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Interactive comment on "The decline of alpine lichen heaths generates atmospheric heating but subsurface cooling during the growing season" *by* Peter Aartsma et al.

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We thank Reviewer 3 for his/her time to review our manuscript and for his/her valuable comments. Below are our answers on his/her comments.

Reviewer comment: This study examines differences in surface energy partitioning and soil microclimate between lichen- and shrub-dominated vegetation in southern Norway using paired measurements made with a set of mobile instrument platforms. The authors find that the lower albedo of shrub canopies leads to higher atmospheric heating, but lichen mats have greater soil heat fluxes and temperatures despite lower net radiation. The latter is attributed shrub canopy shading and a thicker litter layer with

lower thermal conductivity. The results provide important context for understanding how shrub expansion will affect microclimate when shrubs replace lichens. The paper is well written, interesting, and I enjoyed reading it. There are several improvements that could help to strengthen the paper before it is considered further for publication.

Author response: We thank the Reviewer for his/her positive words on the writing style and interest in our paper. Moreover, we thank him/her for his/her advices on how to strengthen the paper.

Reviewer comment: While I don't think there are any technical problems with how the methods were applied, I do find it curious that different approaches were used to analyze the data from different years, and that the data weren't aggregated. Why not use data from 2018 and 2019 in the mixed models to examine differences in microclimate (i.e. in Table 2)?

Author response: Initially, the sample design of this study contained only measurements of the field season of 2018. However, 2018 was an unusually dry and warm year and therefore we decided to include measurements of the field season of 2019, since they support our findings of the field season of 2018 under less extreme conditions. We decided to use only the measurements of the field season of 2018 for the mixed models, because there are quite some differences in the sample design between the two years. For example, the plot pairs of the field season of 2018 consist of two measurement days, while plot pairs of the field season of 2019 consist of six measurement days per plot pair. Moreover, the location of the plots of the field season of 2018 were drawn randomly with ArcMap, while the locations of the plots of the field season of 2019 were chosen subjectively. Therefore the inclusion of the plots of 2019 in the mixed models might introduce selection bias. Therefore, we chose to do the main analysis on the data of 2018 only and not to include the data of 2019. We see that the difference in sample design between both years and the way of analysis might lead to confusion by the reader. Therefore, we will elaborate more on the reasons for the difference in sample design for both years and our way of analysis in the next version of the manuscript.

Reviewer comment: A box and whisker plot showing mean microclimate by vegetation type would be more informative than the individual daily averages shown in Figure 4. Something like Figure 3, but instead showing net radiation, soil heat flux, soil temperature, etc. .

Author response: The Reviewer proposes to replace Figure 4 of the manuscript for Figure 1 of this answer (see below). Although we see the advantages of plotting the difference in microclimatic conditions between the lichen and shrub plots as proposed by the Reviewer, we still think that the current Figure 4 is more informative for the reader. The current Figure 4 adds important information on the timing, as it shows when and how the microclimatic conditions differed between the lichen and paired shrub plots during the field season. Moreover, the current Figure 4 gives information to the reader on how the sample design was set up during the field season of 2018. For example, the reader can see that we measured two days per plot pair and subsequently change to another plot pair. Therefore, we propose to keep Figure 4 in the manuscript. However, if the Reviewer and/or the Editor find the figure above relevant as well, we will add it to the new version of the manuscript.

Reviewer comment: I also wonder whether it would be more appropriate to model microclimate conditions as a function of incident shortwave radiation, rather than temperature, since this likely affects soil temperature and heat flux more so than air temperature (e.g. L163-165)?

