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:
"Thermal offset (TTOP - MAGST) originates from different heat transfer efficiencies of an active layer between summer and winter in a permafrost region at equilibrium state. These mainly result from thermal conductivity of ice being four times of water's. Correspondingly, the product of temperature gradient and time to transfer a given amount of heat by conduction is by a factor of four smaller in the frozen state than it is in the thawed state. With this, the annual mean temperature profile is shifted towards the summer profile and the thermal offset is typically negative. Its value is modified by several aspects, for instance decreased by a snow cover and increased by a soil water content that 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. This phenomenon is prone to appear in the thick stratified active layers on the QTP with summer monsoon dominated precipitation pattern. Generally, 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 adopt 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

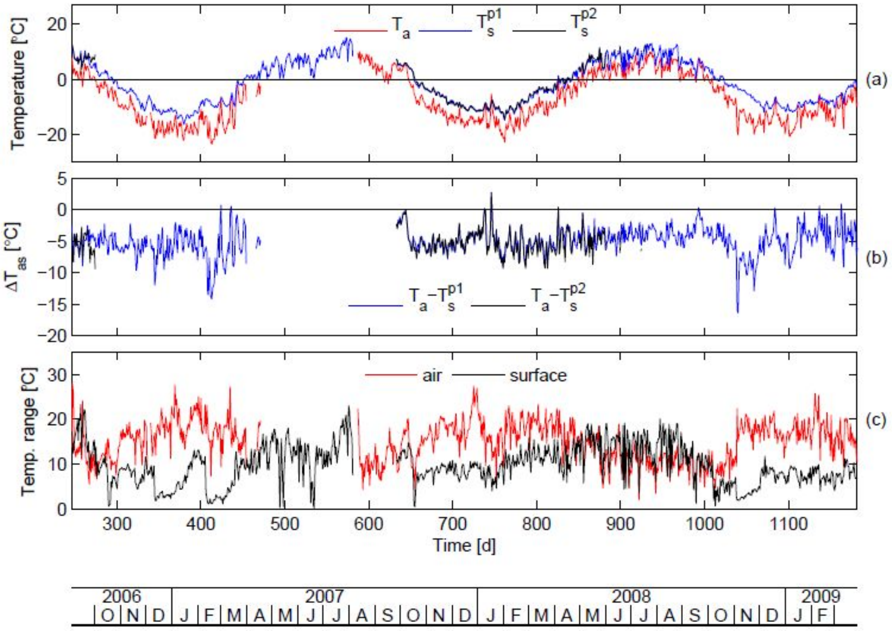
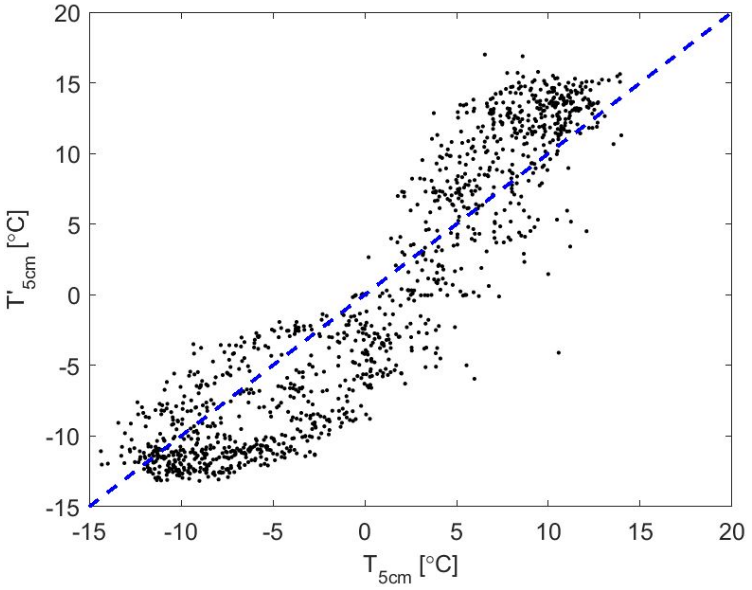
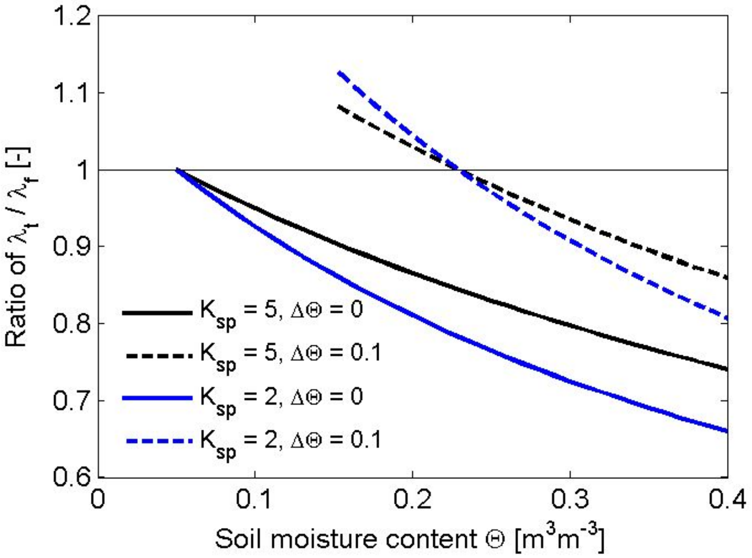


