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Biogeosciences Discussions

Interactive comment on "The sensitivity of microbial processes in Icelandic soils to increasing temperatures" *by* R. Guicharnaud et al.

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Dear referee #1

We are thankful to you for showing interest to our research paper and for many useful comments and observations made.

The title of the paper will be changed to "Short term changes of microbial processes in Icelandic soils to increasing temperatures".

The effect of land use and soil temperature regimes has been added to the aims.

Concerning error bars, it is true that some cases the error bars are quite waste. However, the precision between measurements from each vial was generally less 2% and

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always less than 5 % although the difference between sample replicates was frequently much higher likely reflecting the high heterogeneity associated with soils.

Page 6752 Lines 7-8, sentence badly phrased and will be changed accordingly.

The 2M KCl extraction is considered to be a measure of plant available nitrate and ammonium (Blakemore et al., 1987; Ma and Wu 2008) and therefore we use the term nutrient availability.

The authors do not agree with comments concerning soil bulking limiting the value of the current study. There are a few reasons for this. Soil bulking is a recognized sampling method and involves sampling cores and bulking into one sample for representing a given area (Parkin and Robinson, 1992; Morton et al., 2000). On the other hand, when studying spatial variability in biological measurements within a given field soil bulking would not be recommended (Officer et al., 2006) as pointed out by the referee and the authors agree. However, the aim of this research paper was not to assess spatial variability within a given field but the direct effect of temperature on soil microbial processes with the aid of soil microcosms in a controlled laboratory study. Microcosms are miniature enclosed ecosystems which allow predetermined environmental factors and treatments to be measured in replicated, controlled conditions (Kampichler et al., 2001) as well as reducing environmental variables and allow predetermined environmental factors (Kampichler et al., 2001) which in this case was temperature. Including spatial variability from each site of this study was therefore believed at add environmental factors not relevant to this study. A future research however studying site specific factors on soils temperature respond would be of great value and interest. The authors have conducted a study investigating the suitability of different sampling methods as well as the spatial variability of biological factors of Icelandic soils within a given field area, which has been submitted for publication elsewhere.

The referee fairly points out that site characteristics where different and might have affected the microbial activity more than temperature. This is what the authors con-

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cluded concerning nitrogen dynamics (page 6763 line 28-29 and page 6764 line 1-5). Site specific characteristics like C, N, C: N and pH where tested for significant correlations but no relationship was detected. A sentence stating the results will rightfully be added.

It is mentioned in the comments that it appears that the biomass is greater in Hv compared to Mo, although the activity seems (Fig. 3) to be greater in Mo compared to Hv and that a discussion on the effect of site is missing concerning biomass (Fig. 2) and activity. Again, the aim of this research paper was to asses the temperature dependency of soil microbial processes. Fig. 3 demonstrates that activity generally increased with temperature independent of sampling location. The microbial biomass did generally not differ significantly between sampling locations or temperature treatments (6758 line 14-17) so it goes without saying that there was no correlation between the two. The relationship between biomass and activity is illustrated in Fig. 9 and demonstrate that while the biomass did not change between temperature treatments the activity was lowest at -10°C and at +10°C.

The authors are asked to comment on if the pretreatment had any affect on the soils in question. The pretreatment was followed exactly as Mikan et al. (2002). It is generally acknowleged that pretreatment of soils will always have an affect. Still short term microcosms experiments using homogenized soil microcosms have been widely adopted to study the effect of temperature on soil microbial activity (e.g. Boddy et al., 2007; 2008; Xu et al., 2009). The fact that all soils where pretreated the same way and still differences where measured in microbial activity between temperature treatments gives indication that in fact temperature has an effect on these specific factors. It should also be recognized that microcosms are small scale experiments generating rapid results that often stimulate larger scale experiments and enhance our understanding of fundamental issues (Benton et al., 2007).

