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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 #1

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This is a useful study regarding scale transfer approaches in alpine areas with a substantial degree of innovation. However, it seems that time was short at the end in preparing the manuscript, as a real in-depth application is missing, even though many parts of the model scheme were already presented elsewhere. Either, the generalised modelling aspects should be enhanced (not only the permafrost application, which was already mentioned in the previous publications), or the permafrost example should be discussed in more depth. I have the following general and specific comments:

General:

- I am not completely happy with using the term Land-surface model for the GEOtop

model used in this paper - this suggests a global or hemispheric application of the model, which is as far as I understand not the intended use of the model. On the other hand, if LSM are the potential target of the schemes presented in this study, why not using a real LSM to show and analyse their usefulness ?

- a real application is missing: Figs. 6, 7 & 8 are not useful in this form/resolution (see below). One gets the impression, that the model was developed, but no time was left for real testing and applications. What do you intend to do with the model in future ? You present the results as a test application but without real in-depth analysis or discussion of the permafrost distribution. It becomes not clear whether the permafrost application was one of your real aims or whether it is "just" an example and you plan to do other different examples in the future. This is important as you rely heavily on existing permafrost-relevant validation data (high-resolution GST data), which would be difficult to get for other applications. This model/data set seems to be tailored to your application, but you do not make use of it...

- Figure 6: the plots are too small and details about the abbreviations in the legend, the location of the data point and its representativeness are missing. Too short explanation in the text (page 5864/5865 top): the reader is left alone to understand what Figure 6 is really showing. - Figure 7: why only a "visual comparison" ? What do we learn from this single comparison ? Where is it ? What is shown ? What is teh colour code ?

- conclusion: you should only mention those aspects in the Conclusion which you really assessed/discussed within the paper using your results. This is not always the case.

Detailed comments:

p.5857: l15-17: "...we have an optional informed-scaling training routine, which regresses model results against input predictors after a training run in order to adjust the weighting of each input according to its significance, and in doing so improve the quality of the final results." –> this is much too vague, even if it is describing the method. But without some physical processes and/or explanations which variables/parameters

are optimised, this is a meaningless statement. You can train everything, even if there is no meaningful relation between input and output! Please give more details.

l21: "a "sub-grid aware" aggregation...": nice term, but again too unspecific/vague..."aware" could mean anything in this context.

l22: "with fine-scale ground truth": which type of groud truth do you refer to here ? Is that realistic that this kind of ground truth would be usually available in the context of your present paper ?

p.5858: l12: "The final output is the full set of scaled fluxes...": do you really mean (only) "fluxes" here ? or time series of meteorological variables, from which the fluxes can be calculated ?

l15-16: "...e.g. commonly used lapse rates or parameterisations": again too unspecific –> what are "commonly used parameterisations" ? That depends a lot on the model, temporal and spatial scales, processes included etc.

l21: "it should be noted that this model is not an LSM in the conventional sense...": exactly, so why do you use this term for it ? This is misleading. I suggest to use the model name thorughout the text and make the connection to LSM's only where it is appropriate. (see also general comments)

l27: "...as well as freezing and thawing processes IN THE GROUND".

p.5859:

l1: "...ground temperatures." –> do you mean subsurface temperature or ground surface temperature (GST) ?

l1-2: for what purposes do you use this model in this application ?

l3-4: "..details specific to experiments...": unclear at this point...what experiments ?

p.5860: l1-2: Is this really possible for the "typical" LSM application ? Usually a data set

as used in Schmid et al. 2012 will not be available, that is one has no way of knowing the correct melt date (MD) over a large area. Are you aiming in your paper at a spatially high-resolution case study at your field site (i-buttons or potentially PERMOS, where a lot of GST data are available) or at a generally applicable approach as stated in the introduction ? For the latter, I do not see how this snow correction method could be applied except by using remote sensing. But this, you did not treat in your study!

l2-3: that would be a spatially distributed correction factor if you simulate large areas. How did you extra-/intrapolate that to the full grid in case of data sparsity (see comment above) ?

l19: "4-D-VAR assimilation scheme"

p.5861: l5-8: "Landcover was derived..." –> that is a critical step for surface/subsurface studies in the Alps and should be explained here in more detail! It would also be good to get some indication of the validation and the corresponding uncertainty of these data sets.

l19: GST at IMIS stations: is that measured in the same way as for the other data sets ?

p.5862: l.18-19: was attention paid to the so-called zero-curtain phase (non-changing surface/subsurface temperature data at the freezing point due to freezing/thawing) ?

l.20-23: did you validate only GST values ? Is that generally useful or just the specific focus of your application ?

