

This author response to comments from Anonymous Referee #1 is structured as follows:

Referee comments

Author response

Changes in manuscript or references to “tc-2019-8\_MSchanges.pdf” where these changes can be found

## General Comments

The authors use eddy-covariance to compare late summer surface energy balance between a water track and two non-water track (reference) sites in Taylor alley, Antarctica. They demonstrate that the water track site registers greater energy exchanges and greater relative contributions of evaporative and permafrost heat fluxes to the surface energy balance, while registering lower sensible heat fluxes. It provides a rare account of the effect of wetness on soil thermal behavior, with much implications for biogeochemical and hydrological processes, and is highly relevant in order to further identify the implications of climate change for periglacial and polar desert soils. The absence of vegetation on the site draws on the importance of research on naked watersheds in order to isolate the effect of soil hydrology on its physical, chemical and thermal properties and behavior. The overall originality and presentation quality of the manuscript is good, clearly providing the readers with greater understanding of energetic processes operating in water tracks and in polar desert soils in general.

We thank Referee #1 for the thorough and thoughtful assessment of our manuscript, for acknowledging our experimental approach and our analysis, and for emphasizing the importance of our findings for soil hydrology and climate change impacts on biogeochemical processes in polar desert soils.

At this point, however, the manuscript only presents fair scientific qualities and significance. This is not a criticism of the method and results, but a consequence of the overall theoretical framework of the manuscript and of the lack of discussion and perspective on the work. While the results are sound, the manuscript would benefit from modifications in order to correctly present the narrative behind the measurements, to better address the significance of the findings and to put them in greater perspective and in accord with the existing literature.

These suggestions are very helpful and interesting. We realized that the manuscript focused too much on technical considerations, with too little discussion and consideration of implications of our findings for hydrology and soil ecosystems in the McMurdo Dry Valleys. We therefore performed many changes in our manuscript and are confident that we have put our research into greater perspective now.

The previous statements could be framed within two main criticisms. The first concerns the

ergodic theoretical framework used to compare between water track and reference SEB, in turn rooted in the climate change paradigm used by the authors to present their work. In order to substitute space to time, the authors assume the landscape will become “wetter ” with new water tracks appearing in front of (new?) snowdrift sites or downslope near the valley bottom. It is therefore central to the premise of the manuscript to clearly demonstrate how climate change will increase the spatial distribution of water tracks, using the literature and possibly a conceptual model, yet it is not established clearly enough in the introduction. Specifically, there are some questions that arise on the mechanisms by which dry areas should become water tracks (see specific comments), which goes against earlier findings (see Langdon et al. 2014). In summary, change is not what is measured here, and better definitions and demonstrations of “change ” need to be included in order to anchor the scientific claims of the paper to reality.

Many studies about the McMurdo Dry Valleys (MDV) are showing current landscape and biologic activity changes, are predicting temperature and precipitation increases and are framing the narrative of increasing water availability due to ground ice and snow melt in the MDV. However, we realized that we have not clearly introduced these findings and their importance and connection to our study. In the responses to your specific comments we explain in detail why and how we expect coverage of water tracks and other wetted soils to increase in the MDV. We have included these considerations and background information in the reformulated introduction and discussed our results with respect to the existing literature we included now. We believe that our expectation of soil moisture increase as landscape response to climate change is now rooted in previous research findings and can be accepted by the scientific community.

The second criticism concerns the lack of perspective in the results and discussion section, and to some degree in the introduction. In section 5, a single reference to the literature can be found outside of the first paragraph of section 5.4, and nothing is done to link the findings to other similar comparisons done in the area and to general hydrological, biogeochemical and ecosystemic research concerning the MDV and water tracks, some which were already cited (Ball et al. 2011; Ball & Levy 2015; Ikard et al. 2009; Fountain et al. 2014; Levy et al. 2013; 2016; Schmidt and Levy, 2017; Gooseff et al. 2011; Steven et al. 2013; Paquette et al. 2017; 2018; Comte et al. 2018; Zeglin et al. 2009; 2011; also, see water track literature from Alaska). It is essential to root the research into the existing literature, and to relate the findings to what has already been observed. As the manuscript appears now, it does not demonstrate a thorough understanding of the literature on the subject. In addition, further attention to the specific nomenclature of permafrost soils is required.

