subscript (“theta lrc”) in place of “theta zero”.

- pg. 1704 line 6:  $\epsilon_s$ : add measurement unit;  
done

- pg. 1705 Eq. (10): provide reference;  
done

- pg. 1705 lines 11 and 12: “... so that a snow density of  $350 \text{ kg m}^{-3}$  corresponds to a volumetric ice content of  $\theta_i = 0.35$ ”. (...) Please explain.

We mean the density of dry snow. In the revised version, we have replaced “snow density” by “density of dry snow”.

- pg. 1705 lines 17-21: I imagine you mean that, according to the measurements of heat diffusivities,  $K_{fresh} = 0.3 \text{ Wm}^{-1} \text{ K}^{-1}$  and  $K_{old} = 0.55 \text{ Wm}^{-1} \text{ K}^{-1}$ . However, in Table 3 one reads:  $K_{fresh} = 0.2 \text{ Wm}^{-1} \text{ K}^{-1}$  and  $K_{old} = 0.7 \text{ Wm}^{-1} \text{ K}^{-1}$  for the snow.

The measured values between  $0.3 \text{ Wm}^{-1} \text{ K}^{-1}$  and  $0.55 \text{ Wm}^{-1} \text{ K}^{-1}$  were obtained by fitting a conductive heat transfer model to measured snow temperatures for a snow domain comprising the lowermost 40cm of the snow pack (see Westermann et al. 2009 for details). This array produces the first results for a snow depth of 40cm, so that it is not possible to obtain the thermal conductivity of truly fresh snow. Westermann et al. (2009) note increasing thermal diffusivities over time, while the snow density is relatively constant both over time and within a snow profile (which is most likely

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explained by the strong wind drift and the resulting compaction of the snow). From this, we obtain the two values for the thermal conductivity. The first value of  $0.3\text{Wm}^{-1}\text{K}^{-1}$  was obtained in early December, while the last value of  $0.55\text{Wm}^{-1}\text{K}^{-1}$  stems from March. In the considered years (both in this paper and in Westermann et al. 2009), the first snow fell in September. When assuming the thermal conductivity to linearly increase over time, we have to select the values for  $K_{fresh}$  and  $K_{old}$  given in Table 2 to roughly obtain the measured thermal conductivities at the bottom of then snow pack in December and March.

The concerning text has been modified:

*Considering that the timing of the first snowfall in the study period has been similar to the year studied by Westermann et al. (2009), the linear conductivity scheme with confining values  $k_{fresh}$  and  $k_{old}$  roughly reproduces the measured values for December and March.*

**Furthermore, you say that the thermal diffusivities are measured at the bottom of the snow pack but in Table 3 the bottom ice layer is characterized by higher values. Please explain better the context.**

In the year 2008/2009, when the measurements were performed, a few rain-on-snow events occurred, but the amounts of rain were much smaller than in 2005-2007 (see Westermann et al. 2009 for details). Therefore, the lowermost 40cm of the snow pack were hardly affected by the formation of a basal ice layer, so that the 2008/2009 measurements represent measurements of the snow. For the much thicker basal ice layers found in 2005-2007, we apply the literature value for the thermal conductivity of ice.

We have added a short explanation to the sentence:

*... at the bottom of the snow pack (which has not been strongly affected by rain events in the studied year)...*

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**- pg. 1705 line 23: add measurement unit for P ;**  
done

**- pg. 1706 lines 14-15: "... the thermal properties of the snow remain unchanged during and after an infiltration event". Please add measures or literature references that confirm this sentence.**

This statement describes the thermal properties of the snow as prescribed in the model. To avoid confusion, we have added " ... in the model" to the mentioned sentence. We are fully aware, that the thermal conductivity of the snow is in general not linear with time, but changes as a result of many different factors, most likely also due to rain events. However, this dependence is not in the focus of this study, and interactively calculating the thermal conductivity from internal model parameters would introduce a feedback, which makes it much harder to compare the conduction-only control run to the model run with infiltration of rain water. Therefore, we have chosen the second-simplest dependence of the thermal conductivity, a linear dependence on time constrained by measured values (see above). In fact, choosing a constant thermal conductivity (the simplest dependence) as in Westermann et al. (2009) still results in a fair agreement of GST in our simulations, with wintertime average values being about 0.1 to 0.2K colder. However, the modeled GST is then significantly too cold in the first two months after snowfall, so that we have chosen to present the simulations with linearly increasing thermal conductivity.

