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Interactive comment on "Permafrost degradation risk zone assessment using simulation models" *by* R. P. Daanen et al.

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We would like to thank both reviewers for their comments on our paper, if we are invited to submit a revision this will be much better than the original.

Responds to referee M. Lehning:

We are sorry that the reviewer seems to have missed the overall goal and theme of our paper, we will make this clear in the revisions. This is to discuss how information from regional climate modeling can be related to risk planning in areas with permafrost. We do not test the validity of the regional climate model or the validity of the permafrost model, this has been done elsewhere (Stendel et al. 2007). The permafrost model is the tool used to develop the relation between climate science and engineering on

permafrost. We do not attempt to show the latest developments in modeling and that is why some of the details were left out of the paper, we will address more details where needed in the revised manuscript.

The validation of modeled data with observed data cannot be accurate in the traditional way of model validation. The RCM is driven by a coupled GCM, which implies that (apart from the starting condition) no observations whatsoever go into the model results. A model which is able to depict the statistical properties of today's climate can therefore considered as "good" But, since no observations of El Nino etc. find their way into the model data, we cannot expect that the weather pattern of a particular day is reproduced. However, the statistical properties (in other words: the climate) are reproduced very well, and this is what matters when we talk about the fate of permafrost. Some other literature on this subject (we will make sure that proper references are made) (Mernild et al. 2010). The most important key in the permafrost modeling exercise is the change from its current temperature to a future temperature and how this will affect planning of infrastructure and how this is affected by soil conditions. It is true that each individual construction site will have to be evaluated on its own based on its own design criteria. The tool we provide is a structured assessment of thermal stability based on a climate change projection. This tool will also be useful for a larger scope of cost assessment and planning on a regional scale. We have an objection to the last few sentences of the reviewers overall comments. He states that "The authors state themselves that the risk assessment is not really useful for local planning." This is not true, or at least was not the intention. We definitely find the risk assessment methodology very useful, especially at local, site specific scale. The challenge is that the PTP assessment used in this paper is on a larger scale (25 km), and thus, as we write on page 1036 a higher resolution PTP evaluation is desirable and underway. Nevertheless, the PTP criterion is only one element of the risk assessment, and even with the regional PTP estimate, the risk assessment methodology is far better than what is presently available to entrepreneurs in Greenland (no information).

We would also like to point out that the comment about the climate model represents "a qualitative indication of what may happen." That is a fact, as we are considering only one realization of one climate model, using one scenario. Unfortunately that is what is presently available for Greenland (We are not aware of other 25 km climate model data that covers Greenland). We are working with a geographical area that is not gifted with a multitude of available climate models and scenario runs. But we need to point out that we are aware of this limitation, and that other simulations are in the pipeline. We are currently working on a 5 km simulation, but this is not available now. A last point to the general comments we want to add is that the overall effect of climate warming is known in general. To our knowledge this is the first simulation of its kind for Greenland. Anything previous has been global simulations at completely different scales. So if nothing else, this paper is justified by proving the general assumptions of climatic impact on permafrost degradation in Greenland. We will make this point more clear in the paper.

p. 1023 17: okay

p. 1024 13: This point is very much appreciated. We realize that we need to be more precise about the role of our modelling tools in the risk assessment. We hope this will also assist in conveying our main message more clearly and not distract the focus to the details in the models used. Therefore we have reworked this part considerably to reflect on the reviewers concern.

p. 1024 28: okay

p. 1025 9: we will expand this sentence in the revised version of the manuscript.

p. 1025 14: Thank you for pointing out that supper saturation with ice has a strong effect on the soil properties. The collapse of super saturated soils will in most cases lead to wet surface condition and small total water content in the ground. The stable moisture content in the model is however not necessarily a problem, since permafrost degradation can also drain a soil within the same grid point (25km). On average for

the region there may be very little change. The collapse of ice rich permafrost also affects the overall thickness of the sediment and can therefore underestimate permafrost degradation. In order to include such microscopic effects as changes due to settlements and drainage, very detailed information must be available, which may be the case in research applications, but this will seldom be the case in practical applications. Furthermore it should be spatially very variable, depending on slope, aspect, vegetative cover etc. As we look at – in this context – low spatial resolution, we have focused on macroscopic effects and thus neglected these micro scale processes.

P. 1026 11: we understand that snow modeling is crucially important, but not for this paper, because we deal with larger scale dynamics. The errors between observed and modeled are there because the model receives the data from a climate model that has its own storm pattern. This pattern match the statistical snow pattern very well, however some years will be underestimated and some years over estimated. Observed data in the figure is a point observation and does not represent distributed snow cover, drifting may have contributed in redistribution of snow at this point. Other data also suggests that the observations presented are the result of drifting. Like the DMI 04216 station.

p. 1029 24: okay

p. 1030 3: okay

p. 1030 14: The sentence: 'Thule Air force base is located in a transitional area from low risk to high risk' leads up to the 2.5 m cut off value we choose for this study. It is not our intention to claim this is a result of our study. The result is that most of the settlements in Greenland fall within the high risk area using the set parameters. We will make this point more clear in the revised paper.

p. 1034 13: The temperature in the lower regions of the profile is affected by the bedrock nearby that makes the ground warmer, due to faster warming in the summer. This particular small sediment basin has this problem even if the surface is simulated correctly the 1d model has difficulty simulating the deeper temperatures. Larger basins are more easily simulated in one dimension.