Again, we acknowledge that a broader discussion of our results benefits the manuscript and is necessary to present a comprehensive picture of the implications of our findings and the possibilities and limitations of interpretation. We therefore compared our findings with those from other studies and discussed the relevance of the differences between water tracks and dry soils for soil hydrology and ecosystems in the face of the climate change response of increasing water-track coverage in the MDV we expect. We thank the referees for the suggested references

which we partly included in our discussion. Unfortunately, we were not able to broaden the perspective towards an extensive comparison of the results with studies concerning water tracks outside Antarctica, but we include biogeochemical, biological and hydrological implications for the MDV that will be relevant for future research. The permafrost nomenclature was revised  
5 and is now consistently used and explained in the manuscript.

## Specific comments

p.1 line 7: Sentence needs to be clarified

We rephrased the sentence in a way that explains the way of separate examination of thawed layer and frozen layer more explicitly (p.1, 1.10–13).

- 10 Line 9: 30% to melting the seasonally thawed layer : The active layer is already thawed at the onset of experimentation, so much so that it is considered of a stable depth in the calculations. How is it then that 30 % of the heat is transferred to melting it? Is it meant as warming permafrost under what is called the ice table (QIT)?

The active layer is typically not completely thawed before mid-January in Taylor Valley (*Adlam et al.*, 2010; *Conovitz et al.*, 2006). The ice table – the current transition between thawed  
15 and frozen soil – was therefore assumed to be lowered throughout the measuring period as an advancing thaw front. However, due to limited availability of ice table depth measurements, we decided to calculate  $\Delta S_{TL}$  with a constant ice table depth. We recognize that this results in uncertainties and limitations to the resulting soil heat flux estimates which we noted in the  
20 methods (p.8, 1.27–29; p.9 1.3–6). We discussed these uncertainties in Appendix A, where we estimated an error of  $\approx 15\%$  in  $\Delta S_{TL}$  following a miscalculation in ice table depth by 18 cm. We argue that most of the energy transferred to the ice table as  $Q_{IT}$  was latent heat for melting and the sensible heat transfer had a much lower contribution there.

- 25 Line 13-14: The evaporation from lower TV considers land only or also water bodies (Lake Fryxell, rivers)?

Only land is considered, i.e., soil surfaces. Sentence was changed (p.1 1.19–21).

Line 15: This is a bit overstated, as the manuscript does not address the effect of adding or removing water tracks. Also, ice-sheet free Antarctic regions could be changed to “polar deserts” to broaden perspectives.

- 30 We agree that we have limitations in the interpretation of our results because of the static and short-term design of our experiment. We rephrased the sentence (p.1, 1.25–p.2, 1.4).

Line 16: ...are likely to respond faster to climate change signals...How are they going to respond? Their hydrology is going to change? Their SEB will change? This is never addressed

nor measured in the manuscript, the only landscape change premised is the passage from dry to wet soils, which appears as the main signal of climate sensitivity on the slope. Therefore, if slopes become wet and water track occurrence increases, then the water tracks are rather resilient to change, and their distribution will even “benefit ” from climate change. Also, are the potential changes responses to climate change signals or to climate change? In addition, Langdon et al. (2014) have shown that climate change may cause increases in water track activity, but that they show spatial consistency in their location, since they highly depend on snowdrift accumulation.

