Review: Future permafrost conditions along environmental gradients in Zackenberg, Greenland

S. Westermann, B. Elberling, S. Højlund Pedersen, M. Stendel, B. U. Hansen, and G.E. Liston.

General Comments

This paper sets out to his paper sets out to predict future permafrost conditions for a 4-km long transect of elevation and vegetation change through the Zackenberg valley along the ZERO-line. Considering the depth of research associated with Zackenberg Ecological Research Operations, with 9 or so related papers published on permafrost – carbon dynamics, this is a timely paper. From a methodological perspective, the stepwise downscaling approach taken in this paper enabling 10 m grid cells is intriguing, the inclusion of snow redistribution modeling is important, an consequently the overall approach is of great interest. Model parameterization approach is thoughtfully undertaken, clear, and essential physical elements that contribute to local ground temperature variation are considered appropriately. For example, the elegant use of the peak summer NDVI to compute n_t. The overall structure of the paper is fine as long as it is aligned with the stated purpose which is to present simulations for the transect. Whereas simulations along the ice-edge to sea transect are not tied into the purpose and objectives, and modeling for that transect does not include the degree of downscaling used along the ZERO-line, any related sections should be removed and the title adjusted. There are, however, considerable problems with this paper that relate to field data, specifically how they were used to set model parameters, and the manner in which they are used to verify modeled contemporary conditions. As this downscaling approach predicts permafrost thermal evolution at a local scale, adequate field data are essential; however active-layer data are limited in extent with respect to the possible variation along the ZERO-line transect, no data exist to validate modeled 1-m ground temperatures, and shallow borehole data are not used convincingly to validate modeling. Rectification of these problems, which are outlined in Specific Comments, requires major revisions and additional field data, and as a result the paper should be rejected as it is. However, I strongly suggest that the authors submit a revised paper, as this work will make an important contribution with additional effort.

Specific Comments

Abstract

Clear abstract, provides a concise summary of the ZERO-line related work as presented in the text, but does not mention the ice edge to sea transect.

P 3908, L114-15: “permafrost remains thermally stable until 2100 in most model grid cells” should read “permafrost remains thermally stable until 2100 in all model grid cells”. Fig4 shows10-m temperatures all remain below 0°C until 2100 and Figure 5 shows that active-layer thickness does not exceed 1.2 m (though fell is not included). Therefore permafrost as a condition remains stable, it is either there or is isn’t. It is best not to confound increased active-layer thickness with “permafrost stability”.

P 3908, L15: “Thaw threshold is exceeded” could mean a few different things. I think what you mean to day is that annual mean ground temperatures at 1-m depth were greater than 0°C. However, the paper should point out that active layer depth exceeded 1-m decades earlier in some vegetation classes when the annual mean 1-m temperature is still below 0°C. “Thaw threshold” is used in the introduction, results and discussion, each with a different implication. More appropriate terminology should be adopted.

1. Introduction

Fine for the most part, except the ice edge to sea transect is not mentioned.

2. The Zackenberg site

Though there are already 7 figures, this paper requires a figure of the study area indicating the location of the model grid cells or at least the ZERO-line, in addition to topography, NDVI/vegetation classification, and locations for CALM grids and boreholes. It is difficult to assess the appropriateness of the field data without this critical study design information.

P391, L18-20: “This increasing thickness of the active layer represents only a fraction of the permafrost degradation taking the high ice contents (40–80%) into account.” Is this % excess ice, gravimetric moisture content, or volumetric moisture content? Table 1 indicates most sites have a sand substrate with a 40% volumetric fraction of ice, which is saturation, so it appears that most sites to not have excess ice. Consequently, increasing active layer thickness would be equivalent to the amount of permafrost degradation. In other words there would be little thaw strain. L18-20 would be true if there were excess ice and significant thaw strain.

P391, L21 to P3912, L11: Includes some repetition that can be tightened up.

Write a subsection that details the field study design, including where field data was collected and why so that the reader can better assess their utility and appropriateness for model parameterization and validation. Specifically:

- Why was a transect used?
- What are the site conditions around the boreholes, where are they located, and what depth intervals are temperatures measured at, what is the snow depth?
- Were any 1-m ground temperatures measured, and at what locations?
- What is the range of measured snow depths within each vegetation classification?
- What is the range of active-layer depths along the ZERO-line?

