The Cryosphere
Discussions

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

Anonymous Referee #2

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Review of Zou et al. In this manuscript the authors present a novel approach for characterizing permafrost distribution across the Tibetan Plateau with the commonly used Temperature at the Top of Permafrost model (e.g. TTOP). Comparison with locally collected data and prior maps suggest that the new map provides a better baseline of permafrost distribution in the region than given by previous (more arbitrary maps). The authors use MODIS LST data to force the local climate conditions for the TTOP model.

Overall, I consider the paper to be of sufficient quality to be published in The Cryosphere after revisions have been provided. However, I also believe that there are a number of key points that have to be addressed in order for this paper to be accepted for publication. I have gone back and forth between trying to decide whether this should constitute 'major' or 'minor' revisions.

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Discussion paper

Notably, I consider the following to be major points: [1] MODIS LST The authors use MODIS LSTs as the key input for their model of permafrost distribution. However, MODIS LSTs measure a combination of different surfaces including the snow surface. If, as the authors postulate, there is only minimal snow cover across the region and correspondingly that MODIS LSTs can be used in the winter then this is all fine. However, the authors have not shown conclusively that snow cover impacts on LST retrievals can be ignored for their region. Addressing this point is a necessity for this manuscript to be considered suitable for publication in The Cryosphere. Likewise, there is certainly some effect of canopy cover in the summer which has been ignored by the authors. It would be useful if the authors examined the ecotype related impacts on the LST and correspondingly how this may affect the distribution of permafrost in the region. I also suggest that the authors produce an additional figure which shows a 1st panel with the estimated regional snow depth across the area (either from reanalysis or other datsets) and a 2nd panel that shows the spatial distribution of vegetation classes (broadly) across the region so that as reviewers we can determine the degree to which this issue may be problematic. Another issue with the MODIS LST that I find concerning is that the authors make the claim that MODIS is preferable to interpolation for temperature (it seems to be in the context of air temperature). A number of studies have found issues with MODIS-derived (or aided) air temperature products with only minimal improvements being observed (if at all) in terms of cross-validation.

Although I do think that MODIS products have utility for permafrost purposes, more work must be done to demonstrate that these products offer improvement over high resolution interpolation of station-based temperature products. It is important that the permafrost community ensures that the usage of LSTs from MODIS for driving permafrost models is assessed at each usage given the spatial heterogeneity of the factors influencing MODIS LSTs.

[2] TTOP modelling output The others provide a simple binary term for the presence or absence of permafrost that is useful in the context of total area numbers but also

means that huge amounts of information are unavailable. A map of TTOP temperatures could be useful in interpreting areas most susceptible to future change and also for the purposes of understanding permafrost thicknesses under a variety of environments. I would highly recommend that the authors at least present one map showing the spatial distribution of TTOP temperatures.

[3] Uncertainties Given the uncertainties that may be present in the LST products and in distributing rk across the landscape, it would seem important that some assessment of uncertainty is provided for the estimates of total permafrost area. It also may be a little optimistic to assume that all glacier area would correspond to permafrost area given the vast range of climates in the region. Such an assumption would require a very detailed assessment to rationalize – I'd prefer it be left out.

[4] Non-equilibrium permafrost The authors should consider the results of Riseborough (2007) when evaluating their TTOP model output and particularly in the context of non-equilibrium permafrost. Is the region warming and if so would this be impacting the distribution of permafrost as measured from this equilibrium model? One of the challenges in using a MODIS derived product is that the relatively short period of coverage makes it more challenging to model in hindcast.

Minor points: L16: Remove "mostly". L27-28: Identifying 'thawing regions' seems unclear to me. L38-39: This sentence could use some grammar editing for clarity. L41-42: Urgent is perhaps a bit strong of a word here, as is 'situation'. L46: "there is great variation" -> "there is considerable variation" L49-50: This sentence should be re-written to be clearer. At present, it makes no sense. L51-52: What is the difference between a topographic map and a base map? L54: "On the" -> "on" L55-56: This statement is not true. GIS techniques were used before 2000... L58: What does "stability of elevation" mean? L74-75: I do not agree with this sentence. Temperature and reanalysis data have a higher temporal resolution than MODIS and can be interpolated more accurately. In my experience, MODIS LST products in the Subarctic and Arctic are not suitable alone for characterizing spatial variations in temperature. L80: I agree.

The authors should provide examples of this validation. L87: Remove "plenty of" L89: Remove "perfect" L95: Remove this sentence L96: Remove "combined" L113: What is "drilling method". The grammar seems a bit off. L136-137: The grammar in this sentence should be revised. L157: "mostly widely" -> "most widely" L174: "massive missing values" -> "many missing values" L175: "Harmonic ANalysis Time" -> "Harmonic Analysis Time" L176-177: Remove sentence or combine with earlier sentence L197: What is "stability of the data"? L197: I prefer FDD and TDD or sFDD and sTDD to DDF and DDT. L199: Amend to: "Soil thermal characteristics were modeled according to parameters measured from soil types encountered in the field". L232: We do not need a sentence to tell us that an abbreviation was used. L259-260: Amend: "increases with increasing" and "decreases. . . decreases". L264: This sentence could be shortened with the use of brackets. L280: Boreholes are not "convincing evidence" of permafrost rather they can determine if permafrost exists or not. This sentence should be revised. L311: ". . .correct. . . correct" – please revise L329: "overcomes this shortcoming" – That is not necessarily proven in the study. L360: "lower distance difference" – Please clarify. L389-L392: This sentence is confusing – please revise. L399: "are unevenly" -> "unevenly" L400: What is poor representativeness? L404: "the most accurate" – Remove this sentence. L409: Poor sentence grammar – Please revise. L416: "reflects" -> "reflect" L418: "high representativeness" – what does this mean? L419: The case has not been proven for this statement. L424: Difficulty cannot be "large" L437: I do not believe that this method could be used elsewhere. Most permafrost regions receive snow therefore negating or reducing its potential utility. L440: Misjudgements is not the correct term here. L445: Please revise the grammar in this sentence. L446: Please revise the grammar in this sentence. L454: "In compliance" is not used correctly. L462-463: I do not believe this study has adequately demonstrated this. Please remove.

References Riseborough, D.: The effect of transient conditions on an equilibrium permafrost-climate model, Permafrost and Periglacial Processes, 18(1), 21–32, doi:10.1002/ppp.579, 2007.

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