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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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This paper is a very valuable contribution, highlighting ground surface temperatures (GST) over short distances and in a large or patch scale. The authors stress here the relation between large scale monitoring in heterogeneous alpine terrain and the scale used for regional climate or permafrost models. High spatial variability of environmental parameters important for the ground thermal regime has been addressed before in various publications (e.g. all the BTS-based publications during the last 10-15 years from various places in the world stressed this point), however, this study is after my knowledge the first attempt really systematically quantifying this issue using hundreds

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of small miniature temperature data loggers (MTDs) in various environmental settings in the south-eastern Swiss Alps. This makes this paper an important contribution to the community using MTDs and GST to evaluate permafrost distribution in mountains and possibly use this information for calibrating/validating spatial permafrost models.

However, there are some minor issues which should be addressed before publishing, which I think would increase the value of the paper. In the following, I will give some from my view important points for potentially addressing during the revision, while minor issues I have marked directly in the pdf and uploaded as supplementary information. In general the authors should decide if this paper is a technical/methodological or a more scientific contribution. I guess they want the latter, meaning that both the discussion could be more focussed and the technical details around iButtons strongly reduced, evt. put in as supplementary information or appendix.

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.

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.

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

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may put in a sentence to clarify this issue.

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.

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.

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

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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?

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.

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.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/5/C91/2011/tcd-5-C91-2011-supplement.pdf

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