Reply to

Interactive comment on "Effects of Stratified Active Layers on the High-Altitude Permafrost Warming: A Case Study on the Qinghai-Tibet Plateau" by X. Pan et al.
Anonymous Referee #1

We thank the reviewer for her/his insight comments and suggestions, which make modeling results and interpretation clearer and conciser. We tried our best to implement the suggested changes. This includes the addition of figures, references, as well as wording problems.

With regards to the major comments:

1. "*The authors need to be clearer regarding what causes thermal offsets. They mention vague processes like 'seasonal variability in thermal conductivity' (P2,L15-20; Section 3.2, Section 3.6.2) but they can be much more explicit. It is not complicated. ...*"

Thank you for this suggestion. We have reformulated the last Paragraph in Section 3.6.2 as: "Theoretically, thermal offset (TTOP - MAGST) origins from different heat transfer efficiency of an active layer between summer and winter in a permafrost region at equilibrium state. Since the thermal conductivity of ice is four times of water's, it can significantly enhance thermal conductivity of saturated soils from summer to winter. Given a constant and high soil water content, a negative thermal offset will occur due to a much higher thermal conductivity in winter than that in summer. However, the thermal offset can also be negative when the mean total soil water content is higher in summer than in winter. For instance, the positive thermal offset at equilibrium state in Fig. 10 was mainly led by the high ratio of λ_t/λ_f around 1 via seasonal water content reduction. The schematic mean annual ground temperature profile is closer to the one described by Brown (1970) other than the one suggested by Smith and Riseborough (2002) as shown in Fig. 11. In addition, the concept of thermal offset will be invalid at disequilibrium conditions. For instance, the thermal offsets in Fig. 10 decrease dramatically along with climate warming. It is obvious that the decreasing negative thermal offsets were not caused by $\lambda_f > \lambda_t$ but by the lag between surface and subsurface warming. This is corroborated by the observed thermal offsets (Fig. 4) that positive values occurred in 2007 and 2008 and they decreased to negative in 2009 and 2013. Therefore, the concept of the normal offset is not suitable for the studied case, and the plausible "normal" thermal offset might not necessarily be attributed to $\lambda_f > \lambda_t$, but to permafrost disequilibrium."

2. "*The authors jump right into permafrost thaw in the introduction without any mention of why it is important. I understand that this is a cold regions journal, but some context would be nice. I think the authors should highlight that permafrost thaw has considerable implications for surface and subsurface hydrologic routing (e.g. Kurylyk et al., 2014 ESR), geotechnical failures (Harris et al., 2009, already cited), and carbon dioxide and methane release (Schuur et al., 2015). Those papers are broad review papers on permafrost thaw impacts.* "

Introduction of permafrost thaw impacts are added in the Introduction section.

3. " *Section 3.1. I don't understand how there could be a 5℃ difference between the mean air temperature and mean surface (or very close to surface) temperature. Such differences are commonly observed (e.g. Zhang et al., 2005) but only in regions that experience deep snowpack (Zhang, 2005). This seems like a very high difference without considerable snowpack. Could the authors explain?*"

The notable difference between the mean air temperature (MAAT) and mean ground surface temperature (MAGST) should be attributed to the low absolute humidity in the air at altitude above 4400 m and diurnal freeze-thaw process in the near-surface soils at the study site. As pointed out in the Introduction section, a strong diurnal forcing with some 180 freeze-thaw days (Yang et al., 2007) is very typical on the Qinghai-Tibet Plateau (QTP). Generally, the low absolute humidity can lead to a large diurnal dynamic range in air temperature. As a result, the lower humidity in winter leads to a bigger diurnal range of air temperature, e.g., > 20℃ than that in summer at the study site. In addition, near-surface soil freeze-thaw process strengths the difference. Particularly, the difference in winter is much bigger than that in summer, because the phase change in the frozen soils takes a large amount energy.

Figure 3.5 shows an example of the daily differences between MAAT and MAGST, and their diurnal dynamic ranges at the study site. P1 and P2 shows two monitoring locations. More details can be found in Pan (2011).