The sentence is misleadingly formulated, in a way that it suggests we mainly expect internal processes of hydrology and energy exchange to change in response to climate change. However, we can only speculate that increased melt provision to existing water tracks can potentially cause an increase in discharge, soil moisture and evaporation of water tracks compared to the current state. Therefore we specified our expectations and conclusions to the anticipated area increase of water tracks in the MDV (p.18–19, section 3.4). Water tracks do show persistence in the landscape on inter-annual scales (*Langford et al.*, 2015) and on decadal timescales (*Levy et al.*, 2014). However, not all water tracks are snow-fed. Some derive from snow melt, others from ground ice melt (*Harris et al.*, 2007), and others potentially from soil salt deliquescence (*Levy et al.*, 2012). Water tracks will respond to climate change signals, predominantly the increased availability of liquid water. We expect all water tracks to experience expanded area and activity during warming scenarios, as active layers thaw more deeply and melt previously-stable ground ice, as perennial snow patches melt and thin during summer warming, and as increased evaporation from the Ross Sea and decreased sea ice raises humidity, allowing enhanced soil salt hydration. All these mechanisms point to the potential for expanded extents of wetted soil in the MDV.

Line 17-18: Their spatiotemporal dynamic will be an effect of climate change, but not of sensitivity to it, unless reference sites are discussed here.

Dry soils in the MDV are sensitive to climate change because they can become part of wetted soils in the course of the climate change responses we discussed in the response to your comment above this one.

Line 25: well-documented: Citations needed.

We added more landscape responses including references to the relevant papers (p.2, l.13–17).

p.2 Line 3: could use citation from Gooseff et al. 2016

We added the reference (p.2, l.17).

Line 7: This definition is very regional to TV. A better definition for water tracks can be found in Gooseff et al. 2013.

We provided a more general definition of water tracks with addition of specific properties in the McMurdo Dry Valleys (p.2 1.33–p.3, 1.3).

Line 8-10: This statement fails to explain why water tracks are more sensitive to climate change than non-water tracks.

5 We recognized that our argumentation line concerning the “sensitivity” of water tracks and the “indicator” narrative was overstated in the manuscript. We have not enough support from our data to provide evidence for changes of internal processes resulting from climate change. However, we expect that high thermal diffusivity and energy uptake could increase positive melting feedbacks (*Gooseff et al.*, 2013; *Ikard et al.*, 2009; *Levy and Schmidt*, 2016) for water  
10 tracks compared to dry soils, changing properties like soil moisture, active layer depth and evaporation more significantly for water tracks than for dry soils. We removed this idea from the introduction and abstract now due to its speculative nature and mention it in the discussion instead (p.19, 1.8–14).

Line 16-17: This sentence is the prime assumption to the general “change in the face of climate  
15 change” message of the manuscript. It is however not well documented and demands to be proven before the “change” paradigm can be accepted.

We agree that we did not pay enough respect to justifying our assumption of climate-change induced increase in water-track abundance. We now included elaborate explanations in the introduction (p.3, 1.22–28) and discussion (p.18–19, section 3.4). As mentioned in our response  
20 to the comment above, the change of water track properties due to climate change is not a central point and has been given less focus now.

Line 17: Here we identify an opportunity to investigate the utility of this potentially useful indicator... This is never really what this research is about, as the utility of the indicator (are water tracks really indicators?) is not investigated.

25 As stated above in the response to your comment on p.2, 1.8-10, we acknowledge that we cannot focus on the “indicator” narrative in this study. However, we argue that monitoring the energy and matter exchange – especially evaporation – of water tracks and their spatiotemporal dynamics could serve as an indication of landscape response to climate change. The found differences between water tracks and dry soils can be used as a baseline for monitoring of future changes  
30 in water track energy exchange (p.19, 1.13–14).

Line 21: Latent heat flux is used throughout the manuscript to refer to latent evaporative flux. Since this is a permafrost area and two changes of state are possible, it would be suitable to include evaporation in the wording.

We replaced turbulent “latent heat flux” with “latent evaporative heat flux” throughout the  
35 manuscript. and provided a statement for our interpretation of turbulent vapor flux as evaporation (p.4, 1.10–11).

Line 30: ice table. Please explicitly define this term. It appears to designate the thaw front in Figure 1, but here it seems to also refer to the upper depths of permafrost.