It is possible that sophisticated snow- and soil modeling schemes (e.g. SNOW PACK), where the snow thermal conductivity is a function of model variables, may have a superior performance for daily to weekly time periods compared to our model. However, they would produce similar results for annual averages or decadal simulations (as our model is already very close to the measurements), so that applying a more sophisticated scheme for long-term permafrost modeling (at greater computational costs) would not improve the overall performance. In contrary, not including rain water infiltration for a site with strong rain-on-snow event would result in distinctly different

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results for annual averages and decadal timescales, as Figs. 4-6 demonstrate. It is the purpose of our study to make this fact explicit, and in this context, the choice of the thermal conductivity is adequate.

**- pg. 1706 line 22: In order to distinguish between rain, slush and snow, I would plot also the air temperature, if present, that could help in decreasing the uncertainty;**

The main problem is to characterize the amounts of water and snow present in slush, as this makes up for most of the potential liquid precipitation. On days with precipitation classified as slush, measured air temperatures are generally around 0C, sometimes slightly above and sometimes below. In the preparation of the paper, we attempted to use the hourly record of the Bayelva station to check the correlation between air temperature and the phase of the precipitation (unheated rain gauge records something or not). However, for the critical temperature range around 0C, a clear correlation does not exist. There is both rain at air temperatures slightly below 0C and snowfall at air temperatures slightly above 0C.

Therefore, an analysis of the air temperature can only confirm clear cases of snow or rain, which are already correctly classified by either the analysis of the rain gauge record or the visual observations of Ny-Ålesund. For the critical slush class, the analysis of air temperatures does not provide additional information and does not reduce the uncertainty. Therefore, we do not plot air temperatures in the revised version of the manuscript.

**- pg. 1707 line 18:  $\sigma_s$  and  $\varepsilon_s$ : add measurement unit;**  
done

**- pg. 1707 line 28 and pg. 1798 line 1: the initial condition at 10 m is set to -3.9C equal to the temperature at 1.52 m and then you considered a temperature of 0C**

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**at 100 m depth.**

**please specify if from 1.52 to 10 m you considered a uniform profile equal to -3.9C;**

**please specify the reason for this assumption: the initial condition at depth (approximately below 4 m) take a lot of spin-up time to set to equilibrium with the forcing to the system, so any arbitrary assumption on the initial condition has to be fully detailed and justified.**

**please specify if from 10 m to 100 m which profile you considered (e.g. linear) and justify it.**

In the initial version, we did not assume a uniform profile between 1.52 and 10m, but a linear interpolation between -3.9C at 10m and the measured value from 1 July at 1.52m depth.

However, following the criticism raised in this and other reviews concerning the initialization, we have adopted a more standard initialization procedure in the revised version of the manuscript: we initialize the model below 1.52m depth to represent steady-state conditions for the years 2002 to 2005 by applying the measured 1.52m soil temperatures as upper boundary condition and driving the model for about 1000 years with this forcing. For the uppermost 1.52m, we use measured soil temperatures as before. For 2006/2007, we apply the same initialization, but drive the model with measured 1.52m temperatures from 1 July 2005 to 1 July 2006 to obtain the initial temperature distribution below 1.52m. As a result, the modeled GST changes by a maximum of 0.1K, while the seasonal averages remain unchanged. All statements remain valid, as GST is not strongly sensitive to slight modifications of soil temperatures below 1.52m depth if only a single year is considered. The text has been changed accordingly:

