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Interactive comment on "Soil microbial biomass, activity and community composition along altitudinal gradients in the High Arctic (Billefjorden, Svalbard)" *by* Petr Kotas et al.

Petr Kotas et al.

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Dear Associate Editor and Reviewer, please find below a detailed response to all Reviewer #1 comments and questions regarding our manuscript.

Best regards,

Petr Kotas and co-authors

General comments The authors investigate the effect of horizontal (across a valley) and vertical (altitude) gradients on microbial community structure (PLFA), biomass and activity in High Arctic. They found that both gradient affect microbial parameters, with

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shift in the dominance of bacteria and fungi related to the chemistry of the bedrock. The study target interesting question, is relevant for publication in Biogeosciences and is overall well done. My main criticisms are the method used to measure microbial activity (main issue), and too many assumptions made outside the variables measured, going beyond what the results can show.

The measure of microbial activity seems unrealistic. First, 2 mm soil was used which was frozen and thaw prior to incubation in the lab. So, the microbial community and soil endure 1 freeze-thaw cycle + sieving that will affect OM availability and the microbial community. Then you left the samples for 14 days at 6 degrees before measuring the CO2 for 24h, which you define as “basal respiration”. Fourteen days represent 1/4 of the summer (> 5 degrees) in the Arctic (low altitude) or even your entire summer for the high-altitude site (Table 1), this is a significant amount of time in the Artic. There is no justification and references used to explain why you made these choices. Overall, we can doubt that the high altitude produce high CO2 emissions in in-situ conditions and we can ask the values of your results regarding microbial activity. You did not discuss at any time the limitations of such measurement. We can imagine that the microbial community adapted better or took longer to adapt to incubation condition in high altitude soil explaining the higher CO2 emissions at 14 days. We can also imagine that at low altitude, because of the higher TOC, the CO2 emissions are high rapidly after thawing and after 14 there is not much activity, while for high altitude it took longer to mineralize more complex OM. In other words, your results of microbial activity could be just the results of your incubation/sample preparation. You need to fully acknowledge this in the article, and avoid any conclusion stating that high altitude is a hot spot of microbial activity because your data can’t fully support this. You need to be much more conscious about the microbial activity result. Have you measure CO2 emissions over time?

Author response: We admit that it was inappropriate to call the measured respiration “basal”. The characteristic we measured is rather the “potential respiratory activity”.

We agree with the reviewer that our methodological approach should be explained better. Since we were not able to conduct the measurements on site in freshly collected soils, we had to choose the proper methodology how to store and transport the samples and how to measure microbial activity to get a representative characteristic of the sites. The methodology was chosen based on available knowledge and our experiences with similar experiments. It was shown that refrigeration has stronger effect on microbial activity than freezing (Stenberg et al. 1998, SBB, 30, 393-402). It is likely because microbial activity does not stop at 4°C, which could lead to exhaustion of available substrates during the storage. Slow drying (another alternative for sample storage) followed by rewetting affects the microbial respiration similarly to freezing-thawing (Clein and Schimel, 1994, SBB, 26, 403-406) but it is far from conditions, which the soils face in the Arctic. We are aware of the responses of soil microbes to freezing-thawing cycles. Therefore, we measured soil microbial respiration repeatedly after 4, 12 and 14 days of the incubation at 6 °C (see Fig. 1). The expected respiratory burst occurred in all the samples during first 4 days, similarly as reported elsewhere (Skogland et al. 1998, Soil Ecol. 11, 147-160). It was estimated that up to 50% of the microbial biomass is killed following a single freeze-thaw cycle (Soulides and Allison, 1961, Soil Sci. 91, 291-298), leading to 10-40 fold increase in dissolved sugars and amino acids (Ivarson and Sowden 1966, 1970, Soil Sci. 46, 115-120 and 50, 191-198, respectively). After this CO2 flush, the CO2 production rate decreased and the mean respiration rates measured between days 4-12 and 12-14 did not already differ from each other. This pointed to a stabilization of microbial activities in the soils, as reported by Schimel and Clein (1996, SBB 28, 1061-1066). The respiration burst between days 0-4 were positively correlated with the respiration rates measured later ($r=0.93$ and 0.74, both $P<0.0001$, $n=36$). Therefore, there was a consistent difference among soil microbial activities along vertical gradient during the whole incubation. The samples from high altitudes showed higher flush of CO2 than soils from lower altitudes (except Gr1) as well as higher potential respiration rates after stabilization. The respiration data together with other data which we reported show that the idea that soils in high

