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The Cryosphere Discussions

Interactive comment on "Brief communication "Modeled rain on snow in CLM3 warms soil under thick snow cover and cools it under thin"" *by* J. Putkonen et al.

Anonymous Referee #1

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The Brief communication "Modeled rain on snow in CLM3 warms soil under thick snow cover and cools it under thin" by Putkonen et al. describes simulations of the impact of ROS events on the ground thermal regime. I have a few major comment that I would like the authors to consider in their revised version:

Major comments:

1. How realistic is the discussed case of strong soil cooling? As far as I understand, the authors use the ERA data from 1975 to 1984 to drive their model, and THEN introduce a ROS event with 50 mm. However, in almost all areas with

soil cooling, ROS events have never occurred in this period according to Fig. 15, Groismann et al. (2003). For the majority of the concerned grid cells in Central Siberia, average winter soil and air temperatures are very cold. The snow pack is therefore very cold as well and has a low thermal conductivity (as the snow conductivity in CLM3 is strongly influenced by temperature), so that the ROS event drastically changes the conductivity, which is the reason for the cooling (thermal resistance decreases by a factor of two, Fig. 2). However, the very question is: What would be the effect of such an event in a warmer future climate, under which ROS events could indeed occur? Would this then speed up PF degradation? For a warmer climate, the thermal conductivity of the snow pack would most likely be already larger, so that the increase in the thermal conductivity induced by the ROS event would be smaller, and the cooling hence less pronounced. Furthermore, the snow depth is around 0.2 m at a large number of the concerned grid cells (Fig. 3), corresponding to a snow water equivalent of around 50 mm at realistic snow densities. Therefore, the cooling is in many cases associated with large amounts of rain on small amounts of snow, which I find a rather unrealistic situation. Since ROS is a threshold event, one would expect a similar probability for a ROS event of 50 mm (e.g. at +0.5°C) as for a snowfall event of 50 mm (e.g. at -0.5°C). Plus it's in PF areas, so the probability of snowfall in winter is necessarily larger than for rain. Therefore, I would expect a large ROS event in PF to occur in areas with a thicker snow pack. Here, however, soil warming is found. The cases with soil cooling and a snow depth exceeding 0.2 m are generally associated with a very cold TBOT, where such a ROS event seems more than unrealistic (for the used climate forcing). An analysis on impact of snow (not including ROS events) on future ground conditions, which I find interesting in this context, is given in Lawrence & Slater (2009).
Rennert et al. (2008) analyzed future climate predictions for synoptic conditions which could favour ROS. In Fig. 6 in this work, where a present and future ROS

statistics is shown, areas with a sizable likelyhood for ROS largely coincide with the areas of soil warming in Fig. 1 in the present manuscript, at least for Siberia. Furthermore, this Fig. 6 in Rennert et al. contains ROS with >3 mm of precipitation, so that the likelyhood of a ROS with 50 mm should be much less in most areas. The areas with a large likelyhood of ROS events (and thus a sizable likelyhood for a large event) are almost exclusively in areas, where the present manuscript finds soil warming. Therefore, again, how realistic is it that the scenario of significant soil cooling will ever become reality?

2. CLM3 is a land-surface scheme, whose primary purpose is in large-scale atmospheric simualtions. Nicholsky et al. (2007) tested its performance for simulating the thermal regime in PF areas, and partly found drastic cold-biases of winter soil temperatures in the model. They suggested a number of modifications, which have, as far as the information provided in the manuscript go, not been included in the CLM3 module used for the present study. Furthermore, the snow parameterization in CLM3 is one among many (although considered a sohisticated one), and the authors do not provide any evidence that it can deal properly with ROS. The considerable spread in simulating snow properties between different land-surface schemes is exemplified by Feng et al. (2008). A test against a scheme primarily designed to reproduce snow processes and properties, such as SNOWPACK (Bartelt & Lehning 2003), could provide more clarity.
   I do not suggest that the analysis conducted by the authors is false or cannot be done this way, but the above mentioned studies suggest a considerable uncertainty of CLM3 in this context. These author's findings must be discussed in the light of this uncertainty and the studies on the CLM3 performance must be credited and discussed.

