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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.

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Here we answer to each comment posted by Julia Boike.

Answer to Major comment 1

One of the hypothesis of this paper is that the subsurface water flow has a significant impact on energy flow and active layer thaw, and plays a major role in controlling the spatial variability in active layer depth. Since the study basin is only 1 km^2 in area, it is essential to model the entire catchment area in order to account for all subsurface water flow that affects frost table development. This paper has a deliberate objective of developing an improved method for predicting runoff by linking sub-surface water flow active layer development, and runoff. The focus of this paper is on the first step

of that development – i.e. the spatial and temporal pattern of sub-surface water flow, ground thaw and from this, as explained in this paper, the frost table topography. As such, this paper uses an energy-based method to demonstrate 1) the close coupling of energy and water mass flows, and 2) the re-distribution of moisture within a basin. Both are basin-scale phenomena since they are potentially influenced by horizontal inputs of energy and mass from upslope areas. As a result, we must model the entire basin area. This will then lead into the next phase of this research to apply the methods described in this paper to predict basin runoff hydrographs for a range of input and basin conditions. The paper of Bense et al. (2009) that the reviewer mentions has a completely different focus and addresses much larger spatial and temporal scales. They present a modeling study of the thaw of a very deep permafrost layer (hundreds of meters thick) and consider the long-term effects on discharge in large rivers. Our study focuses instead on a 1-km^2 catchment and the evolution of the frost table topography during a single summer.

Answer to Major comment 2

The reviewer raises issues related to the spatial scale of frost table variability vs. the resolution of the input parameters. Although the reviewer is correct that there are variations in frost depth at scales less than 1 m (for example around mineral earth hummocks the frost table can vary over a horizontal distance of a few cm to dm), this is not the scale of variability that we are interested in. Instead, we are concerned with the spatial variability in frost table depth at a scale of about 10 m. Our field observations show that there are important variations in frost table depth at this scale. We hypothesize that this has important implications to runoff, and we suggest that these can be addressed by modeling the basin with grid sizes of 10 m. In order to model the frost table at this scale it is important to account for sub-grid variability in hydraulic conductivity, to account for the variability in organic soil depth, and to down-scale atmospheric observations to the 10 m scale. These were accomplished as follows:

1) the reviewer notes that we use a bulk parameterization to accommodate for sub-

grid spatial variability. This is correct, and we feel it is appropriate given our objective to consider the spatial variability at scales of 10 m. In order to do this, it would be inappropriate to use point measurements of hydraulic conductivity. Instead we measured hydraulic conductivity over 10 m distances using tracer tests (see Quinton et al., 2008). This captures the sub-grid spatial variability of the hydraulic conductivity at an appropriate scale for this study.

2) an important control on sub-surface water flow and therefore frost table depth is the thickness of the organic soil layer. Organic layer thickness occurs at two scales. (i) Around mineral earth hummocks the organic layer varies over horizontal distances of a few cm to dm. This has important effects on hydraulic conductivity. This is accounted for in the way we parameterized sub-grid hydraulic conductivity as noted above. (ii) The organic layer also varies at a larger scale. This spatial variability was incorporated into the model by using an observed relationship between organic soil thickness and distance from the main stream channel. As shown in Figure 2, most of the change in organic depth occurs within 50 m of the stream channel. Given the grid size of 10 m used in the simulations, this means that organic depth varies over the 5 model grids closest to the channel and is then constant throughout the basin. This is consistent with observations, and demonstrates that the resolution of our input parameters is consistent with the model scale.

3) the properties of the mineral substrate are considered to be uniform over the model domain. This is felt to be appropriate as the mineral material is silt and clay, and since the hydraulic conductivity is very low, water flow is dominated by flow in the organic layer. Second, observations show that the mineral material is relatively uniform over the basin, and any small variations in it, do not play an important role in controlling lateral water flow.

4) although the reviewer is correct that we use input climate data from a single station located within the 1 km^2 Siksik Creek basin, the GEOtop model distributes many of these climate variables based on topography. For example incoming solar radiation is

distributed by slope and aspect, outgoing solar radiation is a function of surface conditions and varies depending on snow or snow-free at the grid level, and wind speed and direction are also distributed by a topographic function for each grid. Air temperature and humidity (more specifically, dew temperature) are also distributed assuming standard lapse rates (e.g. Liston and Elder, 2006). As a result, all of the major atmospheric variables are distributed across the model domain.