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altitudes contain more complex soil OM and that it would take longer time to start and increase microbial activity after freezing-thawing is not correct. Instead, microbial activity in these soils is triggered as rapidly as in soils from lower altitudes. In summary, we insist that the presented respiration data are not an artefact of our storage/preparation procedure. We do not think that sample storage/preparation procedure distorted the differences in potential microbial activities occurring along vertical gradient. Further, our data showed that the respiration likely stabilized earlier than after 14 days in all samples independent of altitude. Nevertheless, we chose to present the stabilized respiration rates, not biased by the respiratory burst following freezing-thawing. However, we have additional data and can add them to the revised manuscript with proper explanation.

The discussion and conclusions are too long and go far beyond what you can say based on your results. There are many sections you discuss about the dynamic of microbial community but you only did one sampling time. You can't make big conclusions about dynamic of the system, such as L362-376. You need to just briefly mention potential dynamic but don't go much further. Similarly, you speak a lot about the effect of plant cover even you did not measure any parameters to characterize the plant cover (you also forgot to mention anything about mosses and lichens despite their importance in the Arctic) such as above ground biomass, root biomass, percentage cover (did you properly assessed it?), diversity. You just described the main vascular plants. So your section 4.4, is simply too long and not fully supported with your data. This entire section could be reduced in few sentences and focus on presence/absence of plants and not linked to microbial dynamic.

Author response: We agree with the reviewer's concern that discussion is too long. We also admit that our discussion interpretations go in some cases behind the measured data. We will revise the discussion to make it more straightforward and concise. Regarding the primary producers, we have data about plant and lichenized soil crust percentage cover at the sampling sites. These data will be added to the result section

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and discussed. However, we didn't assessed the plant diversity and biomass at the sampling sites.

On the other hand, your discussion lack of putting your results in perspective with the literature, for example other studies investigating microbial community in bare/unvegetated soil (there is several article on this in the Arctic). It would be interesting to see if F/B ratio is similar in unvegetated a low altitude compared to other study.

Author response: We extensively searched the WOS and used all relevant literature sources which we found. However, we can't exclude the possibility that some relevant publications were omitted. Based on reviewer's recommendations, we will search for relevant literature again.

Your main conclusion should be the absence of plant rather than altitude effect, especially if we consider that high altitude soil in the Arctic (especially High Arctic) is likely to be rare, and also extremely shallow (only few cm) and not a massive stock of C. So we can wonder of their importance?

Author response: We don't fully understand these remarks. Isn't the decreasing plant abundance due to the altitude effect? Please consider our sampling strategy (L 88-92). What connection is there with rareness of high altitude soils? We don't think that the high altitude soils doesn't deserve scientific interest (despite of their low C stocks and shallow soil profile), especially in context of proceeding global warming and future development of ecosystems in the Arctic. We would like to point out that the high elevation habitats form significant part of the non-glaciated landscape not only in Svalbard, but also in other parts of the northern circumpolar region.

Also you should more conclude on the site effect you have (Gr1) as site is clearly an effect on microbial biomass (consistently lower) and structure for the high altitude site.

Author response: Even though we discussed the site effect in section 4.3., we will focus

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more on the spatial heterogeneity in the data.

Another point you miss in your discussion is the fact that the soil you study is always alkaline (are alkaline soils prevalent on Svalbard or acidic?). You need to discuss or conclude if your results would be similar on acidic soil in the Arctic and compare with the relevant literature as pH is a big driver of microbial community including in the Arctic. This is an important point to make and be more critical about your results.

