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The Cryosphere
Discussions

Interactive comment on "Modelling the spatial pattern of ground thaw in a small basin in the arctic tundra" *by* S. Endrizzi et al.

Anonymous Referee #1

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The paper presents a detailed permafrost process model, which accounts for subsurface heat and water fluxes. The model is applied to a small catchment area in N Canada dominated by permafrost for the period of one summer season. While the data sets driving the model are derived from a climate station, the only validation of the model is the distribution of thaw depths for three small subregions of the catchment. The paper is well organized and written and the main lines of argumentation can be clearly followed. Such models have potential to be used for discharge modeling of streams, but also as base module for future biogeochemical models aiming to predict the release of greenhouse gases from permafrost areas. The authors give an extensive description of the different model parts, which I find adequate for this kind of study.

The main finding of the paper is that the end-of-summer thaw depth is determined by

the soil moisture conditions. The main effects giving rise to that can be summarized as follows: the water drains through the soil from high-lying to low-lying regions, where it creates permanently water-saturated soil conditions. Therefore, in the model catchment areas are found, where a) the surface layer is not water-saturated, while the underlying layer is, and even more important, the still frozen layer below is saturated with ice, too. And b), areas where the surface layer is water-saturated as well, while the rest of the soil is more or less similar to a). As the thermal conductivity of the surface layer of a) is much lower than in b), the dry surface layer will limit the heat flow to the underlying water- and ice-rich layers compared to b). If the surface energy balance consisted only of short-wave radiation, incoming long-wave radiation and the ground heat flux, the surface temperature of the areas a) would increase until the larger temperature gradient across the surface layer compensates for the smaller thermal conductivity and the ground heat flux is similar to b). However, in reality (or at least in the model), the surface temperature of a) is not too different from b), as it is stabilized by increased sensible heat fluxes and emission of long-wave radiation. Therefore, the reduced heat flow through the surface layer of a) will lead to a lower end-of-the season thaw depth, because the ice contents and thus the energy required to thaw a unit soil volume are assumed more or less similar for a) and b).
While the authors describe an interesting and potentially widespread effect, a number of major weaknesses in the manuscript need to be addressed before publication.

1 Major Comments:

1. If or how strong this effect will show in the model results should strongly depend on the particular values of the thermal conductivity assigned to the dry and wet layers. There is no validation for the thermal conductivity presented in this study and the authors do not even give the resulting values obtained from the used

parameterization in the manuscript (except for pure water and "pure peat", none of which occurs in the catchment area). A rather simple parameterization for the thermal conductivity is used, which is not unusual in such studies. In this particular case, however, it is at least possible that the main conclusion drawn in the manuscript is somehow affected by the choice of the parameterization for the thermal conductivity.
In absence of measured values of thermal conductivity from the study area, the authors should i) give typical values for dry peat (as found at the surface in well-drained areas), saturated peat and frozen peat as delivered by the used parameterization, ii) compare these values to the values that would be obtained using other, more sophisticated parameterizations for the thermal conductivity (which are mentioned by the authors) using the same volumetric fractions of the soil constituents and iii) compare to the few published values of in situ measurements of the thermal conductivity (a new publication providing some values is now available in this special issue, Langer et. al. 2011). The authors should then demonstrate that their main results are qualitatively robust and give some assessment of the uncertainty of the thaw depth induced from the choice of parameterization of the thermal conductivity.

2. There is only little validation provided for such a complex model. To me, the distributions of thaw depth shown in Fig. 9 are only marginal evidence that the model reproduces reality. Hereby, my criticism concerns much less the slight deviation of about 10cm (I have serious doubts, models will ever get better than that) than the fact that the shown distributions do not provide any information on the spatial pattern of the thaw depths, which is a main result of the study. It is impossible to tell whether the same factors give rise to the width of the distribution in reality and in the model. If this is feasible with the available field data, the authors should provide a graph: average measured thaw depth vs. distance from the creek (sim-

ilar to Fig. 2a), which should (if the model results are correct) show decreasing thaw depths with distance from the creek. In addition, any field observation on the thaw depths in the drier parts of the catchment would be valuable, as would be measurements or even qualitative observations on the spatial distribution of the soil moisture content. Are measurements of soil temperature profiles available at points in the catchment, to which the model could be compared? Finally, the model should also deliver the discharge of Siksik Creek. Are there field observations/measurements on this?