5) the LIDAR DEM is available at approximately 2 m. However, to match the scale of our interest and the resolution of our observations, we increased the grid size to 10 m. Finally, we feel that the 10 m resolution of the model is appropriate for the objectives of the paper, and that we have dealt with sub-grid variibility when needed, and have also distributed other input data over the 10 m grid, again as needed. As a result, we do not feel we need to modify these aspects of the paper, however we will modify the text to ensure that these points are made clearer to the reader.

Answer to Major comment 3

We thank the reviewer for her comments, all of which are well taken. However we feel that these comments have been addressed by modifying our text and providing more explanation of our methods and results interpretations. We found that the incoming shortwave radiation patterns, dominated by slope and aspect of the topographic surface, do not significantly affect the spatial variability of the depth of thaw. However, shortwave radiation is just one component of the energy balance. In our study the depth of thaw has been always found proportional to the cumulated energy coming from the atmosphere (given by the sum of net radiation and turbulent fluxes of sensible and latent heat). So the relation between energy input from the atmosphere and depth of thaw is not questioned. We show instead that the spatial variability of the depth of thaw is significantly affected by the soil moisture redistribution due to the lateral subsurface flow of water. In particular, wet areas experience deeper thaw than dry areas. A first explanation is that in wet areas the thermal conductivity is significantly higher than in dry areas, and the summer heat wave therefore penetrates more deeply in the soil. However, we show that this effect is complicated by the interaction with the surface energy balance, namely the energy fluxes exchanged with the atmosphere. Since wet areas transmit more heat downwards, they keep the surface temperature relatively low. Given that the energy fluxes from the surface back to the atmosphere generally increase with the surface temperature (with the exception of reflected shortwave radiation), wet areas are able to reduce the energy losses. As a result, wet areas are actually more efficient to capture energy from the atmosphere, and, for this reason, they present deeper thaw. We agree that snow cover and vegetation have to be included to better support the results. However, the approximations made regarding the spatial distribution of the soil properties do not probably affect the overall results shown here. A smaller scale spatial variability of the organic soil thickness than the one used in our paper will perturb this averaged result, since we will have locally deeper (shallower) thaw where the organic soil is shallower (deeper), but not significantly change it.

Answer to suggestion 1

We do not agree with the reviewer that we should use another catchment. The Trail Valley/Siksik Creeks study study sites do have the data necessary for this study. If the reviewer is suggesting using a study site where there are data on the sub-grid variability at the less than 1 m resolution mentioned in another point above, we believe that these data are not available, and if even it was it is likely that computer resources are not available to use it at the basin scale. As a result, we strongly feel that we are applying this approach at the most appropriate site.

Answer to suggestion 2

For the catchment used in the paper the available data include continuous measurements of groundwater level and temperature at 3 depths in one points located downslope of a snow-drift. There are a few measurements in other points, but they were not performed on a regular basis. In addition, discharge measurements at the catchment

outlet were sporadically taken during the summer. However, the position of the measurement points is not known in high-precision, because at that time the GPS use was still affected by the selective availability. We highlight that the purpose of the paper is not performing a full validation of the model. Similarly, the paper is not the place to perform a full sensitivity analysis. There is also no point in doing a sensitivity analysis on a single pixel because it will not include some of the most important effects, i.e. that of subsurface water flow between pixels, and blowing snow between pixels. The purpose is investigating with a distributed model which factors affect the frost table topography during the summer for an eventual better comprehension of the runoff production mechanism. This implies a compromise between simplification of reality and complexity of model. However, in order to better support our results we will use more field measurements. As said before, we feel that there is little relation between the topic addressed in our paper and the study performed by Bense et al. (2009), since the spatial and temporal scales that are considered are completely different.

Answer to specific comments

1) Page 368: Title

We do not say spatial scale, but are simply meaning how variable it is. We will change it to: “Modelling the spatial variability of ground thaw in a small basin in the arctic tundra”.