Author response: We thank the reviewer for pointing out this problem. Soils on Svalbard are neutral or alkaline. The soil pH in other parts of the northern circumpolar region is more variable, ranging from acidic pH (mainly on granite and gneiss bedrock) to highly alkaline pH. We are aware of pH effect on microorganisms. This issue will be discussed more thoroughly.

Finally, you find an effect of altitude on microbial biomass mainly when you divide the data by TOC. This bias the data and don't reveal hidden effect of altitude. Yes, you have less C at high altitude but you don't have more microbial biomass. The fact there is more biomass per unit of C is not of a major interest. Dividing your results by TOC is not important and bias your results. Focus on the altitude effect on microbial community structure and ratio, and acknowledge that there is not a major effect of altitude on biomass but rather a site effect.

Author response: We agree with the reviewer that we should emphasize the spatial variability in the microbial biomass and activity data (not normalized to soil TOC content) and not focus mainly on the normalized data. However, we don′t fully agree with the reviewer′s opinion that normalization bias the data. We didn′t write that we have more microbial biomass at high altitude. Many papers focused on altitudinal gradients or polar areas were published based on microbial data normalized per TOC content (for activity see e.g. Schimel and Clein 1996 SBB 28, 1061-1066, Väre et al, 1997, Arctic and Alpine Res. 29, 93-104; for biomass e.g. Allison et al. 2007, SBB 39, 505-516, Xu et al. 2014, European Journal of Soil Biology, 64, 6-14; Djukic et al.

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2010, SBB 42, 155-161, Väre et al, 1997, Arctic and Alpine Res. 29, 93-104). Author response: The reason for that was considering the differences in soil TOC content between sites. We did the same here as the microbial biomass is usually well correlated with soil TOC content (Wardle 1992, Biological reviews 67, 321-358). Even though there was site effect on microbial biomass and activity (again, we have to stress this in the manuscript), the normalized data show relatively uniform trends of low microbial biomass and activity in soils with highest C stocks and higher microbial biomass and activity with decreasing soil TOC content and increasing elevation (except for the most elevated sites along Gr1). The higher proportion of microbial C within total soil organic C further points to higher lability of the OM, which corresponds well with the high flushes of CO2 from the soils as a response to freezing-thawing. Moreover, the altitudinal trends in microbial biomass and respiration did not always follow the altitudinal trends in TOC content (compare data in Table 3 with Fig. 3). For instance, we found the most pronounced decrease of TOC content with elevation along Gr1, but the microbial characteristics normalized per TOC content did not correspond to this trend. In contrast, the TOC content from the lowest and highest sites along Gr2 did not differ, but the altitudinal trend in microbial characteristics was significant. We consider this information as important characteristic of particular sampling sites.

Specific comments Altitude and transect are both gradient and not only transect. This is really confusing in the text when you speak about gradient, as it is unclear if you speak about vertical or horizontal. You can't refer to the horizontal gradient as "gradient" and the vertical one as altitude. Decide if you speak about vertical or horizontal gradient, or altitude and transect. Change in the entire text, but don't go from "gradient" for horizontal and then use gradient also for "vertical". Be consistent.

Author response: We agree with the reviewer opinion that this needs to be clarified throughout the text. We will clearly distinguish between the effects of altitude (vertical) and transect (horizontal) in the revised manuscript.

Introduction L20: true but it is simply related to less C from a plant origin, it does not

mean that there is more biomass. This is not true as well for GR1. You are telling only one part of the story here.

Author response: We didn't say that there was more microbial biomass at high altitude. However, we agree with the reviewer that the site effect on microbial biomass and activity (not normalized per TOC content) must be emphasized.

L21: "the 2 dominant microbial groups" it sounds like it is unusual or a result on its own but in the same time with PLFA you have access to fungi and bacteria only. Just speak about fungi and bacteria.

Author response: We agree with this comment. Sentence will be revised.