Author response: We have modelled the daily average soil temperature and daily total soil heat flux as a function of the incident shortwave radiation. Subsequently we have determined the marginal r-squared for both models as proposed by Nakagawa & Schielzeth (2013). The marginal r-squared is the variation that is explained by the fixed effects. It turned out that the marginal r-squared was higher when we modelled the microclimatic variables as a function of air temperature ($R2 = 0.58$ for daily total

soil heat flux and $R2 = 0.43$ for daily average soil temperature) than as a function of incident shortwave radiation ($R2 = 0.40$ for daily total soil heat flux and $R2 = 0.14$ for daily average soil temperature). Moreover, we construct models for the average soil temperature and the total soil heat flux separately for nighttime, when incident shortwave radiation is absent. Therefore, we opt to analyze the microclimate as a function of air temperature rather than as a function of incident shortwave radiation.

Reviewer comment: Related, are Figure 5 & 7 showing results of the mixed effects models?

Author response: Yes, Figure 5 shows the results of the daily averages/daily totals and Figure 7 shows the results of the daytime and nighttime averages/totals. We will make this clear to the reader by adding this information in the captions of Figures 5 & 7 in the next version of the manuscript.

Reviewer comment: Also I recommend that the authors consult Loranty et al 2018 published in PLOS One. This paper examines differences in soil temperature, thermal conductivity, surface temperature, and ET between lichen- and shrub-dominated vegetation patches in Siberia. It reaches many of the same conclusions presented in this manuscript, and would provide useful context in the introduction and discussion. Loranty, M.M., Berner, L.T., Taber, E.D., Kropp, H., Natali, S.M., Alexander, H.D., Davydov, S.P. and Zimov, N.S., 2018. Understory vegetation mediates permafrost active layer dynamics and carbon dioxide fluxes in open-canopy larch forests of northeastern Siberia. Plos one, 13(3), p.e0194014.

Author response: We thank the Reviewer for sharing this article with us. We were not aware its existence and see its value for our manuscript. Therefore, we will implement the findings of the paper of Loranty et al. in the introduction and discussion of the next version of our manuscript. Moreover, it shows that the results of our study are consistent with studies at other alpine/Arctic areas.

Reviewer comment: Minor comments: L1: Does your study really address the decline of lichen heaths? The results certainly have implications in this context, but it seems more like a comparison between lichen heaths and shrubs. Something to that effect would be more appropriate in the title.

Author response: We understand the point of the Reviewer and will change the title to the following: "Microclimatic comparison of lichen heaths and shrubs: shrubification generates atmospheric heating but subsurface cooling during the growing season."

Reviewer comment: L45-50: This seems like it refers to another study that uses the same data presented in this manuscript. It would be appropriate to note that.

Author response: We only use to a small extent the same data as for the study of Aartsma et al. (2020). In the earlier study we quantified the difference in albedo between lichen and shrub plots, and in the current manuscript we study the effect of this difference in albedo on other microclimatic variables. As proposed, we will link the findings of the earlier paper (Aartsma et al. 2020) more clearly to this study in the next version of the manuscript.

Reviewer comment: L80: It should be made clear here that the authors know albedo is higher for lichen as a result of previous analyses from this data set.

Author response: We will make this clear in the next version.

Reviewer comment: L85: Is there permafrost at this site?

Author response: No, there is no permafrost at this site. We will mention that in the section "Study Area" in the next version.

Reviewer comment: L216: The wording "As for the net radiation" is a little confusing for me.

Author response: We will rephrase this wording in the next version.

Reviewer comment: L229: It is worth noting here that alpine tundra and lichen mats will also have different albedos, and the while lichen does have high albedo and can

be somewhat abundant, it is not broadly representative of alpine tundra.

Author response: We understand the point that is made by the Reviewer and will give some more context on the representability of lichen heaths in alpine areas in the next version of this manuscript.

Reviewer comment: L240: See Loranty et al 2018 in PLOS One for surface temperature and ET measurements of lichens vs shrubs.

Author response: We will use the findings by Loranty et al. to provide some more context on our results.