2) Page 368 - Line 17: Treeless terrain

The study basin is dominated by tundra vegetation (mosses, grasses, etc) with willow and alder shrubs located along the lower reaches of the Siksik Creek channel. When we refer to peat thermal conductivity we just refer to the thermal conductivity of the peat solids. If the peat is filled with liquid water and/or ice, we have to consider also the thermal conductivities of these substances. We explain later (line 16 and following of page 375) how to calculate the overall thermal conductivity from the conductivities of the individual constituents (peat, water, ice).

3) Page 368 - Line 25ff: .. hillslope drainage..(and overall in introduction section)

The results reported are generally valid only in organic-covered permafrost terrain. These terrains are sufficiently wet that when they are frozen, the zero-degree isotherm (i.e. frost table) is relatively impermeable to water. There are many arctic environments where this does not apply. Most of the high arctic islands in Canada are "polar desert", with little or no soil development. Surface runoff dominates in that environment.

4) Page 369 - Line 3-4: ..defined as the zero-degree isotherm...

For wet organic soils (e.g. tundra and other peatlands), given their high porosity and steep retention curve, most of soil freezing occurs between 0 °C and -0.2 °C. Therefore, the frost table can be reasonably approximated with the 0 °C isotherm, although it is recognized that freezing does not take place sharply at this temperature. In addition, the frost table is a relatively impermeable only during thawing, because the soil below the frost table is almost saturated with ice and a small quantity of unfrozen water. Therefore, its hydraulic conductivity, both vertical and horizontal, is several orders of magnitude smaller than the correspondent conductivities of the soil above the frost table.

5) Page 369 - Line 21: ..direction of flow, are highly variable..

We state that the subsurface flow is driven by the topography of the frost table, as it was a moving bedrock. With high spatial variability of the subsurface flow patterns we mean that, since the hydraulic conductivity strongly varies with depth, different depths of the frost table in space results in different subsurface drainage rates. The frost table can vary at any scale, not necessarily at the small scale only.

6) Page 369 - Line 22: ..therefore critical..

There are several papers that can be referenced to back-up our assumption about the importance of the frost table depth and frost table topography on runoff direction and rate (e.g. Carey and Woo, 2000; Quinton et al., 2009).

7) Page 370 - Line 2: ..spatial pattern of snow disappearance..

We will show in the revised version of the paper that the influence of snow matters.

8) Page 370 - Line 7: ..Subsurface flow.....greater thawing..enhanced evaporation of the ..ground surface

Here we refer to Kane et al. (2001), who state: “In water track areas, the organic mat remains in a saturated state for most of the summer season; thus it has an enhanced thermal conductivity compared to inter-water track areas. This enhanced thermal conductivity effectively supplies a warm boundary condition for the upper surface of the mineral soil layer, thereby producing enhanced thaw in these locations.” We will drop the sentence “. . .even if the rate of energy transfer into the ground is offset by the energy loss due to enhanced evaporation of the relatively wet ground surface.” that is probably a re-interpretation of the findings of Kane et al. (2001) in the light of the conclusion of our work. However, we recognize that this finding is not a general relation, but only something that was observed not only by Kane et al. (2001), but also in other works (e.g. Wright et al., 2009).

9) Page 370 - Line 15: .. a fine resolution.. What is the fine resolution (see major comment 2)

We have a DEM at a 2 m resolution generated from a LIDAR survey. This is considered a fine resolution DEM because only in very few cases a DEM of such resolution is available for remote arctic tundra areas. However, as we said above, we run the model at 10 m of resolution. In this work we are using a fully distributed modeling approach. Therefore, the elementary computational cells are given by the DEM pixels, not by relatively large and homogeneous hydrological response units. The term “fine resolution” is a relative expression, and we tried to discretize the catchment with as many elementary units as possible. Obviously there are computational limits by using large number of cells, and one should seek a compromise. We are aware of course that there are important processes that occur at smaller scales.

10) Page 370 - Line 16 :..limited field observation.. Of what?