P. 1036 10: We would like to rewrite this portion of the text so that it becomes clear that the risk assessment tool is more than the PTP. The risk assessment tool also includes site specific information of material and ice content that make the risk tool valuable on a small scale and for a first assessment. When the design phase would include construction on frozen sediments and more rigorous simulation of the frozen sediment would be needed.

Responds to anonymous reviewer #1:

1) The focus of the paper was on the coupling between climate projections and engineering on permafrost and not so much on the details of the modeling, but we recognize the importance of the model in the process of relating the large scale dynamics with the smaller scale dynamics. In the revised paper we will add more details on the model and its setup. Obviously, we have not been clear enough in stating the theme and objective of the paper. See also our reply to M. Lehning above.

2) The thermal conductivity is mentioned in the text as varying between frozen and unfrozen, as a geometric mean based on the liquid water content in the soils.

3) The snow cover is integrated in the finite difference scheme; the snow accumulates and ablates linearly based on the monthly values given by the regional climate model. The thermal conductivity of snow is related to its density and this was fixed at 0.25 kg/m3. We will include this information in the revised document.

4) Soil classes were used to relate regions to thermal soil properties, because these soil classes identify local thermal conditions. The most important variables are soil organic matter, sand, silt, clay and moisture content. The numbers presented in table 1 are estimated based on general experience. On a 25 km resolution there are numerous sets of numbers that can be picked. The numbers in table 1 are realistic and provide a rather cold permafrost scenario that in some cases will produce a favorable match

with observations. The bedrock simulation is a contrasting simulation that provides the warm end of the spectrum using the same forcing temperatures. For most of the region (25km) ground temperatures are expected to fall between these two extremes.

5) We will provide a better explanation of the goal of the paper in our revised version. The paper was not intended to provide a state of the art model, but rather a concept of linking climate projections to engineering applications. The model is physically based and rests on principles that include heat transfer in the active layer and permafrost. Most models use a simplified approach such as the Stephan solution, which ignores important liquid water characteristics. Based on this physics we believe this model is better at prediction of future permafrost thermal processes than many other models. The improvements that were mentioned are not expected to dramatically change the average permafrost conditions at the scale we are simulating. The smaller the scale of the simulation the more important processes such as hydrology, vegetation, and snow dynamics become. In order the become more accurate there needs to be also better input in terms of a digital elevation model, more detailed soils map, vegetation map and dynamics, precipitation etc.

6) We will look at the structure of the paper and see how we can improve on it.

7) Sorry for not deleting this!

P. 1022 22: okay

p. 1023 1: Permafrost growth is referring to the fact that currently there is permafrost present, so the conditions are favorable to permafrost development.

p. 1023 10: okay

p. 1023 13: okay, will be fixed

p. 1025 4: That is true, but the model does not handle unsaturated conditions as a separate variable.

p. 1025 6: This sentence will be fixed. Yes, the thermal offset is important in this statement.

p. 1025 16: The equation will be adjusted to its proper representation as in the model

p. 1026 3: okay

p. 1026 15: okay

p. 1026 17: okay

p. 1027 7: okay

p. 1028 8: okay

p. 1028 13: 10-50 % permafrost cover will add this to the new version

p. 1029 21: In our revisions we will make it clear that this is the crucial component of the paper, using two scenarios of permafrost simulations that, when combined, will help make better engineering decisions for the future. The bedrock simulation represents the warmer scenario.

p. 1030 19: okay

p. 1031 Sect 2.4.1 We will re-arrange to make it fit better.

p. 1032 5: This comment relates to the three dimensional nature of heat transfer even if the surface is well simulated in a smaller area the larger area influences the deeper temperatures. In this part we refer to the disturbance and bedrock effects within the larger surface area. We will rewrite this portion of the paper to include a better explanation on this section.

p. 1033 15: This is the part of the profile that starts at 4m below the surface till the start of bedrock.

p. 1033 19: okay

p. 1034 15: okay

p. 1034 25: okay

p. 1036 10:okay.

We will add Marchenko 2008 and Tipenko 2001 will be updated.

Table 1: will be fixed

Fig 2: okay

Fig. 5: add cutoffs in revised paper. The sand silt clay portion are standard classifications. The ice content is high when the pore volume is exceeded and it is low when the ice content is below the pore volume.

Fig. 7 and 9 will be updated appropriately.

References Mernild SH, Liston GE, Hiemstra CA, Christensen JH. 2010. Greenland Ice Sheet Surface Mass-Balance Modeling in a 131-Yr Perspective, 1950-2080. Journal of Hydrometeorology 11: 3-25.

Stendel M, Romanovsky VE, Christensen JH, Sazonova T. 2007. Using dynamical downscaling to close the gap between global change scenarios and local permafrost dynamics. Global and Planetary Change 56: 203-214.

Thank you very much.

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