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The Cryosphere
Discussions

Interactive comment on "Transient thermal modeling of permafrost conditions in Southern Norway" *by* S. Westermann et al.

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We like to thank Volker Rath for the comments on our manuscript, which helped to improve quality and comprehensibility of our study. We carefully considered all comments of the review. The answers are highlighted in bold:

This Manuscript deals with a new transient permafrost model, CryoGrid 2, which is described in detail, validated with observations from two profiles of shallow boreholes, and applied subsequently on the whole area of Southern Norway. The methodology used is state of the art, and the paper is in generally well written. I conclude that it can be published with minor revisions. The main section which needs improvement/reorganization is discussion. It is rather lengthy and a bit report-like. Though I

like the thorough discussion of the possible error sources and shortcomings, I suggest to condense it a bit and concentrate on the results obtained and the immediate consequences.

The discussion section is shortened in the revised version.

On the other hand, I would prefer to have also some discussion on how CryoGrid 2 relates to other comparable efforts, like GIPL2 (UoA), instead or complementing the comparison to the earlier version of the own model.

CryoGrid 1 is not an earlier version of CryoGrid 2, but it constitutes a different approach based on an equilibrium model. Both models have their justification (and will continue to have), depending on the available input data, computation resources and the desired output. In Sect. 5.2, we compare the results of CryoGrid 1 with the model results of CryoGrid 2, not the model itself. This comparison is indispensable, since CryoGrid 1 has delivered the only quantitative analysis of permafrost extent and temperatures in Norway so far.
Concerning the principal model physics, there is no difference between CryoGrid 2 and e.g. GIPL2. We have added a remark on this in Sect. 2: "Its model physics is similar to other widely employed permafrost models, such as GIPL2 (e.g. Jafarov et al. 2012): the change of internal energy and temperature in the ground is entirely determined by Fourier's Law of heat conduction,..." There are, however, differences in the employed parameterizations, e.g. concerning the snow pack. Here, it is not a priori clear if and how the model performances would differ, a question that could only be answered by a systematic model intercomparison study.

I think section 5.3 is superfluous.

Sects. 5.2 and 5.3 have been merged and considerably shortened.

Some particular questions/comments concerning the other sections:
Section 2.1: In P5350 L15ff the choice of the mixing law given in eq.4 is based on simplicity (simpler than, e.g., the geometric mean?), and on our general lack of understanding what the best model under permafrost conditions. However, it would be useful to have a short discussion of (a) how important is this choice, and (b) under which conditions there are large differences between the different mixing laws.

Considering that five different phases (air, ice, water, mineral and organic) can vary, there is no simple answer to this question, except for bedrock (more or less only mineral) where the choice of the parameterization does not matter. In addition, more complicated parameterizations of the soil thermal conductivity, such as the deVries-model employed in Westermann et al. 2011, require the knowledge of additional parameters, such as a "water recirculation cutoff", etc., for which almost no measurements are available, and agreement with other formulations will also depend on the choice of these parameters. The main shortcoming of the Cosenza-model from a physical point of view is that all phases are treated equally, which does not account for the geometry in which the phases occur (in soil: poorly connected discontinuous phase for mineral and organic due to the grain/matrix structure, and well-connected continuous phase for at least one out of air, water or ice). As mentioned before, a more physically correct model does not necessarily yield more correct values, if additionally required parameters are poorly constrained.
We deeply agree that a systematic intercomparison of the available parameterizations of soil thermal conductivities would be highly beneficial. However,

adding a paragraph on the theoretical differences between different parameterizations does not shed much light on the effect on the final model performance for ground temperatures, which is the target variable of CryoGrid 2. We would therefore like to leave this aspect to a future model sensitivity study taking into account all sources of uncertainty.

Section 2.3: It would be useful to the reader do give a few more details on the numerical solution. In section it would be nice to have a few words here which finite difference methodology was employed here including the implementation of boundary conditions.

We provide more details in Sect. 2.3, including where exactly the boundary conditions are applied.

Depending on the discretization method it may be advisable to use a smoother change of cell size (there is a factor of 4 at 1.6m). Why not simply use a a log-spaced grid? A better description of the discretization would also ease the understanding of the treatment of the snow cells.

The numerical error will depend on the gradient between adjacent cells. We have checked different discretizations and found only marginal differences in the results, as long as the uppermost layers, approximately down to 1m, are sufficiently resolved. A log-spaced grid would be possible, too.

Also it would be helpful to know the particular solver used in SUNDIALS for the nonlinear MOL solution.

Corresponding information added to Sect. 2.3

It may also be interesting to know what makes the solution so time consuming, and which accuracy are you are you aiming at?

The solution for a single grid-cell is not time-consuming, but if a large number of grid cells shall be computed it is. Basically, having a faster solver and/or a larger number of cores allows to cover larger areas with high spatial resolution. The target numerical resolutions are 10^{-4} (relative) and 10^{-6} (absolute accuracy around zero). In the light of the considerable parameter and model uncertainties, the numerical accuracy is insignificant.

Section 3: How are grid cells with different types of soil/bedrock are treated? Do you use an ensemble for all present types? What is the resolution of the NGU 2010 map?

For each grid cell, the sediment class with the largest aerial fraction within this cell is assigned. We have clarified this in the text (Sect. 3.2): "..., gridded to 1 km resolution by assigning the sediment class with the largest aerial fraction to each cell." In future runs of CryoGrid 2, subgrid variability of the sediment classes could be treated in terms of the aerial fractions for each class, which on the other hand would increase the computational costs considerably. The NGU map is a vector map based on topographic maps of scale 1:50 000, so that features smaller than 1km are reproduced (depending on the accuracy of the classification).

Conclusion: From Table 2 and I would not conclude that most of the data are well reproduced. A deviation of 1K is pretty large at 2m depth. But from my viewpoint there

is a basic problem with a validation at local sites without "local" parametrization, as the authors explained e.g. for Juv-BH4. I suggest to include a more general comment on this problem of "downscaling" in the conclusion section.

The comparison of model results and borehole data suggests, that the model results are somewhere in the range of ground temperatures within a grid cell as given by the natural variability. In this light, we do regard a deviation of 1K at 2m depth a good agreement, considering that no borehole-specific information is used and the model is forced with large-scale data sets, simplified ground stratigraphies and a uniform snow cover. A very similar agreement with borehole temperatures was e.g. reached by Jafarov et al. 2012 in simulations for Alaska with the GIPL2-model. We are aware that transient models perform better if they are calibrated with borehole temperatures for a specific site. However, this calibration can not be easily extend in space, so that little is gained for the model performance on large scales. We agree with the reviewer's comment on the scaling problem, which concerns a principal problem of all grid-based models. In the manuscript we have touched the aspect of subgrid variability several times, most notably in Sect. 4.1, which starts with a statement on the different spatial scales of point measurements and model results and where we discuss improved model performance if the sediment class is adapted to the true borehole conditions, and at the end of of Sect. 5.1., where the prospects of a subgrid representation of snow depth are presented. We have added a sentence to the Conclusion section (L. 25, p. 5382) to clarify the relation between modeled and measured temperatures:
"For most validation sites, borehole temperatures measured at the end of the 50-year model period are reproduced to within 1 K, which is in the range of ground temperatures within a model grid cell given by the natural variability."

I also suggest not to use the itemized style for the conclusion, but this maybe a matter of taste.

We have reduced the itemized style in the conclusion.

Interactive comment on The Cryosphere Discuss., 6, 5345, 2012.