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The Cryosphere
Discussions

Interactive comment on "Inferring snow pack ripening and melt out from distributed ground surface temperature measurements" *by* M.-O. Schmid et al.

Anonymous Referee #2

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Summary:

The authors present a two year study in Switzerland where ground temperature sensors are used to characterize the ripening date (RD) and subsequent melt out date (MD) of seasonal snowpack at sites with varying elevation, aspect, slope, and surface cover. The objectives of the study were (1) to determine how to derive MD and RD with ground temperature sensors, (2) to investigate how these variables vary at fine scales, and (3) to relate these variables to topographic characteristics. The sensors were clustered in groups in 10m x 10m footprints. Intra- and inter-footprint variations in MD and RD were explored and a regression equation was used to relate MD to topographic

characteristics, which produced Rˆ2 of 0.56 to 0.65.

Overall Review:

The data set is extensive, and this is a nice example of how distributed temperature sensors can be used in snow studies. However, it is not clear how the study yields new contributions to snow science. It has already been established that ground temperature sensors can be used to derive MD and RD (e.g., Taras et al. 2002; Lundquist and Lott, 2008; Tyler et al., 2008; Gadek and Kedzia, 2008). (References are included at the end of this review.) Relationships between topography and snow have already been established in other studies (e.g., Anderton et al., 2004; Schmidt et al., 2009; Tappeiner et al., 2001) and therefore the relationships found in the present study do not add to the existing knowledge base or show stronger relationships than previously established. The fact that the elevation and aspect terms have different magnitudes between the two years also suggests that the regression relationships have lower confidence in years when the extensive iButton networks are absent.

It seems the main contribution is the reliability indices developed for deriving MD and RD from the ground temperature data. However, because there are no independent measures of snow cover, the reader is left wondering whether the new methodology improves reliability over existing methods (e.g. Schmidt et al., 2009, Gubler et al., 2011) and must believe the authors' claim that the existing methods were "only partly satisfying" (pg 568, ll. 10-11). The authors state that their proposed method "has been tested in a far wider range of environmental conditions" relative to the existing methods, but this is misleading, because they have only applied the method and have not tested it against other observations. Additionally, no mention is provided about the transferability of the calibrated values of the proposed method to other regions, and this should be addressed in the discussions section to make the study more useful.

In sum, it is my opinion that in its current form, the paper does not add to existing knowledge and introduces a new method without providing supporting evidence for the method's reliability. The paper has potential value, but the authors should reconsider what they are trying to show and how they are demonstrating their technique.

Major Comments:

- In reading the paper, it is gradually revealed that one of the main reasons why someone might conduct this type of study is to check a gridded model. More discussion is needed in the introduction to establish this motivation. A brief mention is included in the introduction, but this should have more substance.

- A figure that shows the study area and footprint locations is needed.

- Please explicitly comment whether vegetation and trees are present at these locations. If these are present, then a discussion on the impacts of trees and vegetation on snow duration is necessary.

- The citations are heavily drawn from European research and would benefit from more intercontinental research. Please cite Tyler et al. (2008) and Lundquist and Lott (2008), who have similar studies in North America. This would fit on page 565, Lines 8-11.

- Page 568, Lines 19-20: How can (d) be asserted when you have no observations of snow depth? It is impossible to assess the reliability of the methodology in these cases without independent observations.

- Clearly, RD cannot be detected at all sites, but it has some correlation between MD ($R2 = 0.59$ to 0.50, page 573). Using your dataset, is it possible to empirically estimate RD based on MD? This might be a useful relationship to investigate.

- What are your diverse environmental conditions? This is referenced throughout the study (e.g., page 566, line 2; page 575, Line 23), but never explained, and thus remains vague.

Minor Comments

- Overall, the manuscript would be improved by having it proofread by a native English

speaker.

- Abstract, Line 7: You say 40 locations here, but 39 footprints later in the manuscript. Please be consistent.

- Were the iButtons buried below the ground surface? If so, how deep? It is strange that some of this information is included in the abstract, but nowhere else in the manuscript. Please explicitly describe this in section 2.2.

- Abstract, Line 19: This last sentence does not make sense in English.

- Figure 1 is never referenced in the manuscript text

- Table A1 would benefit from some context for each site. Please provide the elevation, aspect, slope, and GCT for each footprint

- Page 571, Lines 9-13: what is the purpose of reporting specific footprint results here?

- Page 571, Lines 26-28: Please list which topographic variables you used in this linear regression.

- Page 572, Lines 12-15: Please rewrite this sentence. It is confusing (especially the 2nd half).

- Page 572, Line 18 and Line 24 – Is it 14 locations or 15 locations? You contradict yourself here. Please clarify. I think it is 14 but you counted the mean as a 15th location on Line 24, which is incorrect.

- Page 575, Line 1 – please briefly specify what classes 3 and 4 are for those who have not read Ishikawa (2003).

- Page 575, Lines 13-14: This is a scale-specific issue. While the onset of snow cover was homogenous at your study area, this is not true in other basins, which may span a large elevation range (from rain-snow transition zones to alpine areas). Please qualify this statement.

- Page 575, Lines 14-15: GST only increases at the sites where the ground freezes. Please qualify.

- Fig 3 – the text for GCT1-GCT4 is difficult to read against the white background.

- Figure 5 – In the legends, please order by year. It is confusing to see 2011 first and then 2010, and I misinterpreted the figure because of this strange convention.

- Table 2 – what is the adjusted R2? This needs to be defined.

- Figures 2 and 6 – please write all months in English

- Figure 6 – please plot the Tair difference line with a darker color. It is difficult to see.

- Figure 6 – which year is the Tair line? Is it the difference between Tair and Tground? Or is it the difference between Tair from the two years? It is not clear.

References:

Gadek, B. and S. Kedzia (2008). Winter Ground Surface Temperature Regimes in the Zone of Sporadic Discontinuous Permafrost, Tatra Mountains (Poland and Slovakia), Permafrost and Periglac. Process. 19: 315–321. doi: 10.1002/ppp.623

Lundquist, J. D., & Lott, F. (2008). Using inexpensive temperature sensors to monitor the duration and heterogeneity of snow-covered areas. Water Resources Research, 44, 8-13. doi:10.1029/2008WR007035

Taras, B., M. Sturm, and G. E. Liston (2002), Snow-ground interface temperatures in the Kuparuk River Basin, Arctic Alaska: Measurements and model, J. Hydrometeorol., 3, 377–394, doi:10.1175/1525- 7541(2002)003

Tyler, S. W., Burak, S. A., McNamara, J. P., Lamontagne, A., Selker, J. S., & Dozier, J. (2008). Spatially distributed temperatures at the base of two mountain snowpacks measured with fiber-optic sensors. Journal of Glaciology, 54(187), 673–679. International Glaciological Society.

Interactive comment on The Cryosphere Discuss., 6, 563, 2012.