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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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We are grateful for the constructive comments and suggestions on our manuscript. We are confident that, after having followed your advice, the manuscript has improved. In the following we will provide a detailed account on how we implemented your suggestions. Changes of our manuscript are given in italics, and your suggestions in bold font.

General comments:
The paper is about an important topic within the general discussion about climate change and its impact on permafrost in Arctic regions. I recommend this paper for publications with some revisions.
a) I appreciate the development of a new and simple model, such it is presented in this paper. However, such a new model should be tested by existing and more sophisticated model approaches. A wealth of such models is today freely available and applicable. Therefore, I would recommend to make similar simulations with already existing process models in cryospheric sciences, which are all include the effect of rain water percorlation in a snow pack such as Snowpack (Bartelt and Lehning 2002, Lehning et al. 2002 a, b), Geotop (Zanotti et al. 2004, Rignon et al. 2006), Coup (Gustavsson 2004, Stähli and Jansson 1998), Somars (Greuell and Konzelmann 1994), Sntherm (Jordan 1991) and many more. As this task, may overburden the current paper, I would recommend that such a comparison should be definitively done in a later paper.

The goal of this work is to demonstrate, that refreezing rain water can have a pronounced effect on the thermal regime of permafrost, and that satisfactory modeling of GST can only be achieved by accounting for the rainwater infiltration in snow. As such, we see the model presented in the manusrcipt as a tool to separate the thermal effects of heat conduction from the thermal effects of the refreezing rain water. To have full control over all aspects of the model and to avoid dealing with feedback effects, that are inherent in the fully coupled snow and soil schemes mentioned above, we have chosen to program a scheme ourselves and not to use an existing one. Furthermore, we have an almost complete set of soil and snow parameters available from measurements at the study site, so that it is feasible to prescribe many properties instead of having them generated by the model itself (e.g. the snow thermal conductivity).
At the moment, we do not intend to add the model presented here to the suite of existing models. However, we want to make a strong and distinct contribution to the ongoing discussion on the best operational permafrost modeling scheme, that could be applied over large regions (1,000s to 100,000s of grid cells) and thus has to be as efficient and simple as possible. Currently, even the most advanced of such schemes only take heat conduction with constant soil and snow parameters into

account which in most cases is sufficient to reproduce the thermal regime on a yearly to decadal basis. The main contribution of this work is to show that refreezing rain water does have a pronounced effect on a yearly and possibly also on a decadal basis, so that it should be included in operational permafrost modeling schemes at least in regions with wintertime rain events. We are currently working towards an operational permafrost model for the Atlantic permafrost domain, and the model presented in this study represents an important step towards that goal. However, we are targeting major modifications particularly of the numerical scheme to improve the runtime of the model. Once this work is completed, we intend to present a detailed documentation and performance evaluation, which will include a comparison with other snow and soil modeling schemes, as suggested by the reviewer.
In the revised version, we have acknowledged the existing models in a more thorough fashion by including an introductory paragraph under “3. Model setup”:
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.

b) It is not well explained in the paper how the model reacts, if once ice is generated after the first refreezing of the rain water on the ground surface. As the conditions changes remarkably having ice instead of snow on the ground, the question arises what a second or third rainfall event would cause for a change in the ground temperatures. As once ice is generated, any further

rainfall events will have a much less strong impact on the ground temperatures as the first one.

At the study site on Svalbard, we typically have less than 10 cm of ice overlain by more than half a meter of snow following a major rainfall event, as described in the paper. The thermal conductivity of the snow is considerably lower than that of the ice, which is similar to the conductivity of the frozen soil. Therefore,the impact of a further rainfall event of the same magnitude on the GST is roughly the same as that of the first event on the soil temperature at 10 cm depth, which is quite similar to the ground surface due to the high conductivity of the frozen soil. If many consecutive rain events would eventually lead to ice layers thicker than the snow cover, it would indeed strongly moderate the impact on the ground temperatures. However, such thick ice layers would require either unrealistically high amounts of rain (30cm of ice require roughly 300mm of rain) or strong melting, which does not occur during the polar night at the study site (see below).
We have added a remark to the Results-section:
This relatively conductive ice layer is still overlain by more than 0.5 m *of snow (Fig. 4), so that the heat conduction through the snow pack is not significantly enhanced after the rain-on-snow event.*

This effect is in addition enhanced when considering a slightly undulated topography resulting in a faster runoff and therefore reducing remarkably the effect of the warm rain water to the underlying ground temperatures.
We agree with the reviewer, that redistribution of the rain water within the snow pack due to topography could strongly change the impact of the rainfall. At the study site on Svalbard, the topography is very gentle, though, and runoff in streams and rivers never occurs during the winter season. The Bayelva station is located on top of a hill, which is more or less flat for an area of 50mx50m, so that a 1D-model should be satisfactory.