Figure 3.5. Characteristics of the air temperature T_a (2.0 m above the ground surface) and the ground surface temperature T_s (0.05 m below the surface) at Chumaer. (a) the measured air temperature and the ground surface temperature; (b) the differences between air temperature and ground surface temperature;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

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 #2

We thank the reviewer for her/his insight comments and suggestions, which significantly enhance the quality of our paper. Regard to the suggested quantitative arguments and the implications of the findings, we have added them in the revised manuscript.

With regards to the major comments:

1. "*The formula for « thermal offset » and « surface offset » should be recall (in the introduction) for better clarity. In section 3.2, confusion is introduced about « thermal offset » : it was defined in the introduction as « TTOP – MAGST ». In section 3.2 it is approximated by « T(-2.18 m below surface) – MAGT » with MAGT quite different from MAGST. Please clarify.*"

As suggested, the thermal offset and surface offset are recalled in the revised manuscript. The "MAGST" was accidentally written as "MAGT" in that sentence. It was replaced with "MAGST" in the revised manuscript.

2. "*Section 2.2 is entitled : 2.2 Surface-subsurface monitoring scheme Subsection. « Scheme subsection » could be deleted from the title.*"

The "subsection" is removed.

3. "*The defined soil architectures in Section « 2.4.2 Simulation protocol » are not consistent with the caption of Fig 9 and the explanations of Section 3.6.1. Please make sure the Architecture definition is consistent in the whole document (maybe add a Table).*"

Thank you very much for pointing out this error. It is corrected in the revised manuscript and the Table 2 is also reformulated as appended.

4. "*P8 l 2, L31 : neither the model nor the effect are 'validated' in the current state of the paper. The comments below may give some sense to the validation of the effect through modelling.*"

We apologize for misusing the word "validate". It is replaced with "characterized".

5. "*Concerning the local λt/ λf ratio : Year 2008 is used as an illustration of typical annual conditions. Given that ground temperature and soil water content are being measured at this site since 2006, stepping back from Year 2008 and bringing an interannual perspective would strenghen the paper's conclusion. I at least recommend a Table with the maximum λt/ λf value over the upper 2.18 m of the soil for each year with observations.*"

Appended table 3 is added, and two sentences are inserted as "The inter-annual variation of the maximum λ_t/λ_f in the profile is listed in Table 3. In most years, it is very close or over 1, e.g., 2008 and 2009. The smaller value 0.90 in 2013 is attributed to the wet year with extraordinary rainfall."

6. "*Concerning the impact of the λt/ λf ratio on permafrost warming :*

o Fig 6 could provide the vertical profiles for λt and λf with λm=2.5 W/m/K, in support of the assessment : «In order to exceed the ratio of 1, the seasonal liquid water content has to fall below a certain threshold, which depends on soil thermal conductivity and water content in thawed state. For instance, the soils with high thermal conductivity of soil matrix will need larger liquid water content reduction than that of the soils with small thermal conductivity of soil matrix.»"

The λ_t and λ_f in Figure 6 were calculated with λ_m=5.0 W/m/K. It shows the maximum ratio $\lambda_t/\lambda_f>1$ at the depth of around 1 m. In most years, it is very close or over 1 (Table 3).