Field observations include hourly meteorological data (wind speed and direction, air temperature, relative humidity, and incoming shortwave radiation) as well as frost table measurements at the three plots during the summer. In addition, we have continuous measurements of the water table and temperature (at 3 depths) in one point in the middle plot, and few measurements of the discharge at the lower plot.

11) Page 370 - Line 26ff (entire paragraph until page 371, line 10):

Hinzman et al. (1998) and Marchenko et al. (2008) accurately solved the heat equation in the vertical direction considering phase change, but they did not consider the coupling of the heat equation with the water flow equation in the soil. For this reason, we say that they "neglect the coupling with subsurface lateral water flow". Laurence and Slater (2005) used the Community Land Model, which solves the heat equation, but also describes the water flow with "a simplified method", that is solving the vertical water balance and parameterizing the sub-surface water drainage. On the other hand, GEOtop seeks a numerical solution of the Richards' equation in a fully three-dimensional form. The model of Bense et al. (2009) can be included in the third categories of model (lines 7-10 page 371), and Topoflow used by Schramm et al. (2005) in the second category (lines 4-7 page 371).

12) Page 371 - Line 18: .. 1005 km^2..

In the Abstract the area is correctly listed as 1 km^2. The area was listed as 1.005 km^2 on line 18 in the version we submitted, and are not sure how it was changed to 1005 after we submitted the paper. We are sorry for the confusion this resulted in.

13) Page 371 - Line 21: .. remotely sensed data..

A LIDAR DEM of a 2 m resolution.

14) Page 372 - Line 5: ..diameters of between 0.4 to 1 m...

As we said above, hummocks are relevant for runoff, and have been considered in this study by parameterizing the sub-grid variability.

15) Page 372 - Line 12: ..Other studies...

We listed above the data that are available for Siksik creek.

16) Page 373 - Line 6: ..It is also expected...

In the revised version of the paper the snow cover will be considered, and the above-mentioned assumptions will be proved.

17) Page 373 - Line 10: .. vegetation cover

Shrubs cover a small part of the surface, and we have a map of shrub areas derived from the LIDAR survey. Endrizzi and Marsh (2010) carried out a test and application of GEOtop to better understand the effects of shrubs on snowmelt. We will consider the presence of shrubs in the revised version of the paper.

18) Page 373 - Line 16: ..focused in the inter-hummock..

As we said above, our interest is only in the inter-hummock area, since this is the sole area that contributes to lateral runoff. Hummocks have hydraulic conductivities several orders of magnitude lower. Their effect is as obstacles to runoff. This is however accounted for through sub-grid parameterization. The hummocks have a significantly deeper mineral soil depth than the inter-hummock areas, because the highly insulating organic soil layer is very shallow or even absent on the top of the hummocks. Consequently, there may be lateral heat conduction between hummocks and inter-hummock areas. However, lateral gradients are negligible if compared with vertical gradients.

19) Page 373 - Lines 22ff:

The model uses a 10 m DEM based on the 2 m Lidar data. The accuracy of the LIDAR data is +/-0.12 m. This is sufficiently accurate for this study. Regarding the hummock microtopography, as we said above, in this work we are interested only in the organic soil covered inter-hummock zone, since this is the soil where actually lateral water flow occurs. For this reason we applied the model assigning the thermal properties of inter-hummock soil, so to describe the thermal behavior of the part we are interested in, and using hummock and inter-hummock averages for the hydraulic properties, so to account for the effect of hummock on runoff.

20) Page 374 - Line 15ff: ..heat equation..

The method to solve the heat equation is fully described in Dall'Amico et al. (2011) and relies on the concept of internal energy and "freezing=drying" assumptions. For a given water content, ice content is a function of soil temperature and total (frozen+unfrozen) water content. This function depends on the soil retention curve, which here depends on the Van Genuchten parameters (including porosity). These parameters were derived from results of other studies dealing with the same or similar environment (e.g. Quinton and Gray, 2003; Kuyala et al., 2008). As we said above, the paper is not the place to perform a full sensitivity analysis, given the objective that the paper has. However, we have actually done a small sensitivity analysis in order to consider the effect of uncertainty in the estimation of the Van Genuchten parameters. Probably because the retention curve for the peat is very steep, we found little sensitivity of the results to the Van Genuchten parameters. Therefore, we are confident that the values used here are appropriate. The values of the soil parameters will be shown.