Reference: Pan X.: Hydraulic and Thermal Dynamics at Various Permafrost Sites on the Qinghai-Tibet Plateau. PhD thesis: P57. www.ub.uni-heidelberg.de/**arc**hiv/11934

4. "*Section 2.4.1. Where does the water go in the model if no lateral flows are allowed (P4, L11) and no vertical drainage is allowed out of the bottom (P4, L23)? Are ET and P presumed to be balanced? This is very confusing. Also, P4, L26 implies that the thermal conductivity and porosity are used to compute the geothermal flux, when it is really the conductivity times the gradient. Also, in this paragraph and in many other cases through the manuscript, the authors use the term 'soil matrix' to refer to the solid particles. Sometimes matrix (in the context of thermal conductivity) means the matrix of water and solid grains. I recommend that here and elsewhere the authors change the terminology from soil matrix to 'soil particles' or 'soil grains'.*"

In the 1D GEOtop model, water balance of the soil profile is mainly controlled by the ET, P and soil water storage. Since potential evaporation is usually much bigger than the actual evaporation on the QTP, overland flow is rare for the studied case.

The calculation of the geothermal flux was correct (1.95*0.07≈0.14), where we calculated it as frozen soil as

$$\lambda_b = \left[(1-0.2)\times\sqrt{2} + 0.05\times\sqrt{0.567} + 0.15\times\sqrt{2.29}\right]^2 = 1.95$$

However, since the measured gradient was derived from the measurements below permafrost base, we should calculate it for the thawed soil. Thus, λ_t = 1.64 and the geothermal flux is 0.11 W m^{-2}. It is corrected in all simulations in the revised manuscript. Thank you for pointing out this issue.

The soil matrix is replaced with soil particles in the revised manuscript.

5. "*The modeling results (simulated vs. observed, Fig. 8) are not good. This modeling exercise certainly did not 'validate' (P8, L2) their model. Observed warming is about twice the modeled rate. The authors propose that this is due to (1) not accounting for snow, (2) difference in simulated and observed climate data, and (3) model assumptions. Okay I can buy that. But why not examine (1) by comparing measured and simulated surface temperatures? In other words, if the problem with the model occurs at the atmosphere-soil interface, then you can easily demonstrate that by comparing the measured and simulated temperature at this point. Also, why did the authors use climate model data for a period when they had site data? This makes no sense at all. Just use the climate data for the future period, not for the model performance assessment period. So I believe the authors can easily test (1) and (2) above.*"

We apologize for misusing the word "validate".

Regard to the suggestion (1), the appended Figure 1 shows the measured and simulated near-surface temperature. Evident deviations occur in the winter, which lead to lower surface temperatures over the range of -7 ~ 5℃. This should be caused by occasional snowcovers.

Regard to the suggestion (2), we partially agree with that. Firstly, to make the simulation more realistic, the climate model data were also adapted with local observations (Figure 7). Therefore, the forcing data may provide an opportunity to mimic the observed permafrost warming for the observed period. Secondly, the aim of comparing the observed and simulated warming rates over a 9-year period is to assess the model performance for the whole period from 1980 to the observed period. Although the results are not so good, it exposes the suggested limitations of our simulations. Thirdly,

Figure 8 also provides a hint for the small differences in the thermal regimes for the three soil architectures in Figure 12.

6. "*Section 3.6.3 and Figure 12, The differences in the thermal regimes for the three soil architectures (A1, A2, and A3) seems rather minor in my opinion (for both the left and right columns in Figure 12). Since this is a major point of the paper, I'm left wondering, 'what's the point'? The series are virtually indistinguishable from 2040 onwards*."

We agree with your comment that they are no so significant in Figure 12. The reasons are given as follows.

