Reply to RC2

Replies to the 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.

The introduction is sharpened to one page. Some parts are moved the discussion section.

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.

The objectives are re-organized and presented as a single paragraph at the end of the introduction.

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.

Monitoring period of all parameters is pictured and uniformed. Please see the Figure 4.

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.

The soil temperature in the active layer at the NSP (NSS) and SRP (SRS) were reanalyzed according to the new monitoring data in 2015 and 2016. The current conclusions are very different with the previous. Please see the section 3.5.

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.

The ALT is calculated based on the linear interpolation between temperature measurements of neighboring probes in space and time. Resolution of the ALT depends on the space of probes and frequencies of measurement. Below the depth of 3.0 m, separation distance of neighboring temperature probes is 1 m. Due to the difference of soil properties and unsteady temperature regime, distribution of temperature with the depth is often nonlinear. Therefore, accuration of the ALT should be far more than 1 mm.

Additionally, the ALTs in the discussion manuscript are acquired by using 10 days step in the interpolation calculation. In this revised manuscript, time step is 1 day. As a result, there are some differences between the ALTs of this version and previous version. The maximum difference reaches 14 cm.

The early two year data is not enough to differentiate the difference of ALT change between the both plots due to the small change. However, the ALT was stable since 2015 at the NSP, but the ALT increased by more than 100 cm since 2014.

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 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.

According to your suggestions, thermal effect of snow were analyzed again by adopting new high frequency data, including ground surface temperature (by Apogee), topsoil temperature. The new conclusions are satisfied. Please see the section 3.3, 3.4, 3.5, and 4.1.

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.

According to this suggestions, the cause of the rapid decrease of soil water were discussed in detail (line 447-464, 476-486). The thermal effect of soil moisture loss on the active layer were also analyzed fully. Please see the line 385-411.

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.

From 2012 to 2016, the RH increased by approximately 7%. This change of RH can explain the decrease of soil moisture in the first 2 years. However, it should act as an assistant role because its effect on the both plot is the same. In fact, the CSM reduction at the SRP is higher 8.2% than at the NSP. So, its effect was not given here.

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.

Structure, content, and discussion method all changed a lot comparing with the last version. More importantly, some new findings were got during this process.

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!

The time format is uniformed.

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.

The periods of observation are the same in the Figure 5, 6, and 7. But I did not find an ideal way to put them in one picture because they exhibit the different content. Perhaps, the copy editor could give a good suggestion.

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.

We make some attempts in this version. It is expected that it can do.

Replies to the 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).

It was corrected.

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?

It was deleted in this version.

Line 53: Change into "Low thermal conductivity of snow". Delete coefficient. What do you mean with "The Alps"? The Swiss Alps?

The part was deleted in this version.

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 0C would result in less difference.

The part was shortened greatly in this version, the objective of which is to learn the thermal effect of snow cover with different thickness. Study on the thin snow cover was still scarce.

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.

It is temporal rate change of thermal conductivity per day. The part “W/m K d” and relative content was deleted in this version.

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

This method is valid in the thick snow cover. It may be not appropriate on the QTP due the thin and short-term snow cover.

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

It refer to the perennial frozen soil layer. This part was deleted.

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.

It was removed.

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

It should be “a wide distribution of snow cover”, the aim of which is to tell the readers that there are snow here.

Line 100: snow covered days

This part was deleted.

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

I thought that the snow cover area increased. Now, it was deleted.

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.

It was replaced by a google map, which can include some information on vegetation type.

Line 131: suggested change: "average annual temperatures"

The part was given in the section 3.1.

Line 142: propagation depth at the snow site

This part was deleted.

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.

It was revised. The description on the ground temperature and snow was deleted.

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

This part was deleted.

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

It was described individually in the section 2.2.

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

This part was deleted.

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

Accepted in the line 255-256.

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."

Accepted in the line 252-254.

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

This part was rewrote and moved to the section 2.5 as method introduction.

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

Partially accepted in line 265-266, 281-282.

Figure 8. The averages are made over the period 2014-03-01 – 2015-02-28."

Partially accepted in line 269-271, 285-287.

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.

It was rewrote in line 285-287.

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.

This part was rewrote in line 301-315.

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

I have no. It is only a formula based on the linear interpolation.

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.

Introduction on the data in this figure is given in the section 2.5. (line 188-191)

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

This part was deleted.

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

This part was rewrote in line 348-354.

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"

Accepted.

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.

New explanation was given. Please see line 355-370.

Line 407-408: Please include a reference for this statement, or clarify if statement refers to the same study as referred above.

This part was deleted.

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

This part was rewrote in lines 359-375.

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

The table is deleted.

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.

This is an expression problem. We want to say that the dramatic decrease in soil moisture is the second reason besides the thermal insulator of the snow cover.

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

This part is deleted totally in this new version.