3. I am missing more detailed information on the data sets driving the model, especially the water fluxes. Most data are taken from a climate station in the catchment, which should be described briefly. Furthermore, the authors should state clearly, how they determine the upper boundary for the water budget. Rain? Evaporation? How about melting snow? Are there patches persisting well into summer, which could deliver melt water?

4. In the initial state of the model simulation, the soil is saturated with ice, while in the final state of the simulation, the soil is not water-saturated in most areas, at least close to the surface. Therefore, there must be some recharge mechanism until the following spring, if this is to work as a closed loop. The authors should discuss this point.

5. The authors model the surface energy balance for the catchment. They should at least briefly compare the obtained values with published measurements on the surface energy balance, in particular with the values given in Eugster et al. (2000). Furthermore, I am missing a few measures typically given in model and field studies on the surface energy and water balance, e.g. the Bowen ratio, average ground heat flux divided by average net radiation, evaporation divided by precipitation, creek discharge divided by precipitation.

6. A photo taken in the study area would help the reader to get a better impression

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of the general setting, including the vegetation, the hummocks, maybe the creek, etc.

2 Minor Comments:

P.369, l.24 ff: Is "heat flux transferred into the ground from the atmosphere" simply the ground heat flux? I also suggest including "spatial variability of the surface energy balance" as the comprehensive term in the next sentence ("This implies...").
P.370, l.8 ff: While the soil conductivity increases with soil water content, the energy required to thaw the soil increases with the ice content of the underlying frozen soil, too. Therefore, it will depend on the exact soil composition, whether the thaw depth is increased or decreased. For the cases the authors describe later in the paper I agree, but in this general form in the Introduction, I do not.
P.370, l.20ff: Awkward sentence, rephrase.
P.371, l.17: Not sure about the given surface area of 1005km2. Does that correspond to the area surrounded by the dashed line in Fig. 1? To me, this seems more like 0.6km x 2km, which would be roughly 1km2. Please check and clarify.
P.371, l.19: close bracket
P.372, l.2: Should be "Siksik Creek drainage".
P.372,. l.5: Should be "with diameters between..."
P.372, l.22: Should be "solution of the coupled heat and water flow equations"?
P.372, l.25: Should be "the surface energy balance"
P.373, l.2: This assumption is quite far-reaching and could be a crucial factor for accurately modeling the frost table. How realistic is this assumption, and are there any data on the expected spatial variability of the timing of the snowmelt? I can follow the argumentation given in the two next sentences, if the difference is days, but not if it is weeks. The authors must provide some clarification here. Furthermore, what happens to the water from the melting snow? Is that all gone at the time the model run

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is started?
P.373, l.18: The sections 4 and 5 should better be subsections of section 3, Methodology.
P.374, l.9: The ground heat flux is not only the simple sum of the mentioned contributions of the surface energy balance, but it should depend on the surface temperature, as do the sensible and latent heat fluxes and the outgoing long-wave radiation. Only the incoming and outgoing short-wave and the incoming long-wave radiation are fixed values provided by measurements, all other components of the surface energy balance adjust according to air temperature, wind speed, relative humidity, atmospheric stability, subsurface temperature, etc.
P.374, Eq.1: In the Results section, the authors write: "GEOtop represents the advection of latent heat that occurs when a water particle moving into a mixture of water and ice freezes and releases heat, but it does not describe the advection of sensible heat, resulting from the temperature gradient between the moving liquid water particles and the mixture of water and ice." At this point in 4.1, this is not mentioned or clarified. Although I presume these effects are accounted for by the second term of Eq. 2, it would be better to mention and clearly document all effects leading to a change in soil temperatures in 4.1.
P.374, Eq.2: The first term of the equation is only correct for a system, whose heat capacity is constant with temperature. However, for constant soil air content (water plus ice content constant) and subzero temperatures, this is not the case as the heat capacity changes with the fractions of ice and water and thus depends on temperature as C(T) (this effect has nothing to do with the release of latent heat, which is described in the second term of the equation). The correct equation (assuming the freezing temperature is zero degrees) is

$$U = \int_0^T C(T')dT' + ... \tag{1}$$

For non-constant air content (e.g. in case of water infiltration), U depends on the history of the system starting from a time t0 as

$$U = \int_{t_0}^{t} C(t') \cdot \frac{dT(\tau)}{d\tau}|_{\tau=t'} \, dt' + ... \tag{2}$$

