Editor Comment 1: Reviewer 2 notes that statistical analyses should be improved by considering that most measurements are related to the same source, where 15N was applied. Thus, calculations have to be conducted accordingly, probably by including distance, biocrust type and their interaction in the analysis, as well as experimental plot as random factor or similar. This should be feasible given your experiment design

We have substantially altered the statistical models in the manuscript. We now use mix effects linear models to assess the relationships between 15N signatures in biocrusts and grass leaves relating distance and our proxy for Ascomycota biomass. As suggested by the editor, we now treat the experimental plot as a random effect in all models and directly addresses reviewers 2 main concern accounting for the variation due to sampling biocrusts of grasses within three circular plots. We now nest our distances and Ascomycota gene copy number within each experimental plot. The models are also now included in the manuscript for the reader to see the relationships of all variables involved. The inclusion of the model decreased the significance of our findings but did not substantially alter any of our major findings-NH4+ moved in cyanobacteria-dominated biocrusts and Ascomycota most likely involved. Our initial finding of 15NO3 movement in cyanobacteria was dropped and we have altered the results section to reflect all changes.

We feel that our model changes most accurately present our data and appropriately treat our distance variable. Only one crust type and one N form lead to positive results. There is no benefit to creating an overall model with N form and crust type. If needed we are willing to add another model if the editor still believes it is necessary.

Editor comment 2: Low amount of water added (only 2mm) has been also presented as an important factor that may affect results obtained in moss dominated biocrust. Following reviewer two suggestions authors must, at least, recognize this issue in the manuscript and also moderate related statements, as well as other speculations within the manuscript that are not proven by the experimental data, some of them already explained in the discussion.

We have substantially altered the discussion section to address the low amount of our rainfall event and moss biocrust activity. We now include the following new paragraph.

“The lack of ^{15}N movement in moss-dominated crusts may reside in the nature of our minor rainfall event. Our moss, *S. caninervis*, became photosynthetically active following the 2 mm rainfall event, changing in color from brown to green, but only in the discrete biocrust patches that we watered. Mosses, including *S. caninervis*, are stimulated by minor rainfall events (Wu et al. 2014), with events as small as 1 mm activating moss photosynthesis (Coe et al. 2012). Our rainfall event was intended to wet a small circle of biocrust to a depth of 1 cm. However, the additional aboveground biomass of mosses and the rugose topography of moss-dominated crusts relative to the smooth cyanobacteria-dominated crusts may have limited the depth our minor rainfall event penetrated the soil and, in turn, activated other biocrust components. Also, water from our event might have evaporated more quickly from the mossy biocrust surface, limiting the activity time of all constituents involved. To more conclusively determine the

potential for fungal loops to exist in moss-dominated biocrusts, more information is needed to determine the importance of effective rainfall size in initiating fungal loops."

We have also tempered our statements further in the text by incorporating statements like "the seeming lack of loops in moss-dominated crusts may stem…" We believe that our edits temper our previous message and definitely highlight the concerns surrounding the limitations of the inferences we can make from our design.