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Interactive comment on "Effect of crustose lichen (*Ochrolecia frigida*) on soil CO_2 efflux in a sphagnum moss community over western Alaska tundra" *by* Yongwon Kim et al.

Anonymous Referee #1

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General comments:

This manuscript investigates soil CO2 fluxes in a sphagnum moss community under healthy vs infested by crustose lichen. The authors used novel instrumentation called forced diffusion chamber that allows them to collect high frequency measurement of CO2 fluxes at a microsite during the growing season. From the two growing season observations, the authors show that soil CO2 fluxes in the two microsites are different in a particularly warmer and drier conditions. The authors conclude from these results that higher soil temperature and lower moisture in crustose lichen patches are attributed to enhanced soil CO2 emission. The dataset presented in the manuscript is

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quite novel, where the observations focus on the microsite scale measurements of soil CO2 fluxes in healthy sphagnum community and sphagnum community infested with lichen.

Unfortunately, the writing is rather poorly executed, making the manuscript a mere presentation of the measurements. I have several major concerns throughout the manuscript.

First, the way the manuscript is currently written, authors do not provide much insight towards answering 'why' they observed what they observed. Much of the manuscript focuses on methodology of how they came up with modelling yearlong soil CO2 fluxes, which to me could have been a part of supplementary information. The Introduction section goes over a bit far fetched into the biological effects of crustose lichen, but fails to make the link between how lichen infestation affects microclimate or microsite environmental changes to eventually affect soil CO2 fluxes. To me, a novel dataset cannot automatically be granted a publication unless it is written well with a scientific focus. After reading the whole manuscript, I was left with the question 'why is this interesting and important?'. The main conclusion of this study is that the sphagnum and lichen communities showed different soil CO2 fluxes in one of the growing seasons observed and temperature and soil moisture were important parameters in predicting it. However, it is already a widely accepted knowledge that soil respiration largely depends on temperature and moisture. So the question here should be 'what did the lichen infestation do in those microsites to alter temperature and moisture to affect soil CO2 fluxes?'. But the authors fail to provide that link in this manuscript. It is unclear to me whether the reason soil CO2 fluxes in sphagnum vs lichen communities are different is due to sphagnum community affecting environmental conditions or vise versa. What could help the authors to make the manuscript more interesting is to try to focus on hypothesis testing based on the data they have. Perhaps the authors can focus more on answering the question 'why' throughout the manuscript.

Second, the other major concern I have about the methods is the attempt the authors

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make to compute running Q10s using the two depths of soil temperature and air temperature. The authors go on in depth showing the fit of Q10 and use this in modelling yearlong soil CO2 fluxes. I do not understand why the authors did this exercise at length. Conventionally, temperature sensitivity of soil respiration, Q10, is computed using soil temperature and when computing Q10 the data are pooled to achieve the best fit of Q10. The authors model yearlong soil CO2 fluxes using this Q10 fit in three different model fits they compute for the two different years' of observations, but I also do not understand why the authors did this exercise when they actually have high frequency measurement of soil CO2 fluxes. What is the purpose of modelling soil CO2 fluxes that show three different sensitivity in temperature when they already have observational data? The modelling should only be used as part of gap filling in this case. The authors should provide better justification of this method.

Third, the authors need to be more careful about the use of language (apart from the use of English as a language) in the manuscript such that the language they use is consistent throughout the manuscript. For instance, one of the most important terms they use in this manuscript is 'sphagnum moss communities', however, several different terms are used throughout (e.g. sphagnum moss regime, crustose lichen patches, sphagnum moss colony, intact sphagnum, sphagnum habitat, and etc.). I suggest consistent use of 'sphagnum dominated' vs 'lichen infested (dominated)' community throughout the manuscript. This is just one example and the authors need more careful usage of terminology throughout. The word 'infected' is used throughout this manuscript to describe lichen dominant sphagnum patches. 'Infected' to describe an invasion of microorganisms and thus the authors should use 'lichen infested or lichen affected' throughout the manuscript. The authors acknowledge that the manuscript has gone through a language check by a native speaker of English, however, I still see language issues throughout the manuscript. I suggest the authors to have the final version edited by a native speaker of English more thoroughly.

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Introduction

- The second paragraph is a very important component of the introduction, where it introduces the logical flow of this study. However, it focuses rather too much on the form and biology of sphagnum and lichen rather than the environmental effects of these two. As the paragraph is unfocused, it makes the logical flow unnatural and weak. The second to last sentence of this paragraph even goes into saying that moss could wither and die, losing its preservation of permafrost. This is a bit of an overstatement making the logical flow weak for this study. Please consider revising the paragraph.

Methods

- There needs a section for data analysis. Please specify what tools are used for data analysis and modelling.

2.1 Sampling Descriptions and Methods

- P6L22: The authors state that the air temperature is measured at 2 m height. This also comes up in P9L12. Then what is Air50 in Table 2 and Figure 7? Please specify this in methods.

