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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.

Anonymous Referee #2

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The methods presented in the manuscript are potentially a major step towards improved mapping of the ground thermal regime in areas with strong heterogeneity, but few available in-situ data sets. It combines three previously published submodules (GeoTop, TopoScale, TopoSub) to a modeling scheme, which at least from a technical point of view is a significant step beyond the state-of-the art. However, I am of the opinion that the authors are too quick in assessing the performance for the ground thermal regime. There are a couple of critical issues in particular relating to the snow cover which the authors need to clarify prior to publication:

1. 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.

2. If MAGSTs are warm-biased, why is the estimate of permafrost extent still in the range of the expectations? From Fig. 7, one could get the impression, that the PF extent is underestimated in this particular case? (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.)

3. 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.

4. 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).

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

6. 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.

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

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

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