Reviewer comment: L303: ET would affect the canopy temperature more than that ground temperature, and as noted on line 285 the latter likely has more impact on soil temperature. However, higher ET shrubs may cool canopy temperatures, meaning less LW emitted from the canopy, and LW enhancement by canopies can affect the energy balance at the ground surface (e.g. Todt et al, 2018; Wake et al, 2017). This hasn't been shown in shrub tundra, but might be worth considering here.

Author response: We thank the Reviewer for pointing to this mechanism and the belonging articles. We will mention the mechanism in the new version of the manuscript.

Reviewer comment: L361-365: This section could be developed a bit more. It would be worth discussing how pervasive lichens are across alpine and arctic regions more generally. What types of modeling studies might your measurement help to inform, ecosystem or global scale studies, are there any example citations? Which measurements specifically might be useful for modeling?

Author response: We will develop this part a bit more and elaborate how our findings can be used for further (modelling) studies. Lichen heaths are often not incorporated in land surface models or they are clumped together with mosses, despite their different characteristics (Porada et al., 2016; Stoy et al., 2012; Wullschleger et al., 2014). Our study can help to develop a first version of lichen heaths as a separate plant functional type in land surface models.

Reviewer comment: Figure 7: Why aren't the points included here, but included in Figure 5?

Author response: We decided not to include the points in Figure 7, since the figure would become rather messy when we would include the actual measurements of the microclimatic variables for both daytime and nighttime. We understand that it might lead to confusion for the reader that in Figure 5 the actual measurements are indicated and in Figure 7 not. Therefore, we decided to remove the points from Figure 5.

Reviewer comment: Figure 9: The abbreviations ST, SHF, etc. should be used consistently throughout the figures/manuscript.

Author response: We will take care that these abbreviation are used consistently throughout the next version of the manuscript.

Reviewer comment: References: Todt, M., Rutter, N., Fletcher, C.G., Wake, L.M., Bartlett, P.A., Jonas, T., Kropp, H., Loranty, M.M. and Webster, C., 2018. Simulation of longwave enhancement in boreal and montane forests. Journal of Geophysical Research: Atmospheres, 123(24), pp.13-731.

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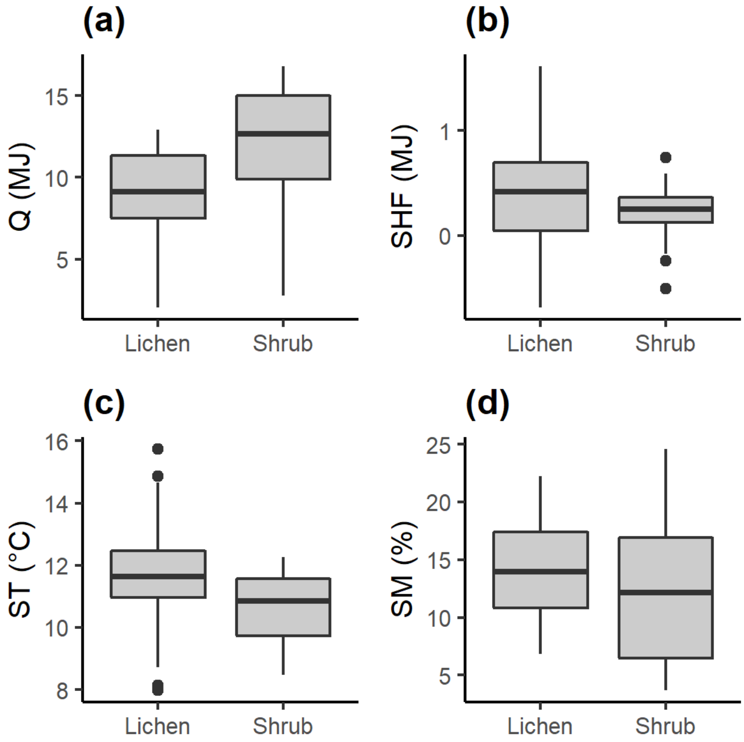

Fig. 1. The figure that should replace Figure 4 of the manuscript as proposed by the Reviewer.