

## ***Interactive comment on “Water tracks intensify surface energy and mass exchange in the Antarctic McMurdo Dry Valleys” by Tobias Linhardt et al.***

### **Anonymous Referee #1**

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**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 Valley, 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, chemi-

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cal 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. 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. 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. 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

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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.

Specific comments p.1 line 7: Sentence needs to be clarified 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)? Line 13-14: The evaporation from lower TV considers land only or also water bodies (Lake Fryxell, rivers)? 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. 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. 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. Line 25: well-documented: Citations needed. p.2 Line 3: could use citation from Gooseff et al. 2016 Line 7: This definition is very regional to TV. A better definition for

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water tracks can be found in Gooseff et al. 2013. Line 8-10: This statement fails to explain why water tracks are more sensitive to climate change than non-water tracks. Line 16-17: This sentence is the prime assumption to the general “change in the face of climate change” message of the manuscript. It is however not well documented and demands to be proven before the “change” paradigm can be accepted. 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. 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. 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. Line 32: QIT needs to be better defined as the sum of latent + sensible heat flux into the frozen soil below the thaw front. p.3 Line 3: dSTL could be defined more straightforwardly as the heat storage in the active layer Line 7: replace “melting” by “soil thawing” Line 16-18: Please reformulate and clarify p.4 Line 5-6 : Stress that water tracks are linear features Line 15: how was CG determined? Were constant moisture conditions assumed between wet and dry soils? Line 17: Important to state late-summer conditions, as it is the only reason why constant thaw depths can be used. Line 19-20: Please reformulate 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. 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. Line 1: The ice table is a 1.9°C? How is this logical? Shouldn't it be assumed that the ice table is at 0°C as is the case in the water track? Line 3: How was this measured? What are

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the values used for the water track and the reference site? This is important for your modelling, and values should be given. 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? 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. 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. Line 28-29: Here QIT is defined as the energy used to melt (sic) permafrost. Clearly 5.2 Mj wasn't 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. 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. Line 4-5: Does this timeline correspond to max solar radiation if you correct for slope aspect? 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. Line 6: What is the surface temperature? Please provide data. p.8 Figure 4 could benefit from showing totals partitioned between references and periods, as a cumulative histogram. Line 1: Figure 4 shows total, and QH is reduced to 0.8 in water tracks, not 0.7. Line 1-2: Add reference to Figure 5 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. 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. 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 per-

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mafrost heat flux (QIT) 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. Line 7: Why are there citations at the end of a question? 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. 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. Line 4: Again, how would increase snow melt increase water track abundance? This suggests increased precipitations and new snowpatches. Line 5: a total of 4.4% of what? 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? Line 10-11: Please reformulate 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. 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. 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.

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. p.1 Line 4: water track instead of water-track. Please correct all other occurrences. Line 6: state-of-the-art

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is used a few times in the manuscript. I would suggest removing this, as it tends to age poorly. Please remove all other occurrences. p.2 Line 2: thermokarst p.3 Line 5: Please define CG and z here Line 18: Please define T and q here. This sentence would benefit being re-written and broken down. p.5 Line 10: corrected instead of correction Line 12: was also applied p.6 Line 15: replace wet water-track soils by water track p.7 Figure 3 caption: replace Eddy-Covariance by eddy-covariance Line 5: at the water track was can be explained p.8 Line 14: lower case r in Reference p.11 Line 8: replace an increase in by greater Line 19: by ither

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