In our simulations, λ_t/λ_f are relative lower than the observed ones. Given current parameterizations, it is difficult to reproduce comparable hydraulic and thermal dynamics as observed in the active layer. Particularly, the used hydraulic parameters in our model are just determined from some statistical relationships, which are rather rough. Therefore, our simulations can only capture the seasonal pattern of soil moisture reduction, but the absolute amount of seasonal soil moisture amount, as well as water content in thawed state, are still not good enough. Since λ_t/λ_f is very sensitive to the seasonal reduction amount and soil moisture content in summer, the simulated λ_t/λ_f is relative low. That's also the major reason for the small differences of permafrost regimes among A1, A2 and A3 (Figure 12). Concerning the relative low impact of soil moisture reduction on λ_t/λ_f for the simulations using λ_m=5.0 W/m/K, simulations using λ_m=2.5 W/m/K are better for visualizing the effects of stratified active layer on permafrost warming.

o "A high λ_t/λ_f ratio is advanced as an important argument for an enhanced permafrost warming rate at the observation site. However, Fig. 12 is the only illustration supporting this thesis (as modelling - Fig 8 - fails to reproduce the observed warming) ; it shows that permafrost warming rate is enhanced in the A3 configuration ; the authors explain that this is due to higher λ_t/λ_f ratio, but this ratio is unfortunately never explicited. I highly recommand adding the mean interannual λ_t/λ_f for each of the 10-year periods preceeding the selected years of Fig. 12, and for each soil architecture. This would make the paper's main argument less vague. This point is a Major Comment."

The decadal mean λ_t/λ_f are added in append Figure 13. The simulations in Fig. 8 fail to reproduce the observed warming because of the passable representation of the land-atmosphere coupling in winter (see more details in reply for #RC 1.) as well as limited representation of hydraulic and thermal dynamics in the active layer. However, simulations in Figure 12 support our hypothesis that the stratified active layer with seasonal soil moisture reduction enhances permafrost warming, although they are not so evident. The added Figure 13 further consolidates it. Overall, the active layers with λ_m=5.0 W/m/K (left column) have higher values of λ_t/λ_f than that with λ_m=2.5 W/m/K (right column), they also lead higher warming rates (Figure 12). However, the smaller differences among the architectures in the left column indicate that the impacts of the seasonal soil moisture reduction on λ_t/λ_f are not as significant as that in the right column. Generally, the A3 have higher values of λ_t/λ_f than the others in both columns at the early stage, and they also have relative higher warming rates.

o "P 10 l 13 : the formulation could be improved (like : high -> higher)"
Done.

7. "P*5 l 30 and P8 l 24 : a crucial thing is to know whether the annual cycle of precipitation in the chosen downscaled projections, is still monsoon-like (as today) or shifts to different patterns in future climate. The authors mention that the projected rainfall may not be accurate. However, given the importance of the annual rainfall pattern on the site specific sub-surface thermal dynamics,* ***more investigations*** *on the projected precipitation pattern in the chosen downscaled climate product is needed, in support of the assessment of the impact of λ_t/λ_f on the warming. This point is a Major Comment.*"

We agree with that the monsoon-like precipitation pattern is crucial for the site specific sub-surface thermal dynamics. The prevailing view for the Asian summer monsoon is caused by the elevated heat source driven by the QTP (e.g., Yeh et al., 1957, Yanai et al., 1992). Observations of energy fluxes over the QTP show climate change may led to a shift of monsoon intensity (Duan et al., 2011). However, the Asian monsoon mechanism might not disappear in the near future due to the dominant factor of high elevated topography. Although the monsoon precipitation intensity might shit a little bit, the annual rainfall pattern will still lead to a notable seasonal soil moisture reduction, as well as high ratio of λ_t/λ_f in the active layer.

In addition, as explained for the general comments this study is aim to address the effects of stratified active layers on the high-altitude permafrost warming, the monsoon precipitation pattern is a precondition. The selected climate model does reproduce a monsoon-like precipitation pattern for the whole period (1850-2100). It assumed that the monsoon was not changed in the future. Since we are not aim to predict the permafrost warming but to test our hypothesis that the stratified active layer enhances permafrost warming due to the high ratio of λ_t/λ_f, the selected climate model data is reasonable.