The ice table was not clearly defined in our manuscript, but is meant to refer to the interface between the unfrozen part of the soil column and the underlying frozen part. The ice table  
5 depth increases during summer thawing from a minimum winter depth to the full active layer thickness at the time of peak melting. It is equivalent to the thaw front during thawing and the freezing front during freezing. It only indicates the site of phase change in the soil, not the directionality of the phase change. It is distinct from the permafrost because it moves in response to thaw and freezing. We define the ice table now at p.3, 1.3–4 and we have improved  
10 the clarity of the conceptual representation of our approach in Figure 1. We rephrased the statement you commented on here (p.4 1.12–13) and we changed another occurrence of wrong usage of “ice table” as well (p.4 1.14–15).

Line 32: QIT needs to be better defined as the sum of latent + sensible heat flux into the frozen soil below the thaw front.

15 We extended the definition of  $Q_{IT}$  (p.4 1.14–15).

p.3 Line 3: dSTL could be defined more straightforwardly as the heat storage in the active layer

The thawed layer is not equal to the active layer since the active layer was not assumed to be thawed completely (see response to your comment on p.1, 1.9).

Line 7: replace “melting” by “soil thawing”

20 Moved this sentence to another section and replaced “melting” by “soil thawing” (p.8 1.28).

Line 16-18: Please reformulate and clarify

We understand that the assumptions made for eddy-covariance method are phrased very shortly here and reformulated them for clarification (p.5 1.3-13).

p.4 Line 5-6 : Stress that water tracks are linear features

25 Changed to “Our intention was to isolate the effect of these small-scale, linear features [...]” (p.6 1.5).

Line 15: how was  $C_G$  determined? Were constant moisture conditions assumed between wet and dry soils?

$C_G$  was determined as effective  $C_G$  including effects of soil moisture. The used values were  
30 averaged from field measurements of 4 and 31 soil samples from the surface of water track and reference 1, respectively (p.8, 1.7–9).

Line 17: Important to state late-summer conditions, as it is the only reason why constant thaw depths can be used.

We added mention of the seasonal conditions (p.6, 1.21). However, we stress again that we did not assume constant thaw depths at the sites, but only calculated a simplified heat budget without changing thaw depth since thickness change of the thawed layer was not recorded over time.

Line 19-20: Please reformulate

We rephrased this sentence in an easier-to-follow way (p.7, 1.2-7). Also, we added tabular information on the eddy-covariance sites (Tab. 1).

Line 20 : Physiographic descriptions are lacking for the sites. Slopes and slope aspects are important elements for polar locations, and any difference in aspect and angle can strongly influence timing and magnitude of solar radiation. Looking at the shading and water track orientation in Figure 2, it seems as if slope aspects are not identical between sites. If slopes angles are low, this might not be a big issue, but it requires clarifications.

The sites were located on generally low slopes and most of the region is on extremely gentle slopes ( $<5^\circ$ ), owing to the abundance of raised paleo deltas at the site. While slope is surely a major player in energy balance high on the walls of the valley, it has hardly any influence on the valley floor. You find a slope map of the sites in supplementary Figure 1. Furthermore mean diurnal variations of insolation over the recording period did not vary notably between Water Track and reference (see attached Figure “Meaninsolations.pdf”).

p.5 Figure 2: It could be more useful to have general map of TV, with a single point to designate the study sites, and pictures of the field sites. Figure 3 could also be made smaller and included in it.

We have added a regional context map showing the location of the sampling sites in Taylor Valley (Figure 2a). However, we did not include the land cover matrix in this figure because the therein presented flux footprint is discussed at a later point.

Line 1: The ice table is a  $1.9^\circ\text{C}$ ? How is this logical? Shouldn't it be assumed that the ice table is at  $0^\circ\text{C}$  as is the case in the water track?

We decided to use the mean temperature of the soil thermometer at 0.3 m depth for any case where no temperature measurement was available at this depth which was the case for the whole record at reference 2. We did this to avoid unphysical spikes in the calculated energy storage change which would have resulted from setting the temperature for these cases to  $0^\circ\text{C}$  at the assumed ice table depth of 0.3 m. One may argue that a mean temperature of  $1.9^\circ\text{C}$  at 0.3 m depth contradicts the assumption that the ice table was located at this depth. But since most of the energy storage change occurred close to the ground surface, this miscalculation of the ice

table depth does hardly matter to the computed energy storage change, as we have shown in Appendix A. To clarify our choice, we included part of this response in the revised manuscript (p.8, l.31–p.9, l.5).

