The Cryosphere Discuss.,
doi:10.5194/tc-2015-201-RC1, 2016

The Cryosphere Discussions
Open Access
EGU

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

Received and published: 19 January 2016

Review of Pan et al., 2016 HESSD

Pan et al. demonstrate that the hydrologic (or hydrogeologic) properties and conditions of the active layer influenced the active layer thermal regime and hence the rate of permafrost warming in response to climate change. In particular, they demonstrate that the active layer hydrologic properties can cause a thermal offset that is opposite in magnitude than what is normally observed (i.e. their TTOP > MAGST) and that not considering this in permafrost models can lead to underestimating future permafrost thaw. I think that the study is of interest and may warrant publication eventually. The authors need to make a number of points more clearly. I also question their modeling results/interpretation.

Full screen / Esc

Printer-friendly version

Discussion paper

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. If the moisture content is the same throughout the year, thermal conductivity is higher in winter than in summer because pore ice has about four times the thermal conductivity of pore water. Thus heat transfer will be more efficient in winter than in summer causing a thermal offset (TTOP is colder than MAGST). In order for the sign of this offset to be switched, heat transfer must be more efficient in the summer. This means that moisture content must be considerably higher in the summer than in the winter. For example, if the pore ice has a thermal conductivity roughly four times the thermal conductivity of pore water (and assuming that air has a negligible thermal conductivity, which it pretty much does), than a homogeneous medium would need over four times the moisture content in the summer compared to the winter to have more efficient summer heat transfer. Correct? I feel like these simple facts are obfuscated in the manuscript. This should especially be addressed at the end of section 3.6.2. The authors suggest that climate warming and permafrost disequilibrium can cause the offset observed by Smith and Riseborough and others. They should explain, phenomenologically, why this would be the case. I would have thought that the lag between surface and subsurface warming would actually make the thermal offset more negative with time under extreme warming. So the results are unintuitive in my opinion. This could potentially make them more interesting, but they have to be explain from a physical perspective.

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.

Kurylyk BL et al. 2014. Climate change impacts on groundwater and soil temperatures in cold and temperate regions: Implications, mathematical theory, and emerging simulation tools. Earth Sci. Rev. 138, 313-334.

Schurr et al. 2015. Climate change and the permafrost carbon feedback. Nature 520: 171-179.

3. Section 3.1. I don't understand how there could be a 5°C 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?

Zhang et al. 2005. Soil temperature in Canada during the twentieth century: complex response to atmospheric climate change. J. Geophys. Res. 110 (D03112)

Zhang T. 2005. Influence of the seasonal snow cover on the ground thermal regime: an overview. Rev. Geophys. 43 (4).

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 computer 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'.

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.

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.

Minor comments

Title, delete 'the' as it is not needed and sounds funny

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

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

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

P1, L26, delete 'the' after 'Generally' and 'the' after 'than'

P1, L28, 'Their response'. . ..whose response?

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)

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.

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

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

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)

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

P2, L23 Delete 'a' before 'recent'

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

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 1.0°C. In fact, it would have to be less than 0°C or it is not permafrost.

Heading for section 2.2 contains an extra 'Subsection'

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.

P3, L29, Change ‘allows to simulate’ to ‘simulates’

P3, L30, Delete ‘the’ before ‘complex’

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

P4, L2, change ‘allows’ to ‘enables the user’ P4, L11 delete ‘given as’

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

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

P5, L18, insert ‘the’ before ‘active layer’. Insert ‘the’ after with

P5, L24, change ‘on’ to ‘of the’

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

P5, L32, change ‘a quick’ to ‘the rapid’

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.

P6, L11, delete ‘well-fitted’

P6, L11, insert ‘in the very shallow subsurface’ after ‘heat transfer’

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

P6, L21. MAGT should be MAGST shouldn’t it?

P6, L26, insert ‘with the fact’ after conflict

P6, L27, change ‘exists’ to ‘exhibits’

P7, L11, change ‘else’ to ‘otherwise’

P7, L12, change ‘from’ to ‘that’

Last sentence in P7 sounds like it should be in introduction not $\frac{3}{4}$ of the way through the paper

P8, L7, Delete ‘it is higher. . ...Wu et al. (2015)’. This is not relevant given how different the periods are.

P8, L31, ‘validated’ should be ‘investigated’ or something like this

P9, L7, change ‘contrast’ to ‘the’

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?’

P9, L18, I’m confused by the statement ‘and disappears till talik present’

P9, L26, ‘more close’ should be ‘closer’

P10, L8, delete ‘to’

P10, L17-19, this is a fragment

P10, L20, is this ‘extraordinary permafrost warming rate’ referring to observed or simulated warming?

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

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

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?

Figure 7 caption: change ‘on’ to ‘of’ in both places.

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.

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.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2015-201, 2016.