The Cryosphere Discuss.,
doi:10.5194/tc-2016-134-RC2, 2016

The Cryosphere Discussions
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Interactive comment on "Effects of Seasonal Snow Cover on Hydrothermal Conditions of the Active Layer in the Northeastern Qinghai-Tibet Plateau" *by* Ji Chen et al.

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Received and published: 24 October 2016

Introduction:

This paper analyzes the snow cover's effect on the hydrothermal regime of the active layer. In contrast to the majority of previous studies on this topic, this study is conducted in the very cold and dry climate of the Qinghai-Tibet Plateau. The snow cover season is marginal, with snow cover duration normally no longer than 5 days, and snow heights typically 5 cm. To analyze the effect of the snow cover on the ground thermal regime, snow is removed after every snowfall, and ground temperature and soil moisture measurements are compared between this site and a natural site. The authors find that the removal of the marginal snow cover still has an effect on the ground, and

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drying and cooling of the ground is observed at the snow removal site.

This study addresses a topic much investigated before. The climate of the study location, however, is very different from the environmental setting of most other studies of this type, making the study both interesting and an important scientific contribution. The very short duration of snow covered ground and very shallow snow cover results in other dominating effects from the snow cover on the underlying ground, and the large amount of field data allows for a detailed analysis of the hydrothermal effects.

Despite this, the paper lacks a logical structure in the argumentation, and it is difficult for the reader to understand the background for the focus of the discussion and the conclusions drawn. The structure and the presentation of the results must be significantly improved, and the results that the statements in the discussion are founded on must be highlighted and presented in a clearer way. The paper in its current form is difficult to evaluate with respect to the scientific content of the discussion and conclusion. For this reason, I recommend a major revision of this paper before publishing. A list of general and more specific comments and suggestions to the paper is provided below.

General comments:

1. The introduction chapter includes a very detailed literature review. I recommend to shorten this part, and only include the background necessary to put the paper in a larger context. Highlight why this study is unique and needed in context of previous studies on the same topic in the introduction, but avoid starting the discussion here. Rather move parts of it (with many of the references) to the discussion chapter where you discuss the results in relation to previous findings.

2. I miss a presentation of the objectives in the paper. Please include clear objectives, e.g. in the last paragraph of the introduction chapter.

3. The different observation periods for each variable is confusing for the reader. Please make a figure or table illustrating the period of measurement for each vari-

able/instrument, e.g. as a timeline of observations. In addition, I cannot find explicit information on the period when the snow removal is done. I assume this was done for the entire period 2012 to 2015? This is crucial information in this paper! Please specify.

4. The authors state that the temperatures at NSS are warmer than at SRS over a calendar year, and suggest that removal of the snow has a cooling effect on the ground. However, I miss a clear quantification of the difference between the sites and how this changes with time, supporting these statements. Does the difference increase by each year? Functions of running mean annual temperatures of some selected temperature loggers (depths) would be useful, as well as MAGT for each year at each site.

5. How can the ALT be determined with an accuracy of mm in the range 3.4 - 3.6 m, when the soil temperature measurements are only located at 3 m and 4 m depth? These depths are derived from (I assume linear?) interpolation of the temperature logger data. Because of variation in ice content and ground material this may not be entirely true, and the use of mm precision does not make sense. The ALT derived from interpolation can therefore not be used to differentiate the change in ALT between the two sites. I would say it could perhaps give an indication of ALT thickness within 10 cm, but it has to be noted in the paper that this is an approximation. By this you could still say that ALT at both sites are increasing, but you cannot differentiate the ALT change. In order to assess the differences between the sites, please compare observed temperatures at 3 and 4 m depths between the sites.

