Response to Referee #1

We appreciate you very much for your comments concerning our manuscript entitled "A New Map of The Permafrost Distribution on The Tibetan Plateau" (MS No.: tc-2016-187). Those comments are valuable and helpful for improving our manuscript. We followed all comments and made revision and answers carefully. Revised portions are marked in red in the revised manuscript. The page, line, and figure numbers refer to our revised manuscript. And, a point-by-point reply to the comments are listed below.

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 it is 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 LST 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).

Response:

It is a good question that also raised by another reviewer. We discussed this issue in a great detail within our research group. Ground Surface Temperature (GST) is defined as the surface or near-surface temperature of the ground (bedrock or surficial deposit), measured in the uppermost centimeters of the ground. The snow and vegetation might play significant influences on the relationship between the remote sensing LSTs and the GSTs, the influences is depending on the snow depth and duration (Zhang, 2005), vegetation height and coverage.

The spatial distribution of snow cover over the Tibetan Plateau (TP) varies quite greatly. The most wide-spread snow cover were found in southeastern part of the TP, and some alpine regions with the elevation higher than 6000 m (Qin et al., 2006; Pu et al., 2007); the snow cover is rare, shallow (< 3 cm) and existed in a short duration (mostly existed less than one day for one single snow event) due to very strong solar radiation and wind in the vast interior and the north of the TP, where the permafrost most developed, and is our major study area in this manuscript (Che et al., 2008; Huang et al., 2017). The thin snow cover with short duration mainly has a cooling effect on LST due to the high surface albedo of fresh snow and a rapid process of snowmelt (Zhang, 2005). The duration of the cooling effect may be very short, thus it may have very little effect on the LST in average for certain period of time.

Generally, the soil surface beneath the vegetation layer have a higher temperature than the canopy surface, depending on the height and coverage. The alpine ecosystem in permafrost regions and its vicinity are all composed of grassland, characterized by dwarf and sparse plants. The vegetation coverage in most of the permafrost region was less than 30% (Lehnert et al., 2015), and even less than 10% in the middle and western TP.

In view of the actual condition of both snow cover and vegetation on the TP, there are only slight differences between MODIS LST and GST measured in meteorological stations in average, and the differences in thawing and freezing indices from both datasets were much small in our study area. In the revision of this manuscript, we added two figures of the snow depth (Fig.7, edited after Che et al.(2008)) and vegetation types (Fig.8, edited after Wang et al.(2016)) to show the possible regions influenced by the snow cover and vegetation.

In the revised version, we explained this in the *Section 4* (P.13 L.381-397).

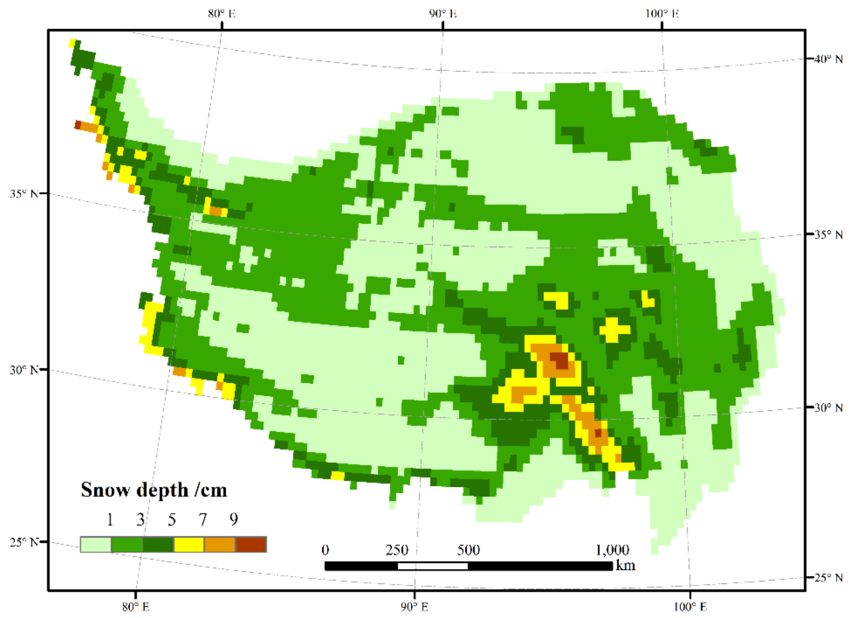

Figure 7. Annual average snow-depth distributions on the Tibetan Plateau from 1979 to 2014 (edited after Che et al.(2008))

