The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-8-RC3, 2019

The Cryosphere
Discussions
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EGU

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

Anonymous Referee #3

Received and published: 27 February 2019

This manuscript presents a comparison of surface energy balance for a water track and for two reference locations in the Taylor Valley during 26 days in summer of 2012-2013. The main aim of the study was to evaluate the hypothesis that water tracks alter the surface energy budget, mainly due to the higher moisture content of the soil. The energy balance estimation relied on eddy-covariance (EC) method, net radiation measurements and soil temperature profiles.

I am not an expert on these cold ecosystems, yet I do have some experience on the EC method and will concentrate on EC related issues in my review. It seems that the other two reviewers are experts on the Antarctic ecosystems and hence our fields of

expertise complement each other.

In general the paper is well-written and it has a clear structure which is easy to follow. The available data set is not large, only 26 days, and hence far reaching conclusions cannot be drawn based on it. Nevertheless, the authors do their best and it should be also acknowledged that this is the first EC study in this remote location. However, there is one major flaw in this study which is related to forcing the energy balance to be closed. This should be changed before the manuscript can be accepted for publication. Please see more details below. In my view the manuscript is otherwise good and hence I recommend accepting it for publication after minor revisions suggested below, in addition to the modifications suggested by the other two reviewers.

SPECIFIC COMMENTS

1) The authors estimated QIT (the energy input to the permafrost) as a residual of the surface energy balance, meaning that they force the energy balance to be closed. However, there is a plethora of publications out there that show that at EC sites the energy balance is almost never closed, meaning that incoming energy exceeds the sum of outgoing energy and energy stored in the system (see e.g. Reed et al., 2018; Stoy et al., 2013; Hendriks Franssen et al., 2010; Leuning et al., 2012). Usually 10-30 % of the energy is missing. It is still unclear why the energy balance is not typically closed at EC sites, yet it is often hypothesized to be related to sampling mismatch between instruments, instrumental problems, under sampled large eddies transporting heat, terrain heterogeneity or energy storage in soils, air and biomass below the measurement height (Wilson et al., 2002; Leuning et al., 2012). The reasons might also be different for different sites. Be that as it may, this issue should be acknowledged also in this manuscript in question. Hence, I argue that the energy balance residual cannot be assumed to be equal to QIT and the authors should analyze it only as energy balance residual, not some specific real energy flux. Furthermore, if QIT is related to energy input to the permafrost shouldn't it be directed downwards (i.e. it should be negative) since melting ice requires energy? Now the QIT estimated from the residual is mostly

positive which seems physically implausible. Also Anonymous referee #1 pointed out some unphysical behavior of the estimated QIT when compared with dSTL which might stem from the fact that the energy balance residual is not equal to the real QIT.

2) The measurement campaign lasted only 26 days and hence far-reaching conclusions cannot be based on it. This is something that should be discussed in the text. I mean, there are likely seasonal changes in the energy fluxes in both areas, reference and water tracks, and these seasonal changes are not necessarily the same. The authors should discuss what part of the summer these measurements likely represent well and how the energy fluxes are likely changing during the summer.

MINOR COMMENTS

page 1 line 4 To me “during the Antarctic summer of 2012-2013” sounds like that the measurements lasted for several months. Please explicitly mention the length of the measurement campaign (26 days) here.

p. 4 Section 3 More information about the EC setup is needed. What was the sonic anemometer measurement height? Was the gas analyser next to the sonic or below it? What was the horizontal and vertical separation between the two sensors? Were the EC instruments clearly above the surface roughness elements (e.g. rocks)? How rough was the surface in the three locations? Give estimates e.g. for the roughness lengths

p. 5 Section 4 Did you filter out the low turbulence periods from the EC data? During low turbulence EC is not measuring accurately the surface gas exchange and hence these periods should be filtered out e.g. by removing periods with low friction velocity. This applies also to water vapor fluxes. This is somewhat linked to the non-closure of energy balance at EC sites (Wilson et al., 2002)

p. 5 l. 11 Buoyancy correction by Schotanus et al. (1983) is slightly wrong and one should follow van Dijk et al. (2004) instead. Schotanus et al. (1983) is missing a term

0.51<q><Ts’w’> from the right-hand side of their equation (8). Here brackets denote temporal averaging, q air specific humidity (kg kg-1), Ts temperature (K) measured with the sonic anemometer and w vertical wind speed (m s-1).

p. 5 l. 13 Foken et al. (2004) quality flagging scheme gives quality flags between 1 (best quality) and 9 (worst quality). Here you mention that periods with quality flags equal or smaller than 1 are filtered out. Please clarify

p. 6 l. 5-7 It is a bit unclear what was the overall data coverage after filtering. Please clarify and mention it explicitly for each site.

p. 6 l. 11 This 79 % is quite peculiar value. Why not to use nice round number like e.g. 80 %?

p. 6 l. 8-21 and elsewhere Please use consistent naming for the different surface types. For example, here “stream channel” equals “River” in Fig. 3, right?

p. 7 l.12 – p.8 l. 2 It is unclear what is done here. What is the difference between the ratios reported here e.g. for QH? Please rewrite and clarify.

p. 8. l. 3 How much later QH peaked at the water track? Two hours? Please add this information to make it more concrete

TECHNICAL CORRECTIONS

p.4 l. 10 You are referring to Fig. 3 before Fig. 2. You need to go in order.

p. 4 l. 27 You can remove “and carbon dioxide” from the sentence since those measurements were not used in this study.

p. 5 l. 10 Please replace “spectrally correction” with “corrected for low- and high-pass filtering”

p. 5 l. 12 You can remove “and carbon dioxide fluxes” from the sentence since those measurements were not used in this study.

p. 5 l. 9-10 To be exact the method by Wilczak et al. (2001) does not eliminate the mean vertical wind. It tries to eliminate the mean vertical wind caused by tilted sonic anemometer.

p. 6 l. 21 “Each class was assigned an adequate momentum surface roughness length from literature and used in footprint calculations”, right?

p. 7 Fig.3 “Glacial Till” and “Ice” cannot be separated from each other in grayscale figure. Please change the colors.

p. 7 l. 5 remove “was” after water track.

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