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Comment

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

Anonymous Referee #1

Received and published: 10 August 2009

General comments

The paper deals with a case study on microbial processes in Icelandic soils. The main aim of the study was: “to assess the response of soil microbial processes and nutrients dynamics in Icelandic soils to changes in temperatures”. It would be more correct to say short term changes. Apparently further aims were to study the effect of land use, grass or barley on the soil microbial activity and to compare frigid and cryic soil temperature regimes. These should be included as aims in the introduction.

To achieve this, soils were sampled in four different areas from a barley field and a grass field on each site. Eight replicate samples from each sampling site were mixed to give a representative sample from each site. By this it is not possible to assess the variation within each field which is a limitation and limits the value of the study for

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global interpretation as the authors attempt to do. The authors use standard methods with recent modifications and methods developed to investigate microbial activity at low temperatures.

The soils used are from different parts of the country and have very different characteristics. The soils at Hv have low pH (4.5 and 4.3) high organic C (20 and 25%) and a C/N ratio of about 14. In contrast the soils at Mo have the highest pH (6.4 and 7.0) the lowest C content (5.9 and 5.1) and a C/N ratio of 11.7. This would suggest that the soils and site characteristics are substantially different and may, if not probably, be of more importance for the microbial activity than the temperature regimes. However the authors do not make any attempt to elaborate on this. This is surprising as the results suggest that these differences are real. For instance from Fig. 2 it appears that the microbial biomass is greater at Hv than at Mo but that the microbial activity is greater at Mo than at Hv (Figs 3 and 4) independent of treatment. An analysis and discussion on the effect of site is missing and should be added.

In this study the soils were incubated for only two weeks at temperatures ranging from -10 to +10 °C after they had been kept frozen for a prolonged period. The authors leave it open if the pre treatment had any effect or not. This would need to be commented on. Generally the increase in biological activity between -2 and +2 is greater than between +2 and +10 °C. As the freezing point is a threshold value it would need some attention. The authors point out that microorganisms live in a thin unfrozen film of water and from the DOC dissolved in this water. Thus the question raises which proportion of DOC would be in that film and how much of it is effectively available. In his comment on the article Leifeld discusses the effect of possibly decreasing availability of readily decomposable SOM at higher temperatures and a drop in the Q10 factor as a consequence. The authors discuss the question of substrate availability (page 6762, lines 19 onwards) and although the importance of the readily decomposable fraction of the DOC is not questioned other factors that possibly inhibit the accessibility of labile C need to be discussed. Icelandic soils are generally high in organic C and N but they are

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also rich in allophone and soluble Fe and Al which are thought to bind organic matter. In how far this effects the readily available C fraction needs to be understood before overall conclusions on the effect of global warming on microbial processes in Iceland are made.

The replicate analyses vary considerably. This can be seen by the error bars in Figs. 2. to 7. The triplicate analyses were done by taking a subsample from a bulked sample. The samples were moist, however an error exceeding 10% should hardly be accepted. The large errors are not systematically connected to one analysis or to certain samples. This calls for a comment from the authors on the general accuracy of the analysis and how that affects the interpretation of the results.

The article demonstrates that biological activity is taking place in Icelandic soils at sub zero temperatures and this is of considerable value as this has not been investigated before. It shows that the microbial activity compares quite well with what is found in arctic and subarctic regions and helps in understanding the processes that are taking place in this cold environment.

Specific comments

Page 6750 Line 6. “nutrient availability” here rather N transformation. Nutrient availability is used throughout the article when in fact it deals with a part of the N cycle. Lines 20-23. The last sentence is too far reaching. The results do not indicate any clear direction in which the carbon dynamics will be going upon global warming and it is going further than the authors do in their aims and conclusions. Better just omit this sentence from the abstract.

Page 6752 Lines 7-8. Soils with low bulk density are generally not difficult to manage. What should make the Icelandic soils so different?

Page 6758 Line 1. The quote Millet et al 2007 is out of place as you are only reporting on your own results

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Lines 13 to 18. Here a single Q10 value disrupts the general picture. How reliable is this? Should this value be taken into account at all? See general comment on the reliability of analysis.

Page 6760 Lines 23 to 28. If the Q10 values do not relate to the temperature treatments, land use or soil temperature regimes it is tempting to ask if they relate to any of the site specific factors (pH, C or N). It is suggested that the authors try this although it may not be obvious.

Page 6761 Lines 8 to 14. One can perhaps say that the Q10 values were generally greater in the range +2 to +10 than at -2 to +2 and later that dehydrogenase activity was generally greater in grassland soils. These are not as clear cut results as the authors suggest in their discussion. It is interesting if certain enzyme activity is greater in arable than in grassland soils, however it should be stated that generally soil biological activity is enhanced by ploughing causing a breakdown of organic material. Should the Icelandic soils behave differently that would certainly be an interesting result. However this study will not be able to answer that question.

Lines 28 to 29. Has the soil been conditioned? (by whom). Would it not be more correct to say that the soil microorganisms have adapted to the low temperatures. Ecosystems adapt to certain environments and environmental changes like the global warming. This will also apply to the biology of Icelandic soil and the adaptation will take time. This case study cannot give any indication of how these changes might or will be. Therefore conclusions on large spatial or temporal scales as done in the abstract are not possible.

Page 6762 Lines 4 to 29. This is a very good part of the discussion and highlights the problems of the complex system the soil is. The availability of readily decomposable OM, here characterized by DOC, is one thing and the accessibility of this fraction to the microorganisms another. In his comment Leifeld questions if the incubation method is the right thing to do to detect temperature sensitivity. The authors are asked to respond

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to his comment. The authors continue and mention recalcitrant organic matter that is not accessible to the soil microorganisms. In recent years a number of publications have focused on the stabilization of soil organic matter including DOM (e.g. Issue 171 of Journal of Plant Nutrition and Soil Science, February 2008). It would be interesting to have the authors comment on this in particular the effect of changes in temperature on the readily available DOC fraction. The authors continue and cite research that came to the conclusion that the activity of the microorganisms was to a greater extent controlled by readily decomposable substrate than the temperature. This seems to defy any discussion on long term conclusion from the two weeks incubation used in this study.

Page 6764 Lines 18 to 26. These are very general statements rather conclusions from the study. I feel the authors can do better than this. The conclusions should be drawn from the results and beyond that only as far as this research allows.

Technical corrections

Page 6759 Lines 25-26. ..with there was.. correct sentence

Page 6751 Line 2. “could result in the release”. correct sentence Line 8-9. “impacted by permafrost” Is this really GB English?

Page 6752 Line 26. (Keeny, 1982; Curtin and McCallum, 2004) Not in References

Page 6756 Line 5. change tow into two

Page 6758 Lines 20-21. (Figs 5 and 6)

Page 6759 Line 1. add: (Fig. 6)

Page 6759 Lines 25-26. rewrite sentence

Page 6760 Line 19. soil temperature regime

Page 6763 Line 20. Müller et.al. 2002 not in References

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Page 6764 Line 3. in the Arctic??

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

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6, C1483–C1488, 2009

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