21) Page 376 - Line 10, line 25: ...assuming constant albedo and surface roughness..

The effects of snow and vegetation will be considered in the revised version of the paper.

22) Page 379 - Line 12 ff:.."inactive porosity"..

Like all the soil properties used in this study, we considered averaged values over the elementary computational cell. The Van Genuchten properties are, in a certain sense, larger scale parameters that describe smaller scale properties, like, for example, the

distribution of pore spaces.

23) Page 381-382 Results and discussion

This is not completely true. We present a comparison between measured and modeled frost table in different moments during the summer (Fig. 9, we have a few moments more, but had to reduce for space reasons), maps of modeled frost table during the summer (Fig. 4), and all the maps of energy fluxes are actually cumulated fluxes over the summer (that is balances). However, in order to allow a better comparison between observations and simulations, we will add a chart with the seasonal course of frost table for the 3 plots.

24) Page 384 - Line 26ff:.. north facing slopes and greater thaw..

We remark that the results we have obtained regarding the effect of the surface topography on the thaw depth spatial variability do not have the pretension to be valid in any arctic tundra environment, but are only characteristic of this catchment given its topographical, soil and climatic characteristics. In the revised version of the paper we will also prove that the effects of snow melting patterns, dominated by shortwave radiation spatial variability, on the depth of thaw are here not significantly prolonged in the summer. In addition, differences in slope and aspect are not so large in Siksik, and the relatively small spatial variability of shortwave radiation does not affect the spatial variability of the depth of thaw. This is not true for the site studied by Carey and Woo (2001), which, instead, is a sub-alpine arctic site, with much more pronounced slope and aspect spatial variability than in Siksik Creek (rolling tundra). This is reflected in significantly higher snow melting spatial variability, playing a stronger effect in the distribution of the depth of thaw.

25) Page 385 - Line 2ff:..evaporation higher on south facing slopes..

Yes, this can be quantified, but this comment is not correct. Here we are not referring to the reference simulation (the one that includes all the processes and the results of which are presented in Fig. 3), but are separating the single factors that affect the spatial variability depth of thaw. In this paragraph we are only evaluating how the spatial variability of depth of thaw would be if there were no subsurface water flow.

26) Page 386 - Line 6ff: ...spatial distribution of the ground heat flux...

This is considered in the revised version of the manuscript

27) Page 387 Line 21:..fine resolution..

Already discussed above

28) Figure 4

Since we did not consider snow, the spatial variability of the depth of thaw is dominated by the subsurface water flow since the beginning of the simulation. The revised version of the paper, which will also consider snow, will show that there is a certain period of time after snow removal when the patterns of thaw depth are also influenced by snow melt patterns. However, this influence vanishes shortly after snowmelt. We do not agree that the patterns show the limitation of the spatial resolution, which is sufficiently small (10 m). Probably this comment was affected by the erroneous indication of the area of the catchment (1005 km^2 instead of 1.005 km^2).

29) Figure 5

Only slope and aspect determine the spatial variability of net shortwave radiation, because albedo was set as uniform parameter.

30) Figure 9

We discussed this in the paper: the peaks of the distribution compare reasonably well. However, the model tends to overestimate the depth of thaw. We list in the paper possible reasons of deviation: i) snow, ii) scale issues related to the soil properties and microtopography, iii) local deviations of the organic soil thickness with respect to the thickness distribution used in the model, iv) effects of transpiration of small vegetation.

We will make the comparison more consistent in the revised version of the paper, in the sense that we will compare the plot observations with model results correspondent with the area of the plots, not with results relative to the whole strip at the right hand side of the main stream. However, we think that the paper responds to the prefixed objectives and provides an explication of the thaw depth spatial variability (in particular that occurring at the end of the summer) that is based on the surface energy balance and the soil properties. Not many table measurements were available. These data, however, tell us that the range of variability of the observed thawed depths is similar to the modeled ones. In addition, the data of the Middle plot, which, differently from the other 2, shows a significant slope gradient, confirm that the frost table depths generally increases as we move downslope.

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