Note: in the following document, the original comments made by the reviewer are copied in black, while the authors' responses to these comments follow in blue.

Authors' response to comments submitted by Reviewer #1

The authors have responded to the comments made in the previous review round, partly modified the manuscript, and added a paragraph about measurement uncertainties as appendix. Despite some improvements, I still see points that should be addressed before publication:

The authors state that the performed quality assessment has slightly changed their results. In total, the difference between the previous and the revised version is on the order of about 4 Watt per square meter which is indeed a minor shift. The authors, however, aim to evaluate flux differences on the order of a few Watt per square meter so that minor shifts might be relevant for some interpretation. For example, the authors have clearly stated in the previous version that atmospheric fluxes are increased at the drained site. This finding was the basis for speculations on positive feedback mechanisms towards permafrost degradation. The new results have significantly changed this point of the study.

Even though the trend in the changes of the Bowen-Ratio remains the same, I think it is necessary to address the sensitivity of the results to the performed quality assessment in the manuscript.
Starting the experiment that is presented herein in summer 2013, it took our research group numerous iterations to finally arrive at an eddy-covariance data processing procedure that we were fully confident with. The last iterations, which also affected this manuscript, were related to developing an optimum balance between data quality filtering and the subsequent gap-filling procedure. In other words, we needed to adjust quality measures that we added to the regular flagging procedure by Foken et al. The resulting optimum solution for our experiment now reliably detects biased fluxes, while producing a lower frequency of gaps with an adequate distribution across the seasons.

The shift in flux budget results between the first and second draft of this manuscript was caused by our choice to revise quality flag selection and gap-filling according to the description above. The resulting changes in net energy flux results demonstrate that the eddy-covariance method overall is quite susceptible to data processing choices by the user, and that a skillful selection and fine-tuning of methods is a prerequisite for producing reliable results. However, we believe that we by now found the optimum approach for our sites, and that shifts in net fluxes as experienced between the previous two manuscript versions will not occur again. Still, we decided to add a reference to the potential influence of this procedure in the appendix of this manuscript.

Furthermore, the title of the manuscript should be changed since the atmospheric heat fluxes are obviously not increased only the flux partition is moderately changed.
The authors acknowledge having missed to adjust the manuscript title when working in the updated eddy-covariance results into the revised version of the manuscript. We therefore highly appreciate that the reviewer pointed this out here, since of course we agree that the old title does not reflect the core message of the study anymore. Our modified title now reads "Shifts in energy fluxes linked to drainage-induced changes in permafrost ecosystem structure increase Bowen Ratios, but reduce thaw depth"

p.1, l.22: The conclusion that an increased sensible heat flux might lead to a positive feedback on permafrost degradation is very arbitrary and after correcting the results I do not see any indication in this study that supports this statement. A very similar argument could be used in order to point out an increased latent heat flux as reason for a positive feedback on permafrost degradation. Water vapor strongly changes the radiation balance and is a very potent GHG. I suggest to focus on the results presented in this study as already outlined in the previous review round.

Our statement referred to potential effects of shifts in the energy flux partitioning on the local to regional scale lower atmospheric boundary layer. Energy in form of latent heat does not affect local temperatures directly, but instead is often vertically removed through boundary layer transport processes. Sensible heat fluxes, on the other hand, can directly influence local temperatures by heating up the lower boundary layers. Accordingly, a shift towards higher Bowen Ratios following drainage disturbance at least holds the potential to create warmer conditions locally.

Still, we acknowledge that a direct link between local temperature shifts caused by increased Bowen Ratios on the one hand and deeper thaw depths and permafrost degradation on the other remains speculative. We therefore edited the last sub-sentence of the abstract to "..,which may trigger a warming of the lower atmospheric surface layer."

p.3, l.18: Same comment as above. Besides that, the statement is misleading as it reads like that the net heat transfer into the atmosphere is increased. Following the new results this is obviously not true.

We decided to change this last paragraph of the introduction, also based on comments made by reviewer #2. As a result, the statement that reviewer #1 cites here has been removed.

p.10, l.31: The fact that the latent heat flux is not consistently decreased at the drained site in both years indicates that changes in the energy partition depend on various factors. Even though the Bowen-Ratio is consistently higher at the drained site, there is a high interannual variability as stated by the authors themselves. Thus, general conclusions based on observations of two years should be made very carefully if differences in energy partition between the sites also strongly depend on synoptic conditions.

We fully agree with the reviewer on this issue. To emphasize the uncertain long-term effect, we added the following sentence to the end of this paragraph: "However, due to the pronounced interannual variability this mean value may not be representative over longer time periods, and more data years would be required to constrain a net drainage impact.".

p.12, l.11: I think this statement is misleading. Soil hydrology is the only factor modified in this study. How is it possible to identify it as the dominate control factor without testing other cofactors such as atmospheric conditions? It is important to be precises with such statements.