Clearly, the ZERO-line transect was used because data were available, but that reason alone is not adequate. The paper must demonstrate that use of a transect was a part of the study design because it allows testing of specific hypotheses related to differences in elevation and vegetation. Otherwise, one might argue that if elevation change was not important, it might have been better to choose completely random sites to parameterize and test the model. Is variation between vegetation classes greater than variation due to elevation change alone? In addition, active-layer soil moisture, an important control on ground temperatures, varies substantially along the ZERO-line gradient, but its importance to variation of the ground thermal conditions in space and through time is not addressed.

Very generally, ground temperatures are influenced by the surface conditions in a radius approximately equivalent to 3 times the depth the temperature is measured at. Thus with a 30-m radius, nearly a third of a CALM grid would influence the 10-m ground temperature. It would be very good to know if the boreholes were located in homogeneous settings and represented two distinct vegetation classes, or whether the boreholes represent composite classes. This also relates to snow conditions, as one site has “more regular snow” and the other is “at a site with a snowdrift”. If the snow drift is not extensive, it may not have a substantial influence on the 10-m temperature. Further, as you rightly mention on P3924,L19-20, deeper ground temperatures are influenced by longer-term temperature forcing, and consequently there is a depth of zero annual amplitude (DZAA) above which ground temperatures reflect short-term trends. Temperature change below the DZZA is related to long-term climate forcing. Based upon the cold permafrost temperatures, it would not be surprising if the DZAA was below 10-m, but the DZAA is not stated. Other studies that model permafrost thermal evolution, get around time lag effects by comparing a measured annual mean temperature profile with a predicted profile (e.g., Burn and Zhang, 2009, doi:10.1029/2008JF001087). As the model run begins in 1960, this kind of validation approach should be possible. Such validation would give much more strength to the model parameterization, rather than simply stating that the 10-m borehole temperatures are within the range predicted across all vegetation types. With such a high resolution model we should be able to see specific outcomes related to vegetation type and snow conditions, rather than generalized outcomes that one might expect from a non-downscaled modeling scheme.

1-m ground temperatures are modeled, so it seems necessary, or at least quite beneficial, that some near-surface ground temperature measurements should be compared against modelled results. The active-layer is treated this in this manner, and it should be done with 1-m ground temperatures. The 1-m data should represent all vegetation classes modelled. It would be even better if this shallow-ground temperature dataset also included a thin snow site and deep snow site. Then specific questions can be addressed such as: Is the model better at predicting sites next to sea level, or at 1040 m a.s.l.? Is elevation more important to ground temperature variation than snow depth overall?

Much of the novelty in the modeling approach comes from incorporation of MicroMet/SnowModel, and the agreement between predicted and observed snow depth observations is not quantified during validation. It seems that manual snow depths were determined within the CALM grids, and this implies that snow depth at fell was not assessed. In addition, as many CALM grids are established on relatively level topography, thus the effects larger snow drifts related to topography may not be adequately treated in the paper. Is there any significant difference in snow depth along the transect, or between ecosystem types?

Active-layer data are critical to this paper and they are derived solely from the CALM grids. This is problematic because the Fell classification, which may ultimately thaw more deeply, is not included, and because the within grid variation in a given year may or may not be representative of the variation along the transect. 4-km is not a great distance to cover, and it would be possible to determine a range of active-layer depths within several of the model grid cells for each of the four vegetation classes. The CALM data are used to validate the predicted active-layer increase over time (as is done in the paper), but the additional sites would be used to test model’s ability to predict the spatial component of active layer variation., i.e., are there any patterns associated with change along the transect? This test seems necessary due to the model’s fine resolution and incorporation of processes that vary at the local scale.

3. Modeling tools

P3913, L15: Without a map of the study area, it is difficult to assess what part of the ZERO-line was modeled.

3.1 The permafrost model CryoGrid 2

P 3915, L15 to P3916,L6: Tighten up this paragraph. Figure 2, mentioned at the bottom of the paragraph shows the relation described by Equation 3 at the top, but what is the degree of explanation (the R^2, the p-value)?

P3917: The wetland class includes grassland and fen, but as fens are in wet depressions with a saturated active layer, they probably have a distinctly different climate-permafrost relation than the grasslands that are likely more well drained (based upon the study area descriptions given on P3912 this seems true). Unless shown otherwise, the fen is likely an end member of the spectrum of climate-permafrost relations and it should probably be treated separately. Similarly, *Salix* snow-bed delineates a class that is likely thermally distinct from others because of its relation to snow rather than soil moisture, so merging it with wetland is a problem. This uncertainty is glossed over to some degree, and the implications on spatial variation of ground thermal conditions are not discussed at the end of the paper. These merging problems arise, in part because the classes have to be differentiated according to NDVI values that are subsequently related to n_t, but NDVI is also influenced by soil moisture. Soil moisture adjusted vegetation indices such as the SAVI (Huete, 1988, doi:10.1016/0034-4257(88)90106-X) or the new MAVI (Zhu et al., 2014, doi:10.1371/journal.pone.0102560) are probably better able to differentiate between vegetation classes, especially moist versus saturated soils, and it is likely that a strong relation can be established between either index and n_t. These alternative indices may be worth exploring.