To make clarify the persistence over time of the hypothesis, we add a paragraph to clarify this issue in the revised manuscript as follows.

" The effects of soil architecture on permafrost warming evolve in time. First of all, the Asian summer monsoon, caused by the elevated heat source driven by the QTP (e.g., Yeh et al., 1957, Yanai et al., 1992) may not disappear in the near future, but the monsoon intensity might shift due to climate change (Duan et al., 2011). Secondly, thickening active layers can weaken the role of soil architecture in permafrost warming via to exert impact on the suprapermafrost water level as well as soil water content distribution. Given a monsoon dominated precipitation pattern in the projected climate model data, Fig. 13 shows the evolution of seasonal λ_t/λ_f of the shallow soils (0-1.5 m) over the simulating period of 1980-2100. Decadal mean λ_t/λ_f were calculated for all the simulations. Compared to the same soil architectures but different thermal conductivites of soil particles, the maximum decadal mean λ_t/λ_f in the left panel with λ_{sp} = 5.0 W m^{-1} K^{-1} are higher than that in the right panel with λ_{sp} = 2.5 W m^{-1} K^{-1}. But the differences of λ_t/λ_f among the architectures in the right panel are bigger than in the left panel. Besides, the maximum values of λ_t/λ_f in A3 are bigger than the other two from 1980s to 2040s, then they gradually become smaller. Generally, the shrinking differences in decadal mean λ_t/λ_f among the three architectures indicate that the effects of the stratified active layers on permafrost warming are significant in the early state of permafrost degradation, and they will decrease afterward. These results are all consistent with the thermal regimes in Fig. 12."

Yeh, T.C., Luo, S.W., and Chu, P.C.: The wind structure and heat balance in the lower troposphere over Tibetan Plateau and its surrounding, Acta Meteor. Sin., 28, 108–121, 1957.

Yanai, M., Li, C., and Song, Z.: Seasonal heating of the Tibetan Plateau and its effects on the evolution of the Asian summer monsoon. J. Meteor. Soc. Japan, 70, 319–351, 1992.

Duan, A., Li, F., Wang, M., and Wu, G.: Persistent weakening trend in the spring sensible heat source over the Tibetan Plateau and its impact on the Asian summer monsoon, J. Climate, 24, 5671–5682, 2011.

Technical Corrections

1. "*- Very frequently the authors confuse « whereas » with « while » or « in the opposite ». (p4 l 23 ; p5 l 14 ; p6 l 13 and l 24 ; p9 l 28 ; ...)*"

Done.

2. "*- P2 l 14 : basing -> based*"

Done.

3. "*- P3 l 15 : humility -> humidity*"

Done.

4. "*P4 l 2 : incomplete sentence*"

It is rephrased as "It invokes a relation between the soil freezing characteristic and the soil water characteristic and assumes a rigid soil scheme without change in volume for water phase transition (Kurylyk and Watanabe, 2013)".

5. "*P5 line 13 to 16 : unclear, please reformulate*"
   It is moved and merged into the introduction section.

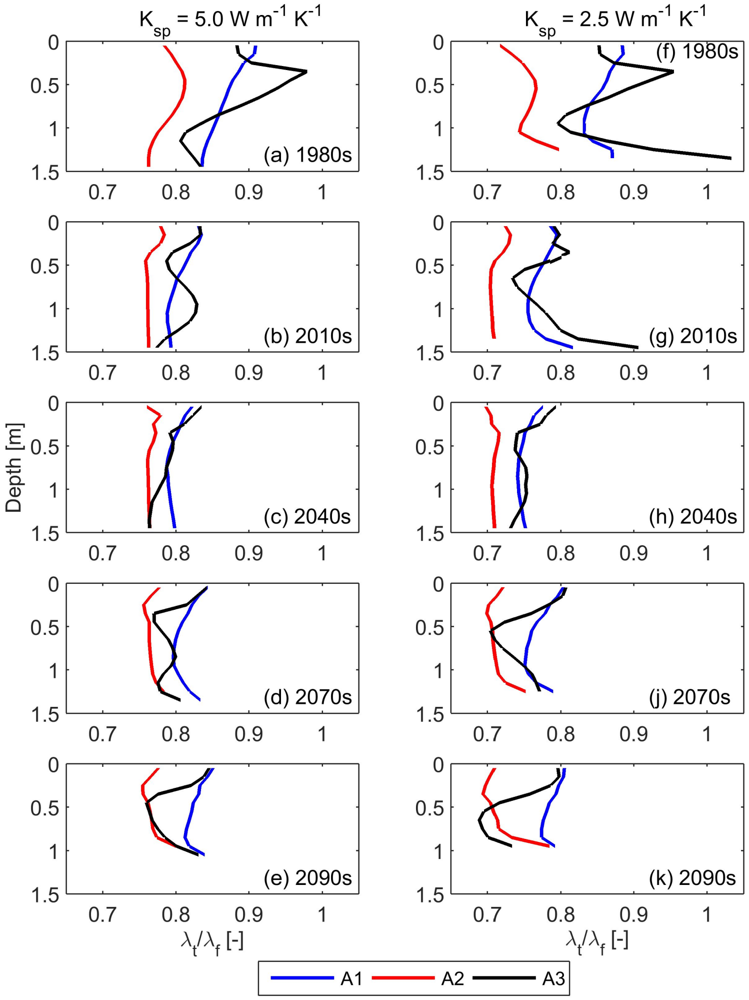
6. "*- P9 l 18 : till talik -> when talik*"
   Done.