L23: you didn't measure microbial dynamic over time, only the change in soil temperature. So, I would focus on what you measured and not make assumption, especially in the abstract. Keep this for the discussion

Author response: Assumption will be removed from the abstract.

L25-26: the conclusion is an overstatement. In general, unvegetated area should be considered as previous studies showed. Speaking about high elevation as hotspots of microbial activity based on 1 measurement is an overstatement (see main comment).

Author response: Conclusions will be revised.

L36-41: this is normal as high altitude usually have no soil present or are extremely shallow (few cm) and may not be as important in their distribution and volume than low altitude (see main comment).

Author response: We completely agree with the reviewer that the high altitude soils are more important in distribution and volume compared to high elevation soils. However, we wanted to point out in these lines that the soil microbial properties were not thoroughly studied yet along the altitudinal gradients in the Arctic. We consider the information given in these lines relevant.

L42: not true, you can assess the effect of changing microclimate not using altitudinal climate as you can be using different sampling time (which you did not do), different exposition, open top chambers etc. So, just focus on altitudinal but you can't say that other studies can't assess change of microclimate.

Author response: The sentence will be revised according to reviewer′s comment.

L47: are the ranges of altitude comparable and are the ecosystems comparable?

Author response: No, they are not. We are writing here about general altitudinal trends. The referred studies investigated microbial communities in different latitudes and across different altitudinal ranges (please see our next response).

L51: you cite articles on complete different ecosystems, such as Fierer et al 2011 (tropical), Meng et al 2013 (forest)... Focus on the Arctic and no other biomes, and same ecosystems (i.e. tundra and not forest) as you will not expect to have the same trends. Clearly state the location and ecosystems the studies you cited are based on.

Author response: We are aware of that. General latitudinal, but also altitudinal trends in diversity of animals and plants are one of the most widely recognized patterns in ecology. However, they are not valid for the microbial diversity as we wanted to show here. The number of references about microbial diversity or community structure along altitudinal gradients from the Arctic is strongly limited. We will mention the ecosystems the studies we cited are based on.

L59: this is not true. Your study is also true at your sites and at other sites the effects will differ. The number of studies help us to determine the common drivers across different sites. Your study is not better than others at that level. For generalisation, you could have cited Chu et al 2010 as global study of microbial diversity across the Arctic (Soil bacterial diversity in the Arctic is not fundamentally different from that found in other biomes)

Author response: We completely agree with the reviewer opinion – our study is not

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better than others and the effect is site specific, which does not allow generalization – as we have written here. What is not true then? We didn't want to highlight our study here.

L60: "Fundamental" this is a strong word, please rephrase.

Author response: Sentence will be rephrased.

Materials and Methods L80-84: any idea of the percentage plant cover? Did you measure it? You don't mention mosses and lichens, but they represent a large part of the plant cover in Arctic tundra and are completely missing from your description.

Author response: Regarding the primary producers, we have data about plant and lichenized soil crust percentage cover at the sampling sites. These data will be added to the result section and discussed. However, we didn't assessed the plant diversity and biomass at the sampling sites.

L85: you speak about the bedrock in the entire article but you never define/describe it. Could you give some information on it.

Author response: We would like to thank the reviewer for pointing out this deficit. We have detailed information about geology of the Petunia Bay. The information will be added to the site description.

L86: was an organic horizon present in the low altitude soils?

Author response: No, the soil profile is poorly developed. Based on our experience there is rather litter layer on the soil surface and then relatively homogeneous mineral soil layer overlaying coarse gravel.

L93: "kept frozen" at which temperature?

Author response: At -20 °C

L122: section 2.4 there are no references and no justification of your measurements

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choices: temperature, duration of incubation. . .?

Author response: The information will be added. The incubation temperature of 6 °C was chosen as it represents mean summer soil temperature across the whole elevational gradient (mean summer temperatures for particular elevational levels ranged from 5.3 to 7.1, see Table 1; the mean summer temperature along the whole altitude range was 6 °C). Please find the justification of our measurement choices in our response to general comments.

