**The Cryosphere
Discussions**

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

Anonymous Referee #1

Received and published: 30 May 2011

The paper presents model predictions of the future thermal regime of permafrost in Greenland, based on the output of the Regional Climate Model HIRHAM and the previously published GIPL 2 model. Furthermore, the authors propose an impact assessment strategy for construction on permafrost. The topic is of relevance for The Cryosphere and the paper is well organized and written. My main criticism is the lack of crucial information on model setup and explanation for the choice of model parameters.

Major Comments:

1. Details on the model setup are missing, especially initialization procedure and

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choice of lower boundary condition.

2. How is the thermal conductivity calculated? Does it switch from unfrozen to frozen at $0°C$ or is there a parameterization in terms of water and ice contents as given by the freeze curve used? The same question would apply for the heat capacity (although the parameterization is straight-forward here).

3. How is the snow cover treated in the permafrost model? In particular, which thermal conductivity is used? Is the snow pack treated explicitely in the finite difference scheme, including build-up and ablation? None of the cited papers seem to contain this information.

4. Why do sediment groups 1 to 10 have these layering and thermal properties? The authors state that they are "based on experience ... and measurements form numerous permafrost regions in the world", without citing previous studies that could support this rather general statement. I agree that linking soil or landcover classes to a set of thermal properties is indispensable for large-scale permafrost modeling. Therefore, the values presented in Table 1 could become an important point of reference for future studies, if the autors can provide some more rea-soning how and why they are linked to FAO soil classes. I consider this issue important enough that it deserves its own subsection.

5. I am missing an explicit discussion point on future model improvements – it is only mentioned briefly, that a hydrological model is missing. At other locations, the authors mention subgrid variability, snow redistribution, etc. So, where would the authors file the results of this study on a scale ranging from proof-of-principle to accurate prediction of future state variables? What are key improvements to advance further on this scale?

6. I suggest to split Sect. 2 in two sections, one for the methods and one for the results.

7. I suggest removing the section between Acknowledgements and References. I have problems connecting it to the rest of the paper...

Minor Comments:

P. 1022, L22–23: Awkward sentence, rephrase.

P.1023, L. 1: What is meant by "permafrost growth"? Shallow active layer? Or growth of the soil surface through decomposition of plant material?

P.1023, L. 10: until now

P. 1023, L. 13ff, P 1024, L. 19ff, L. 25ff: Simply refer to a bedrock and a sediment model run, not to a "colder" and "warmer" run. At the appropriate location, it should be explained that sediments are colder due to the thermal offset, but mentioning it three times is unnecessary.

P. 1025, L. 4: That's only true if the pores are filled with water/ice.

P. 1025, L. 6ff: Apart from being an awkward sentence, I don't understand the logic (although the statement might be true). Is this based on model runs with constant thermal conductivity, or how do you separate from the thermal offset effect?

P. 1025, L. 16: For $B < 0$, the unfrozen water content has a singularity for $T = D$, so that D is not the freezing point of the material, at least not exactly. For the parameters presented in Table 1, $\theta_l = \theta_{tot}$ for temperatures in the range of -10^{-3} to $-10^{-5}°C$, so quite close to zero. Thus, I presume the effect on the calculation is negligible. For the formula to be exact, put

$$\theta_l = \theta_{tot} \frac{\delta^B}{(\delta + D - T)^B} \,, \tag{1}$$

where the parameter $A \approx \delta^B$.

P. 1026, L. 3: $D = 0°C$

P. 1026, L. 15: standard symbol for gram is "g"

P. 1026, L. 17: were

P. 1027, L. 7: Give the exact periods here already.

P. 1028, L. 8: warming

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P. 1028, L. 13: What's the criterion for ocurrence of sporadic permafrost?

P. 1029, L. 21: Why use the bedrock simulation? Construction on bedrock should be comparatively stable, no matter what happens to the permafrost.

P. 1030, L. 19: The PTP threshold should be clearly stated before results are presented. It could be moved to one of the two previous sections.

P. 1031, Sect. 2.4.1: In this form, the section has little to do with "validation on community scale". I suggest to move it up to the Introduction or the first paragraph of Sect. 2.

P. 1032, L. 5ff: I don't understand the reasoning. Vegetation is disturbed, so the model fits close to the surface (i.e. the region of disturbance), but not in deeper layers. Explain!

P. 1033, L. 15: What is meant by " in the deeper parts of the profile 4m till bedrock"?

P. 1033, L. 19: How is the gravimetric water content defined here? Is that water mass over dry mass? As water mass over total mass is maybe a more common definition, I suggest using "super-saturated with ice" or similar to avoid confusion.

P. 1034, L. 15: full citation for Olesen

P. 1034, L. 25: 3.7m

P. 1036, L. 10: 25km

Reference list: Marchenko et al. 2008 is missing.

Reference list: In the data bases accessible to me, Tipenko & Romanovsky 2001 appears as Tipenko 2001. The AGU recommends citing abstracts as "EOS Trans. . . .".

Table 1: Wrong units in columns 1, 5-8.

Fig. 2: Time axis labeling is confusing, years are enough. "Simulated 3" should be Simulated. Figure heading and figure caption are almost identical, so the heading could be skipped.

Fig: 5: What are the thresholds for Gravel/Sand and Silt/Clay to be high or low? The PTP threshold could be indicated, too.

Figs. 7 and 9: I can't see much in these diagrams for the periods when observations are available. The symbols are way too big. I suggest to make additional diagrams for

these periods.

Interactive comment on The Cryosphere Discuss., 5, 1021, 2011.

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