The Cryosphere Discuss., 6, C817–C820, 2012
www.the-cryosphere-discuss.net/6/C817/2012/

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.

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Received and published: 20 June 2012

We would like to thank the referee for his constructive comments which helped to improve this paper.

Small corrections and grammatical errors were corrected without further comments. As all the referees have pointed out that the linear regression we used to explain the MD with the topography does not add to existing knowledge we removed this section of the paper. We also agree that the proposed methods would benefit from further validation. The fact that we use temperature as a proxy for the presence of snow to detect MD makes a validation with independent data important. As field measurements with a

temporal and spatial resolution adequate for the iButton scale are not available, we now use simulated data, instead. We perform a high number of point simulations for diverse locations and environmental conditions and then use simulated ground temperatures to estimate MD. This can then be evaluated against MD derived from the simulated snow water equivalent. We use the physically based numerical model GEOtop (Dall'Amico et al., 2011) driven with environmental conditions typical for the test area. To not turn this paper into a modelling study, we do not include a validation. This is justified as we mainly require physical consistency of the results and not so much the absolute fit to individual measurements. Minor changes were made in the algorithm to detect the RD and the MD. The threshold for the mean daily standard deviation in the month Jan-Mar of the GST where we predict an insulating snow cover is now set to 0.2 instead 0.4. Due to this, only for a few iButtons was no RD or MD detected and the overall picture did not change.

Major Comments or Questions:

RC: An example showing the standard deviation and why 0.4 was selected as a threshold would be really helpful as no support is shown for this threshold and the scale in later figures is not sufficient to allow the reader to evaluate this very significant decision.

AC: We decided to choose a more conservative threshold and now used 0.2 as a maximum standard deviation. This reduced the number of iButtons where we detected MD and RD slightly. In Fig. 2 you can now see iButton AOa03 with a standard deviation of 0.19, which is the highest value where we still predict an insulating snow cover.

RC: Table A1 is not useful if there is no information about individual site characteristics is given... There isn't even a basic map showing the study area location and where these sites are. There needs to be. There also needs to be footprint-level information, either incorporated into Table A1 or in a separate place, describing the slope, GCT, etc.

AC: We have now made it clear that Gubler et al. (2011) should be read for this infor-

mation.

RC: Review of North American references would be helpful as these seem mainly from European sites and there has been a number of recent, relevant work on snowpack distribution in the States and Canada.

AC: We have included a broader suite of references, now.

Minor edits and/or suggestions:

RC:. Pg 568, line 1-4 – For which iButtons? Rephrase as daily stdev is used for days???

AC: We do not understand this comment.

RC: Section 3.2. Citations or documentation is needed based upon whether this is a theoretical discussion or one based upon observations at this site.

AC: This is a theoretical discussion and now also backed up with references. The revised text now reflects this as: "This point in time, the ripening date (RD), is detected as the beginning of the zero curtain period in spring and marks the beginning of melt water runoff or percolation into the ground (Taras et al., 2002; Tyler et al., 2008). The development of preferential flow paths in snow (Williams et al., 2010) increases the lateral variability between cold and isothermal portions of the snow pack and ground below and, as a consequence, also the lateral variability of RD."

RC: Pg 570. Section 3.3. One might assume that this was also aggregated up to the footprint level, but it should be specified clearly.

AC: The paper reads now: "As at all footprints at least five measurement series were recorded, the MAGST could be aggregated to the footprint level at all locations."

RC: Figure 5 – Could show GCT here as well? It isn't v ery clear what the authors are illustrating with this graph as there is no order to the data – perhaps a box-and-whiskers plot would be more effective? There needs to be some additional information.

AC: We agree with you that including GCT and presenting it as a box-and-whiskers plot would increase the information content. However, in our opinion the extra information would make it more difficult to get the two main points of this figure. These being that (1) it is possible to define the melting period based on RD and MD (2) the differences in the length of the melting period between 2010 and 2011 are for nearly all footprints smaller than the absolute shift in days.

RC: Pg 575. Mention no validation – might consider the recent discussion on Cryolist about remote digital cameras.

AC: A testing of the developed method is now done with independent data from GEOtop, where as we think a validation with remote digital cameras goes far beyond the scope of this study, but might be a possible application for the future.

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