L131: why did you adjust the amount of soil based on TOC and in which way? Could this bring a bias if you have more soil for example in high altitude to compare you results?

Author response: We optimized the PLFA extraction protocol in our laboratory to suit wide range of different soil types (with respect to the amount of lipids and size of the SPE cartridge for lipid fractionation, analytical procedure and stock sample aliquots for eventual reanalysis). As we use 0.7-1g of soil with TOC content around 5% and the microbial biomass is usually proportional to TOC content, we adjust the sample size for C "poor" soils. This modification could not bias our results since the extraction efficiency is not affected and the PLFA yield is thus comparable for all samples. Rather the opposite is true – not accounting for low TOC content could lead to concentrations of particular PLFAs below the detection limit.

L154, 156: change "mL" to "ml"

Author response: The abbreviation mL was used in all recent articles published in BGS.

L161: I guess you checked also for homoscedasticity?

Author response: Yes, we checked the data also for homoscedasticity. We will mention this in the Materials and Methods section.

L161: state clearly if you transformed or not the PLFA data.

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Author response: The relative data were long-transformed. We will mention this in the Materials and Methods section.

L163-164: the horizontal and vertical transect are both gradient (one vertical one horizontal). See comment at the beginning.

Author response: We agree with the reviewer. We will clearly distinguish between the effects of altitude and transect in the revised manuscript.

L166: how was the forward selection done?

Author response: It was performed using CANOCO 5.0 software. The soil geochemical parameters were used as explanatory variables while the relative abundances of microbial groups (MCS) were used as the dependent variables (RDA). The test offer list of candidate variables sorted according to their contribution to total explained variation in the dependent variables, together with their significance. After selection of the best candidate, the contribution and significance of remaining candidates is recalculated to explain the remaining variability in the dependent data. Then the next candidate can be selected (of course only if significant).

L167: why did you use only P values adjusted by Holms corrections. Any reference for that?

Author response: We used the significant values adjustment to reflect the multiple tests performed on the same dataset. The Holm's correction follows the approach described in Holm (1979): A simple sequentially rejective multiple test procedure. Scand. J. Stat. 6: 65-70. This procedure is slightly less conservative compared to the often recommended Bonferroni correction. On the other hand, it is a sequential procedure and takes into account that the candidate predictors with stronger effect were selected first. Thus it suits better for the forward selection procedure (please see above our comment on the forward selection).

L172-173: it is really confusing when you speak about whole-plots vs splits-plots when

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you don't have a plot experiment. I am not sure what you refer to here.

Author response: We described the permutation strategy in the constrained multivariate tests (RDA) here. As we mentioned in lines 171-172, we could assume that the characteristics of each sample will be autocorrelated with characteristics from other two samples taken from the same site (otherwise we had 9 independent transects and not three, which is not the case). In other words, the samples from the triplicate cannot be considered as independent samples due to relatively low inter-sample distance. The sampling design was in this context hierarchical with repeated measurements for each sampling site.

L176: what type of correlation did you use? Why there is no direct reference to correlation in the previous sentence?

Author response: We used correlations to find out how tightly were two variables related to each other. We will mention the correlations in the previous sentence.

Results L190-192: this is a repetition of L 204. L192, it is also wrong what you say as the low altitude site show higher soil moisture than high altitude. Delete the sentence.

Author response: Sentence will be deleted.

L187, 203: which gradient are you talking about, be clear. L214: cite the Table you refer to L217-218: say if the correlation is positive or negative when you mention correlation even if it is given in brackets. L220: finish the sentence by "while increased in Gr2 and Gr3".

Author response: The changes will be done according to reviewers comments.

L223-225: this should be given in the materials and methods and justify why you should use it. This problematic for me and can bias your results as mentioned in the main comment.

Author response: Please see or response to the reviewer's main comments.

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L233: what is the “whole PLFA profile”? You did not use all the biomarkers in previous tests, it is not the same than MCS?