Both expressions differentiated with respect to time yield $C(T) \cdot dT/dt$, so that the well-known form of the heat flow equation is reproduced. As it is unnecessary to discuss this matter in the paper in detail, I suggest writing Eq. 1 exclusively in terms of temperature and skipping the definition of the internal energy (including Eq. 2).
P.374, l.21: use "surface energy balance" instead of "surface heat flux"
P.375, l.1: heat capacity
P.375, l.2: "density of water" is sufficient (as opposed to "density of ice")
P.375, l.3ff: The section on the soil freezing characteristic is too long. What is important here is: 1. The effect is included in the model, and 2. How was/were the curve(s) used in the simulations determined? Information on the second point is missing. Is there spatial variability, for instance in z-direction between organic and mineral soils. Does it depend on porosity? Are there studies on this issue for peat? The authors should briefly address these issues and not only refer to Dall'Amico et al. (2010).
P.375, l.19: "heat capacity" instead of "thermal capacity"
P.376, l.1: Which value is used as the conductivity of pure "soil solids"? This will strongly depend on the mineral and organic contents, and there are very, very few studies on the thermal conductivity of peat. On P.381, l.28, a value of 0.21W/mK is given for "pure peat". What is this value based upon?
P.376, l.2: "respectively" should be at the end of the sentence
P.376, l.7: Use "surface energy balance" instead of "surface heat flux"
P.376, l.11: Why use 0.2 as value for the albedo? Is this based on measurements or literature? Considering the study site description, spatial differences of the albedo, e.g. due to differences in vegetation, are not unlikely. A (not unrealistic) albedo difference of 0.05 would result in an average difference of 7 to 8 W/m2 in the average

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net short-wave radiation, which is in the range of the spatial differences caused by the relief (as given in Fig. 5).
P.377, l.1: Does the soil water content of the uppermost soil cell enter in the calculation of the soil resistance? How about transpiration from plants (mentioned later, but not here)?
P.377, l.4: Which universal stability functions are used?
P.377, l.7: partitioning
P.377, l.11: How are the boundary conditions for the water flow part? Measured rainfall, melting snow, evapotranspiration as determined from the surface energy balance? Information on this should be included (see also Major Comments).
P.377, Eq.5: Is the source term used in the model? Is this the way, the melting ice in the active layer is accounted for? Please comment briefly.
P.377, l.24: “further decreases” or “is further decreased”
P.378, l.3ff: How is the creek itself treated in the water flow model? Is it somehow incised (in reality and in the model)? Is it important for the soil moisture regime in the adjacent areas?
P.378, l.6: computation time
P.378, l.6: Awkward sentence. Rephrase.
P.378, l.13: two transects
P.379, l.9: Rephrase this sentence.
P.379, l.18: “This has been represented in the model by setting...”
P.379, l.20: Why can’t the water trapped in the “inactive pores” freeze? Heat conduction should not be affected at all.
P.380, l.10: How was the hydraulic conductivity of the hummock zones determined?
P.380, l.11: several magnitudes “lower”?
P.380, l.17: Why has this particular temperature been chosen? Measurements?
P.380, l.17: What is meant by “saturation front at the surface”?
P.380, l.17ff: The soil is discretized down to 3.5m and the lower boundary is at 8m? Please clarify!