p.5863: l6: "(10 times, 1979-1983 period)" –> why this spinup set-up ? is there a specific reason to use this period ?

l8: "defined in Gubler et al. 2013": as far as I understood this study aimed at analysing a very specific region, which was also used as input data in the present study. But these data concern a very small region; would that not give a bias towards a good validation in exactly this region, but not necessarily a good performance within all the

other areas in the present study ?

l8-10: mean annual values: but as you calculate also the daily values, why not validating them as well, at least some statistics of them ? At least if you really aim at some general applications as written in the introduction. Else, focus on your application (I would prefer that) and reduce the "generalisation focus" of the introduction.

p.5864: l10: Is mean annual snow depth really a good validation variable here ? as you mentioned yourself earlier, melt out and/or number of snow days are much more relevant and meaningful in this context. A strong under/overestimation in snow height changes MASD but may not be important for frozen ground, as along as MD is correct.

l20: how do you know it's precipitation input and not snow drift/parameterisation of snow metamorphism etc ?

l26: "...snow depths can be underestimated...": is there a discussion of the associated problems following somewhere ? performance depends on teh snow regime: if large, no problems associated, if small there are potential biases in winter through additional cooling of the subsurface

p5865: l8-10: "Comparison of methods is only intended for...": so what is the aim of this rather arbitrary and purely visual comparison ? Why not comparing against ground truth ? Both models are based on temperature (GST, MAAT) estimates which are partly (Boeckli) calibrated with specific ground truth data...again with a bias towards the same data sets (PERMOS, ibuttons)...so what do you exactly learn from this comparison ?

l20-26: the error magnitudes are difficult to see in Fig. 8 - in this resolution the only thing one can clearly distinguish are the colours/sign of the bias.

l26-27: "...fit magnitudes of precipitation...": do you have a reference for that ? l27: "...first stations...": what do you mean by "first" ?

p5866: l13-14: "The exception being precipitation.": not clear what you mean

l16: "climate models": do you mean atmospheric models ?

l19-20: "...and (b) it relies on GST...": if you have already GST measurements: do you really need the model to predict whether there is permafrost at these places?

5867: l10-12: "...In addition there are advantages of the gridded ERA dataset over interpolated station data...": too vague, please explain! There is also a new high-resolution gridded data set for Switzerland for air temperature and precipitation (MeteoSuisse) , which you may want to mention/use.

l24ff: "Landcover could however...": not clear to me: it seems to me a VERY IMPORTANT source of uncertainty ?! Especially in this kind of heterogeneous terrain ! If you take the results from Gubler et al. with GST variations of several degrees over a few metres, how do you get something reliable out of your scheme which is more than a typical MAAT-based estimate without this type of GST ground truth data as input data, like in your study ?

5868: l14-16: I do not understand: why/where do you have clay silt and peat sand in the mountains ? and there are many other process uncertainties on subgrid level, e.g. the effect of large boulders at the surface which are very important regarding the permafrost distribution

l19-22: ok, but then you should do one example application yourself and show that it is feasible in terms of these uncertainty analyses...

5869: l2-3: unclear to me what you mean

l7-11: yes exactly, see my comments above

l15: "driving fields"

l17-18: "like the Mattertal": why do you point out this psecific example if you do not focus on regions except the Engadine in the reminder of the paper ? The focus of the study is not clear to me.

l24-28: operational weather forecast models can still not simulate this type of sub-grid processes in mountain terrain...

l28: new paragraph after "...few years."

5870: l17ff: Do you see evidences of these limitations in your validation experiments or are they "just" examples of a list of potential uncertainties/limitations without knowing the relative importance of them ? If you have any evidences (e.g. for the cold bias or the snow deficit) this would be useful to mention here.

5871: l4: "... as a test case.": I am not so sure you could really call it a test case. You present it as a test application but without real in-depth analysis or discussion of the results. It becomes not clear whether the permafrost application was one of your real aims or whether it is "just" an example and you plan to do other different examples in the future. This is important as you rely heavily on existing permafrost-relevant validation data (high-resolution GST data), which are difficult to get for other applications.

l5-6: a word is missing towards the end of the sentence ?

l12: "...that consider significant uncertainties in the model chain...": but this is at the cost of having introduced additional uncertainties due to the simplified approach ?

l13-17: please rephrase this sentence: it is quite vague and difficult to understand

l24: "due to biases in driving data": you did not show this in your analysis

5872: l6-7: "...such as changing sub-surface material properties...": thi sdepends a lot on the subsurface properties which are generally only known at site level! You did not address or discuss that in your paper

l15-20: repetition to above

5877: Table 1: why do you give the values to the third decimal ? are these averaged values of the evaluation data set ? is it necessary in this context ?

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