Table 2. Six simulations with different combinations of soil architecture and thermal conductivity of soil particles (λ_{sp}) for the shallow soils (0-3.0 m). A1, A2 and A3 stand for three types of soil architecture in Table 1.

λ_{sp} / W m^{-1} K^{-1}	Soil architecture		
	A1	A2	A3
5.0	1	2	3
2.5	4	5	6

Table 3. Inter-annual variation of the maximum seasonal thermal conductivity ratio (λ_t/λ_f) in the monitoring profile. * Hydrological year: from May 1 to the next April 30.

Year*	2007	2008	2009	2010	2011	2012	2013
λ_t/λ_f	0.99	1.01	1.01	0.97	-	1.00	0.90



Append Figure 13. Comparison of the influence of soil architecture and thermal conductivity of soil particles (λ_{sp}) on the seasonal thermal conductivity ratio λ_t/λ_f in the shallow soils (0 - 1.5 m) over the period from 1980 to 2100. (a) - (e) Decadal mean λ_t/λ_f of A1, A2 and A3 with a high λ_{sp} = 5.0 W m^{-1} K^{-1} at selected decades; (f) - (j) the same as (a) - (e) but with a low λ_{sp} = 2.5 W m^{-1} K^{-1}.

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.

Referee #3

We thank Dr. Endrizzi for his insight comments of the modelling issue. Regard to the suggestions about model sensitivity analysis, although we could not take all the suggestions in this paper, they will motivate us to do a thorough investigation in future.

With regards to the major comments:

1. “*The model settings are extremely important since they strongly affect the results. However, the paper does not fully describe them. For example, the paper should list the van Genuchten parameters, since the behaviour during freezing/thawing is based on them. It is not enough to refer to neural network routines.*”

The van Genuchten parameters are listed in revised Table 1.

2. “*The characteristics of the 3 soil architectures A1, A2, A3 are not completely clear to me. You should put a table or drawing that clarifies the soil layers with correspondent properties and parameters*”

Soil properties of the three architectures were detailed in Line 12 - 27 in page 5 as well as shown in the revised Table 1 (attached at the end).

3. “*In the papers the parameters are assigned in a deterministic way. Apart considering 3 soil architectures, no or little sensitivity to parameters is performed. This is extremely important, since many parameters are actually idealised or strongly simplified. The van Genuchten parameters result from a strongly simplified model of soil retention, and, since the results are dependent on them, a sensitivity analysis is essential. Pedotransfer functions and, probably, neural network routines have limitations and cannot be fully trusted. The sensitivity to other parameters should also be considered, for example, when no data are available, for bottom soil, snow precipitation, lateral flow, albedo, etc. In addition, you set the vegetation coverage to 0.3, referring to Gubler at al. (2013), but in this paper we did not consider vegetation.*”

We agree with the reviewer concerning the nature of the model parameters and the validity of methods to estimate them independently. The approach in this paper is to use best available independent information for the simulation. Besides corroborating the general understanding of the observed processes, this also demonstrates the challenges for quantifying situations where data are not available, which is the vast majority, unfortunately.