P3918,L13-14: I assume depth to bedrock is not known from either borehole, otherwise it could be used here.

3.3 Modelling snow distribution by MicroMet/SnowModel

As discussed above, it was surprising not to see a quantitative comparison of modeled and measured snow depths. At bare minimum, since the area is probably wind swept, give the *shd* for each vegetation class since the snow depths will be close to these except where drifting is related to topography. The reader has no idea what the normal range of snow depth is in this region.

How well was snow depth replicated outside of the CALM grids such as at topographic drifts like the one above one of the boreholes?

3.5 Permafrost simulations along the ice edge to sea transect

This section comes as a surprise as it is not mentioned in the introduction. There are no data to validate any results, and the simulation does not include downscaling.

4 Model results

4.1 Comparison to field data

Given the scope of the modeling, the field data are largely inadequate.

- There seems to be no relevant field data for the fell classification. Consequently the validity of the total range of AL thicknesses and permafrost temperatures is questionable as the contribution by fell is unclear. Fell should be treated with caution and probably removed from analysis, validation, and prediction.

- Active-layer thickness; P3924, L7-18; Figure 3:
    - It seems very counterintuitive that active-layer thickness, a function of summer conditions, is presented and discussed in relation to snow. If there is a clear physical link between antecedent snow depths and active-layer thickness in this study area then it should be established and discussed. This relation is not commonly observed, but it can occur.
    - Figure 3 shows modeled and measured thaw depths are reasonably consistent over time within the CALM grid. An unanswered question is whether or not the downscaling approach was able to reproduce thaw depth variation along elevation and moisture gradients associated with the ZONE-line, controlling as best as is possible for vegetation/snow depth.

- Permafrost temperatures; P3924,L19 to P3925,L2:
    - What kind of surface/vegetation/snow setting does each borehole represent? Is 10-m depth below the DZAA?
    - Compare modelled results with mean ground temperature profiles from the deep boreholes. To say that the two point measurements are within a range of modeled temperatures is reasonable for coarse resolution models, but is not a strong argument for a model designed for high spatial resolution predictions. It would be better to be able to say that modeled output closely fit measured ground temperature data. How good is the agreement under specific conditions?
    - Evolution of highly variable 1-m ground temperatures is presented in the next section, but no field data are presented for validation.

4.2 Evolution of active layer thickness and ground temperatures

P3925,L9-11: A “weak spot” is not a description that adds to our understanding of the evolution of ground temperatures. See earlier comments about permafrost “stability” and increased active layer thickness and exceeding the “thawing threshold”.

What are the vegetation/snow conditions associated with the “weak spots”? This should be answerable due to the nature of the model.

P3925,L20-22: No, it cannot be due to high ground ice content at the top of permafrost because all vegetation types were assigned the same volumetric ground ice content at the top of permafrost (see P3918,L10-13). It should be due to the saturated wetland active layer, that year over year has a much higher water content than the next wettest vegetation type *Cassiope*.

P3925,L24-25: Have you any field data to support these modeled results?

4.3 Ice edge to sea transect

Probably remove this section.

5. Discussion

5.1 Model uncertainty

A big caveat is presented, that the results should be considered a first order approximation. Considering the light amount the field data used to parameterize and validate model results, this caveat should be emphasised in the abstract, and conclusions.

5.2 From model results to permafrost landscape development

Readers want to know if the scaling strategies from GCM to plot scale enable improved understanding about the spatial variation of thaw depth and ground temperatures, and their co-evolution over time? Rather than producing overall ranges of modeled results, such as from a sensitivity study or from sub-grid estimates based upon a coarse model, readers are interested in knowing if the downscaling enables more specific questions to be addressed, such as:

- How good was the temperature fit between snow drifts and bare-blown locations?
- Was the ground thermal regime in each vegetation class reproduced satisfactorily?
- Are there particular vegetation/snow conditions associated with the highest/lowest increase in active-layer thaw or ground temperature increase?
- Was snow depth the greatest driver of spatial variation of ground temperature or soil moisture, and at what scale?

Unfortunately, the field study design prevents many of these questions from being tested, making it harder to demonstrate the added benefit of undertaking this downscaling approach.

