

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

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General: The paper presents the application of a permafrost model (GIPL) to all of Greenland with two applications: a) the development of permafrost as a result of climate change as predicted by HIRHAM; b) a risk assessment with respect to construction on permafrost. The text is in general clear and concise. However, the paper appears to have been put together too hastily and has a lot of unnecessary mistakes, which should have been caught by careful reading (e.g. of co-authors). Also, while the quality of the figures is good in general, the so-called model validation would require a completely different approach. I find it impossible to make a fair comparison between measurements and observations based on those figures but what I see is that the data are probably insufficient for a model validation and that they don't support the

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conclusion that the model is an adequate representation of local permafrost dynamics in Greenland. My main problem with the paper is, however, that it does not position itself clearly as a science paper (e.g. predicted changes in Greenland permafrost) or a technical report on how to judge permafrost degradation risk. Maybe because it tries to achieve both goals it does not convince in either one. While the climate change simulations lack a better validation and in my opinion can only be taken as a qualitative indication of what may happen (and the qualitative results are largely known – see statement in the abstract), the authors state themselves that the risk assessment is not really useful for local planning. Therefore and for the reasons detailed below, I suggest rejection of the paper.

Detailed Comments:

Abstract: The introductory sentence is too general and therefore does not add value. It is very important that the major science results are clearly stated in the abstract. Useful is not spelled “usefull”.

p. 1023, l. 17: too big not “to big”

p. 1024, l. 13ff: If you claim to have a “good estimate for air temperature”, you should present a validation for this. In my opinion, a 25 km model resolution is grossly inadequate to estimate climate for the critical permafrost areas, namely the steep fjord areas. Typically, local to regional predictions are made using a downscaling procedure. While an RCM with 25 km resolution may give you correct regional trends, in my opinion you can't use the RCM output as it is for doing the kind of permafrost predictions you want to do.

p. 1024, l. 28: The “colder run” terminology should be replaced because the reader automatically thinks about forcing when reading this expression. Just stick with the bedrock versus sediment terminology.

p. 1025, l. 9ff: You should explain this difference since this is an important feed-back

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mechanism.

p. 1025, l. 14ff: While this assumption is discussed later in the paper, it should already be mentioned here that this assumption is very problematic in particular if climate change simulations are made, which will certainly cause structural changes to the soil.

p. 1026, l. 11ff: While it is stated that snow is an important factor, the negligent treatment of snow is another critical point in this model study. The snow dynamics are not at all well represented by the model as shown in Figure 2. Since in some years the snow depth is underestimated by a factor 4!, the model predictions have to be regarded as not quantitatively reliable. The Liston snow model would for example offer a possibility for downscaling of meteorological parameters (as a possibility to have a downscaling module) and I highly recommend that also the drifting snow feature of the Liston model is used to have a more reliable simulation set-up. In general, the role of any changes in snow cover is not discussed at all and should be discussed in much detail in particular with respect to fundamental investigations presented by other authors (e.g. Zhang, Lütschg).

p. 1029 l. 24: Fig. 4b not "Figs. 4b"

p. 1030 l. 3-4: Variables were not "Variable where"

p. 1030 l. 14-15: This sentence is one example of where general knowledge is sold as a result from this study.

p. 1034 l. 13 ff: You are basically saying that you have to deal with a three-dimensional problem here without discussing of why the reader can assume that this only applies to this particular measurement location and is not a general problem of the permafrost soils in Greenland.

p. 1036 l.10 ff: Here you say that the tool is not suited for planning of individual projects (with this I agree) and later in section 3.2 you say that it should "help contractors to assess the local risk of permafrost degradation", which is contradictory to me.

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

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