We agree with the reviewer that our analyses are not comprehensive enough to support the identification of 'the dominant' control factor. Even though we could rule out strong impacts by some factors (atmospheric conditions, for example, were virtually the same for both treatment areas), we therefore changed the last sentence to "..the impact of soil hydrology was identified as a major control, ..".

p.15, l.2: Please present an estimate of how much snow could be melted earlier due to differences in soil temperature. The statement that an increased snow depth leads to increased soil temperatures and, thus, to earlier snow melt requires a sound basis.

Based on suggestions by reviewer #2, we changed our interpretation of the mechanisms leading to warmer soil temperatures at the drained site towards the end of winter. In the revised version, we now argue that the high thermal conductivity in ice-filled pores (more abundant in the control section), compared to a low conductivity in air-filled pores (more abundant in the drained section) lead to steeper soil temperature gradients with time, once all the water in the control section has been frozen (latent heat effect). Accordingly, this difference in net thermal conductivity also in winter should be the main controlling factor on trajectories in wintertime soil temperatures, with better insulation through deeper snow cover at the drained site potentially contributing to the process.

We have observational evidence from several years of continuous measurements that the drained soils have warmer (less negative) soil temperatures at the end of winter. We can only speculate on the mechanisms behind this observation, but regard the thermal conductivity of ice-rich soils as a plausible interpretation. In any case, Eugster et al. (2000) demonstrated that underlying soil temperatures may have an influence on snow melt dynamics. We changed the last sentence in this paragraph to clarify these relationships. A quantitative estimate on the temporal shifts in snow melt linked to this effect at our site, however, is clearly beyond the scope of this study, also because we lack more specific information such as density and water content of snow.

p.15, l.12: This estimate assumes the closure term to be constant. It might be possible that the two sites feature different closure terms and that the closure terms change with time.
We fully agree with the reviewer that the 20% as an estimate of non-closure of the energy balance is only a rough estimate, and that the value may vary between years and between sites. It is clearly stated in this sentence that under the 'assumption' of a 20% non-closure, the residual would be changed as provided in the manuscript. To emphasize that the non-closure is variable, we changed the fixed value of 20% to a range of 15-25%, and adjusted the remaining residuals accordingly.

p.15, l.27: Strictly spoken, the results (thaw depth and uppermost soil temperatures) show indication of a reduced soil heat flux.
The sentence was changed accordingly.

p.16, l.29: After the quality assessment the total turbulent fluxes are not observed to be higher.
We changed this to 'higher mechanically generated turbulence'.

p.17, l.24: I do not see any result in this study justifying speculations on "tipping points". The same is true for speculations on atmosphere-permafrost feedback processes as already remarked above. The authors present nice measurements that demonstrate that the Bowen-Ratio changes due to drainage. Furthermore, they demonstrate that drainage has impacts on thaw depth and the soil thermal regime (at least within the upper decimeters). I strongly recommend that the discussion focuses on the solid results of this study without pushing speculations too far. Without results that clearly show effects such as tipping points and feedback mechanisms the made speculations appear either arbitrary or overstated.
Agreed. We removed the last sentence in this paragraph that contained the 'tipping point' statement.

p.19, l.14: What does "multi-disciplinary" mean in this context? Why is this information necessary?
We agree that this term isn't necessary to make the intended statement, and therefore removed it.

p. 19, l.16: It should be pointed out that temperature changes are limited to the upper most decimeters. The differences in soil temperature are only 0.27K in 64 cm depth. This leads to further questions such as: What is the accuracy of the soil temperature sensors used? How accurate were the soil temperature sensors installed in the ground? How was the soil surface defined? What do these uncertainties mean for the soil temperature comparison (also the observed seasonal differences)?
The information of maximum sensor depth for these analyses has been added to the paragraph (but it was also clearly given already on p.9, where these numbers of soil temperatures were presented first).
We agree with the reviewer that soil temperature measurements are subject to uncertainties, with those listed in the statement above being the most important sources for errors. However, given the temperature gradients over depth observed at our sites, we can postulate that a vertical displacement of the sensors as a result of installation bias cannot significantly change the overall picture. Linearly interpolated soil temperature gradients between 32cm and 64cm, as

well as between 64cm and 128cm (which are available at other sensors not used within the context of this study) yield values ranging between -0.006 K/cm and 0 K/cm, therefore even a height bias of 5cm would only lead to a temperature shift up to 0.03K. The absolute calibration of the sensors can be compared during the zero-curtain period in fall, when very stable temperature levels are reached by all sensors for a prolonged period of time. Also through this comparison, we can confirm that no offset exists that can call into question the validity of the overall shifts in soil temperatures found between wet and dry microsites, respectively.

p.19, l.17: This statement should include the wording "most likely" since the study neither presents data on soil heat capacities, thermal conductivities, nor soil heat fluxes.
OK

p.19, l. 30: Why is there a profound impact on forecasts of the sustainability of Arctic permafrost ecosystems under future climate change? Changes in the atmospheric fluxes below 10% might be judged as rather moderate impact. In particular when the natural spatial heterogeneity of tundra is taken into account.
We toned down the statement to "..,the demonstrated effects could therefore be relevant for forecasts of the sustainability of Arctic permafrost ecosystems under future climate change.".