The effect of the seasonal soil moisture reduction on λ_t/λ_f is mainly controlled by the thermal conductivity of soil particles and the seasonal soil moisture reduction. Since λ_t/λ_f is very sensitive to the seasonal reduction amount and initial water content, the effect of the seasonal soil moisture reduction on λ_t/λ_f is related to the soil architecture. The appended Figure 2 gives an example of the impact of summer water content on λ_t/λ_f . Given a constant moisture reduction of 10%, the smaller the summer water content, the higher the λ_t/λ_f. Unfortunately, given current parameterization it is challenging for our model to capture the field reality, as a result, the simulated seasonal soil moisture reduction is not as significant as the observed one (Figure 9c v.s. Figure 9c'). In Figure 12, the differences are bigger in the right column than in the left column, and this is because of the smaller thermal conductivity of soil particles. While A3 is warmer than A1 and A2, particularly in the right column. The soil architecture A3 does form a dry middle layer (around 1 m in Figure 9c), where smaller seasonal soil moisture reduction could lead to a higher λ_t/λ_f.

In general, Figure 12 demonstrates the effect of soil architectures with the different seasonal soil moisture reductions on permafrost warming, although it is not perfect. Besides, it is possible to get more significant differencesif the simulated seasonal soil moisture reduction is more accurate by using better parameterization for hydraulic properties in future.

The results from 2040 onwards provide a hint of the persistence of our hypothesis over time (see reply to the specific comment 7 in #RC 1), apart from the point that the stratified active layers and associated seasonal soil moisture reduction enhance the high-altitude permafrost warming.

Minor comments

1. *Title, delete 'the' as it is not needed and sounds funny*
Done

2. *P1, L16-17, delete 'with thickness larger than some 1.5 m' and insert '(>1.5 m) after 'thick'*
Done

3. *P1, L17, 'It is additionally furthered' is unclear. This should be something like 'The conductivity ratio can be further increased' or something like this*
Done.

4. P1, L23, 'high mountains' should be 'alpine regions'. The Alps is not a mountain, for example.
Done.

5. P1, L26, delete 'the' after 'Generally' and 'the' after 'than'
Done.

6. P1, L28, 'Their response': : :.whose response?
It is rephrased as "Responses of permafrost controlled by these local factors to climate change is thus also expected to differ."

7. The last sentence on P1 should be moved before the sentence beginning with 'For instance' The first paragraph reads a bit like a Wu et al. fan club press release. I think it would be better to incorporate some of the implications of permafrost thaw in this paragraph (see major comment above)

Rephrased in the revised manuscript.

8. P2, L3, The reported permafrost warming rate is half of the air temperature warming rate (we would expect it to be lower, so that is fine); however, the paragraph reads as if the permafrost warming rate is higher. I'm not convinced that it makes sense to compare the permafrost warming to air temperature warming over a 13 year period (2002-2014). The subsurface warming rate is lagged (and typically damped) in response to a surface (or atmospheric) warming period. The lag is not that important when you are talking about a 100 year period, but it certainly is over 13 year period.

We agree with that it is problematic to compare the warming rates of permafrost and air temperature without considering time lag. Nevertheless, that is cited from the referenced paper. But the inference should be reasonable if the indicated the background of climate warming for the past few decades on the QTP is added. The sentence is rephrased as " Comparing to the rate of 0.3˚C per decade for the past five decades over the QTP (Piao et al., 2010), there was no extraordinary increase in air temperature (0.02˚C yr^{-1}) at the investigated area. Thus, an average increasing rate of 0.01˚C yr^{-1} at 10-m depth in permafrost temperature sounds rather high.".

Piao, S.L., Ciais, P., Huang, Y., Shen, Z.H., Peng, S.S., Li, J.S., Zhou, L.P., Liu, H.Y., Ma, Y.C., Ding, Y.H., Friedlingstein, P., Liu, C.Z., Tan, K., Yu, Y.Q., Zhang, T.Y., and Fang, J.Y.: The impacts of climate change on water resources and agriculture in China, Nature, 467, 43 – 51, 2010.

9. P2, L8, insert 'the' before 'atmosphere'

Done

10. P2, L11, I'm confused by the comment regarding diurnal forcing and freeze-thaw days. Permafrost is not really diurnally forced.

See responses for major comment 3. The diurnal freeze-thawing modifies the coupling between atmosphere and land surface, as well as permafrost.