Author response: As we mentioned in L162-163, the MCS is the relative abundance of microbial groups (fungi, Gram-positive and Gram-negative bacteria etc.). We also performed the tests using the whole PLFA profile, ie. relative abundances of individual microbial PLFAs. We thank the reviewer for this comments, we must mention this in the section 2.7.

L229-237: should cite figure 5? For example, L230 which figure you refer to?

Author response: There is no figure showing the results commented on L229-231. We think that figures showing altitude effect (Fig. 5) and relation between selected environmental variables and MCS (Fig. 4) are more important. However, we would like to modify Fig. 4. by inclusion of sample points and envelopes to depict how much variation is between the altitude replicates and how distinct are the samples from particular transects. Please see the modified figure below.

L250: change “typical” by “characterized”

Author response: Sentence will be revised.

Discussion L256: do you mean “did not” or “did” correspond. Looking at your plot, you have the same trend between soil and atmospheric, just few degrees’ differences. Nothing surprising here. Your explanation is not logical, snow will insulate the soil from air temperature, so having less snow should make the soil temperature more similar to air temperature but at the beginning you say they don’t correspond. So, what are you trying to say?

Author response: We mean “did not” as written in line 256. Yes, the trends are similar. The main message here is about altitudinal stratification, not about trends. While the soil temperatures are stratified according to altitude (Fig. 1 and S1a), the air temperatures are not (Fig. S1b). The latter is not surprising, but different mean winter soil

temperatures along the altitudinal gradient, ranging from -4 to -10 °C between the least and the most elevated sites, can have strong implications for microbial activity (please see e.g. Drotz et al. 2010, PNAS 107, 201046-21051, and references therein). We also see logic in our explanation – more snow at lower elevation insulates the soil. Consequently, the difference between soil and air temperatures is much higher at low elevations compared to more elevated sites, where the soil and air temperatures correspond much better.

L255-264: this whole section I am not sure what message you are trying to deliver. Nothing is really new here, and could be condensed in a shorter section.

Author response: We will clarify and shorten our statements in the first paragraph of section 4.1.

L281: OM does not grow but increase. Change “growing” by “increasing” L285: “documented growing contribution of “, rephrase this is difficult English to understand

Author response: Sentences will be rephrased.

L288- 290: ok there is no vascular plant, but what about mosses and lichens? Could they be partly responsible for presence of sitosterol? You have to discuss about mosses and lichens in the article, you completely omitted to mentioned them, and I don’t think they are not present on the soil. Lichens have a distribution up to high-altitude and you often find them on Svalbard on top of mountain even without any soil.

Author response: We agree with the reviewer′s comment that the importance of lichens must be thoroughly discussed.

L306: this reference is a bit old, is there any more recent references done on a larger number of bacteria?

Author response: Unfortunately not, we didn′t find any other relevant publications.

L307: what is the parental material?

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Author response: It refers to bedrock. Will be changed.

L309-314: of course if you divide microbial biomass and activity by TOC you will find an altitude effect, because there is less plant input at higher altitude so lower TOC. It does not mean your microbial biomass is higher at high altitude or there is higher microbial activity. Is it really important or interesting to know that there is higher proportion of living microbial biomass per soil TOC content? Your site effect if stronger on microbial biomass (PLFA /g soil) than altitude (only present for GR1) which is an interesting result on its own and show the variability of the suppose vertical gradient.

Author response: Even though we agree with the reviewer that the spatial variability in the microbial biomass and activity data (not normalized to soil TOC content) should be emphasized, we consider the microbial characteristics normalized to TOC content important indicator of functioning of the soil system. If the microbial biomass was uniformly proportional to TOC content, the normalized microbial characteristics didn′t show any altitudinal trend. We didn′t say that total microbial biomass or activity is higher at higher elevations. Also, we don′t think that the trends in normalized microbial variables are not interesting and important as they point to differences in a lability of organic matter and C sequestration. We consider this information as important characteristic of particular sampling sites.