Specific comments:

1. p. 1699, line 3: I would suggest to use the expression process-based model instead of physical model

changed

2. p. 1699, line 10 and 11: There exist a lot of literature about this topic beside of Kane et al. 2001 -> see added literature below.

Additional references inserted

3. p. 1700, line 21: years instead of year

done

4. p. 1707, line 28: the authors set the temperature at 10 m constant to -3.9 C with the same average temperature at a measured depth of 1.52 m. This would imply that from 1.52 m to 10 m is no gradient at all and therefore no heat flux would occur. Are the authors sure that this assumption can be made?

In the old version, there was in fact a gradient, as the 10m-temperature in the initialization was the average 1.52m-temperature over three years, while the 1.52m-temperature in the initialization was the measured 1.52m-temperature on 1 July 2005. However, following the criticism of two of the reviewers, we have modified the initialization procedure for all model runs. We now initialize the model below 1.52m depth to steady-state conditions for the years 2002 to 2005 by applying the measured 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 1July 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 1.5m 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 (Table 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 record of the lowermost temperature sensor at 1.52*m *(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 1.52*m*. This results in a temperature of -2.9*C *at 1.5*m*, -3.8*C *at 3*m*, -3.1*C *at 10*m *and -3.1*C *at 20*m *depth. Below, a stable gradient of 0.024*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 150*m *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 1.52*m *is obtained by forcing the 2002-2005 steady-state conditions with measured 1.52*m*-temperatures from July 2005 to June 2006.*

5. p. 1710, line 26: Why do the authors make this control run with only heat conduction. Nobody would apply such a model today for the objectives the authors give in their paper. It would be much better to compare the model of the authors to an already developed sophisticated model as mentioned above. Therefore, I recommend to remove this comparison with the model, where the infiltration routine is deactivated from the paper.

It is the very goal of this work to demonstrate, that rainwater infiltration in the snow has an impact on soil temperatures on an annual timescale, which can not be modeled with an only-heat-conduction scheme. Contrary to what the reviewer states, none of the thermal models designed and used for operational permafrost modeling includes any other effect than heat conduction (see also our reply to the point “a” raised by the reviewer). The control run allows quantifying the enhanced performance of the scheme with rain water infiltration. Therefore, the control run with only heat conduction is essential for this study.

6. p.1712, line 16, and p. 1713, line 25: The production of snowmelt water should be definitely implemented in a final model version. May, it would improve the current model results remarkably.
We will implement the production of meltwater in the final model version. However, studies on the surface energy balance at the study site (Westermann et al. 2009) suggest that significant meltwater production does not occur during the polar night even during rain events, as the energy available from sensible heat fluxes and incoming long-wave radiation (which are the only potential energy sources for melt during polar night) is generally small compared to the net short-wave radiation during the actual melt period from end of May to June. Therefore, we expect an improved performance only at the end of the snow season in May, where the deviation from modeled and measured GST is indeed strongest.

6. p.1716, line 2: impact on the soil instead of impact the soil
changed

7. p.1716, line 25 to 27: see general comment 2 above
see above

9. p.1717, line 6: the authors state that wintertime rain events may amplify the

warming of permafrost temperatures. However, they should consider that once a rain fall event occurred having generated one or several ice layers on the ground surface: a) the snow cover thickness will be reduced and the thermal resistance of the snowpack will therefore be reduced and b) the new ice layers have in addition a much higher heat conductivity. Both effects would allow a much more efficient cooling if an atmospheric cooling would follow the warm rain fall events, somehow counteracting the warming effects of the rain fall events before. See also general comment 2 above. Is the here presented model able to include such effects?
We do not measure a significant reduction of snow cover thickness after a rain event (on the order of a few cm, see Figs. 4 and 5). The rain water has a temperature close to 0C, so that it does not trigger significant snow melt. Furthermore, measurements of the surface energy balance suggest that strong additional melt does not occur during the polar night (see above). In the described cases, the ice layer forming at the bottom is much thinner than the remaining snow pack, and the thermal resistance is thus not strongly reduced. In the model, the bottom ice layer is assigned the thermal conductivity of ice, so that the effect of enhanced conductivity of the bottom ice layer is indeed included. A potential change of the thermal conductivity and a compaction of the snow pack due to the rain event is not included, as we change the thermal conductivity independently according to measurements. This, however, is a point that should be investigated further.
We have added a remark about the impact of the enhanced conductivity of the bottom ice layer to the Results-section (see Major Comments b):
This relatively conductive ice layer is still overlain by more than 0.5m of snow (Fig. 4), so that the heat conduction through the snow pack is not significantly enhanced after the rain-on-snow event.

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Interactive comment on The Cryosphere Discuss., 5, 1697, 2011.