The Cryosphere Discuss., 6, C807–C810, 2012
www.the-cryosphere-discuss.net/6/C807/2012/

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: However, could the intra-footprint variability influenced by meteorological conditions, snow drift and deposition be better predicted?

AC: Certainly, even at distances of very few meters, snow redistribution will be predictable to a degree. We have, however, to decide on a scale at which to perform the analysis and below which we do not try to resolve patterns of variation.

RC: The discussion of both the inter-footprint variability and the inter-annual MAGST variations does not provide new insight; it is generally known that MD is later with increasing elevation, northern slope aspects and less steep slopes.

AC: We shortened the discussion of the inter-footprint variability by removing the linear regression.

RC: For the MAGST, it is clear that it mostly depends on the snow conditions, which in turn are highly variable from year to year due to the natural variability of the meteorological conditions. I would not include the aspect of a changing climate here.

AC: We agree that to experts this should be a trivial fact. Nevertheless, the fact that a change in one variable (e.g. climate, often expressed as changed air temperatures) can have contrary impacts on another (e.g. temperature) is important and due to the strongly replicated measurement setup clearly demonstrable. We prefer to keep this aspect in the text.

Specific comments:

RC: 565/16-17: there are many newer grid-based snow modeling approaches available in the literature, including models for processes like wind-induced snow transport, snow-canopy interaction or heat conduction from the ground. Please update Your overview and indicate at which spatial resolution these models have been applied, and what implications You can provide from Your findings for these applications and their validation.

AC: We have extended this section to now include references for canopy interaction and wind transport, heat conduction from the ground is implicitly contained in some of the original references. We have also indicated the range of possible grid resolutions, but find a comprehensive review to go beyond the scope of this paper. 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: 567/10-12: is there no better way to determine these variables than from a resolution more than twice as coarse than the footprint size?

AC: Yes – we also have a 10m DEM that yields similar results. Now that we have deleted the linear regression analysis, this issue is not crucial. We kept the 25m information for consistency with the Gubler et al. 2011 paper.

RC: Fig 4: explanation of this phenomenon is poor (572/15-17). Please either go into more detail (of the respective processes; is there a spatial pattern in the observation?), or skip the figure and explanation.

AC: Removed the Figure

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