The issue of relating in more detail Q10 values and drop in temperature to a possible decreasing SOM availability, as mentioned both by this current referee and Leifeld, are

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very just and valuable. We tested for correlations between the CO2 flux and Q10 at lower temperatures for all the sites. Respiration at -10°C and -2°C did not correlate with Q10 for the temperature interval -10 to -2°C (r 0.89 P 0.85 and r 0.92 P 0.16 respectively). The same was observed when respiration at +2 and +10°C where correlated against Q10 obtained from the +2 to +10°C temperature interval (r 0.91 P 0.57 and r 0.95 P 0.06 respectively). The only correlation found was between respiration at -2°C and Q10 for the -2 and +2°C temperature interval (r 0.84, P 0.03) which generally displayed highest Q10 values (mean Q10 of 4, Table 2). The correlation found between respiration at -2°C and Q10 -2 to +2°C was the same relationship as reported by Leifeld and Fuhrer (2005). This could be an indication of a relationship with the quality of SOM as suggested by Leifeld and that at this temperature interval SOM might be less available to soil microorganisms. We feel that it is though difficult to make such assumption as our study did not investigate either long term changes in SOM or the different fractions of SOM within soils making it difficult to draw any definite conclusions on the decomposability of OM in this study. However such a study would be a valuable input in understanding the carbon dynamics of Icelandic soils and the authors fully intent to conduct such a study. Also, if this was an indication of more recalcitrant SOM at -2°C what did we not get similar result at -10°C where no relationships where found between respiration and Q10 values.

The authors are well aware that Icelandic soils are rich in allophane and soluble Fe and Al which promote carbon sequestration. This is mentioned in the research paper (line 3-14 page 6752) and (line 26-27 page 6763). In this study the absorption capacity of these soils was not studied specifically and neither was the microbial activity associated with different fractions of the soils, so it would be difficult for the authors to comment specifically on the affect of e.g. amorphous clay minerals on the soils microbial activity. That aside, despite the high binding capacity of Icelandic soils and other soils of volcanic origin, differences in DOC, cumulative CO2 and enzymatic activities where measured at different temperatures suggesting that although Icelandic soils are affected by their pedological properties (e.g. high C sequestration and low turnover

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rates) they are sensitive to differences in temperature.

Specific comments:

The authors actually discussed this while writing the paper and decided to include this high Q10 as so high values and even higher (Q10: 237.0; 191.3; 164.7) have been reported in the literature (e.g. Mikan et al., 2002). Lowest values in the Mikan et al. (2002) study where 4.6 and 4.9. Leifeld and Fuhrer (2005) also reported strong variability in Q10 values between samples.

Page 6570, lines 20-23. The last sentence in the abstract will be removed.

Page 6760, 23 to 28. This was tested for and did not produce any significant correlations which might have been due to soil bulking. But this issue has already been discussed and justified in answers to previous comments above.

Page 6761 Lines 8-14. Will be taken out of the discussion paper as authors now feel that this is not relevant to the paper.

Lines 28-29. " the soils have been conditioned", the referee suggest to rather use the word adaption. To be conditioned and to be adapted has the same meaning so this is rather a matter of taste rather than having a scientific impact on the research paper. The authors do not agree with the referee about not being able to transfer results from this study to a larger scale. The research paper showed that increasing temperature enhances microbial activity in soils under investigation (enzymatic activities, DOC, and soil CO2 production). However the sentence will be rephrased in a more unbiased way and will be made more as a general observation. It should though be noted that many soil microcosm experiments studying the effect of soil warming on soils microbial activity use laboratory results and transfer them to a wider scale (e.g. Mikan et al., 2002; Leifeld and Fuhrer 2005; Boddy et al., 2007; 2008; Conant et al., 2008; Xu et al., 2009). This is done as microcosm studies can isolate the effect of a predetermined variable on specific factors. Just as the authors have been criticized to try to transfer

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these values to a wider scale the referee can not state that Icelandic soils will adapt to temperature changes as this has not been tested for. Moreover it will take time for soil to adjust to a new climate and while doing so it is likely that the soil carbon dynamics will be altered as has been suggested and measured in this research with the use of laboratory experiments.

Technical corrections have been dealt with accordingly.

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Interactive comment on Biogeosciences Discuss., 6, 6749, 2009.