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The Cryosphere Discussions

TCD

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Interactive Comment

Interactive comment on "Scale-dependent measurement and analysis of ground surface temperature variability in alpine terrain" *by* S. Gubler et al.

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We thank the referee for his construtive comments.

Obvious grammatical or syntax errors have been changed without further comment. The paper has been slightly modified since the first publication for TCD, consisting mainly of the insertion of a fourth ground cover type variable, which includes all footprints with highly heterogenous ground cover. Furthermore, the sine of the aspect was included in models (4) and (5) to model differences between east and west-facing slopes. The coefficients of both models have therefore slightly changed.

1. *The general aim of the paper is somewhat vague, stated as "to obtain and anal-*

yse a spatially-distributed and dense GST dataset in an alpine environment". This is achieved, but the problem is how this information is intended to be used for larger scientiïňĄc questions. It is not quite clear whether the paper focuses on technology/methodology/the theoretical issue of scale, or science. The discussion doesn't help much in this respect, providing more of a summary of results than real assessments and conversations with other studies elsewhere, especially outside of the Alps. As I see it, the obtained information should be stretched into either a more theoretical discussion of scaling issues, or towards consequences and recommandations for permafrost mapping and modelling.

The same issue was mentioned by the first referee, B. Etzelmüller. The paper was changed according to the suggestions made by both referees, including a restructuration, and especially rewriting the discussion. We think that the focus of the paper is a scientific one, and mostly addresses to diverse questions of uncertainties when using measurements to validate or calibrate models. These questions have been addressed in hydrology only recently, however for permafrost modelling, we are not aware of a similar approach of studying model uncertainties related to measurement uncertainties (in this case mostly coming from scaling issues). This is now in more detail included in the discussion.

2. With respect to experiment design, two spatial scales are investigated – intrafootprint (10x10m) and interfootprint (some km2). This choice is clearly relevant to address typical spatial scales and sources for variability encountered in pemafrost mapping, but maybe less so in a general discussion of scaling relationships. A choice has for instance been made NOT to investigate the range of spatial autocorrelation, maybe based on knowledge of these effects from earlier BTS measurements, but this could have been commented more. A more regular nested sampling design may also have provided interesting data.

The spatial autocorrelations have now been addressed, resulting in no spatial autocorrelations of the residuals of both models (4) and (5). Since the two chosen scales are relevant for permafrost research (as mentioned by the referee), we think that the sampling design presented here is very useful. Certainly other sampling designs would also provide useful data, depending on the questions the researcher aims at answering.

3. The sources of variability is another issue that could be discussed more. As I see it, the GST is a complex function, probably exhibiting deterministic chaotic behaviour due to the interactions between snowcover, vegetation, water availability, soil type and terrain, and their effects on the energy ïňĆuxes. Thus, any picture (of GST variability) that emerges at the footprint scale for typical ground conditions is very helpful to aid the researcher where to be cautious and to address this small-scale variability in GST in future model calibration or validation. Further, the parameterization of GCT is very brieïňĆy presented. I get the point with using a dummy variable, however the different GCTs that go into the model - what are they?

The sources of variability are discussed, in more detail, in the discussion. The definition of the GCTs was missing in the paper, thank you for reminding us.

4. The multiple regression models are a nice outcome of the study. However, we do not know anything about how general these relationship are. All data go into the model construction, and the power of extrapolation to new data therefore remains unknown. Especially the model for footprint variability may be promising, as this gives important hints regarding sources of error when calibrating or validating permafrost models. However, as with all regression models, the physical relationships between the predictor variables and the model outcome should be discussed.

To validate the model and test its applicability for prediction, a ten-fold cross validation has now been included in the paper. The results are found in Chapter 3.3. In our point of view, the physical relationships are discussed, however the discussion is expanded in this point.