11. P2, somewhere the authors could consider citing Hayashi et al. 2007 who proposed a Stefan type algorithm to deal with the problem the paper focuses on (i.e. Changing moisture content through the season).

Hayashi et al. 2007. A simple heat conduction method for simulating the frost-table depth in hydrological models. Hydrol. Process. 21(19)

Done

12. P2, L22, Delete 'the' before 'permafrost'

Done

13. P2, L23 Delete 'a' before 'recent'

Done

14. P2, L25, I don't think diagnose is the right word here: : :.maybe characterize?

Done

15. P3, L12. If the permafrost is 25 m, than the soil at a depth of 10 m must be permafrost. So of course, the temperature would have to be less than 1.0C. In fact, it would have to be less than 0C or it is not permafrost.

Missing a sign "-". Corrected.

16. Heading for section 2.2 contains an extra 'Subsection'

Done.

17. P3, L27, Delete 'the' before 'GEOtop'. Also this sentence would read much better if 'surface and soil, and the soil freezing' were replaced with 'surface and soil as well as the soil freezing and thawing'. Otherwise it sounds like freezing and thawing is included in the list containing atmosphere, surface, and soil.

Done.

18. P3, L29, Change 'allows to simulate' to 'simulates'

Done

19. P3, L30, Delete 'the' before 'complex'

Done

20. P4, L2, the concept of relating the soil freezing curve and soil drying curve is quite foreign to most permafrost scientists. Consider citing the review on this topic.

Kurylyk and Watanabe. 2013. The mathematical representation of freezing and thawing processes in variably-saturated, non-deformable soils. Adv. Wat. Res. 60, 160-177

Done

21. P4, L2, change 'allows' to 'enables the user'

Done

22. P4, L11 delete 'given as'

Done

23. P4, L15, change 'with a high resolution in size of 10 cm: : :and was gradually reduced' to something like 'with elements with a height of 10 cm: : :and reducing to: : :' or something like that

Done

24. P5, L14-16, This is a fragment and confusing

It is moved and merged into the introduction section.

25. P5, L18, insert 'the' before 'active layer'. Insert 'the' after with

Done

26. P5, L24, change 'on' to 'of the'

Done

27. P5, L27. There should be an appropriate citation for CMIP5. If I remember correctly, there is a brief paper published describing the dataset

Done.

28. P5, L32, change 'a quick' to 'the rapid'

Done

29. P6, L3. I'm curious how many 10 year periods were run for the spin up (i.e. how many cycles). This should be mentioned.

We used 15 times of 10-year period for spin up. It is added in the revised manuscript.

30. P6, L11, delete 'well-fitted'

Done

31. P6, L11, insert 'in the very shallow subsurface' after 'heat transfer'
Done

32. P6, L16, insert 'mean annual' before 'thermal profiles. Delete '. It covers the mean annual temperature data', i.e. combine first two sentences into 1.
Done

33. P6, L21. MAGT should be MAGST shouldn't it?
Done

34. P6, L26, insert 'with the fact' after conflict
Done

35. P6, L27, change 'exists' to 'exhibits'
Done

36. P7, L11, change 'else' to 'otherwise'
Done

37. P7, L12, change 'from' to 'that'
Done

38. Last sentence in P7 sounds like it should be in introduction not 3/4 of the way through the paper
*R*emoved in the revised manuscript.

38. P8, L7, Delete 'it is higher: : :..Wu et al. (2015)'. This is not relevant given how different the periods are.
Done

39. P8, L31, 'validated' should be 'investigated' or something like this
Done

40. P9, L7, change 'contrast' to 'the'
Done

41. P9, L13, 'underestimated permafrost temperature' is not really a good physical explanation for why thawing is slower in the model than in observations. Of course, this is caused by underestimated permafrost temperature, but the question that should be addressed is 'why is the permafrost temperature underestimated?'
Revised in the manuscript as "This is mainly related to the different soil water content distributions between observed and simulated ones. The overestimated soil water storage in the shallow layer in the simulation lagged its thawing rate. "

42. P9, L18, I'm confused by the statement 'and disappears till talik present'
Revised as "and disappears when talik presents".

