

Interactive comment on “Soil bacterial community and functional shifts in response to thermal insulation in moist acidic tundra of Northern Alaska” by M. P. Ricketts et al.

Anonymous Referee #1

Received and published: 7 February 2016

In this manuscript, the authors describe the changes in bacterial community composition as a result of increased snow cover in a moist arctic Tundra. The study shows that increased snow cover led to changes in bacterial community composition along changes in soil chemistry and the plant community. The authors conclude that the observed changes in bacterial community composition and function might lead to reduced decomposition of SOM in these arctic systems. The manuscript is well written and structured and the story is for the most part easy to follow. After careful revisions the manuscript should be of great interest to the readership of SOIL. However there are some issues that need to be addressed or discussed in more detail to improve the manuscript. 1. Soil depth: As the authors point out, that there is a huge difference in

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edaphic factors between organic and mineral horizons in this study. Such depth related differences have been shown to potentially influence microbial community structure and function and the potential controls on those (Eilers 2012 SBB, Schneckler 2015 SBB). The authors should also test the effects of depth as well as treatment and potential interactions on the individual bacterial groups, their relations to soil factors and beta diversity using Adonis and perform the mantel tests with the edaphic factors separately for organic and mineral horizons. 2. Vegetation and decomposition: The authors state that an increased snow cover ultimately leads to reduced decomposition and C loss from the system since NPP is increased and might offset potential losses of C. While their results show a reduced potential for decomposition in the bacterial community and other studies have found increased NPP in shrubby tundra compared to tussock tundra, the C contents in organic and mineral horizons decreased significantly. This huge loss could have either happened during the transition from tussock to shrubby vegetation, which would mean that NPP did not offset decomposition or during the transition into a sedge dominated fen, which would indicate that decomposition was not reduced despite the reduction of the bacterial potential for decomposition. 3. Fungi and oxidative enzymes: The authors should more strongly point out that this study is focused on bacterial community composition and function throughout the text and that fungi might play an important part especially in the production of oxidative enzymes which have been found in arctic soils (Tveit 2012 ISMEJ). 4. The authors should be more careful with the interpretation of the ancestral state reconstruction, since these results are strictly based on the sequencing results of the bacterial community. Changes in the so obtained functions can only be interpreted as changes in the bacterial community composition. Any statements concerning enzyme kinetics, enzyme transcription, activity or even in situ functional gene copy number can only be speculated on and should be clearly marked as speculation (especially Page 17 Lines 1-19) 5. The authors should consider that any changes in the bacterial community composition could be independent of SOM properties and be a result of changes in temperature, moisture vegetation length and so on and could vary with depth (Schneckler 2014 Plos One,

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Gittel 2013 ISMEJ)

Detailed comments: Title: Since multiple environmental factors are changed with increased snow cover, “thermal insulation” should be replaced with “altered snow cover” or similar. Introduction: Page 3 Lines 17-23: Since there is another paragraph on SOM in the arctic this one could be omitted. Especially since the numbers for global C storage here and in the paragraph on arctic C storage are not the same.

Page 4 Line 1-2: The nutrient limitation of Arctic soils has recently been challenged (Wild 2015 GBC, Melle 2015 SBB). Page 5 Lines 5-18: The Authors should consider using testable hypotheses instead. Structure and O₂ availability were not measured in this study. The change in plant species composition might not be a consequence of increased nutrient availability but the result of changed water status. With the experimental setup it cannot be distinguished between substrate effects and environmental effects. Material and Methods: Please mention which program was used to perform the statistical analyses. As mentioned before the measured parameters, including beta diversity should be tested for depth effects and interactions of depth and treatment. All correlative tests should also be performed separately for organic and mineral horizons. Results: Page 10 Lines 26- Page 11 Line 2: These results should be presented in a separate table. Page 11 Lines 10-11: The reported p-values are not significant. Page 11 Lines 24-25: This interpretation should be moved in the Discussion section of the manuscript and “microbial communities” should be replaced with “bacterial communities”. Discussion: Page 12 Line 11: As stated before, while the bacterial functional potential might indicate reduced SOM decomposition, the decrease in C content from control to DEEP suggests otherwise. Page 12 Lines 12-17: An alternative explanation might be that the microbial community composition is shaped by the environmental factors and less so by SOM properties. Page 12 Line 16: Blanc-Bates et al. 2015 is missing in the Reference list. Is this the same that is listed as submitted in Page 16 Line 12. If this is the case and if this study was conducted at the same site, mentioning this and a short description of the findings would help the reader understanding the

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author’s arguments about changes in SOC dynamics. Page 13 Line 26: The strong correlation of Acidobacteria with pH and the non-significant correlation with C:N questions that statement. Page 13 Lines 28-30: This sentence can be omitted. Page 14 Line 28: This could be a depth effect and not a result of the altered snow cover. Page 15 Lines 7-11: Is there any indication that increased tanning occurred at the studied site? Page 15 Lines 23-27: Binding of enzymes to tannins could happen to any enzyme. Oxidative enzymes could actually degrade tannins and might thus be upregulated. Page 16 Line 13: Sistla et al 2013 did not use a snowfence study. Page 16 Lines 13-28: While this study explains to some extent some of the author’s statements, it is over represented for its current publication status. Page 17 Lines 1-19: see general comments above.

Interactive comment on SOIL Discuss., doi:10.5194/soil-2015-89, 2016.

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