6. The actual effect of the snow removal on ground temperatures is not clear to the reader (see points 5 and 6). It is therefore also difficult to follow the discussion of why snow removal has a cooling effect. However, IF the effect is cooling at SRS compared to NSS, the discussion must focus on establishing the cause of this effect. Is the reason a change in thermal insulation, albedo, efficiency of longwave radiation exchange, energy lost to snow melt or infiltration of meltwater/soil moisture (see e.g. summary in Zhang, 2005)? In most areas with a developed snow cover the first effect (thermal insu-

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lation) would dominate, and the result of snow removal would be cooling of the ground. However, as the authors correctly highlight, a 5 cm thick snow cover is normally considered too thin to have an insulating effect on the ground. Still, the authors spend most of the paper discussing whether the thermal insulation from snow is the reason for the cooling effect. As a reader, I would really doubt that this is the case, and therefore it is crucial that you support this discussion with observations. The most obvious would be to compare hourly temperature observations from the air to the uppermost logger (5 cm) in both boreholes. In this way you could see if there is a pronounced dampening of the daily temperature amplitude after a snowfall at 5 cm depth at the NSS site, and not at the 5cm observations at the SRS site.

7. The entire discussion and logical structure behind arguments has to be improved throughout the paper. It is difficult for the reader to relate the discussion around effects to the presented results. The soil moisture data presented in Figure 10 shows interesting results, with a general drying of the SRS site. There is rapid decreases at some of the depths, which has to be commented upon. What is the accuracy of these observations? The authors also link the drying of the ground to reduction of melt water infiltration and increase of evaporation at the SRS site. This seems very likely, but I miss the explicit link from this to the thermal effect it would have on the ground. In general, a soil with less moisture would reduce the exchange of latent heat, as latent heat from freezing of soil moisture is a large energy source, while melting of ice is a similarly large energy sink. Over a year the latent heat energy in and out of the system would be equal, if the soil moisture is not changed. However, in this case there is a gradual drying of the ground at the SRS site during the period, potentially resulting in more melting and evaporation than freezing of water during one year. This would be an energy sink and consequently cool the ground. The authors comment both of these effects, which seems very likely in light of the observed soil moisture data. Still, the authors spend more time discussing the effect of thermal insulation in the discussion chapter. This seems strange, as the presented data indicate that the reduced latent heat effect from drying of the soil may be an explanatory factor, and there are no results

demonstrating that the thermal insulation from the snow cover play a major role.

8. In Figure 4 you present the relative humidity observations. However, these are used only to give the average annual relative humidity. It would be nice to connect these observations to the discussion of the effect of increased evaporation in chapter 4.2.

9. The paper would benefit from a better structure and consistency. Some results are mixed into methods chapter (line 138- 146 and line 159-161). The logical structure of the discussion points also has to be improved. Please refer to the results when you discuss them, and ideally quantify the finding, i.e. not only refer to "a cooling effect", but give how much cooling compared to reference site.

10. Please keep a consistent time format throughout the paper. At least four different date-formats are used in figures and in the text. Please clear this up!

11. Figure 4 to 6 and also ideally Figure 7 should be presented together (in the same figure or below each other at one page) with the same date format so that the data can be related to each other! Now they represent different periods of observation, all have different date format, and they are not in the same figure.

12. The text would benefit from a simpler and clearer language. Some sentences are lengthy and could be simplified. This may partly be a language problem, but the general content could also be sharpened.

Specific comments:

Line 21: "Maybe" in the abstract is a bit vague. Rather use "likely", or "we believe". This is also valid for the conclusions (point 3, L 524).

Line 21: What do you mean with "the delay of snowfall time in autumn"? Please clarify, and relate it to the physical process – does this also refer to the insulating effect of snow, or other effects?

Line 53: Change into "Low thermal conductivity of snow". Delete coefficient. What do

you mean with "The Alps"? The Swiss Alps?

Line 58 – 85: Why is the observed effect, given in degrees C, of variation in snow depths higher in some areas than in others? I would expect this to be directly coupled to the climate (very cold winters or maritime mild winters?) Since snow cover with a critical thickness (typically 60-80 cm depending on the snow properties) disconnects the ground surface temperatures from the air temperature due to very low thermal conductivity, it follows that the difference in ground temperatures between a site with 40 cm snow and a site with 80 cm thick snow is closely related to the air temperature. A very cold winter would result in a large difference, while a warmer (maritime) winter with temperatures close to 0°C would result in less difference.

Line 56: Thermal conductivity is normally given as W/m K, or better W m-1 K-1, where Kelvin is denoted with capital K. The latter notation is used later in the paper; please be consistent. Also clarify the meaning of "d" in W/m K d. If this is temporal rate change of thermal conductivity per day, change into W/m K day or W m-1 K-1 day-1.