43. P9, L26, 'more close' should be 'closer'
Done

44. P10, L8, delete 'to'
Done

45. P10, L17-19, this is a fragment

It is removed in the revised manuscript.

46. P10, L20, is this 'extraordinary permafrost warming rate' referring to observed or simulated warming?

That's observed one. Revised in the revised manuscript.

47. P10, L25-27. This sentence seems to contradict itself (although I know what the authors mean): 'In contrast to the normal offset caused by the seasonally variable thermal conductivity, a reversed thermal offset at equilibrium state is formed due to the remarkable high ratio of seasonal thermal conductivity

It is rephrased in the revised manuscript as

"In contrast to the normal thermal offset caused by the low ratio of seasonal thermal conductivity, a reversed thermal offset at equilibrium state is formed due to the remarkable high ratio of seasonal thermal conductivity, namely close 1.0 or even higher, given such a weather pattern and soil properties."

48. Figure 4 – different colours for the series would be helpful (after all TC is all online anyway)

Done

49. Figure 6 caption. The thermal conductivities in (b) are calculated via Eq. (1) right? If so, this should be stated in the caption. Also, how is the ice content obtained for this equation? Somewhere it is stated that the moisture content is assumed to stay the same in the winter. So then the ice is calculated as the total minus liquid?

Yes, The thermal conductivities were calculated with Eq. (1). Added in the caption in revised manuscript. The ice content is calculated as soil water content measured just before freezing minus the measured liquid water content in frozen soils. Here we assume the soil moisture migration is negligible due to the coarse soils.

50. Figure 7 caption: change 'on' to 'of' in both places.

Done

51. Figure 9, 10, and 12. The authors should clearly highlight the differences between the left and right columns (Figure 9 and 12) and the top and bottom (Figure 10). I think it is better to label the figure panels rather than put this info in the caption. Otherwise the reader is scanning up and down.

Done.

52. Table 1. How was the solid particle thermal conductivity of 5 W/(m K) chosen? This is rather high for sand grains in my experience.

The value was estimated from local time series of soil temperature measurements. The apparent thermal diffusivity can be inversely estimated by using transfer function, then the effective thermal conductivity can be calculated with thermal properties of soil components. More details can be found in Pan (2011). Finally, the thermal conductivity of the soil particle can be inversely estimated with Eq. (1).

Reference: Pan X.: Hydraulic and Thermal Dynamics at Various Permafrost Sites on the Qinghai-Tibet Plateau. PhD thesis: P57. www.ub.uni-heidelberg.de/**arc**hiv/11934

