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The Cryosphere Discussions

# *Interactive comment on* "Shifts in permafrost ecosystem structure following a decade-long drainage increase energy transfer to the atmosphere, but reduce thaw depth" *by* Mathias Göckede et al.

**Anonymous Referee #1**

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Summary: The study "Shifts in permafrost ecosystem structure following a decade-long drainage increase energy transfer to the atmosphere, but reduce thaw depth" by Göckede et al. investigates the impact of a drainage experiment within a wet tundra landscape on components of the surface energy balance. The study seeks to quantify differences in micro climate between a drainage site and a control site which show marked differences in vegetation cover and soil moisture. The authors, therefore, compare sensible and latent heat fluxes which were measured simultaneously at both sites by eddy covariance. In addition, differences in radiation, near surface soil temperatures, and active layer depths are presented. The found differences be-



tween the drained site and the control site are discussed in the context of differences in vegetation cover and soil moisture. The study reports increased sensible heat fluxes at the drained site, whereas much small differences are reported for the latent heat flux. Only minor differences are also reported for the net radiation. The authors argue that the increased sensible heat flux is related to a decreased ground heat flux which is reflected in decreased soil temperatures and lower active layer depths. The study focuses on a very important issue in permafrost research that addresses possible hydrological changes in the Arctic due to climate warming and permafrost degradation. This study is, therefore, within the scientific scope of the journal The Cryosphere. The study is written in a clear and understandable manner and is well structured. There are, however, some major issues that should be addressed before publication.

General comments: The measured differences in the turbulent heat fluxes and the radiation are essential to the argumentation of this study since the ground heat flux is not determined independently. Thus, it would be highly recommended to perform a comprehensive uncertainty analysis and quality assessment of the fluxes. Uncertainty ranges should be calculated in order to evaluate whether the measured differences are significant. It must be excluded that the differences in the heat fluxes are subject to instrument biases and/or site related methodological biases. Due to the importance of reliable measurements, it seems to be inadequate to just refer to an other study for quality assessment.

The authors state that they do not see any differences in the long wave radiation budget. This is surprising since increased sensible heat fluxes are often related to increased surface temperatures. I think a discussion of this point would be highly interesting for understanding the reasons for the increased sensible heat fluxes.

The authors present differences in friction velocity as indicator for differences in vegetation structure (hight/density). In this context, I think it would be more instructive to calculate roughness lengths during neutral conditions. In addition, a footprint analysis might be useful since vegetation seems to vary within the footprint area of the eddy

covariance tower (Figure 1). It might be interesting for the process understanding to further investigate the impact of changed surface roughness on flux partition.

In general, the data analysis presented in this study is very limited and should be expanded. Besides footprint analysis, the analysis of the diurnal and seasonal signals can provide deeper insights into the processes behind flux partition. Figure 9b, for example, clearly demonstrates that more than just average differences can be observed in the time series of sensible heat fluxes. I think that the very strong and general statements made in the conclusions and the abstract are not possible based on the very limited data analysis presented here and, thus, should be toned down. Without further process analysis leading to a better understanding of heat transfer processes within and below shrubs, the presented data and results must be considered site specific and are not directly transferable to other tundra landscapes.

The authors do not present any data or results related to snow. However, several paragraphs in the discussion include speculations on snow including the impact of vegetation on snow, and the impact of ground heat fluxes on snow melt. As these subjects are essentially not part of the presented investigations, I strongly recommend to exclude them from the discussion and to focus on the presented results.

Specific comments: p.8,l.1: Sections 0 and 0?

p.8, l.2: Why not including the LW budget in the analysis, in particular the outgoing LW radiation?

p.8, l.6: What is the absolute accuracy of the used radiation sensor? What is the footprint of the radiation sensor? Does the sensor measure a representative area of the drained site?

p.9, l.1: Here and throughout the manuscript it should be thermal conductivity.

p.9,l.3-5: This is a very general statement which does not provide any quantitative information on the heat transfer processes within the soil. Please note that increased



soil moisture means a higher content of latent heat which besides thermal conductivity and heat capacity determines the duration of the zero curtain.

p.9,l.25-27: Besides the ground heat flux, the thaw depth is determined by the soil ice content. Are there any information on differences in soil composition between both sites?

p.10,l.6: This is a very small number (1,8%), is this within the accuracy of the sensor?

p.10,l.13: Is it possible to distinguish between the impact of drainage and changed vegetation cover within this study? Or is it more likely that the vegetation cover has adapted to drainage which then has modified the surface energy balance? I think it would be important to separate these things since there drainage can happen on relatively short time scales while changes in vegetation cover requires some time.

p.11,l.13: Does 'slightly lower' mean a more negative sensible heat flux at the drained site?

p.12,l.6-15: See general comment on snow cover discussion.

p.12,l.23-30: Why so much emphasis on discussing the impact of mosses if mosses are absent at the study site?

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