Line 3: How was this measured? What are the values used for the water track and the reference site? This is important for your modelling, and values should be given.

We measured thermal properties of the soil from several samples close to the surface with a Decagon KD2 Pro device. We added information on the measurements (p.8 l.7–9) and on the results (Tab. 2).

p.6 Line 17: Levy et al. 2011 say that surface darkening occurs on 1-3 m, yet a width of 10 m is used. Why?

While many water tracks are quite narrow, others have widths of 5-10, or even 10-20 m. Water tracks in this region of eastern Taylor Valley are notably wide. 10 m is taken as a typical, representative value.

Results: It would have been useful to have access to the meteorological data and ground temperature data, either as appendices or supplemental material, or even to show them as results instead of Figure 3, which could be included in Figure 2.

We agree that background information on the measurements is useful. Please find the most important meteorological measurements in supplementary Figure 3 and soil temperatures in supplementary Figure 4, along with interpolated soil profiles.

Line 22: How much smaller? Please state with %, maybe mean % and standard deviation. In addition, appendix B shows that  $Q^*$ 's isn't really smaller in one of two instances.

Since we compared spatial differences to temporal differences, it is not easy to come up with simple numbers which is why we referred to the Appendix here. However, we can compare the different root mean-squared error estimates calculated in the appendix representing differences between water track and reference, and between Reference 1 and Reference 2, respectively. We added this in the Results section (p.12, l.5) and in the Appendix (p.24, l.4).

Line 28-29: Here QIT is defined as the energy used to melt (sic) permafrost. Clearly 5.2 Mj wasnt used to further lower the thaw front, or it would have moved significantly. In fact, QIT includes both the energy transferred to permafrost as sensible heat and the latent heat used to thaw permafrost (or to melt the ice in permafrost). It could be said this energy is used to warm and thaw permafrost.

We did not intend to relate  $Q_{IT}$  to melting of permafrost, but rather to melting of the seasonally thawed active layer. Still, as we only present results at this point in the MS, we only state there now that the energy is taken up by the frozen part of the soil (p.13, l.5). However, we assume



that latent melting energy played the major part in  $Q_{IT}$ , which is supported by several SEB studies in the Arctic (*Lloyd et al.*, 2001; *Lund et al.*, 2014; *Westermann et al.*, 2009) (p.16, 1.18–21).

p.7 Figure 3: The scale is too small for what is actually shown. It could be smaller and included in figure 2. What are the density lines showing? Density of water track contribution? If so, the % seem to be inverted as your smaller area only has 50 % of water track contribution.

The footprint is a probability density function of the source area of the observed property. This probability density function is visualized by the density lines. We annotated this in the caption to Fig. 3. We acknowledge your notion that the figure size should be reduced, so we changed the scale now. However, we did not include the figure in Figure 2 since it is intended to refer mainly to the flux footprint which is discussed much later than the site locations.

Line 4-5: Does this timeline correspond to max solar radiation if you correct for slope aspect?

As we have shown in our response to your comment on p.4 1.20, slopes are irrelevant for insolation at the sites we investigated.

Line 4-5: Albedo was stated to be 0.15 in water tracks and 0.22 in non-water tracks soils (*Levy et al.* 2013). This is the kind of comparison that could be discussed.

We compared our albedo results with those found by *Levy et al.* (2014) (p.14, 1.1–4).

Line 6: What is the surface temperature? Please provide data.

We added average and maximum radiative surface temperatures for references and water track (p.14, 1.5–7).

p.8 Figure 4 could benefit from showing totals partitioned between references and periods, as a cumulative histogram.

We appreciate your suggestion which certainly would add more information to the figure. However, we can not improve this figure in the way you proposed. We decided to calculate mean daily totals of SEB components – now slightly transformed to average fluxes – from mean diurnal variations, so there is only one value available for the whole recording period (p.11, 1.9–11). We did this because coverage of representative data for Water Track was far too sparse for individual days to calculate SEB (see supplementary Figure 2).