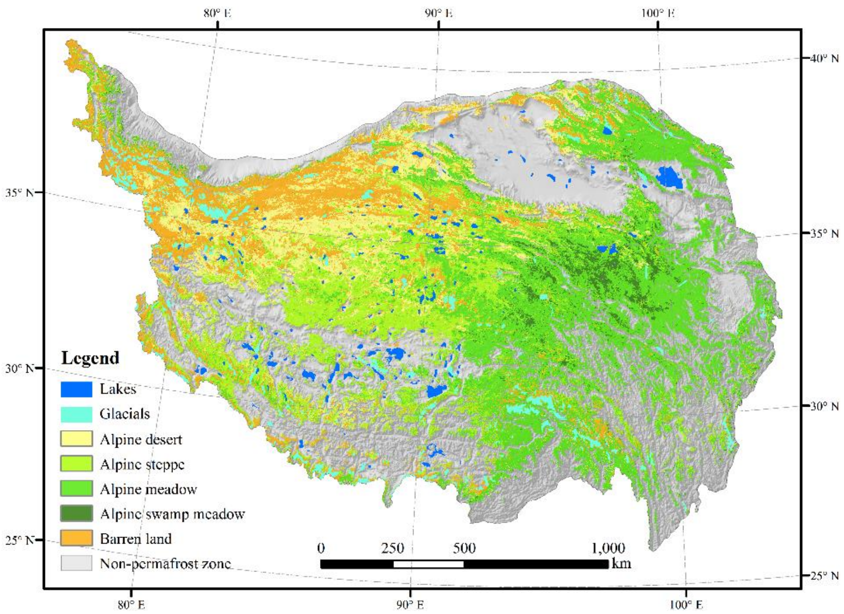

Figure 8. Vegetation types of the permafrost zone on the Tibetan Plateau (edited after Wang et al.(2016))

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 temperatures (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.

Response:

Thanks for the comments and suggestions. The evaluation of the modified MODIS LST averages in the permafrost region on the TP has been done, which was described in detail in Zou et al. (2014). Briefly, the multiple linear regression model combined all four observations of Terra and Aqua MODIS LST were established to estimate the mean daily GST. The model validation in three permafrost sites (e.g. Xidatan, Wudaoliang and Tanggula) showed that the determination (R^2), mean error (ME), mean absolute error (MAE) and root mean squared error (RMSE) was 0.91 to 0.93, -0.21 to 1 ℃, 2.28 to 2.42 ℃ and 2.96 to 3.05 ℃, respectively (Zou et al., 2014). The uncertainties were mainly came from the following factors:1) temperature differences caused by the time offset between half-hour interval of ground-based observation and satellite overpass time; 2) the mismatch of spatial scales between point and pixel observation. Moreover, the three sites (with different vegetation types: including alpine meadow, alpine steppe and alpine desert) located at the continuous permafrost zone on the TP with altitude above 4000 m, the observed GST data could be used to calibrate the MODIS LST due to their high representativeness of climate condition in the permafrost region.

In the revised version, we have added the evaluation of MODIS LST briefly in the *Section 2.2.2* (P.6 L.189-191).

On the other hand, we cited some papers about the evaluation of global and regional MODIS LST products. The results of the radiance-based and temperature-based validation indicated that the accuracy of the global MODIS LST product is better than 1 ℃ in most cases, including lakes, homogeneous vegetation and soil sites in clear-sky conditions (Wan et al., 2002 and 2004; Coll et al., 2005; Wan and Li., 2008; Wan., 2008). In addition to validate the MODIS LST with the in-situ measurements at the same time within the footprint of the satellite sensor, some studies focused on the accuracy of LST averages for longer time periods, which is crucial for permafrost modelling. Langer et al. (2010) and Westermann et al. (2011) focused on weekly averages and demonstrated an agreement generally better than 2 ℃ for MODIS LST for the summer season, at permafrost sites in Siberia and Svalbard, respectively.

In the revised version, we cited the papers in the *Section 2.2.2* (P.6 L.164-169).

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.

Response:

In our opinion, the changes of LST as well as air temperature with the horizontal distance are much greater in the north slope of Himalaya and Gangdis, where the slope is much steeper than that in the interior TP. It is said that the LST differences between both adjacent pixels are much larger, and is more sensitive to boundary identification. Moreover, there is almost no validated permafrost distribution data in these mountainous regions, due to the difficulties to carry out investigation.

In the revised version, we discussed the performance of TP-2016 in complex terrain in *Section 3.3* (P.10 L.294-298).