5. An important point is the outcome of the study in terms of how these results may improve permafrost modelling. As ground temperatures are effectively ïňĄltered both

TCD
5, C315–C320, 2011

Interactive Comment

in the temporal and spatial domain, I am not certain that the consequences are large. Of course, large scale permafrost models are driven more by large scale forcings, such as MAAT, while smaller scale or more detailed grid-based models can incorporate the effects of terrain parameters other than altitude etc. Even with detailed information, incorporating effects of sub-grid variability, there will always be extrapolations involved in the models - with additional and unknown problems involved. Permafrost models should serve two purposes: screening and experimenting. In the ïňĄrst case, the initial screening must be followed up with more detailed ïňĄeld measurments anyway. In the second case, running a permafrost model involves an experiment - ïňĄrst directly relating to investigating energy ïňĆux - ground coupling (testing our knowledge of physical or empirical relationships involved in permafrost distribution and its transient behaviour) and then (providing such proper knowledge exists) for instance for coupling GCMs and long-term fate of SOC in permafrost areas. In doing experiments, clear knowledge of errors are vital, but the scale of the experiments are also important, and your type of data may be most relevant in high resolution studies. Thus, a clearer discussion of relevance, coupling to permafrost models (or other relevant environmental models, e.g. ecological models) etc. would beneïňĄt the paper and serve the interesting approach more justice.

We totally aggree.

Most of the small comments were adapted without further discussion as suggested by the referee. Some issues that we seperately address are mentioned here:

Page 309, Line 7: Why does it increase? Scaling issues are generally relevant (although too seldom properly addressed).

The importance of addressing scaling issues when comparing model outputs to measurements increases when models are used in terrain with high topographic variability since changes occur at much smaller scales than in homogenous terrain, due to more variable ground cover, water availability and infiltration, vegetation, etc. If the terrain is

TCD
5, C315–C320, 2011

less variable, we can assume that changes only occur at larger scales. However, the scaling issue is important for any model application.

Page 311, Line 3: Although interesting in itself, I am not too convinced that very small scale temperature variability is important in terms of permafrost modelling - except in terms of us relying on point measurements for model calibration. This is because permafrost at depth will filter both small scale temporal and spatial variations. However, such information may be highly valuable for biologists.

It is certainly true that permafrost at depth filters small scale variations, and thus the spatial variability of ground temperatures (GT) at depth are certainly smaller than at the surface, as measured in our study. However, similar data of small scale GT variability does not exist, and we can thus not prove that variability at larger depths is not existent. And it is still probable that very close GT measurements differ (more than the measurement precision) due to different local ground types, local water infiltration, etc. We think it is therefore important to keep the (un-)representativeness of measurements in mind when comparing model outputs to measurements, independent of the application.

Page 315, Line 9: PIT-tags could improve retrieval rates.

We agree. We have thought so during the first iButton campaign, however decided not to use tags in order to not pull the attention of hikers, farmers, etc. on the measurements. Luckily, in most cases this strategy worked well.

Page 318, Line 5: earlier you have argued for the importance of snow conditions. I realize that recording snow conditions for each of the ibuttons, or for each footprint, is not easily feasible and maybe not necessary considering the scope of the work. However, as soon as a model is introduced, how well do you think snow conditions correlate with the explanatory variables? Your results indicate that the combination of slope and aspect capture both effects relating to radiation and snow.

Snow height and snow water equivalent have been measured, at least at the footprint

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Interactive Discussion

Discussion Paper

level, whereever accesible. Further, snow duration can be estimated from the GST measurements using an adaptation of the Danby and Hik (2007) algorithm. Right now, a master student (Marcol Schmid) is studying the effect of the topographic variables on snow length, zero curtains, etc, and showed that the correlations with the topographic variables are high. The results of his study will however appear in a second paper.

Page 321, Line 4: Is the influence smaller in coarse blocks? I guess MAGST is 2 C lower in coarse blocks?

Yes, you are right. The sentence is reformulated for better understanding.

Interactive comment on The Cryosphere Discuss., 5, 307, 2011.