P3930,L5-8: No evidence of excess ground ice has been presented, so this text is probably not relevant.

P3930,L14-15: “the spatial variability of ground temperatures to a large extent caused by spatially variable snow depths”. No analyses are presented in a figure or statistically to support this statement, and there is no line in *Model results* connecting ground temperature variation with snow depth. However, Figure 3 suggests that soil moisture is probably important to thaw depth and it is therefore probably important to overall ground temperature variation.

P3931:
The model approach should also capture within ecosystem class differences related to spatial variation along the ZERO-line.

Simulated ground temperatures are not adequately compared with borehole data.

As discussed earlier, '"Weak" spots' is not informative. "Onset of permafrost thaw" is not indicated by when the annual mean temperature at 1 m is above 0°C, it happens earlier when the increasing active layer thickness includes that depth.

The final conclusion is not clear, and perhaps this is due to wording. Why is it important that the range of temperatures due to spatial change is near to the amount of increased ground temperature change? Do you mean to say something like: The projected increase of ground temperatures until the end of this century varies spatially within a few kilometers according to ecosystem class, ranging overall from about 3 to 5°C. This conclusion regarding ground temperatures does not have strong support because of the validation approach which does not include fell, and the lack of clarity of the representativeness of the borehole sites. Also, is this 1-m or 10-m depth? Rather than K, °C should be used as it is throughout the paper.

This model predicted a variable ground thermal regime, but it is not clear how that spatial variation is divided amongst the various ecosystem classes.

The high-resolution model should allow for site specific questions to be addressed, demonstrating that the downscaling approach has the ability to simulate spatial variation of actual field measurements. If it can do that, then consequently, we would have a much improved understanding of future permafrost conditions and their variation along environmental gradients. However, only active-layer conditions were considered from this perspective, but in a limited way. As a result, the paper is not convincing. This modeling is next-level, modeling a high degree of spatial variation, and therefore requires a great amount of field data to support the increased spatial resolution.

Considering the comments above and the model's high spatial resolution, there are at least three options available for a revised manuscript, and the discussion in any case should explicitly treat the spatial dynamic associated with the distinct vegetation classes and range of snow depths.

1. Focus on the active layer. Much of the permafrost related work in this study area treats active-layer conditions and relation to greenhouse gas dynamics, and the active-layer field data appear to be the most extensive in space and time. With more intensive active-layer data collected from grid cells along the transect this coming field season, there would be strong support for any conclusions and the benefit from downscaling would be clear.
2. Rather than positioning the paper as predictive, explicitly treat the paper as a sensitivity study, and in addition to future air temperature forcing under multiple predicted trajectories, explore the implications of changing snow depth regimes that may arise from long-term increasing or decreasing future winter precipitation and/or vegetation change.
3. Maintain stated objectives, and collect more ground temperature data in addition to active-layer data.

Technical Corrections

P3908,L20: Change "discussed in the" to "discussed regarding the".

L22: Delete “as well as in the light of”.

L24 to P3909,Ll1: Clarify. Near-surface permafrost degrades because of significant deepening of the active layer, so provide threshold depth that should be referred to here to qualify a decrease in extent?

P3909,L5: change “assessment for” to “assessment of”.

L8: Delete”e.g.”

P3910,L11: Change “Zhang et al., 2013” to “Zhang et al., 2012, 2013”.

P3912,L20: Change “are dominating” to “dominate”.

P3913,L4: Change “sample” to “determine”.

L15-16: Change to “The different parts of the scheme and their interplay are described as follows”.

P3914,L13: Change “The shape” to” The characteristic curve”.

P3916,L7: change “accumulated” to “determined”.

L9-13: Change to “Whereas the acquisition date is close to the annual maximum NDVI values, it represents a single point in the time and there is strong seasonal and interannual variability in plant growth and consequent evolution of NDVI values (Tamstorf et al., 2007). While this error source is hard to quantify”

P3918,L1 change “oriented at” to “derived from”.

L8-9: change to “The volumetric organic material contents are low in all classes (5 % or less)”.

P3919,L27: Change “too high albedo values” to “albedo values that are too high”.

P3924,L24: Change “the meteorological” to “at the meteorological”.

P3927,L12: Change “thus e.g. reproduce” to “thus reproducing”.

P3928,L11: Change “it must remain unclear” to “it remains unclear”.

P3930,L11: Change “could capture” to “captured”.

Figure 4: Delete the number 38 on the figure.

Figure 5: Heath is classified as *Cassiope* in the rest of the text.

Figure 6: Delete the number 40 on the figure.

Figure 7: Probably do not need this figure.