L316-324: This is repetition of L309-324. You need to merge both section and make it shorter, and again that you have higher microbial biomass per TOC is not of a major interest.

Author response: We will merge both sections.

L330-331: keep in mind that you work on alkaline soil and it might be difficult to compare to other soil which are acidic. . .L348: you are not working on “dynamics” because you only have one sampling time. Remove dynamic from the title as you can only make some assumption

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Author response: We completely agree with the reviewer.

L349: Why do you think it is low quality litter? Compare to what? The fact you have a high C/N does not mean low quality but just more OM and less degraded. Low OM quality usually have low C/N. This also could contradict what you say L354 “which released easily assimilable”. Is low quality litter easily assimilable?

Author response: Low quality litter is not easily assimilable, but we referred in L354 to exudates and they are easily assimilable. However, the exudation represents small fraction of plant OM input. We considered the shift in substrate origin from N poor and structural compounds rich plant detritus at low elevations to N-rich microbial products at high elevations as a reason for higher microbial activity at the most elevated sites. We definitely need to discuss the presence of lichens here.

L352: So what? Ok it reduces the microbial biomass per unit of C, it just means that there is more C in the soil. You describe this as an issue but I don’t see why it should be an issue? The microbial biomass does not decrease with increasing altitude! Sorry to repeat myself but you use the results you prefer to support your theory without considering your entire results.

Author response: Again, we admit that we have to focus to spatial variability in not-normalized microbial biomass and activity.

L356-357: this is a strange wording which make the sentence difficult to understand. What do you mean by “inverse consequences from soil MCS compared to development of microbial communities”? In which way this is “inverse” and how do you have an inverse microbial community structure (or just different) and why you speak about development or young soil when you don’t measure microbial growth or dynamic and the age of the soil?

Author response: The meaning of “inverse” is as follows: while during succession is the increasing plant productivity accompanied by increasing fungal abundance in the

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microbial communities, the fungal abundance along the elevational gradients increases together with decreasing plant occurrence. We didn't compare here the age of the soils, but successional development versus altitudinal climosequence – it has very similar attributes, e.g. gradient in plant occurrence and productivity, organic matter content etc. That is why we compared the F/B ratios in lines 359-361. The sentence will be clarified.

L358: Are you talking of your study or not? You are not working on a "succession". You are again making too many assumption and extrapolation based on your results. This is also contradictory to what you say in the previous sentence. Here you say at the "maximal plant biomass" the fungi dominate, while you say bacteria dominate in the previous section and in general in the article.

Author response: We are not talking about our study here. We are talking about the contrasting F/B ratios in early successional stages of soil development and soil at the most elevated sites. Despite the fact that both have low TOC contents and low plant biomass, the F/B ratio strongly differs.

L360: so, any conclusion? Can you really make this comparison based on 1 study? L362-376: your results do not support what you say. You have one sampling time point, don't make assumption on what you don't measure: dynamic. Focus on your results. You can say that the presence of ergosterol coincide with continuous dominance of fungi at high altitude sites, but you don't need an entire section about it. Delete most of this section into one or few sentences.

Author response: We agree with the reviewer opinion that this section must be significantly shortened.

Conclusion: L379: move "were" just before "characterized" L380: this is not true. Unless you divide by TOC, there is no consistent effect of altitude on biomass and activity. L381: can you really say that there is negligible effect of microclimatic conditions over the summer with only one date of sampling? Do you think you have enough resolution

