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The Cryosphere Discussions

Interactive comment on "A New Map of the Permafrost Distribution on the Tibetan Plateau" *by* Defu Zou et al.

Anonymous Referee #1

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Apologies to all for late post.

Summary

This manuscript constructs and evaluates a new permafrost map for the Tibetan Plateau (TP). The map is created by using MODIS LST and soil types based on in-situ observations and an empirical model. These data are used to drive the TTOP model (Brown & Riseborough 1996) to determine spatial distribution of permafrost (defined as TTOP lessthan 0degC). The map is evaluated against extensive datasets of ground temperature measurements from boreholes, infrastructure monitoring systems and previous field campaigns. The new map is also compared to two previous generations of regional permafrost maps. This manuscript presents an interesting attempt to combine remote sensing, observational and modelled data to improve understand-

ing of permafrost distribution on the Tibetan Plateau - I think this is a good approach and useful contribution. However, I think several issues need to be dealt with more rigorously prior to publication.

Main comments

1. LST obviously is not equal to ground surface temperature (GST) that is required to drive TTOP. You acknowledge this in Section 3.1, however: (A) this needs to be much more explicit as you describe your methods. (B) Does it not make it extremely difficult to interpret values obtained under snowcover as snow surface temperatures are likely to be much lower than GST even in shallow (albeit likely cold and dry, therefore lower thermal conductivities than a temperate snowpack) snowpacks of the TP, especially on clear nights where high emissivities will cool the snow surface much more than the GST. I think its really important you at the very least quantify how significant this problem will be in your study region and probably try to introduce a term that accounts for the offset between GST and surface temperatures under snow on the ground conditions. Additionally I think this statement is wrong:

"In this study, the land surface temperature was directly used as the upper boundary conditions in the model; therefore, the LST calculation procedure with air temperature and n factor was omitted."

LST is likely to be very similar to near surface air temperature - this approach therefore ignores the n-factors which are important in describing the offset between air and ground surface conditions particularly under snowcover as described above, also effects of vegetation (less significant perhaps).

2. You don't give any evaluation of the MODIS product - how well does it perform in the region? WHat are the uncertainties under snowcover (wet/dry), vegetation, arid soils etc. You have AWS data from the permafrost field campaign you mention in Section 2.1 which may give you some clue if you measure surface temps (of coarse point/spatial scaling needs to be acknowledged). Cite the literature that has looked at uncertainties

in MODIS LST regionally/globally and give some incites on how you expect this to affect your model setup. This forms the basis of your method and is therefore really important to critically discuss.

3. MODIS LST is a coarse resolution 1 km product. This of course will make any kind of discrimination of permafrost units at the subgrid scale difficult - mainly important on the north slope of Himalaya and other mountain regions of complex topography on the TP. It should be thoroughly discussed what limitations this poses for your results.

4. Bedrock and debris slopes do not seem to be included in your "soil" classes and presumably are important land classes in your region. How do you deal with these?

5. Why not present actual values derived from the TTOP model instead of just a binary map? This would be interesting to see eg. where warm/cold permafrost exists.

6. How do you incorporate the effect of solar radiation (slope, aspect + possibly horizon, sky view factor) into your five investigated regions (IR) that you use as validation? From reading section 2.1.2 it appears that you determine a lower limit of permafrost (LLPs) and extrapolate this across your IR. However this sentence:

"The permafrost map was generated for each IR based on the criteria of LLPs in different conditions combined the digital elevation model (DEM) data..."

Suggests you do something which may account for at least aspect. This needs to be well described as forms the basis of you evaluation. We need to know how well we can trust this and what uncertainties are involved in these validation datasets.

7. In the validation exercise with the 2016 map we see large differences in kappa values: 0.38 – 0.78. Some discussion is given (p.14 l.439–448) which as far as I can tell indicates that smaller IR are more accurate due to density of measurements. However, best (0.78, WQ) and worst performing (0.38, AEJ) are roughly the same size. It is important you discuss these differences in performance with respect to how well you think your model performs in the various regions eg. uncertainties such as complex

topography, LST-GST offsets etc.

8. How long are your borehole records? These need to be better described in your data section. You show in Figure 3 how the new map better represent seasonally frozen ground eg. subset a3-c3. But how do we know whether the model is better or simply that the permafrost has thawed in this region over the last 20 years. The borehole measurements are not contemporary with the old permafrost maps as I understand - but that needs to be described as stated above. Additionally, in validation you compare a map derived from 2003–2012 MODIS data with borehole records of possibly another period. Basic point: it seems that comparability of different maps and validation datasets is problematic and these issues should be discussed well.

9. In section 3.5 you compare maps 1996, 2006 and 2016. What is the main message from this comparison? How do you disentangle changes in permafrost distribution computed from possible actual changes in MAAT over the last 20 years and differences due to different methods and sources of uncertainty? It would be good to be clearer about what the various differences that are observed are correlate with i.e. complex topography, latitude, data scarcity etc.

10. Issue of permafrost conditions out of equilibrium with todays climate i.e. warming permafrost conditions, should be discussed. Surface forcing could indicate no permafrost according to todays conditions - but there exists a long response time of permafrost bodies to modern atmospheric conditions. Therefore any map based on a contemporary forcing likely underestimates permafrost extent and especially, arguably the most interesting/ disruptive warming/thawing permafrost bodies. This fact does not have an easy solution, but certainly should be discussed.

11. How do you identify ’thawing regions’ (Section 5 l.460 and mentioned throughout text). This would require some form of transient modelling that demonstrates a transition from permafrost to non-permafrost conditions? As far as I can tell you are equating detection of seasonally frozen ground to thawing conditions. If you use ’sea-

sonally frozen ground' in figures also use this in text, otherwise confusing to reader that likely associates the word 'thaw' with a change in permafrost conditions.

12. Language of Section 4 is very poor in sharp contrast to rest of paper which is generally fairly good.

Minor comments

1. Might be worth citing Gruber 2012 (cited later in paper) in your introduction where you discuss TP permafrost maps.

2. p.6 l.174: massive –> numerous.

3. p.6 l.175: describe what HANTS is and why you use it. Details can be left to the reference but reader needs to know the basic purpose of this method.

4. p.6 l.183: what are the MODIS overpass times? How many Swathes used?

5. p.7 eq.4: mention that kt/kf comes from properties derived in Section 2.2.3. Make this link more obvious in text.

6. p.9 l260 "decrease gradient" –> "decreases linearly", is that what you mean?

7. p. 13 l.415: I would rather say medium spatio-temporal resolution. I don't think 4 daily values at 1 km qualifies as 'high res' on either dimension.

8. p.13 l.416: "it can reflects" –> "it can represent".

9. p.14 l421-422: "The improvement of upper boundary conditions of permafrost model and the employed of massive reliable in situ observed datasets make the high modelling accuracy achieved. " –> "The improvement of upper boundary conditions of the permafrost model and the use of large quantities of reliable in situ observed datasets, leads to a high modelling accuracy."

10. Based on comment above about comparability of maps and validation data, I don't think this statement is so straightforward (p14 l439): "Although TP-2016 performed

better than TP-1996 and TP-2006 and showed substantial agreement with the investigated results, it still results in some misjudgments"

11. What is the permafrost distribution of figure 1 based on?

12. Acknowledgments: remove "Level 1 and"

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