*To a depth of 1.52 m, the initial condition is inferred from soil temperature measurements at the Bayelva station (Tab. 1), between which the temperatures are linearly interpolated. Below 1.52 m, no temperature measurements are available, so that the temperature distribution can only be estimated. For the season 2005/2006, we use the*

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record of the lowermost temperature sensor at 1.52m (which has been continuously in frozen ground) from July 2002 to June 2005 to generate the steady-state temperature distribution for this forcing, which is employed as initial condition below 1.52m. This results in a temperature of -2.9C at 1.5m, -3.8C at 3m, -3.1C at 10m and -3.1C at 20m depth. Below, a stable gradient of  $0.024\text{Km}^{-1}$  (determined by the heat flux through the lower boundary and the conductivity of the bedrock, Tab. 2) forms, thus placing the base of the permafrost at 150m depth, which is in agreement with estimates of permafrost thickness in coastal areas of Svalbard (Humlum, 2005). For the season 2006/2007, the initial condition below 1.52m is obtained by forcing the 2002-2005 steady-state conditions with measured 1.52m-temperatures from July 2005 to June 2006.

- pg. 1716 lines 17-19: "... the freezing would ... ". The text is confused. What does it mean that the soil is first warmed by the latent heat and then cooled by heat conduction through the snow? At pg. 1711 line 11 you say that the heat conduction is impeded by the overlying snow layers. Please explain.

The process is the following: first, the soil is warmed towards 0 degrees C by the release of latent heat from the freezing of water. This energy stored in the soil as sensible heat has not been accounted for in the simple estimation of the time required for total refreezing, so that the times given must be seen as upper bounds. Subsequently, i.e. when all water has refrozen, the soil cools again by means of heat conduction through the snow and the energy is released. We have changed the corresponding text passage to provide clarity:

*In reality, the freezing would occur faster than this simple estimate suggests since the downward heat flux in the ground, which leads to a warming of the underlying soil, dissipates some energy in addition to the upward heat flux through the snow pack.*

- Table 1 pg. 1728: add column with measurement unit;

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column with units added. For consistency, we have done the same for Tabs. 2 and 3.

- Table 2 pg. 1729: you show the values for bedrock and soil: what porosity are you considering for soil and bedrock? In general you should specify the values used for  $\theta_w$ ,  $\theta_i$ ,  $\theta_a$ ,  $\theta_m$  and  $\theta_o$  to derive the values of  $c_{frozen}$ ,  $c_{thawed}$ ,  $K_{frozen}$  and  $K_{thawed}$  both for soil and bedrock.

Other than for the soil, we do not have any information on the thermal properties of the bedrock, neither on the thermal conductivity itself nor on the porosity and constituents. The chosen values are therefore just the simplest possible choice (zero water content, values constant in space and time, values in the range of thermal conductivities documented for bedrock), and this lack of knowledge can only be treated in terms of an uncertainty analysis. However, as our modeling mainly focuses on GST and on periods of one year, the choice of the thermal properties of the bedrock below 10m depth has no effect on the results of the simulations. In the hypothetical 10-year runs presented in Fig. 6, choosing a significantly higher conductivity of e.g.  $4.5\text{Wm}^{-1}\text{K}^{-1}$  below 10m depth slightly changes the obtained soil temperatures especially between 2 and 4m depth, but the qualitative picture remains unchanged and all statements given in the paper remain fully valid.

We have added a statement to Sect. 4.3:

*While the initialization below 1.52m depth (Sect. 3.3) and the assumptions on the thermal conductivity of the bedrock (Sect. 3.1) introduce some degree of uncertainty for the ten ten-year simulation, all conclusions remain fully valid for slightly perturbed input values.*

**Comments on the equations**

**In general, I would require a deeper explanation on the modeling hypothesis and assumptions, with clearer passages in order to ease the comprehension. This paper, indeed, is based on modeling and therefore must precisely explain the**

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details. Below some inherent questions:

(...)