P.380, l.22: The model yields a considerable spatial variability of the summer ground heat flux leading to strongly different thaw depths. Why should the lower boundary temperature be spatially constant then? I agree that it is hard to avoid such simplifications in modeling, but at least it should be outlined as a potential error source.
P.381, l.2: ranges from
P.381, l.4: What is meant with “diverging curvature”?
P.381, l.21: “times” instead of “moments”
P.381, l.28: Where is the value for “peat solids” taken from?
P.382, l.5: heat capacity
P.382, l.8: Should be documented in 4.1, see P374, Eq.1.
P.382, l.10: Not sure what the authors mean with “the advection of sensible heat, resulting from the temperature gradient between the moving liquid water particles and the mixture of water and ice”, since every point (or grid cell) has one temperature defining ice and water contents according to the freezing characteristic. In any case, it should be documented in 4.1, which effects that give rise to a change of soil temperatures are accounted for and which effects are omitted.
P.382, l.20: higher absolute spatial variability
P.382, l.20ff: Again, albedo differences are not unlikely.
P383, l.10: heat capacity
P.385, l.1: Is the hydraulic conductivity set to zero only in lateral, or also in vertical direction?
P.385, l.22: Fig. 8d?
P.386, l.8: Before (P.383, l21), the authors write that the wet areas have higher latent heat fluxes, so MORE evaporative losses. They remain cool, BECAUSE they have higher evaporative losses, and only to a minor degree, because they are more conductive. The absolute spatial differences in the ground heat flux are significantly smaller than the differences in the sensible and latent heat fluxes, as are the absolute numbers, so the principal governing factor for the surface temperatures are the sensible and latent heat fluxes and only to a minor degree the ground heat flux.

P387, l.9: I wouldn't call the tail towards high values of measured thaw depths “less accentuated”. There simply are no measured thaw depths of more than 55cm in the last graph. Therefore, I can't entirely follow the argument with the subgrid variability, as I suspect, that there would be fewer measurements with deep thaw depths, but not any at all.
P.387, l.12ff: Transpiration would not simply increase the total amount of the latent heat flux, but some part of the water lost by evaporation in the model would then be lost by transpiration. The effects of plants or vegetation on sensible and latent heat fluxes and the ground surface temperature are diverse and the sign and magnitude of the net effect is not clear. Only detailed field studies could help here. Therefore, I don't think that this speculation is helpful here, considering that the used parameterizations of sensible and latent heat fluxes are great simplifications anyway.
P.388, l.6: The dry sites are mainly warmer because of the lower latent heat fluxes (which lead to increased sensible heat fluxes), and only to a minor part because they are poor conductors. Compare the magnitudes of sensible and latent heat fluxes and the ground heat flux. The sensible and latent heat fluxes more or less interchange their roles between wet and dry areas, while the differences in the ground heat fluxes are comparatively small. Next sentence: the sensible heat flux and the outgoing LW radiation are NOT a result of the ground heat flux. All fluxes adjust as given by the surface energy balance equation. Outgoing long-wave radiation, sensible heat flux, latent heat flux and ground heat flux are functions of the surface temperature, so that the surface temperature can be calculated from conservation of energy, i.e. a closed surface energy balance, yielding all fluxes.
Fig. 2b: I have the impression, that the underlying shading of the DEM is superimposed on the color scale. At least I see some variation of the peat thickness at distances of more than 50m from the stream, which is not in agreement with the curve in 2a. Please remove the shading of the DEM, if my interpretation is correct. Please check for the effect in the other figure, too.
The authors should also improve the labels of some of the axis, e.g. the 2 in W/m2

should be superscript, it should be degree C and not only C (Fig. 6b), it should not overlap with the labels of the color bars (Fig. 7b/d).

3 References

Eugster, W., Rouse, W., Pielke Sr, R., McFadden, J., Baldocchi, D., Kittel, T., Chapin, F., Liston, G., Vidale, P., Vaganov, E., Chambers, S., 2000. Land– atmosphere energy exchange in Arctic tundra and boreal forest: available data and feedbacks to climate. Global Change Biology 6 (1), 84–115.

Langer, M., Westermann, S., Muster, S., Piel, K., and Boike, J. 2011: The surface energy balance of a polygonal tundra site in northern Siberia – Part 1: Spring to fall, The Cryosphere, 5, 151-171.

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