Line 77: Here I would also refer to Haeberli and the "Bottom temperature of snow" (BTS)-method.

Line 89 and 90: What is the permafrost "shell"? Please clarify.

Line 92-94: I guess there are also several newer models developed for this purpose. Is there a reason why you mention this old one in particular? If not, please remove.

Line 98-99: I don't understand the meaning here. Do you mean "a wide distribution of snow depths"?

Line 100: snow covered days

Line 100-101: gradual increase in the height of the stable snow cover

Figure 1b: This figure does not tell much. Either leave out and give distance between the sites and the elevation in the text, or include some background information on

vegetation type or similar.

Line 131: suggested change: "average annual temperatures"

Line 142: propagation depth at the snow site

Line 138 – 161: The description of the monitoring site (2.1) is quite lengthy. This is partly due to results mixed into the methods chapter (e.g. line 159 – 162). Avoid this and be more concise.

Line 177-178: This sentence does not fit in the methods chapter.

Line 186-188: This is an important part of the method (the removal of snow) which is hidden away.

Line 201 – 208: This information is included in the table, and it is therefore not necessary to repeat it in the text.

Line 274: What is "thawing and freezing process curves"? Please be more precise, e.g.: "continuous plots of interpolated ground temperatures"?

Line 271-278: It is enough to specify how you determine the active layer thickness and the actual thickness; e.g. "Continuous plot of interpolated ground temperatures for the period xx to xx are shown in Figure 7. Here we define the active layer thickness as the maximum depth of the 0 °C isotherm (Muller, 1974). From the continuous plots we find that the ALTs of the two sites are xx cm and xx cm in 2013 and xx and xx in 2014."

Line 291 – 296: This part is unclear. Either refer to observations or cite previous studies.

Line 299: Suggested change: "Profiles of seasonal average soil temperatures interpolated between the loggers from 0.5 m to 4 m depths at SRS and NSS are shown in Figure 8. The averages are made over the period 2014-03-01 – 2015-02-28."

Figure 8: Inclusion of the season (e.g. Mar – May) on each plot would make it easier

to read. Similar x-axis would also make it easier to see the relative variation in temperature differences. In Figure 8f (or in a separate figure) it would be good to also include the evolution of temperatures in the NSS. This would make it easier for the reader to understand if the change in SRS is only due to climatic changes, or if it is due to the removal of snow, and if the initial situation at the two sites where similar or if the variation where as large as in figure 8e also in 2011-2012.

Line 350 – 358: Do you refer to Figure 9 or 10 here? Please specify and make references in the text. Also make sure you highlight interesting points from the figure, and don't reproduce the figure in the text.

Line 371: Do you have a reference on Eq. 1?

Figure 10: Specify that this is soil moisture at maximum thaw penetration (October) each year. Also consider placing this figure together with Figure 9, and indicate the timing of the calculations in Figure 10 with lines in Figure 9.

Line 396-399: Repetition form introduction. Please reduce the amount of redundancy.

Line 399: The ground temperature in the SRS should therefore increase after snow removal.

Line 400 – 403: Repetition. Delete "the thickness of the snow cover was smaller than the critical snow cover thickness" and include "snow removal, while the average soil temperature"

Line 404-405: You state above that thermal insulation from the snowpack is not a dominating effect with snow heights lower than 20 cm. Here you still argument that this could possibly be an effect. Why do you believe so? You have to support this with observations! Again, this can e.g. be done by comparing daily temperature amplitudes in the air and in the topsoil (5 cm depth) before and after snowfall, at the SRS and at the NSS.

Line 407-408: Please include a reference for this statement, or clarify if statement

refers to the same study as referred above.

Line 408 – 417: Is this applicable if the snow cover is NOT thick enough to have an insulating effect?

Table 3: Is snow clearing still in effect in 2015 and 2016? Please be explicit.

Line 434: The "other significant factor" than what? Thermal insulation? There are far more indices that the decrease in soil moisture is an effect than the thermal insulation, which has no effect proven from the data.

Line 435 – 449: Please relate the differences in fluxes to physical processes. Please be more explicit.

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