2.2 Forced Diffusion (FD) CO2 Efflux Chamber

- Please specify what soil CO2 efflux includes in this study. If surface vegetation (sphagnum/lichen) have been removed, please clarify how lichen infestation may affect soil CO2 efflux.

- Figure1 and associated text (P7L20-26). This is a technical part that does not add much to the science of this manuscript. I suggest moving this part to supplementary information.

2.3 Simulated Soil CO 2 Efflux

- It would be helpful for the readers to understand why the authors compute temperature sensitivity in this study and why this is important. Some background information

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and justification of methods used here would be necessary.

3.1 Temporal Variations in Environmental Parameters

- This section can be more focused around how environmental conditions are different under the two different communities investigated and explain why that should be. At this stage, it is rather too lengthy and unfocused. As a result, it is very difficult to grasp what the main findings are.

- P10L7-10 is better suited in the next sub-section.

- P10L22-25: I have a hard time understanding this sentence. It should be revised and perhaps adding a reference would be helpful.

- P11L5-7: This contradicts to the earlier statement 'Peaks in soil moisture during the soil thawing of early May were found at 2- and 5-cm depths in 2015 and 2016 (Figure 2), suggesting the response from soil moisture at 2- and 5-cm depths for intact sphagnum is much more sensitive to soil thawing than at crustose regime.'. Please clarify.

- P11L11-14: Why is it that in 2016 soil moisture was higher in crustose at 2cm depth? The soil temperature and moisture dynamics in relation to lichen dominance should be explained a bit better.

3.2 Seasonal Variations in Soil CO2 Emissions

- P11L23-28: This part largely overlaps with methods and should be moved to methods section.

- P12L11-14: Usually when moisture increases, the rate of organic matter decomposition also increases. Why is it the other way around in this case?

-P12L24: I'm not sure if this is a good comparison as Svalbard soil is very low in soil C compared to AK.

3.3 Sensitivity of Soil CO2 Emissions to Temperature and Soil Moisture

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- P13L16: The authors discuss seasonal dependence of soil CO2 efflux here. I do not understand this explanation. The only two environmental variable measured in this study are temperature (at various depths) and soil moisture. So what is the seasonality that regulates soil CO2 efflux in this case? Usually seasonal dependence of ecosystem C exchange is due to physiological changes of vegetation through the season or temperature dependence of respiration with season. In this study, photosynthesis does not come into play and the authors tease out temperature sensitivity in this section, but then what is the seasonal dependence are they referring to? Please clarify.

- P14L7-9: This is also a key explanation the authors keep referring to. I am curious why this is. It would be important to link theories with observations in this case. Otherwise, one of the most important support would largely remain as part of speculation.

- P16 last paragraph: The authors are discussing the usefulness of using FD chambers in this paragraph, but I think it is a bit too far fetched from the main point of the study. Please consider making the final part of the discussion rather focused on the main point of the study.

Technical corrections:

- P3L3: Either 'in time and space' or 'on temporal or spatial scales'

- P3L5-8: This only applies to high latitude ecosystems. Please specify.

- P4L16-19: Please revise this sentence.

- P5L21-23: This sentence already appears in the Introduction section. Please remove.

- P5L24: ecosystem 'dominated by'

- P6L6: these should be mean annual temperature and mean annual precipitation

- P6L11: Is 'average ambient temperature' air temperature? Please clarify.

- P7L17: Please specify that this is due to loss of power.

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- P9L7: equation (6) should be (5) instead. Throughout the manuscript, equation (6) is referred to. This needs to be revised.

- P9L18: Soil temperature is 'higher'. This should be consistent throughout.

- P11L1: a sharp jump 'in'.

- P11L16: 'These changes' should be 'The changes'.

- P13L17: Please clarify whether this is combined effects or not.

- P13L21: temperature is 'the' most significant

- P13L28: either 'in' or 'during' August

- P14L12-13: Q10 values at . . . Delete this sentence.

- P16L10-12: This sentence is very difficult to understand and grammatically incorrect. Please revise.

- P16: delete '-measurement' from 'soil CO2 efflux-measurement'.

- P16L21: Please clarify why sunny sky matters. This is winter measurements we're referring to.

- Table1: I do not think this table is very useful. It largely overlaps with the information shown in Figure 2&3. Please consider making it a supplementary information.

- Figure2: Delete the small a) inside the figure. The lines(solid/dotted) of Crustose T and M are easy to identify as they are different colours, but Intact T and M are very difficult to distinguish. Please consider using a different colour for one of them. To me, soil temperature and moisture can both be effect variables and response variables at the same time in this study. This means that soil T and M are the two variables that affects CO2 efflux, but at the same time, they can be affected by the presence of lichen. Therefore, this figure should include air temperature and rainfall data and the description of results should focus around how soil T and M change under variation of

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air T and rainfall and how intact and crustose moss affect soil T and M during these events (rainfall) and why that is.

- Figure3: The left y axis should be ‘CO2 efflux’. What are the different colours for SD (pink and light purple)? Can the colours (or line form) for two different communities consistent throughout the manuscript? - Figure4: Make a legend indicating dotted and solid lines.

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