Line 1: Figure 4 shows total, and QH is reduced to 0.8 in water tracks, not 0.7.

We referenced the figure at a wrong position. This reference has been relocated now (p.14, 1.17–19).

Line 1-2: Add reference to Figure 5

The reference was wrongly placed one sentence before and has been added here instead now (p.14, 1.19).

Line 10: This section could benefit from links to the existing literature, as active layer depths are known for water tracks and non-water tracks in the area.

- 5 We compared our ice table depths with findings of active layer depths of water tracks in Taylor Valley and with typical active layer depths in lower Taylor Valley (p.15, 1.5–9).

Line 20: How was thermal conductivity measured? It would be interesting to quantify the respective roles of increased energy input and thermal conductivity in the daily energy budget.

Thermal conductivity was measured alongside volumetric heat capacity. We added this information in the manuscript (p.8, 1.7–9). Because we have no measurements of dynamics of the thermal conductivity, we cannot distinguish between the influences of energy input and thermal conductive on energy balances.

p. 9 Line 6: It is suggested that energy travels more rapidly toward permafrost in the water track, yet this doesn't appear clearly in Figure 6. As  $Q_s^*$  increases in the reference, so does the active layer temperature (dSTL), with permafrost heat flux ( $Q_{IT}$ ) following closely. In the water track, this latter heat flux seems delayed by about 18 hours. Otherwise, how could heat flux toward permafrost occur before the soil even begins to warm in a downward process? Please clarify.

$Q_{IT}$  was delayed by around one day for both reference and Water Track: Lag times of temperature wave penetration  $t_{lag}$  into depth  $z$  can be calculated after Woo (2012) as  $t_{lag} = 0.5z(L \pi^{-1} D_T^{-1})^{0.5}$ , with period  $L = 1$  day and thermal diffusivity  $D_T$ . The results for penetration to ice table depth were 12.8 hours for reference stations and 15.3 hours for Water Track with measured thermal diffusivities of  $0.29 \text{ mm}^2 \text{ s}^{-1}$  for reference and  $0.52 \text{ mm}^2 \text{ s}^{-1}$  for Water Track indicating that the observed daily peaks of  $Q_{IT}$  corresponded to the peaks of  $Q_G$  from the previous day. At the reference  $Q_G$  and  $Q_{IT}$  peaked at 11:00 UTC+12 and at the Water Track maximum  $Q_G$  was reached at 12:00 UTC+12 and  $Q_{IT}$  peaked at (Figure 6). Therefore, we assume a delay of 24 and 21 hours at reference and Water Track, respectively, even though these penetration lags deduced from measurements do not match the modeled lags. The reduced lag at Water Track relative to reference – contradicting the increased lag at Water Track in modeled values – is due to the strongly increasing soil moisture with depth in the Water Track (Levy et al., 2011), which substantially enhances thermal conductivity and convective transfer of heat relative to the surface where thermal diffusivity used for the penetration lag calculation was measured. We therefore deduce that heat was transported to the ice table faster at Water Track than at the reference. We added some of this information in the methods (p.10, 1.1–3) and results (p.16, 1.24–30) sections.

Line 7: Why are there citations at the end of a question?

The citations referred to the expected climate change in the MDV. We moved them to the right position (p.18 1.13–14).

Line 10: These scenarios should include the same parameters (precipitations, insolation, temperature). The first is not a climate scenario, rather an arbitrary 50 % increase in water track abundance. It is not clear how this could occur, as it would require new snowpatches locations. A simpler, more straightforward approach would be to determine if the future would be wetter or drier. This could be done using increases-decreases in area % of water track surfaces, and computing the respectful SEB components for each increment.

