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Interactive comment on "Large area land surface simulations in heterogeneous terrain driven by global datasets: application to mountain permafrost" *by* J. Fiddes et al.

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AUTHORS REPLY TO REFEREE #2

We thank Anonymous Referee #2 for the effort in evaluating and commenting this manuscript. In the following text the referee's comments are marked "RC" and author comments "AC". We have subdivided comments alphabetically where appropriate. All RC references refer to original manuscript. Figures with prefix R correspond to figures presented only in this response, not in manuscript, e.g. Figure R1. These can be found in attached supplement. Comments have been subdivided (a,b,c,..) where appropriate in this response. A corrected manuscript with additions in red is included as attachment to aid cross-referencing.

SPECIFIC COMMENTS

RC1: Snow depth is a crucial factor in thermal modeling of the ground. Fig. 4 is evidence that the snow depth is systematically underestimated in the model approach. As a result, winter GST and thus also MAGSTs should be significantly biased but this is actually not seen. This must be explained in more detail, I wonder whether two model errors could cancel in this case. MAGSTs are somehow warm-biased, in particular in the critical region around 0 degree C (Fig. 4). Is this an effect of too late melt-out due to overestimated snow depths? The authors should compare the melt-out date with the in-situ observations from the minilogger which they used anyway for the snow correction routine. Then they should evaluate the effect of the biased snow depths on the MAGST and permafrost extent simulations.

AC1: (a) The effect of warming by insulation diminishes with increasing snow thickness. Figure 4 shows that there is already quite some snow thickness (500 mm MASD) by the time that the bias becomes significant, so the increment may not be so big, here. (b) PERMOS 2 are rock temperatures, largely in steep rock, PERMOS 1 are in fact modelled mostly warmer than measured. This could be due to the prevalence of coarse blocks that have ventilation through the snow cover. This final point underscores the fact that it may be too simplistic just to make the relationship between snowpack depth and MAGST. While of course snow depth is an extremely important driver of MAGST, it interacts with other variables (topography, sub/surface) in a non-straightforward way. In addition we have changed Figure 6 to show how MASD bias correction affects MAGST.

RC2: (a) If MAGSTs are warm-biased, why is the estimate of permafrost extent still in the range of the expectations? (b) From Fig. 7, one could get the impression, that the PF extent is underestimated in this particular case? (c) (I think the match is actually quite impressive, but it is hard to tell without scale in the graphs, and it is nevertheless important to discuss the error sources.)

TCD

7, C3578–C3582, 2014

Interactive Comment

AC2: A quantitative comparison is given in the aggregate estimate which shows that this study produces a lower estimate than Boeckli (2012), but is in broad agreement. This is not an 'underestimate' as Boeckli (2012) does not necessarily represent the 'truth', but is another 'estimate' derived from a different approach. The fundamental problem is that we cannot really validate any of these results – just test aspects of the modelling procedure that in the end produce the result. As in climate models we hope that if several approaches #(i.e. climate ensembles) produce similar results then we are likely to be close to the truth.

RC3: The effect of wind drift of snow is only briefly mentioned, but actually is of outstanding significance and potentially one of the most critical limitations for the modeling scheme. How about subpixel effects, snowdrifts, etc., which can occur despite of the 30m resolution.

AC3: Section 6.3 'Important limitations' reads: "... we do not model redistribution of snow by wind or avalanche. This has an important effect on the surface energy balance where melt dates can be several weeks later due to heavy accumulations at bases of avalanche slopes (Harris et al., 2009) or earlier on wind eroded slopes (Bernhardt et al., 2010)." To clarify further, we have added: "This sub-grid effect can be parameterized by computing multiple cases for increased/decreased snow cover, but corresponding results will be difficult to spatialize.".

RC4: The correction routine for the snow actually relies on in-situ data and thus counteracts the original intention to only rely on globally available data sets. The authors should clearly state this or discuss how melt-out-dates could be determined on large scales (e.g. remote sensing).

AC4: Yes this is true. The idea in future is to use a snowcover product to identify meltdates which allows scalability. Added text:

p5866 : "Two notes of caution are worth mentioning with respect to this method, (a) this method is only valid currently at site scale, and (b) it relies on GST measurements.

TCD
7, C3578–C3582, 2014

However, the approach shown here could potentially be used together with satellite imagery in order to estimate snowpack bias based on MD to enable scalability of the method. However this is beyond the scope of this manuscript."

RC5: The authors should at least mention that some validation for Tair is already performed in the publication of TopoScale.

AC5: This is mentioned in 6.1 (2) and we have added the following to improve clarity:

“Other driving fields (including TAIR) were previously evaluated in FG2013.” “in FG2013 we show that TopoSCALE is able to achieve an RMSE of 1.93 on daily TAIR values.”

RC6: In how far is the employed subsurface classification a source of error for the permafrost extent? This may play a significant role in areas other than the Alps, to which the scheme may be applied in the future.

AC6: This is correct, however, in this contribution we aim to introduce and demonstrate a method. For this, a reduced set of surface and subsurface properties is sufficient and we hope future studies will improve on the actual application of this method and the data provisioning for it. To clarify we have edited the introduction to contain:

“The main aim of this study is to establish this combined method as a proof-of-concept and perform an initial evaluation of its performance. That said, the aim is not to provide a best-possible result but to provide a demonstration of method using simple datasets.”

AC7: Fig. 7 needs some sort of scale both for temperature, distance and altitude.

RC7: Approximate scale bars for distance, permafrost index and permafrost presence added.

AC8: p5861: the abbreviation SLF IMIS should be explained.

RC8: ’WSL-Institut für Schnee- und Lawinenforschung (SLF), Intercantonal Measurement and Information System (IMIS)’ has been added to text.

TCD
7, C3578–C3582, 2014

Interactive comment on The Cryosphere Discuss., 7, 5853, 2013.