The Cryosphere Discuss., 6, C811–C816, 2012
www.the-cryosphere-discuss.net/6/C811/2012/

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.

RC: 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.)

AC: Even though previous studies have dealt with similar topics, this study shows a more robust way to detect MD and RD. Together with the low cost for the miniature temperature loggers we believe this makes this study an interesting measurement setup for snow cover distribution related applications. We have incorporated the suggested references into the revised version.

RC: 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.

AC: We removed the section with the linear regression for the MD.

RC: 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.

AC: Applying methods from different studies (e.g. only temperature or variance threshold) on the iButton dataset from Piz Corvatsch did not deliver sufficient results for the snow cover duration, which can be explained by the limited range of environmental conditions and sample size from those studies. We agree on the need for validation, especially for MD and have developed the manuscript further in this direction. We have now additionally tested the algorithm with simulated data that offers the benefits of physical consistency, differing environmental conditions and precise control on MD.

Major Comments:

RC: 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.

AC: We extended this part and added also more references. The revised text now reflects this as: "Grid-based snow cover distribution models are often used to estimate snow cover evolution (Bartelt and Lehning, 2002; Blöschl et al., 1991a, 1991b; Lehning et al., 2002a, 2002b; Luce et al., 1998) or ground temperatures (Dall'Amico et al., 2011; Luetschg and Haeberli, 2005). Scales of gridded applications range from grid sizes of few meters (e.g., Groot Zwaaftink et al., 2011; Marsh et al., 2012) to tens or hundreds of kilometers in climate models (Best et al., 2011; Essery and Clark, 2003; Tribbeck et al., 2004). Often, the interaction with vegetation (e.g., Endrizzi and Marsh, 2010; Rutter et al., 2009) and processes of snow redistribution (e.g., Groot Zwaaftink et al., 2011; Pomeroy et al., 1997) are simulated as well."

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

AC: We referred to this and further information about the research area is given by Gubler et al. (2011).

RC: 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.

AC: As we removed the discussion of the relation between topography and the melt out date, the impact of vegetation and trees to the snow duration is of minor relevance for this study.

RC: 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.

AC: In the winter 2009/10 at most of the locations the snow cover was measured, thus it is known that a thick snow cover was present. The revised text in Chapter 2.2 now reflects this as: "The snow cover was measured at most sites during three campaigns in January, March and April in 2010 (Gubler et al., 2011; Schmid, 2011)."

RC: 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.

AC: With the detected correlation between MD and RD and the fact that RD was only detected for approximately half of the footprints it was not possible to find a robust relationship.

RC: 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.

AC: With diverse environmental conditions we refer to the differences in elevation, slope, aspect and GCT. This is now mentioned in Chapter 2.2. and reads like this: "These span diverse environmental conditions with elevations of 2100–3300 m a.s.l., slope aspects North, South, East, West and slope angles of 0–55° and all kind of ground cover types (GCT)."

Minor Comments

RC: 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.

AC: The paper reads now in section 2.2 like this "The devices were buried a few cm below the ground surface, at places with no fine grained material they were placed in funnels and gaps."

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

AC: We referred to this and further information about the research area is given by Gubler et al. (2011).

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

AC: What is important here is that these footprints were excluded due to less than 5 valid iButtons (see 3.1).

RC: 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.

AC: We do not say the onset of the snow cover is homogeneous in an absolute sense, we say that it is in general homogeneous in comparison to the melt-out date.

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