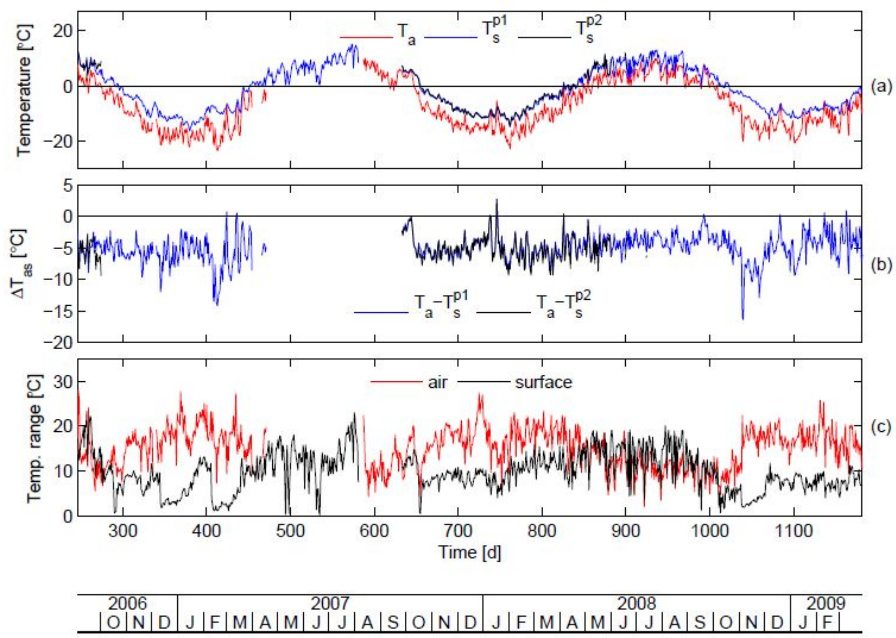
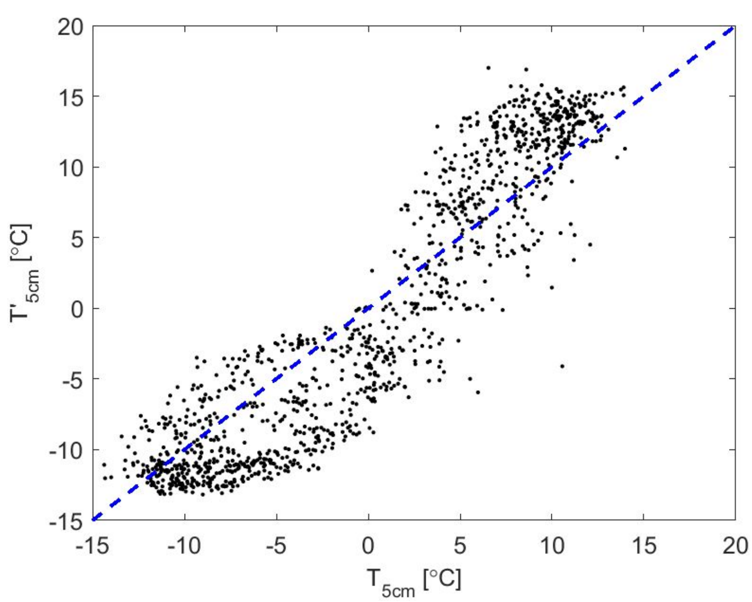
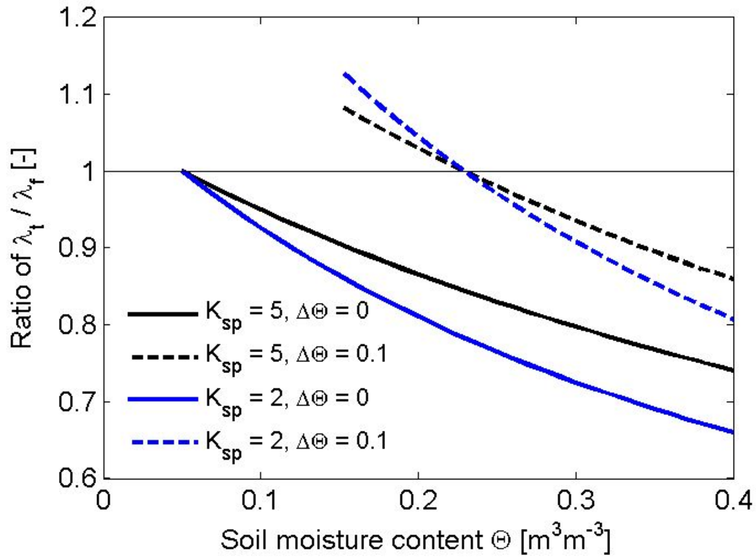


Figure 3.5. Characteristics of the air temperature T_a (2.0 m above the ground surface) and the ground surface temperature T_s (0.05 m below the surface) at Chumaer. (a) the measured air temperature and the ground surface temperature; (b) the differences between air temperature and ground surface temperature; (c) the diurnal range of air temperature and ground surface temperature.



Append Figure 1 Comparison of the simulated near-surface soil temperature (T'_{5cm}) and observed one (T_{5cm}) over the period of 2006-2014.



Append Figure 2 Influence of the summer soil moisture content and thermal conductivity of soil particles (K_{sp}) on the ratio of seasonal thermal conductivity (λ_t/λ_f).