Review of

Modeling the spatial pattern of ground thaw in a small basin in the arctic tundra

By Endrizzi, Quinton, Marsh

Such modeling approaches as described in this paper are greatly needed, since there is a great lack of knowledge on how regional scale variability affect permafrost dynamics. Furthermore, the paper is well organized and written. My initial excitement at the paper was disappointed, however. I have three major comments about the present study which should be addressed before publication. The discussion and conclusion will have to be substantially revised based on the outcome of the new data analysis. However, the authors will have a much stronger manuscript as a result.

Major comment #1:

Why model a catchment?

It is not clear why a catchment area is modeled in this paper, since modeling results are not compared with catchment scale observations. I suggest that it is convenient, but adds nothing to the paper. The paper is a more or less a theoretical study and, as such, must go beyond the studies that have been published already (for example the theoretical study by Bense et al. GEOPHYSICAL RESEARCH LETTERS, VOL. 36, L22401, doi:10.1029/2009GL039225, 2009). It would be of great interest for the hydrological community to show the applicability of the model using field data using measured data as input and for validation (whichever is available: runoff, soil moisture, ground water level, evapotranspiration,…).

Major comment #2:

The scaling issue: spatial pattern at what spatial scale?

Subsurface flow in the shallow active layer is determined by fine scale subsurface topography of the seasonal progressing thaw depth. The field measurements at this site show a high spatial variability of thaw depth ranging between 20 to 50 cm at the end of summer, changes in soil and hydraulic properties occur on even shorter length-scales. This stands in contrast to the resolution of the input parameters (DEM 1-10 m, climate data from a single location, more or less homogenous surface and subsurface characteristics). Given these coarse input parameters, it is not possible to model the high spatial variability of thaw depth on the required small scale (cms to dms scale). The authors do describe their method of accommodating for sub-grid spatial variability using a bulk parameterization. This is almost the opposite of exploring the effects of small-scale spatial variability on model results, however.

Major comment #3:

Main conclusion questionable due to model parameterization

The main conclusion is that topographically induced, spatially different thaw depth is related to subsurface flow. The authors state that this is the result of high hydraulic K of organic soil and thus high water content and high thermal conductivity of the peat. In addition, the conclusion is that the modeled low spatial variability of frost table depth is not a result of surface energy balance.

The model greatly simplifies the input parameters by neglecting snow cover and melt processes and spatially variably surface (snow cover, vegetation, surface roughness, albedo) and subsurface properties (soil texture, ice content, porosity).

All these parameters, varying in space and time, determine the surface and subsurface energy and water exchanges and can result in a highly variable thaw depth.

Suggestions:

Overall, the paper presents a broad and theoretical model sensitivity study, similar to the one performed by Bense et al. (2009, GRL). I thus recommend going beyond this study:

1. Use a catchment for which hydrological field data (runoff, groundwater level, soil moisture) are available. From the cited literature these data seem to be available from the author's study sites.
2. Perform an in depth sensitivity analysis of the model on a single pixel including the following parameters: snow cover, vegetation, subsurface ice content, surface features (hummocks).

Specific comments

Page 368

Title: The title should be specified- what spatial pattern at what spatial scale?

Line17: Treeless terrain-

There are no willows and shrubs?

Line 20: ..low thermal conductivity..

Only when dry, when wet it has a high thermal conductivity (Yoshikawa et al.), thus making it high spatially and temporally variable.

Line 25ff: .. hillslope drainage..(and overall in introduction section)

The reader gets the impression that these results from past publications are generally valid for arctic landscapes and hillslope hydrology. I suggest to clarify that these results are characteristic for these catchments. There is a wide spectrum of results from other arctic catchments and hillslope hydrology studies available from the Canadian (High) Arctic for example by Woo et al. and Alaska (North Slope including Imnavait creek, Toolik Lake, Kuparuk)..

Page 369

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

Rarely, if ever does impermeable surface correspond to 0°C isotherm (T freezing < 0°C)

Line 21: ..direction of flow, are highly variable..

Within 0.5 m of the ground surface, i.e. only constrained high spatial variability (at the small scale).

Line 22: ..therefore critical..

To my knowledge, this has not been proven or demonstrated in published literature. It may be a useful hypothesis for this paper. Suggest to change sentence to: We explore the idea that….

Page 370

Line 2: ..spatial pattern of snow disappearance..

Here in fact you describe that the assumption of homogenous thaw depth after snowmelt is not a good one.

Line 7: ..Subsurface flow…..greater thawing..enhanced evaporation of the ..ground surface

To my knowledge, there is no publication that has shown this as a unique relationship. In fact, for some sites the opposite is true: sites with high water content and groundwater level have smaller thaw depth compared to drier sites with greater thaw depth (as observed at one of our study sites, Boike et al. 2008, JGR).

Line 15: .. a fine resolution..