with your sampling strategy to assess the effect of summer microclimate? L382: again, you use gradient without saying which gradient you are talking about and when you define in the material and methods “gradient” to refer to horizontal not vertical. L383: you need to clearly state the decrease in pH. The decrease is less than a pH unit and the soil remains slightly alkaline. This is important because your results are likely to be completely different on acidic soil. L384-385: again, what is the bedrock at your sampling site? L386-388: well, there is plenty of unvegetated area at low altitude and even when there are plants. Your thinking must be developed to unvegetated area not only at high altitude. Do plants will colonise high altitude soil which are only few cm thick with global warming? Also give a reference for the potential increase in plant cover in the Arctic as several articles were recently published. L389: you can’t really say that it diminishes the variability because it depends on plant species colonizing new area, the bedrock as you say. You don’t measure variability with PLFA, the resolution in the method you use is not high enough. L390: you don’t measure microbial diversity, how do you know this could have a negative effect? L393: again not true, you don’t have a considerable microbial biomass and your measure of microbial activity is questionable. You just can’t make this conclusion L379-394: there is no mention of the site effect even if you clearly have a site effect on microbial biomass and activity. This should be clearly stated as the vertical gradient is directly affect by the horizontal one in relation (in your study) to bedrock.

Author response: The conclusions will be revised.

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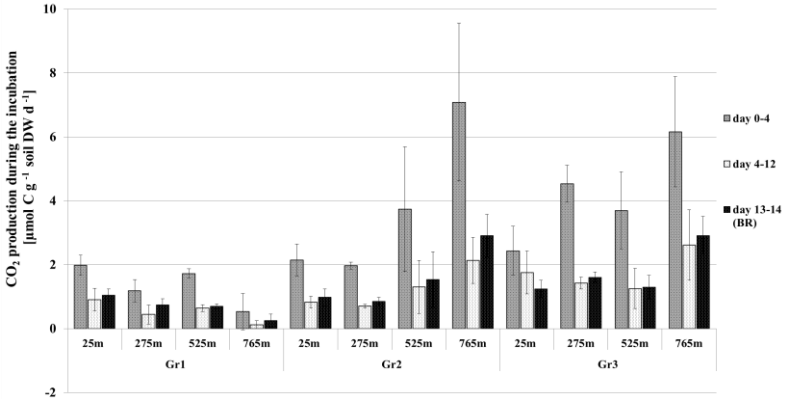

Fig. 1 Comparison of mean daily CO_2 production at days 0-4, 4-12 and 13-14 (respiration presented in the manuscript).

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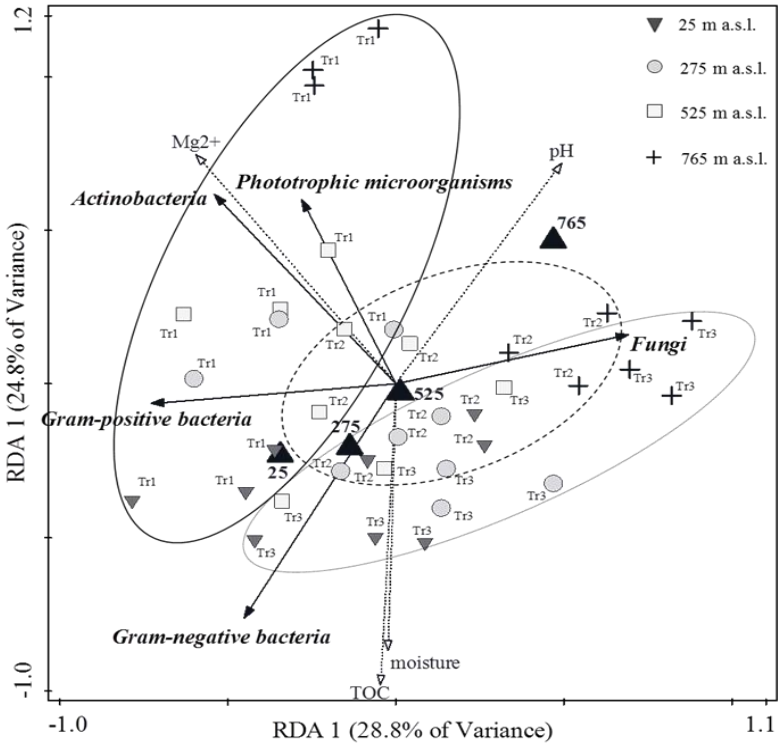

Revised Fig. 4

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Discussion paper