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Authors' response to comments submitted by Reviewer #2

This study of a long-term drainage experiment with a focus on energy budget effects is interesting and relevant to ongoing efforts to understand how ecosystem change in the Arctic might feedback on belowground and atmospheric processes. I have a number of comments listed below that I hope the authors will find useful.

Pg 2, line 19 – Improve wording. Perhaps, "In addition to warming, shifts in the water balance in this region are also expected to trigger profound …"
We took over the wording suggested by the reviewer.

Pg 3, paragraph starting line 3 – Include a reference to the Kittler et al. Biogeosciences paper that reports on the CO_2 flux analysis at this study site.
As suggested by the reviewer, we added a citation for the paper by Kittler et al. (Biogeosciences, 2016), which analyzed the effects of long-term drainage on summertime CO_2 fluxes at the Chersky site.

Pg 3, line 14 – Suggest wording change to " …to quantify several secondary disturbance effects linked to lower water tables, including changes in vegetation community, radiation budget and soil thermal regime."
The sentence was changed according to the suggestion.

Pg 3, lines 18-21 – These last 2 sentences repeat the results summarized in the abstract. I suggest these be rephrased in terms of a hypothesis or leave out entirely.
We removed those parts of the sentences in the last paragraph of the introduction that summarized results.

Pg 4, lines 9 – 12 – This last sentence reviewing Merbold et al. (2009) results is not needed in the methods and it is a repeat of information from the introduction. I suggest it be removed.
As suggested by the reviewer, the last sentence was removed.

Pg 6, Section 3.1.2 – It isn't clear to me why this analysis/detail on long-term temperature and precipitation is needed in this manuscript which focuses on the results of a manipulation experiment.
We agree with the reviewer that the inclusion of long-term climate trends for this region is slightly outside the core focus area of this study, i.e. the comparison of energy processes between the two treatments on our site. Still, we believe that information on trends in regional climate is helpful to put the changes associated with the drainage disturbance in perspective. For this purpose, the survey on decadal mean temperatures and interannual variability presented in Section 3.1.2 is very useful.

Pg 7, line 30 – The method used to calculate aerodynamic roughness needs to be included in the methods.
We added the sentence 'Aerodynamic roughness length was derived here based on flux-profile relationships using friction velocity and wind speed at tower top under neutral atmospheric stratification.' to this paragraph.

Pg 9, lines 10-11 – Is it reasonable to attribute the warmer conditions in the dry microsite to deeper snow as a) no snow depths were measured and b) this microsite might be relatively close in space to the wet microsite (as both are within the drained transect)? Could cooler

winter temperatures at the wet microsite also or instead be due to greater thermal conductivity promoting heat loss in the saturated and frozen vs. dry soil? Or do you expect the dry micro-site surface peat to become saturated in fall and be relatively similar?
The dry and the wet microsites equipped with additional instrumentation for continuous soil monitoring are approximately 50m apart in both transects. In case of the results presented in Fig. 7, all values were taken from the 2 sites within the drained transect.
We do not expect the dry microsites to become fully saturated with water when the early fall precipitation events occur. We have witnessed shorter periods of time when repeated heavy rainfall events have created inundated conditions at parts of the drained section during summer, but this water is usually removed through lateral export soon after precipitation stops. The difference in water content is also emphasized by the prolonged zero-curtain period at the wet microsites (item 2 in this list)
Since the thermal conductivity of ice even exceeds that of water, we agree with the reviewer that, once all the water in the wet soil has been frozen, the higher ice content compared to the dry microsites should lead to a higher net thermal conductivity. Since, due to missing direct measurements of snow depth at several locations, our hypothesized snow cover effect must remain speculative, we modified the 3. item on this list with a new emphasis on thermal conductivity.

Pg 9, line 15 – Were these results from just the 2 microsites at the control and the 2 microsites at the drained site or dry vs. wet microsites at the drained site only as shown in Figure 7?
These results are based on the comparison of dry vs. wet microsite within the drained transect, i.e. using the same data source as for Fig.7. To clarify this, we amended the first sentence of this paragraph.