We agree that the climate change scenarios were not properly separated from the arbitrary increase in water-track coverage we assumed. We therefore restructured and reformulated this section. Several findings (see *Harris et al.* (2007); *Langford et al.* (2015); *Levy et al.* (2012), previous responses and MS) point towards higher spatial extent of wetted soils in a warming climate, which is not mainly dependent on new snow patch formation, but rather on increased melt of existing snow patches and ground ice. Our mechanistic scenarios support this expectation. Therefore, we only investigate the effect of increasing water-track coverage on evaporation in lower Taylor Valley, and neglect a decrease. We liked your suggestion of incremental increase for all SEB components though which is why we included this in Tab. 3.

p.10 Figure 6 caption: Negative energy fluxes. . . These do not appear except for dSTL, so this sentence could be removed. The following sentence could specify how out of the thawed layer (aka the active layer) is both into permafrost (QIT) and toward the atmosphere as QLE or QH.  $Q_G$  also shows negative values. We understand that more information on the directions of  $\Delta S_{TL}$  is helpful so we restructured the directional explanation in the caption to Fig. 5.

Line 4: Again, how would increase snow melt increase water track abundance? This suggests increased precipitations and new snowpatches.

See response to your comment on p.9, l.10.

Line 5: a total of 4.4% of what?

Fraction of total soil surface in lower Taylor Valley (Tab. 3; p.19, 1.1–4).

p.11 Line 1: Increased solar radiation will create a feedback that would decrease solar radiation? Does this mean that no increase in solar radiation is possible in the Dry Valleys?

We argue that there will be a negative feedback on insolation increase (p.20, 1.7–8). However, we have no means of quantifying how strong this feedback might be and we do not hypothesize that there can be no increase in insolation since there are many other influencing factors, most of all the control by larger atmospheric circulation.

Line 10-11: Please reformulate

We expect you refer here again to your concern about interpretation of  $Q_{IT}$  as melting energy for lowering of the ice table. As we stated in our response to your comment on p.1, l.9 in the discussion paper, we still use this interpretation.

- 5 Line 16: This is the central message of the paper, and should be what is put forward in the abstract and what the introduction leads to. The climate change aspects are secondary to this scientific finding, and are not as sound as this sentence is.

We agree that the impact of water tracks on SEB is the most powerful finding of our study, and we have tried to focus more on this in the introduction. However, as our findings have  
10 implications for expected increase of wetted soil area in the MDV, we still discuss these in our manuscript.

Line 17: respond faster... Why? It seems as if water tracks as hydrological features are resilient to change, and might even benefit from warmer temperatures.

The sentence was removed (p.21, l.19–20). See our responses to your comments on p.1, l.16 and  
15 p.2, l.8–10 of the discussion paper.

p.14 Table C1: The second row of Water Track is redundant. The first row could simply say 26/12 to 21/01. Otherwise please explain in the caption.

There were two thermocouples deployed at 0.4 cm depth until 04/01, and only one after. We changed Table C1 for clarification.

## 20 Technical corrections

General comment: Whenever possible, please abstain from using abbreviations, except for long terms which appear often. For example, eddy covariance could be written in the long form throughout the text.

Replaced all occurrences of “EC” by “eddy-covariance” and checked for other unnecessary ab-  
25 breviations.

p.1 Line 4: water track instead of water-track. Please correct all other occurrences.

Replaced all occurrences.

Line 6: state-of-the-art is used a few times in the manuscript. I would suggest removing this, as it tends to age poorly. Please remove all other occurrences.

30 Removed all occurrences.

p.2 Line 2: thermokarst

Corrected (p.2, 1.16).

p.3 Line 5: Please define CG and z here

Moved the  $C_G$  definition to this location and defined  $z$  (p.4, 1.23–24).

Line 18: Please define T and q here. This sentence would benefit being re-written and broken  
5 down.

We restructured the sentence (p.5, 1.10–12).

p.5 Line 10: corrected instead of correction

Corrected to corrected (p.8, 1.19).

Line 12: was also applied

10 We do not see the need to add an “also” here, so we did not include it.

p.6 Line 15: replace wet water-track soils by water track

Replaced p.10, 1.18–19.

p.7 Figure 3 caption: replace Eddy-Covariance by eddy-covariance

Replaced.

15 Line 5: at the water track was can be explained

Removed “was”.

p.8 Line 14: lower case r in Reference

Replaced all upper case “R” in “Reference” by lower case “r”.

p.11 Line 8: replace an increase in by greater

20 Replaced.

Line 19: by ither

Replaced with “either”.