What is the fine resolution (see major comment #2).

Line 16 :..limited field observation..

Of what?

Line 26ff (entire paragraph until page 371, line 10):

This paragraph should be rephrased to list previous work without necessarily claiming to go beyond it. This includes the theoretical study by Bense et al. (2009, GRL), Schramm et al. (2007, JGR) and various papers by Pohl et al.

I do not agree with the statement "…or address with simple methods…" since the method used in this paper is comparable to the Hinzman et al. publications.

Page 371

Line 18: .. 1005 km2..

I would not consider 1005 km2 a small catchment.

Line 21: .. remotely sensed data..

What remotely sensed data are used in the paper?

Page 372

Line 5: ..diameters of between 0.4 to 1 m…

The topography varies at the 0-1 m scale with vertical differences at the same spatial scale as thaw depth. See major comment #2.

Line 12: ..Other studies…

These citations suggest that hydrological data should be available for the catchment.

Page 373

Line 6: ..It is also expected…

I consider this assumption a great weakness of the paper. Different snow depths on the slopes will contribute significant different flows, especially during major hydrological events. Depending on the snow cover duration and thickness these effects may perpetuate throughout the summer. Furthermore, the snow cover has a direct effect on the soil's thermal regime and thus the active layer thaw depth.

Data on snow cover and thaw depth variability should be shown to give proof for the assumption or alternatively, the effect should be explored in a sensitivity test.

Line 10: .. vegetation cover

Shrub studies (for example Blok et al. 2010. Global Change Biology 16, 1296–1305, doi: 10.1111/j.1365-2486.2009.02110.) have shown the importance of shrubs for the thermal regime and active layer thaw depths. Differences in thaw depth between shrub and non-shrub areas can be very significant (also due to the entrapment of snow).

Similar as suggested for the snow cover, this effect should be explored in a sensitivity test (if field or remote sensing data are not available).

Line 16: ..focused in the inter-hummock..

What bias is introduced when measurements were only performed in the inter- hummock zone?

Lines 22ff:

The model is run using a 1-10 m DEM. Please provide details on the DEM- grid cell size and vertical accuracy? If it is > 0.5 m then what is being modeled?

A 1-10 m model scale does not reflect the microtopography of the hummock.

Page 374

Line 15ff: ..heat equation..

The model includes sensible and latent heat effects, but no further information is given how the ice content in the ground is affecting the ground heat flux. Specifically, It is not clear from the method section what ice contents and porosity values were used.

This would be another model parameter for a sensitivity analysis.

Page 376

Line 10, line 25: …assuming constant albedo and surface roughness..

The variability of surface characteristics (albedo, aerodynamic resistance) due to variable snow cover and vegetation should be discussed (modeled or measured). See major comment # 3.

Page 377

Line 10:..effect of vegetation not considered..

See comment above: Due to this, the effect of vegetation surface characteristics on turbulent heat fluxes are neglected.

Page 379

Line 12 ff:..”inactive porosity”..

How important is this effect with respect to assuming homogenous surface and subsurface (mineral soil) properties?

Page 381-382

Results and discussion

Generally, the results are presented for only one datum at the end of the summer period (averaged over the simulation time). No seasonal course or comparison with observed data is shown. Thus, it is difficult to interpret end of the summer water table depth or averaged fluxes over the entire period.

The lack of measured data (or references to published data) there is no knowing if the magnitude and direction of the modeled data are correct.

I suggest showing the seasonal course or balances (for example energy or water balance).

Page 384

Line 26ff:.. north facing slopes and greater thaw..

This contradicts the results of Carey and Woo (2001; mentioned on page 369): near surface moisture is higher on north facing slopes as a result of shallow thaw depths.

Page 385

Line 2ff:..evaporation higher on south facing slopes..

Can this be quantified? If the water level is close to the surface how can this have an important effect? Figure 3 shows little variability, only about 15 cm within non stream areas.

Page 386

Line 6ff: …spatial distribution of the ground heat flux…

Again, the timing is neglected: south facing slopes will be snow free and thawing earlier before north facing slopes. The shortwave radiation varies not only spatially but also temporally due to snow and vegetation properties.

Page 388

Line 21:..fine resolution..

I do not agree with the term “fine resolution” since the subgrid variability of thaw depth is not addressed.

Figures

Figure 4

The pattern of thaw depth at two different times does not show much variability over time, i.e. the same pattern is shown for different times. It also shows the limitations of the spatial resolution. The thaw depth more or less reflects the ground heat flux pattern and thus the peat surface.

Figure 5.

What determines the variability in net short wave radiation?

Figure 9.

The differences between model and measurements are significant (starting in June) considering that generally shallow thaw depth. The measurements show a high spatial variability (range from 20 to 50 cm at end of summer). Furthermore, why is there a higher variability of thaw depth for the late summer model results?