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Interactive comment on "Microbial dynamics in a High-Arctic glacier forefield: a combined field, laboratory, and modelling approach" *by* J. A. Bradley et al.

Anonymous Referee #2

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This combined laboratory and modeling study of microbial community development in glacier forefields was well designed and well written, but narrowly limited to a very specialized environment. In addition, although the Midtre Lovenbreen forefield seems ideal for the study of soil microbial community succession in an extreme oligotrophic system, it also seems to be an outlier among such systems, by having no vegetation. Thus, the results are interesting with respect to potential microbial succession without the influence of plants, but may have little relationship to any natural system. It would have more general appeal if the authors try to relate their work to more common soil microbial communities or, alternatively, to those in other extreme environments, like Antarctic soils, desert microbial crusts, cryptoendoliths, etc.

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For similar reasons, the argument that these communities might make important contributions to atmospheric CO2 requires more information about the total amount of land involved over a defined time frame and thus the quantities of CO2 likely to be emitted. If microbial communities actually drive the net accumulation of organic matter and nutrients in the centuries after glacial retreat, this argues for net C-sequestration rather than release. The authors make their best case for this study with regard to uncertainties in the sources and fates of organic matter and nutrients in these systems, rather than extrapolations to global C cycling.

The key terms for model sensitivity, i.e., heterotrophic growth rate, bacterial growth efficiency and temperature response, are generally the parameters that are important to other models. That these terms were assumed to be the same for all microbial groups is problematic, as many other field, laboratory and modeling studies have reported otherwise. For example, it is unlikely that the maximum growth rate is so high and yet BGE is so low and both are the same for all autotrophs and heterotrophs. Moreover, citing Allison 2005 for exudation rates, given that his earlier work was based on Hawaiian sites, seems a strange match to this study.

Model: despite the supplemental information, I needed to read Bradley et al. 2015 for the details of SHIMMER. I'm not certain that anyone could easily decipher the manuscript without doing so.

Line 192: SOC quality might reasonably select community composition. If empirical data suggest otherwise, please show these results. Are changes in SOC quality characteristics over time (labile/refractory) known from field sites?

Line 289: What is the quality of allochthonous inputs? Is it the same as initial materials? Perhaps I missed that information. The SHIMMER model description appears to conflate factors controlling the utilization rates of the labile and refractory substrates, so the dynamics aren't easy to anticipate.

The lab results show high variability for low means, which provided no resolution of

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treatment effects. Clearly, the methods employed to determine BGE were too insensitive for these systems. The extraordinarily low BGE values in this system are fascinating, and contrast with other soil systems. This deserves more discussion and justification for remaining constant across groups and time.

Line 352: I assume that respiration rate was ug C/ g day?

Lines 465-467: “Recycled” may not be the best term for the mineralization of C, N and P. In other microbial literature, this term refers to the reincorporation C, N and P into biomass from dead organisms.

Lines 510-512: The large difference in community structure between 16S and microscopy data deserves more discussion. This is a big departure from expectations (and simulations). What’s the reasoning? The same is true later (lines 536-538), although the spatial heterogeneity of Nostoc colonies provides a potential explanation: are observations available to contrast the N-characteristics of Nostoc +/- locations?

Line 602: I’m not convinced that it is possible to evaluate key processes independently of one another, as they occur simultaneously and interactively. So, I’m not certain what the authors are trying to say with this statement.

Throughout, the relationships between allochthonous inputs, microbial production and necromass are uncertain. More clarity is needed in tracing the dynamics and interactions of these C pools.

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