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The Cryosphere Discussions

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 structure of the paper should be improved, following standard scientific papers.

Chapter 2 should be "Instruments and methods", with sub-chapters "Study site", "Instruments", "Experimental design", "Logger placement". "Campaign automation" could be deleted, it does not contribute much to the message of the paper. In addition, the calculation scheme of MAGST and variability in footprints etc should be a paragraph in the method chapter. Chapter 4 should be "Results", including quality and variability of the measurements.

The structure has been changed according to the suggestions made by the referee. The subsection "Campaign automation“ was deleted. However, a small part of it was put at the end of subsection "Instruments“, since we believe that the software development to facilitate (re-)programming in the field should be mentioned in this paper. Methods and results were separated and arranged differently.

2. Spatial autocorrelation (SA): You mention on p. 318 that SA was not taken into account. I would say that the paper would improve if you would have included an analysis on this matter, e.g. within a footprint. It is ok that Nelson et al 1998 came to this conclusion that SA did not matter, but you could make a check here.

SA of the model residuals was now included (Chapter 3.3), showing that the residuals are not autocorrelated, and thus the variability of MAGST can be explained only by the topographic variables. We thus came to the same conclusion as Nelson1998.

3. Snow issues: You mentioned snow as an important issue several places but did not show any quantifications. You did not have any information about snow depth at the footprints as this would be easily obtainable either using iButtons along a pole (like Lewkowicz's PPP paper) or through a field visit during late winter (snow depth). You may put in a sentence to clarify this issue.

Snow depth and snow water equivalent measurements were taken 3 times during winter time at several accesible footprints. These measurements confirm the statements about the effect of snow on GST, and support the statements made about isolating snow covers. This is now briefly mentioned in the paper (Chapter 2.3 and 3.1).

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4. Calibration or validation of small-scale permafrost models are of course difficult based on GST measurements, especially of you have 4 km grid cells which in the Alps or other high mountains would include hundreds of m in elevation change. In Arctic lowlands this task would be easier, however, give examples of studies which used single GST measurements to calibrate/validate spatial permafrost models. Further, in areas with large GST variability there is never a good relation between GT (ground temperature) in say 20 m depth and GST, as GT integrates over large surface areas because of lateral heat flow. This could be evt. addressed in the discussion. E.g. validation in mountain areas based on point measurements should maybe only relate to permafrost present or not or a probability, like derived from the old BTS campaigns. When looking in Fig. 6 only two footprints (BK and AE) really crossing the 0-C-boundary, all others are either above or below or the lowest/highest logger show 0. This is worth a further discussion, too.

It is certainly true that the validation/calibration of medium-scale permafrost models is difficult with only GST measurements, especially in mountainous regions where differences occur at very short distances. However, this paper further addresses the more general question of model uncertainty due to uncertainties in response measurements, such as GST or ground temperatures (GT). This issue has only in the last few years been discussed within the hydrologic community (see Gupta2005, Di Baldassarre2009), where beforehands many studies investigated model uncertainty due to parameter and model structural uncertainty neglecting measurement uncertainties. Due to the lack of data, models are often compared to one measurement, replications of measurements to estimate measurement uncertainty are, in most cases not available, as so also in meteorological forecasting (personal communication with V. Stauch, MeteoSwiss). This study is one of the few studies addressing uncertainties in measurements due to scaling issues, and we think that the results are of interest for a much broader application range than permafrost modeling. The discussion on this issue has been expanded and goes now much deeper. It is furthermore certainly true that GT integrate over larger areas, however also borehole measurements at greater depths are susceptible to diverse uncertainties, which include representativeness and spatial variability (changes in water availability, ground properties, etc.), even though the magnitude is certainly smaller than at the surface.

5. Regression model: I liked that, but I wonder why you not used PISR (potential incoming solar radiation) calculated from a DEM (which you have for the area). This would be a much better variable than aspect with all distribution circularity problems.

A model including PISR (as annual mean solar radiation) calculated from the same DEM has been further investigated. However, no convincing results were obtained, PISR was even excluded from the models explanatory variables when making a step wise model reduction after Akaike. When including PISR model (4) as a additional variable, the R^2 rises slightly, however, in our opinion, this insertion is not very reasonable, since PISR is already represented by slope, aspect and the sky view factor.

6. Discussion: The discussion is a weaker point. The authors have done a remarkable peace of data sampling and analysis, and should put their results in a wider scientific context. The discussion now appears often more or less as a summary of statements given before in the text. The authors should try to discuss more the value of their results, maybe also in relation to old BTS sampling. Those publications often clearly document large BTS variations over short distances. Look e.g. at eq. 3, the regression model. The result there gives you an average footprint GST, based on topography and land cover. This could of course be evolved further, testing for larger areas, making a map, comparing with older BTS measurements etc, at least in a discussion. Further, a discussion of the importance of surficial material could be more focussed. E.g. the role of water content of the upper soil layer, Fig. 6. nicely show much less variation in areas with fine material than with course. You attributed this to more homogenous snow, but also soil moisture is important as e.g. demonstrated earlier for Arctic lowlands. And: What do you now recommend in detail for the further work, especially related to space-dependencies as stated in the title?

The discussion has been extended, and the above mentioned points are included. Thank you for the inputs given here.

7. Figures: Fig. 2 is not necessary for the message of the paper. Fig. 3: Give a key map of where the area is situated in Europe/Switzerland. Fig. 6: How about giving the standard deviation for each point as thin bars. This would give an indication of the variability around GST=0C. Fig. 7: Give grid lines, easier to sea the pattern in relation to the axis values. Fig. 8: Same as above. Axis could be labelled as "Measured" and "Modelled". Give units for the axis.

Fig. 2 is excluded now. Fig. 3: Map of Europe/Switzerland included. Fig. 6 was changed to boxplots. Fig. 7. and Fig. 8: Done.

8. References, are a bit biased to the Alps, but also in other areas the GST variability was addressed, both in relation to snow and other environmental factors and in various scales, and often in combination with BTS, but not in the systematic matter you did in your study (e.g. in southern Norway: Hauck et al. PPP 2004, Isaksen et al NJG 2003, Iceland: Etzelmüller et al./Farbrot et al PPP/JGR2007, Mongolia: Heggem et al PPP 2003, Sharkhuu et al JGR 2007, North America/Yukon: Bonnaventure Lewkowicz PPP 2009, 2010 and I guess other studies in Austria, France or Japan would relate to the topic). Figure 7 was here interesting from my point of view, if you have a look on Fig. 4a in Etzelmüller et al 2007 about Iceland, a very similar pattern is observed, with a big scatter in an elevation range where GST below or above 0C is mostly depending on snow. Maybe the paper would benefit to a broader look into the literature, the same issue applies to the reference to block fields. An interesting finding is of course that the pattern of your Fig. 7 is a scaling issue and is possibly worth being discussed in more detail.

More widespread references were included and relations to the study were further discussed. Thank you for the input, we think that the paper has been improved. The similar pattern in Fig. 4a) in Etzermüller 2007 is indeed of interest, this is further discussed in the paper.

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

Page 308, Line 7: Well, do you know of studies who have done so explicitly? If yes, please give examples.

There are certainly many examples of studies comparing model results to point measurements, however we don't think these examples fit in the abstract. Some examples can be found in the discussion.

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

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