4. Bedrocks 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?

Response:

In this study, the bedrocks and debris slopes were classified into Gelisols according to the soil classification criterion described in Li et al. (2015a and b). Owing to bedrocks and debris slopes are not much and generally located at much higher elevation than the lower limit of permafrost, mostly around the high mountain peaks (eg. The Kunlun, Tanggula, Himalayas and Gangdis mountains) where were glaciated and strongly weathered. It was said that even the simulation for such regions were not so accurate, however it does not affect the permafrost distribution modeling.

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.

Response:

Thanks for your excellent advice. We have change the binary map with the actual values derived from the TTOP model. In additional, the TTOP values were divided into six intervals so that the reviewers and potential readers can read and analyze conveniently.

The revised figure is as below, and could be found in the P.25.

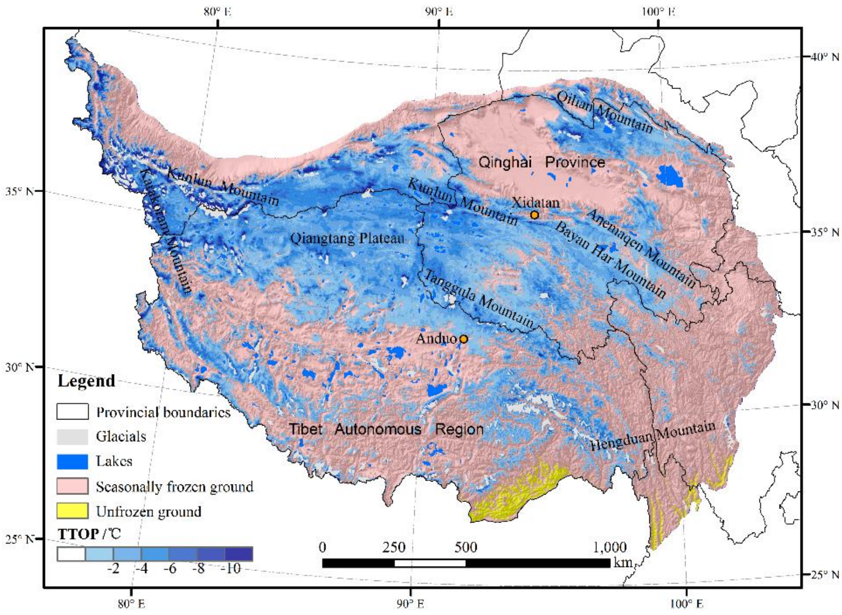

Figure 2. Spatial distribution of permafrost with the derived TTOP on the Tibetan Plateau

6. How do you incorporate the effect of solar radiation (slope, aspect + possible 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 your evaluation. We need to know how well we can trust this and what uncertainties are involved in these validation datasets.

Response:

In the investigation, the influence of aspect on the LLP have been considered when the boreholes were set. The LLP was determined based on the linear regression relationship between MAGT and elevation of boreholes, and the elevation where MAGT equals to 0 ℃ was regarded as the LLP. In the permafrost mapping of each IR, the boreholes were classified into three types: north-facing, south-facing and east-west facing slopes and the LLP of each type was determined respectively; then, the permafrost distribution were generated based on the LLPs of different aspects and the digital elevation model (DEM) data, and a portion of the observed results of boreholes and geophysical methods (GPR and TEM) was reserved to validate the maps (Li et al., 2012; Zhang et al., 2012). For example, the results of GZ IR showed that the LLP was about 4950 m for north-facing, 5000m for east-west-facing, and 5100 m for south-facing slopes (Chen et al., 2016).

In the revised version, we added this statement in the *Section 2.1.1* (P.4-5 L.130-138).

7. In the validation exercise with the 2016 map we see large difference 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 difference in performance with respect to how well you think your model performs in the various regions eg. Uncertainties such as complex topography, LST-GST offset etc.

Response:

The TTOP model identify the permafrost boundary with a temperature threshold, which is sensitive to the temperature differences in horizontal distance. As we described in the *Comments 3*, the TTOP model might perform better to identify the permafrost boundary in the regions with complex terrain because of sharp changes in LSTs within short distances, such as the WQ, B-Q and XKL IR. For GZ and AEJ IRs, where land surfaces are much flat, and so called lower surface relief, LSTs changes with distances are small, the TTOP model performs not as good as other IRs. For AEJ IR, the performance of TTOP model was worst, because of there is no soil pits in the investigation and the soil condition inferred completely from the relationship between the environmental factors and the soil samples of the other four IRs.

In the revised version, we added this statement in the *Section 3.3* (P.10 L.294-298).

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 past 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.