## References

- 25 Adlam, L. S., M. R. Barks, C. A. Seybold, and D. I. Campbell, Temporal and spatial variation  
in active layer depth in the mcmurdo sound region, antarctica, *Antarct. Sci.*, 22(01), 45,  
doi:10.1017/S0954102009990460, 2010.

- Conovitz, P. A., L. H. MacDonald, and D. M. McKnight, Spatial and temporal active layer dynamics along three glacial meltwater streams in the mcmurdo dry valleys, antarctica, *Arct. Antarct. Alp. Res.*, *38*(1), 42–53, doi:10.1657/1523-0430(2006)038[0042:SATALD]2.0.CO;2, 2006.
- 5 Gooseff, M. N., J. E. Barrett, and J. S. Levy, Shallow groundwater systems in a polar desert, mcmurdo dry valleys, antarctica, *Hydrogeol. J.*, *21*(1), 171–183, doi:10.1007/s10040-012-0926-3, 2013.
- Harris, K. J., A. E. Carey, W. B. Lyons, K. A. Welch, and A. G. Fountain, Solute and isotope geochemistry of subsurface ice melt seeps in taylor valley, antarctica, *Geoderma*, *119*(5-6),  
 10 548–555, doi:10.1130/B25913.1, 2007.
- Ikard, S. J., M. N. Gooseff, J. E. Barrett, and C. Takacs-Vesbach, Thermal characterisation of active layer across a soil moisture gradient in the mcmurdo dry valleys, antarctica, *Permafrost Periglac.*, *20*(1), 27–39, doi:10.1002/ppp.634, 2009.
- Langford, Z. L., M. N. Gooseff, and D. J. Lampkin, Spatiotemporal dynamics of  
 15 wetted soils across a polar desert landscape, *Antarct. Sci.*, *27*(02), 197–209, doi:10.1017/S0954102014000601, 2015.
- Levy, J. S., and L. M. Schmidt, Thermal properties of antarctic soils: Wetting controls subsurface thermal state, *Antarct. Sci.*, *28*(05), 361–370, doi:10.1017/S0954102016000201, 2016.
- Levy, J. S., A. G. Fountain, M. N. Gooseff, K. A. Welch, and W. B. Lyons, Water tracks and  
 20 permafrost in taylor valley, antarctica: Extensive and shallow groundwater connectivity in a cold desert ecosystem, *Geol. Soc. Am. Bull.*, *123*(11-12), 2295–2311, doi:10.1130/B30436.1, 2011.
- Levy, J. S., A. G. Fountain, K. A. Welch, and W. B. Lyons, Hypersaline “wet patches” in taylor valley, antarctica, *Geophys. Res. Lett.*, *39*(5), n/a–n/a, doi:10.1029/2012GL050898, 2012.
- 25 Levy, J. S., A. G. Fountain, M. N. Gooseff, J. E. Barrett, R. Vantreesse, K. A. Welch, W. B. Lyons, U. N. Nielsen, and D. H. Wall, Water track modification of soil ecosystems in the lake hoare basin, taylor valley, antarctica, *Antarct. Sci.*, *26*(02), 153–162, doi:10.1017/S095410201300045X, 2014.
- Lloyd, C. R., R. J. Harding, T. Friberg, and M. Aurela, Surface fluxes of heat and water vapour from sites in the european arctic, *Theor. Appl. Climatol.*, *70*(1-4), 19–33, doi:  
 30 10.1007/s007040170003, 2001.
- Lund, M., B. U. Hansen, S. H. Pedersen, C. Stiegler, and M. P. Tamstorf, Characteristics of summer-time energy exchange in a high arctic tundra heath 2000–2010, *Tellus B*, *66*(1), 21,631, doi:10.3402/tellusb.v66.21631, 2014.
- 35 Westermann, S., J. Lüers, M. Langer, K. Piel, and J. Boike, The annual surface energy budget of a high-arctic permafrost site on svalbard, norway, *The Cryosphere*, *3*(2), 245–263, doi:10.5194/tc-3-245-2009, 2009.