The questions are:

**how can one derive Eq. (A1) and (A2) at pg. 1720 from the above Eq. (10);**

The choice of the infiltration rates (Eq. (A1) and (A2)) ensures the correct physical behavior of the system. When inserted in Eq. 10, one can see that the temperatures of a cell with non-zero liquid water content reach 0 C, while at the same time an amount of water corresponding to the required energy refreezes. As this is not directly obvious from the equations, we have illustrated the application of the scheme on page 1722 (old manuscript).

**\* where do  $\tau_1$  and  $\tau_2$  come from?**

The two time constants are introduced to obtain ODE's with the variable time for the liquid and total water content of each snow cell, which can be solved by the efficient MATLAB ODE solver. Their physical meaning is the timescale required to infiltrate in a single grid cell. As detailed in Sect. 5.1, choosing the two time constants very short compared to the typical timescale of heat transfer (which we do by the choice of 10 seconds) leads to the formation of a wetting front, as almost all new precipitation is absorbed by the lowermost cell, which has not reached the maximum liquid water content. If the time constants were chosen very long, all grid cells would absorb the same amount of water simultaneously. We illustrate the effect of the time constants  $\tau_1$  and  $\tau_2$  at the end of the Appendix, where we give the equations for the water content of a grid cell following infiltration. These contain terms as  $e^{t/\tau_1}$ , which show that the water content of a grid cell saturates exponentially to the maximum content for infiltration rates  $I$ , with time constant  $\tau_1$ .

**3. Eq. (A3) pg. 1720: what are the measurement units? ( $\text{mm s}^{-1}$ )?**

A3 contained an error. As we apply finite differences in space in this step, the infiltration

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rate for each cell must be multiplied with the grid spacing, so that the correct inequality is

$$\frac{\Delta P}{\Delta t} \leq \sum_{j=1}^N I_j^{\text{snow}} \Delta z_j, \quad (1)$$

The units for the first term ( $\text{m}^3\text{m}^{-2}\text{s}^{-1}$ ) and for  $I$  ( $\text{s}^{-1}$ ) are already given in the text before.

**What is the index “ $n$ ” in Eq. (A5) at pg. 1721?**

$n$  stands for the  $n$ -th grid cell. See line 18, p.1720, old manuscript. For clarity, we have added the range, over which  $n$  runs, and added that this is a recursive scheme (which so far had only be mentioned in the “Model setup”-section).

**4. I have seen just the energy balance (see Eq. (1) for the soil and Eq. (10) for the snow). What about the mass balance? What assumption and equation are you using? I think that should be thoroughly explained. See Dall’Amico et al. (2011) for an extended description of water and energy balance equations.**

The water balance of the model system is fully defined by four principles:

1. The water/mass of the soil is constant in time. Mass change/ change of the water/ice content only occurs within the snow pack.
2. The system gains dry snow with a defined density of  $350 \text{ kg m}^{-3}$ , when the measurements of the snow height at the Bayelva station indicate an increase in snow depth.
3. All liquid precipitation that is put in the system refreezes either at the bottom of the snow pack or internally, which is ensured by Eqs. A4 to A7 (old manuscript). This results in variable total volumetric water/ice contents throughout the snow pack.
4. When the measurements of the snow height at the Bayelva station indicate a decrease in snow depth, the system loses snow/ice/water as given by the total

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volumetric water/ice content of the removed grid cell. (The physical nature of the loss process, i.e. sublimation, evaporation, lateral runoff, is hereby irrelevant.)

This procedure ensures that the mass gained and the mass lost during one winter season are equivalent.

We have inserted an introductory sentence to Sect. 3.2:

*We derive both the build-up and ablation of the snow cover from snow depth measurements at the Bayelva station, which in conjunction with measured precipitation rates determine the mass balance of the snow pack.*

Literature:

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