Response:

It is common that the ground temperatures of permafrost were increasing during the last several decades, but the increasing rates were much lower than that in circumpolar regions, and it was even much lower for warm permafrost (Wu and Liu, 2004; Wu and Zhang, 2008; Smith et al., 2005; Romanovsky et al., 2010; Smith et al., 2010; Zhao et al., 2010). In the other words, the monitored changes of permafrost temperature near the permafrost boundary on the TP was not so much, and there is no data showed that the permafrost in these regions have disappeared. For example, we have deployed two permafrost temperature monitoring sites since the 1970s: Liangdaohe (N31.82°, E91.74°), at the south permafrost margin area where the permafrost is sporadic with an area of less than 1 km^2 and its thickness is more than 60m, and Xidatan (N35.72°, E94.09°), the north margin and the thickness is about 20m.The permafrost at these two margin areas does not disappear until now. Furthermore, an ice-rich layer commonly exists at the top of a permafrost layer that the thawing process needs much more time and more energy input. Therefore, the changes of the permafrost distribution on the TP is limited in the past several decades and that is the basis of comparison between the boreholes investigation and different maps. Although the permafrost degradation was obvious, it mainly occurred as the temperature increasing of ground temperature and deepening of active layer, rather than the disappearance of permafrost.

In the revised version, we added this description briefly in the *Section 4* (P.14 L.440-444).

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.

Response:

The purpose of the comparison was to show that the result in this study was the more accurate map of the permafrost distribution in the contemporary climate. There is a quite small change in permafrost distribution on the TP over the past several decades as we described in *Comment 8*. Therefore, the primary causes for differences in the three maps were the different methods and data sources (data quantity, accuracy, and regional representativeness). The difference areas are mainly distributed in the periphery of the continuous permafrost and high mountainous regions where local factors play more important roles. In view of the TP-2016 has a better performance in complex terrain, it is reasonable to infer that the accuracy of the TP-2016 is higher than that of the other two maps in the difference areas. In addition, the changes in the permafrost distribution should be much smaller than the difference caused by the different methods and data source.

In the revised version, we discussed this issue in the *Section 4* (P.14 L.440-445).

10. Issue of permafrost conditions out of equilibrium with today's climate i.e. warming permafrost conditions, should be discussed. Surface forcing could indicate no permafrost according to today's 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.

Response:

It is really a good question. Permafrost on the TP is out of equilibrium under global climate warming. As the reviewer said, there exists a long response time of permafrost bodies to atmospheric conditions, even millions of years. All the maps using modern climate conditions are difficult to solve the problem because the evolution of permafrost is a large time scale issue. In the view of the solution of permafrost identify, the boundary of permafrost is the most sensitive region to the climate change and has close relation with the contemporary climate. Therefore, the essential of modern permafrost mapping is how to improve the accuracy of surface forcing and the soil parameters to identify the boundary. In this study, the TTOP modelling based on remote sensing LST and plenty of current in situ soil parameters observation shows a high accuracy in the validation. However, the results might not capture thawing permafrost bodies, and more works still were needed to be done.

In the revised version, we discussed this issue in the *Section 4* (P.14 L.436-447).

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 “seasonally 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.

Response:

To avoid any confusions, all the “thawing region” was instead of “seasonally frozen ground” in the revised manuscript. Thanks for pointing out.

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

Response:

The language of *Section 4* has been polished carefully.

Minor comments

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

Response:

The reference of *Gruber (2012)* has cited in the *Introduction* and removed from the *Discussion* section (P.2 L.61-64).

2. p.6 l.174: massive -> numerous

Response:

The “massive” has revised to “numerous” (P.6 L.174).

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.

Response:

The description of HANTS has been added and the details was left to the reference (P.6 L.176-179).

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

Response:

The overpass times and the total swathes of MODIS have been added (P.6 L.180-183).

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.

Response:

The mention that k_t/k_f comes from properties derived in Section 2.2.3 has been added (P.8 L.230).

6. p.9 l.260: “decreases gradient” -> “decreases linearly”, is that what you mean?

Response:

That is exactly what we mean. We have revised accordingly (P.9 L.261).

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.

Response:

Thanks for the comments. The “high” has changed to “medium” (P.14 L.416).

8. p.13 l.416: “it can reflects” -> “it can represent”

Response:

We have revised accordingly (P.14 L.417).

9. p.14 l.421-422: “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.”

Response:

Thanks. Changed accordingly (P.14 L.421-422).

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".

Response:

To avoid any confusions, the sentence has removed.

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

Response:

The permafrost distribution of figure 1 is the result in this study without the unfrozen ground that we want to show the IRs are right on the boundary. To avoid any confusion, it has been changed to the permafrost map made in 1996 in the revised version (P.24).

12. Acknowledgments: remove "Level 1 and".

Response:

The "Level 1 and" has removed from the Acknowledgments. Thanks.

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