Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-203-RC2, 2020

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "Influence of microorganisms on initial soil formation along a glacier forefield on King George Island, maritime Antarctica" *by* Patryk Krauze et al.

Anonymous Referee #2

Received and published: 21 September 2020

Krauze at al. investigated the interplay between the microbial community and soil formation after glacier retreat at Maritime Antarctica (here: King George Island). This is a fascinating topic which fits very well to the scope of Biogeosciences. The paper is well written and the quality of English language is high (only spelling error I found is in line 364, ice fee instead of ice free). All in all the manuscript is a good and inspiring read. Unfortunately some doubts on the experimental design and the drawn conclusions of the study arise while reading and cloud the pleasure remarkably. The absence of hypotheses is puzzling, especially since the chosen ecological model of very young soils, only a few decades after glacier retreat, should have led to several hypotheses around the topic of temporal dynamics of C and N accumulation and the involved microbial

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functional traits, thresholds or tipping points in the system regarding soil development etc. A study only aspiring an objective of “identifying processes” but not having any assumptions on which processes it should look, is probably not meeting contemporary standards of good scientific communication anymore. Further, the experimental design is challenged by a very low repetition number (only one field repetition per age class). In Antarctica, harsh conditions melt down iniotially planned sampling schemes for sure, but only 4 soils in single replication in close vicinity to a research station, at least raises the question, why there was not a valid experimental design possible. The authors additionally should make the age determination of their sites much more transparent as well as the data of vegetation on the sites should be reported in full detail. For instance, the authors discuss the role of vegetation for the microbial community in their manuscript, but write simply “mosses, 5% coverage” in the table. This is an almost useless information, as some Antarctic moss species are nourished by aeolian input without interfering much with the soil, while other moss species deeply penetrate into soil with their rhizoids. Additionally, it is highly unlikely, that only one species of lichens occurs, which suggests erratic identification. The age dating of the sites is the crucial issue for the study. The appearance of both higher plants native to Maritime Antarctica already on the second youngest soils is directly leading to the question if the dating was done correctly, since this appearance is either limited to highly developed soils or ornithogenic soils. As the soil parameters suggest a young soil, not having enough N for maintaining populations of higher plants on the long run, it is very likely, that these encounters are due to the frequently seen bird dropping effect (excrements containing seed of Deschampsia or Colobanthus stage the appearance of these plants for a season or two, until the N from the excrement is used up). A coverage of 10% of higher plants on such young soils suggest a high frequency of bird visits, which was excluded by the authors, as this would largely question the results of a real soil development situation of the microbial community, as higher plants alien to the respective community would have been introduced. With the given data all this stays mere speculation, of course. To solve this issue, at least a much better map of the sites, ideally

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a satellite map showing the features of the sites as well as their surroundings, along with a concise description of the found vegetation should be offered by the authors, please. If possible some proof of the assumed age, for instance by a combination by 14C and 15N analyses of the soils, showing the age of organic carbon and the source of N (fixed from air or carried in by birds?) would help explain this very rare combination of observed features reported here. These aspects should be discussed in detail, too. Regarding the DNA-based identification of bacteria it stays largely unclear to the reader where the usually pretty high number of unidentified OTUs (so to say the bacteria not yet known to science) in soils of Maritime Antarctica has gone. The abundance graph reads as if the authors could attribute every single OTU to a phylum. In the discussion, as already indicated by the missing hypotheses, the reader misses a true discussion of the "identification of processes" promised in the objectives. Not even the "influences of microorganisms on soil formation" promised by the title were discussed in detail, what anyway would have been impossible by a study leaving the fungi completely aside. After what is discussed in the paper, this study is more investigating the influence of vegetation on the bacterial community of young soils in Maritime Antarctica. A big step towards the originally intended process elucidation of soil formation would be for instance a detailed discussion on the functional traits and abundances of bacteria involved in e.g. C and N accumulation or weathering processes. In the current form, the study is a family list of bacteria of four Antarctic soils, trying to mimic process identification by naming rather weak coincidences. There is surely more to it.

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