Pg 9, line 26 – Perhaps reword to "Here, higher soil moisture promotes higher soil temperatures…"
OK

Pg 12, lines 22 – 27 – Unless I missed it, there was no direct data to support a finding that snow was deeper in the drained site with greater shrub cover (which was also not assessed for height as noted on pg 13, line 1). Make sure to be clear that your data is not direct evidence for snow depth effects or include some additional data that helps support this conclusion.
We acknowledge that there is no observational evidence on shifts in snow cover depth, since there is only one snow level sensor installed at our experimental site. Therefore, we agree with the reviewer that snow depth should be removed from this discussion item. We changed the paragraph accordingly.

Pg 14, line 13 – Can you speculate on the mechanism leading to this difference in H+LE response to reduced net radiation in 2015?
There are two short periods, i.e. in early July and early September, when R_{net} was much higher in 2015 compared to 2014, offsetting parts of the negative difference between the energy budgets of both years. In both cases, LE contributed the largest share of energy flux reaction to this change in energy input, and the additional flux was found much higher in the drained section than in the control section. The largest part of the different magnitude in interannual variability of net energy exchange between the two treatments can be attributed to these few days.
Our interpretation of the underlying mechanism combines two influence factors: First, our data clearly demonstrates that LE is the dominant factor behind both the interannual variability in net energy exchange (for both treatments) and the differences in interannual variability between treatments. Here, we already stated in the manuscript (same section, previous paragraph) that LE flux rates are highly susceptible to day-to-day variability in radiative energy input. Second, since the reaction in LE to variability in R_{net} differ between treatments, it is likely that soil moisture levels have an influence in this process. However, we can only speculate on the reason why the combination of these factors leads to differences in interannual

variability between treatments. A possible explanation is that antecedent moisture levels play a role, i.e. the timing of precipitation events related to the timing of variability (spikes) in R_{net} may play a role. We added the following section to the paragraph:
"We speculate that the deviating interannual variability between the sections may be driven by differences in soil moisture levels between data years, e.g. linked to the timing of precipitation events, which influenced the feedback of LE flux rates to variability in net radiation."

Pg 15, line 12 – But could this heat storage in water be included in the ground heat flux term if the surface is defined as the water surface?
In this study, we ignored heat storage effects in the soil water, and the potential impact of transfer of energy through the lateral export of soil water, which may be particularly important in the drainage section. Since we have no data available that may enable us to quantify this effect, we can only mention it here as a potential source of uncertainty.

Pg 15, line 13 – This was an increase of 8%? In other words, more energy may have been going to soil heat fluxes in 2015 at the control site?
That is correct. To clarify this, we modified the last sub-sentence to '..,values increased by about 8 % in the control section,.. "

Pg 15, line 28 – Any heat transfer impacts in winter?
As already stated in our answer to comment 'p9, l10-11', we agree with the reviewer that differences in the thermal conductivity of ice-filled (more abundant in control section) and air-filled (more abundant in drained section) pores are likely to cause the steeper negative gradients in soil temperatures in the control section in winter, once all soil water has been frozen. In this specific paragraph, we changed a sentence to "..which in our case resulted in a reduction of heat transfer into the soil across the seasons." to acknowledge this fact.

Pg 16, line 15 – Improve wording.
We changed the sentence to "Sophisticated numerical models are needed for assessing the complex feedback processes between permafrost ecosystems and climate change, but is unclear yet which processes need to be explicitly resolved in these models, and which input parameters need to be provided at what resolution, to avoid systematic biases in simulation results."

Pg 17, line 5 – Reword "multiple links with factors" to improve clarity.
We changed this sub-sentence to "..,and numerous studies have identified links with factors such as snow cover (Sturm et al., 2001a; Pomeroy et al., 2006), radiation regime (Bewley et al., 2007) and nutrient cycling (Myers-Smith and Hik, 2013; DeMarco et al., 2014b)."

Pg 17, line 8 – Suggest revise wording to "…that the capture of drifting snow by shrubs increases snow depth and soil temperatures in winter…"
We changed the sentence according to the suggestion.

Pg 19, line 24 – Figure 10 needs to be introduced/discussed before the conclusion and in greater detail. It could instead be removed if preferred.
We decided to remove Figure 10.

Pg 26, line 19 – Suggest wording change to "were amended for this study by flags also reflecting overall…"
OK

Pg 27, line 7 – Remove "e.g."
OK

Pg 34, Fig 4 caption – What does the height of 0 cm reference to?

We arbitrarily chose a zero-level while conducting the terrain surveys in 2013 and 2014. The absolute values therefore do not carry any information. We added this information to the figure caption.

Pg 40, Fig 10 – What do the green quadrilateral shapes represent?
As mentioned above, we removed Figure 10 from the revised manuscript version. Those shapes the reviewer was referring to were meant to represent tussocks, and their shift in abundance and size following the sustained drainage.