3. Independent from the above point, the authors should briefly describe how the CLM3 snow routine works.

4. Snow depth in Fig. 2: Directly after the artificial rain event, the snow depths of the two runs are almost equal, until about timestamp 2.5. Then, the snow depths deviate significantly. I guess this is due to stronger compaction of the snow pack without the ROS event, whereas the snow was already compacted before in the ROS run? This should be explained.

5. I would recommend to remove or at least reduce the narrative style used in parts of the paper. Words like “surprisingly”, “we expected, ... but ...” are at least not common in scientific literature. Although I respect the personal writing style of the authors, it is my opinion that they can make a stronger contribution by concentrating on their findings.

Minor comments:

p. 2558, l. 5: awkward sentence, rephrase.

l. 13: “are leading in the warming of the globe” awkward sentence, rephrase.

l. 13. “Since ...” rephrase to two sentences

p. 2559, l. 6: the more recent work of Westermann et al. (2011) in The Cryosphere should be acknowledged

l. 10: Global Climate Models

l. 23: this effect is quantified by Westermann et al. (2011)

l. 28: How was CCSM3 used in this study? If only the off-line CLM3 was used, remove CCSM3 from the Introduction.

p. 2562, Eq. 2: the original publication, Sturm et al. (1997), should be cited. Furthermore, this equation is only valid for snow densities above 0.156g/cm^3. It should be clarified whether such model snow densities occur in CLM3, and how this is treated. In addition, a different parameterization for snow (Jordan 1991) is used in CLM3. Why use two different ones?

p. 2564, l. 25: This statement needs to be backed up by literature. Fig. 15 in Groismann et al. (2003) suggests, that ROS events far from oceans have not occurred in winter in the past, but are limited to spring, when the effect on the thermal regime of the ground would most likely be negligible.

l. 27ff: Does this statement refer to present or future climate conditions? Can there be literature cited to support this? The study by Rennert et al. (2008) seems to contradict this sentence for large areas in Siberia.

The resolution of Figs. 2 and 3 is poor and should be improved.

Fig. 2: The time axis should be labelled with dates or months, not 1, 2, 3. Also, the rain event should be marked, e.g. with an arrow.

References:

Bartelt, P. and Lehning, M.: A physical SNOWPACK model for the Swiss avalanche

warning. Part I: numerical model, Cold Reg. Sci. Technol., 35, 123–145, 2002.
Feng, X., Sahoo, A., Arsenault, K., Houser, P., Luo, Y. and Troy, T.J.: The impact of snow model complexity at three CLPX sites, Journal of Hydrometeorology 9 (6), 1464-1481, 2008.
Groisman, P., Sun, B., Vose, R., Lawrimore, J., Whitfield, P., Førland, E., Hanssen-Bauer, I., Serreze, M., Razuvaev, V., and Alekseev, G.: Contemporary climate changes in high latitudes of the Northern Hemisphere: Daily time resolution, in: Preprints, 14th Symp. on Global Change and Climate Variations, Long Beach, 2003.
Jordan R.: A one-dimensional temperature model for a snow cover: technical documentation for SNTHERM.89. Special Report 91-16. U.S. Army Cold Regions Research and Engineering Laboratory, Boulder USA, American Meteorological Society, vol. 4, 1991.
Lawrence, D. and Slater, A.: The contribution of snow condition trends to future ground climate, Climate Dynamics 34 (7-8), 969-981, 2009.
Nicolsky, D. J., V. E. Romanovsky, V. A. Alexeev, and D. M. Lawrence (2007), Improved modeling of permafrost dynamics in a GCM land-surface scheme, Geophys. Res. Lett., 34, L08501, doi:10.1029/2007GL029525.
Sturm, M., J. Holmgren, M. König, and K. Morris, The thermal conductivity of seasonal snow, J. Glaciol., 43, 26–41, 1997.
Westermann, S., Boike, J., Langer, M., Schuler, T. V., and Etzelmüller, B.: Modeling the impact of wintertime rain events on the thermal regime of permafrost, The Cryosphere, 5, 945-959, doi:10.5194/tc-5-945-2011, 2011.

Interactive comment on The Cryosphere Discuss., 5, 2557, 2011.