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Interactive comment on "From substrate to soil in a pristine environment – pedochemical, micromorphological and microbiological properties from soils on James Ross Island, Antarctica" *by* Lars A. Meier et al.

Anonymous Referee #2

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The study aims at linking microbiological properties and their role in soil formation in the absence of plants. The study was performed at James Ross Island, Antarctica, where no vascular plants are occurring, and the authors identified two plots with different precipitation and sea spray input. Besides the performing microbial community studies based on high throughput sequence analyses, soils were investigated by pedochemical and micromorphological methods.

The manuscript fits well into the topic of Biogeosciences, and it presents new and interesting data on bacterial taxa of these soils, depending on soil environmental conditions

on the one hand and having a possible contribution to soil development on the other hand.

However, I have a problem that the intention of the manuscript is not clearly presented. From the introduction, one may understand that the manuscript is devoted to: - increase the general understanding of soils developed in the transitional zone of the eastern APR (l. 109-111), - add to the understanding of drivers of soil microbial diversity in high latitude soils (l. 125-126), - perfrom micromorphological studies on soils of the eastern APR (l. 132-134).

At the end of the introduction it appears that it is all a little bit (l. 139-143). Further, the mentioned goals are not embedded into a theoretical framework. This makes it a bit hard to prepare the potential reader of what can be learned by reading the manuscript, which goes beyond a list of microorganisms. Here, the authors may consider reworking the introduction incl. the objectives chapter.

A further problem that I encounter is that only two profiles are compared. I understand that at such regions of the world, it is often not possible to carry out a longer-term field study. But one must be aware that this is not a very solid basis for identifying cause-and-effect relations between the soil environment and the microbiota. Multivariate statistics could be performed, because the soil increments were considered as being independent form each other (if I understand the Bray-Curtis dissimilarity right). But at the other hand the authors also reported of water and solute flow through the profiles, thus linking the different horizons. But I think that this problem can be solved by a more careful discussion.

Abstract Also in the Abstract the goal of the study is written only in a quite vague manner. It is not clear, how the lee and luv position should impact the soil development? Was it the different input of salts with sea spray? Also the rest of the abstract is quite vague. E.g., what are the changes in soil microstructure below 20 cm depth and what is the potential impact on water availability and matter fluxes.

l. 53: Is it fair to say that the soils are dominated by bacterial taxa, when obviously no fungal taxa were investigated? But I believe as well that fungi most likely are of minor importance in these soils.

Introduction

Please, see my general comments given above.

The introduction largely emphasis the different soil forming conditions, primarily related to climate, at different regions of Antarctica. Even though there are usually no figures in the introduction, here I would suggest to show a map of Antarctica highlighting the different areas that are mentioned in the discussion (it can be a slightly modified version of the present Fig. 1). But, of course, this also depends on whether the editors will accept this suggestion.

l. 123-125: This sentence is not clear, actually sating that the microbial activity has an influence on the microbial composition . . . Please, rephrase.

Regional setting of James Ross Island, maritime Antarctica

Can be first subchapter of Material and Methods.

Material and Methods

l. 221: Please, indicate in what solution pH was measured.

l. 223-228: I do not understand how Cinorg (the abbreviation has not been introduced) can be measured by dry combustion after fumigation of the carbonates with HCl. I rather assume that Corg was measured and Cinorg was calculated by difference of Ctot and Corg. Otherwise, methods are properly described.

Results

l. 347: Why "virtually" unvegetated?

l. 357-360: Since this property was not identified in the field I would shift this paragraph

to the presentation of the micromorphological features.

l. 375-376: Present the TIC content as mg g-1. How can a TIC content transform to a TOC content? Consider rewording.

l. 378-380: Is there any explanation for the very low C/N ratios, most often much lower than in microbial biomass?

l. 395: Move this sentence to the beginning of the paragraph.

Discussion

In l. 192 a strong wind ablation was mentioned at BB. What is the role of the stronger ablation of fine material at BB on the chemical soil parameters? Can the selective erosion of a particular particle size blurr the results of the different weathering indices?

l. 499-501: I would rewrite the sentence "Due to the absence of vascular plants, the ice-free area of JRI is a pristine laboratory and offers the exceptional opportunity to improve our understanding of the interrelations between soil formation and microbiological properties" as "The JRI offers an exceptional opportunity to improve our understanding of the interrelations between soil formation and microbiological properties in the absence of plants".

l. 512-513: Present TOC and N contents as mg g-1.

l. 516-517: If low P contents refer to total P, then this cannot be taken to indicate a relative juvenility of the soils. Soils rather loose P with development than they gain. In the soils under study, there is no P input by birds and I assume that also the atmospheric P input is negligible.

l. 557-561: Here, I do not understand the line of argumentation.

l. 562-567: This part is quite speculative, but could have been easily proven. Why has Na not been leached before the total elemental analysis of the soil minerals? I cannot imagine the formation of stable secondary mineral phases entrapping Na.

l. 572-577: This is an important finding.

l. 585-609: Nice discussion based on micromorphology.

l. 610-674: The discussion on the different taxa is well written, and it is a good message that this initial stage of soil development, chemolithoautotrophic lifestyles plays an important role for the generation of biomass and initial accumulation of soil organic carbon and nitrogen (even though this finding is not really new). But might be this offers also a good opportunity for an introduction, in order to base it better on a conceptional background.

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