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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 #2

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The paper by Kim et al. presents an interesting premise: that crustose lichen may affect the CO_2 flux of *Sphagnum* moss, and that this infection may also affect the stability of the permafrost beneath. They argue this by presenting data from two flux chambers in a patch of *Sphagnum* moss in western Alaska. This kind of research is valuable, since not much is known on how the spread of lichens influences CO_2 fluxes, but unfortunately this study falls short on too many fronts to make meaningful conclusions about this phenomenon. I don't think that the presented data convincingly show that there is a strong effect of crustose lichen on the CO_2 flux from these ecosystems.

To begin with the study setup: it's commonly known that CO_2 fluxes vary strongly spa-

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tially, and it's therefore advisable to use multiple measurements within each vegetation type to reliably determine whether two vegetation types exhibit different CO_2 fluxes. The same goes for soil moisture and soil temperature. This experiment, however, uses only one chamber in healthy *Sphagnum* and one in crustose-infected *Sphagnum*. The results then show minute differences between the two, which the authors extrapolate to say something about CO_2 fluxes between infected and non-infected *Sphagnum* in general. But without knowing what the variation within each group is, we don't know whether the differences between infected and non-infected *Sphagnum* are meaningful.

The authors claim that the two are different, based on a one-way ANOVA, but this statistical method is not suitable for this study. A one-way ANOVA is used to show whether two groups are taken from the same population by studying the variance between and among groups. In this study, we don't have two groups. Just two time series of repeated measurements. In this situation, a one-way ANOVA is not applicable since the repeated measures are not independent.

With just two measurement locations, it's not possible to show that the two populations from which these measurements were taken (intact and infected *Sphagnum*) exhibit statistically different fluxes since we don't know the variation within each group. In any case, the differences are very small. Visually, it appears that the only period where there are clear differences is for two weeks in June 2016 but the overlap between the two is huge for the rest of the time, which shows that more samples from each group would be required to argue that a difference exists.

Furthermore, the authors claim that their study shows that the spread of crustose lichen would lead to the rapid degradation of permafrost, but not a single measurement of active layer depth is shown in this study. Actually, at a depth of 2 cm, temperatures are lower under the crustose lichen. This is unsurprising, since the photo of the field plots shows that these lichens are completely white, and therefore have a high albedo. This means that a lot of sunlight is reflected, which would actually cool the surface and prevent permafrost degradation. This important property of these lichens is not

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mentioned in the paper, and the conclusion that these would lead to rapid permafrost degradation is unsupported.

The presentation of the paper, unfortunately, is also lacking. The writing is often confusing (despite a language check) and many statements are not well-supported by either the data or a citation to another study. The authors try to solve some of the problems caused by the limited data by applying a model, but this shows a poor performance and is subsequently extended to the full winter, a time period on which it was not tested. It's better to focus on the measurements themselves instead of using an imperfect model to draw conclusions.

Overall, I think it's a pity the authors did not do a better job because the data itself is truly interesting. But too many questions remain. For example: the flux measurements are only soil respiration, not net ecosystem exchange. Perhaps the growth of crustose lichen compensates for the loss of carbon from the infected *Sphagnum*? Unfortunately, due to the flaws in the study setup and analysis we are not closer to understanding whether crustose lichens do actually affect the CO_2 flux of *Sphagnum* mosses.

More specific comments:

Page 4, line 21-23: this statement is essential to the premise of this paper, but it's not supported by a citation.

Page 4, line 27-30: this is a very basic statement but for some reason the authors need to cite 13 studies including 6 by the main author himself! One citation would suffice.

Page 6, lines 2-3: this paper does not specify which species of *Sphagnum* the measurements are done on. Judging from the photo, I assume it's *Sphagnum fuscum*?

Page 6, line 22: has this sensor been calibrated for moss? It's normally only calibrated for mineral soil (which *Sphagnum* certainly isn't).

Page 8, line 3: this makes no sense. You estimated CO_2 flux sensitivity from the exponential relationship between air and soil temperature?

Page 9, line 3: air temperature at 0.5 m, but you measure at 2 m!

Section 3: Why are the results and discussion combined? These should be separated in two sections.

Page 10, line 24: what do you mean with 'forfeiture' in this context?

Page 10, line 26: indicate where this is shown in the figure.

Page 11, line 2: how were these thawing rates calculated?

Page 11, line 5: a thawing rate of 0 cm/day?

Page 11, line 9-10: the sudden drop in soil moisture (and the sudden rise in spring) are probably due to the fact that your moisture sensor doesn't work below 0° C. This is clear from your temperature sensor. Moisture data from days with temperatures below freezing should not be used.

Page 11, line 16-21: these snow depth measurements appear to be from a different location, judging from the vegetation. Why not point a timelapse camera at your plots so you know when the snow melted there, rather than at another place which may not be representative of your measurement location?

Page 11, line 11-14: it is pure speculation to say that this is due to a hotter and drier environment. Again, soil temperatures at 2 cm are lower in the crustose lichen location. Soil moisture is also regularly higher under the crustose lichen. Besides, there is no large difference in 2015 despite similar differences in moisture.

Page 14, line 8-9: this relation with soil moisture is not shown in this study.

Page 15, line 8: it's commonly known that air temperature governs soil temperature. There's no need to cite yourself twice to support that statement.

Page 17, line 17: the data presented in this paper do not show a loss of ecological and thermal functions.

Page 17, line 27-28: by only measuring soil respiration, rather than net ecosystem exchange, it's impossible to say whether shriveled *Sphagnum* moss is a source of CO_2 to the atmosphere.

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