The next step will encompass a site-specific sensitivity analysis of the simulation followed by a proper inversion for the parameters. This will then also provide the statistical basis for better assessing the true uncertainties. That next step is beyond the scope of the current paper, however.

4. “*The simulation settings also assume simplified conditions that are described only at the end of paragraph 3.5, namely to justify disagreements between observations and model results. The simplifications should be listed at the beginning, and their plausibility discussed in advance.*”

Agree. It is revised in the new manuscript.

5. "*In par. 3.6 you write that the effect of stratified active layer is validated with modelling. Validate is a strong word. You are not validating, but you are using the model to understand physical processes.*"

Agree. See more explanation in the reply for referee #1.

6. "*The formula of Cosenza et al. (2003) is just one parameterisation of bulk thermal conductivity. GEOtop gives also the possibility to use other formulae (De Vries for example). Maybe it would be worth checking if there are significant differences in the results if other formulae are used.*"

First of all, we do believe that there might be significant differences in the results if we use improper formulae for the bulk thermal conductivity. Here we choose the formula of Cosenza et al. (2003), because it has been verified with some published data in satisfactory agreement both for saturated rocks and for unsaturated soils.

7. "*In Fig. 6a you consider only unfrozen water content. However, bulk thermal conductivity also depends on ice content. You should discuss this point.*"

We did consider the ice content for calculating the bulk thermal conductivity. The total water content in the caption means the sum of unfrozen water content and ice content. To avoid misunderstanding, the total liquid water content has been replaced with "total water content" in the text.

8. "*I do not understand why in Fig. 5b the 0 °C isotherm is not close to the curve of the unfrozen water content decrease.*"

This is quite common in field observations. First of all, the freezing point of soil water will be reduced below 0°C due to soil salinity. Secondly, the soil temperature gradient is so small within the zero-curtain that the 0°C isotherm is not close to the curve of the unfrozen water content decrease.

9. "*In the paper you often use temperature/time as a proxy of permafrost warming. However, temperature only describes the effect of sensible heat, but not the latent heat. If permafrost has a temperature close to 0°C, more heat is needed to increase soil temperature, because some energy is needed for thawing. Therefore, I do not think that a temperature difference of 0.01°C to end spinup is good. Performing a good spinup is also essential to have good model results. This should be more completely described. For how many years the spinup simulation was run? You should also check that water and ice content differences are small to end spinup.*"

We agree that checking temperature difference in conjunction with water and ice content differences would be more reliable. In this study, to reach a temperature difference of 0.01, the spinup simulation runs for 150 years, and the total mean annual water content difference is less than 0.01. This should be fine.

10. "*In 2014 I wrote a paper describing the model, in particular the version 2.00. Although you used a previous version, you should have a look and cite the paper. This is the link to the paper: http://www.geosci-model-dev.net/7/2831/2014/gmd-7-2831-2014.html*"

Done.

11. "*I saw some errors in the English language. Please correct them.*"

Done.

Table 1. Soil properties of shallow soils (A: 0-3.0 m) and underlying soils (B: 3.0-30 m) for three soil profiles (A1/B, A2/B and A3/B). K_s: saturated hydraulic conductivity; α and n: van Genuchten parameters; θ_r and θ_s: residual and saturated soil water content, respectively; λ_{sp}: thermal conductivity of soil particles; C: thermal capacity.

Soil architecture		A1	A2	A3		B
		0-3.0 m	0-3.0 m	0-0.3 m	0.3-3.0 m	3.0-30 m
Soil texture %	sand	66.3	92.2	66.3	92.2	-
	silt	12.0	3.8	12.0	3.8	-
	clay	21.7	4.0	21.7	4.0	-
Hydraulic properties	K_s / m d^{-1}	0.19	4.68	0.19	4.68	2.2×10^{-3}
	α / cm^{-1}	0.03	0.03	0.03	0.03	0.01
	n / -	1.33	2.85	1.33	2.85	1.5
	θ_r / m^3 m^{-3}	0.06	0.05	0.06	0.05	0.10
	θ_s / m^3 m^{-3}	0.38	0.38	0.38	0.38	0.2
Thermal properties	λ_{sp} / W m^{-1} K^{-1}	5.0				2.0
	C / J m^{-3} K^{-1}	2×10^6				