

Interactive
Comment

***Interactive comment on “Ecosystem respiration,
vegetation development and soil nitrogen in
relation to breeding density of seagulls on a
pristine volcanic island, Surtsey, Iceland” by
B. D. Sigurdsson and B. Magnusson***

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The authors wish to begin with thanking the two referees for their constructive criticism, which has significantly improved the manuscript. Their comments can be seen in the following sections. Author’s replies are listed below each comment.

Anonymous Referee 1 Received and published: 19 September 2009 The two authors present a study from an interesting place. They chose ecosystem respiration(RE) as a measure of biological activity and discuss this in terms of ecosystem succession

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as driven by seagull colonisation via N input to the system. The Authors found highly significant differences in ecosystem structure, soil substrate and RE between colonised (C1) and non- colonised (C0) plots.

Autor's response: OK

The spatial design of the study is well appropriate to cover statistical relationships between different plots. The style of the manuscript is brief and the language is correct and easy to understand.

Autor's response: OK

There is one essential problem with the study. Respiration is known to vary across diurnal, seasonal and interannual time scales due to its sensitivity to the physical and chemical environment. The methods chapter tells that each of the 21 plots has been measured 4 times resulting in 84 respiration measurements during day time. The description of the measurements does not note at which time of the day every single measurement has been taken and how the authors corrected for the effect of the diurnal RE variation on their results and conclusions. It neither describes which time period a single measurement represents. Normally a respiration measurement takes only a few minutes. The investigation sheds thus only a spotlight on a very short episode of a highly variable time series and quantitative estimates should be interpreted with great care, which the Authors do not apply.

Author's response: This is a fair point. The authors are well aware of the DIURNAL variation of RE, which essentially is controlled by soil temperature fluctuations. There are also other factors which control RE at longer timescales (weekly, monthly, seasonal), such as soil water availability, growth phenology (autotrophic respiration) and microbe and soil animal population dynamics (heterotrophic respiration). Therefore the above criticism applies to two separate issues; a) diurnal variation and b) seasonal/annual variation.

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6, C3750–C3755, 2009

Interactive
Comment

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Interactive Discussion

Discussion Paper



Author actions: Since soil T measurements were made with every single RE measurement the authors can and will create a Q10 function from the measurements within each ecosystem type and use those to calculate the RE at a common T_{soil} across all measurements. This is a commonly used method to deal with diurnal variation in RE datasets, when measurements are done at different locations/times over relatively short period of time. As explained in the Discussion (page 8-lines 7-9) this is foreseen to further increase the observed difference between the two ecosystem types since C1 plots were “colder”. Such T_{soil}-corrected RE values do not show the real difference between the two ecosystems (which was the reason for the authors to choose the present way of displaying the data); but can indeed serve to remove any doubts that the general results are “flawed” because not all plots could be measured simultaneously. An appropriate description will be added to MM and subsequent figure (two columns \pm SE for the average RE at a constant T_{soil} for both ecosystem types) will be added to the Results. A discussion will also be added on the possible impacts of both diurnal and seasonal variation in RE on the present findings.

The manuscript mentions, e.g., a large temperature difference between C1 and C0 plots (T_{C0}T_{C1}), *which they explain with shading by vegetation. It is not discussed, that (how) these differences depend on time* because of the diurnal variation in RE.

Author actions: This point will be addressed (see above).

Looking at differences in structural ecosystem traits, as also described in earlier publications, one could well have expected that a well developed ecosystem with plants and soil organic matter (SOM) would respire more than a barren sand soil. The Authors compare RE rates with N concentration in SOM. Before discussing the comparably high RE(N) correlation (Fig 3) one needs to show that the criteria for such regression are fulfilled, as two very different data sets are analysed in one regression. To investigate this, one needs to do the same analysis for each of the two data sets, C1 and C0, and investigate the distribution(s) of the residuals.

Author actions: This was actually done; a more thorough statistical description will be

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6, C3750–C3755, 2009

Interactive
Comment

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Interactive Discussion

Discussion Paper



added to the MM chapter.

Apart from this I would find it more appropriate to compare at least also area related RE with area related N and C stocks. It is not only the quality of a substrate but also the amount of substrate that matters for respiration.

Autor's response: Good point! Unfortunately no soil bulk density measurements exist from the two ecosystem types in Surtsey, so such a conversion of the measured soil N and C concentrations is not possible. The authors agree that this would have further strengthened the present results, but deem it unlikely that converting concentrations to contents would have altered the findings (i.e. the authors agree of course with the reviewer that the concentrations are showing general differences in C and N contents).
Author actions: A note on this will be added to the Discussion.

Given the presented RE / N relationship the Authors tend to see N as the most important driver for vegetation cover and ecosystem function. Although very likely from everything what ecological textbooks tell, I wonder how this statement is related to the study. Wouldn't one need to investigate all other possible drivers, e.g., phosphate and show that they are less important for ecosystem development than N? Autor's response: Fair point. Author actions: A note on this will be added to the Discussion – and the general statement on N as the “main driver” will be accompanied with further discussion on other potential influential factors. Such discussion is partly found in the Discussion (Page 8; line 2-5; water availability), but it will be expanded.

Due to the scarce data basis (too short investigation period to end up with substantial, general conclusions) and in parts poor and descriptive analysis I do not recommend publishing this manuscript.

Autor's response: Since the two referees don't agree on this point, the editor will have to deem if the major revisions (improvements) based on the reviewer's comments of the present manuscript will be enough to accept it for publication in the NECC special issue in Biogeosciences.

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Interactive Discussion

Discussion Paper



Does the paper address relevant scientific questions within the scope of BG? Yes Author's response: OK

Does the paper present novel concepts, ideas, tools, or data? No, no, no, yes Author's response: OK

Are substantial conclusions reached? Yes, but not supported by the novel data from the study Author actions: The conclusions will be modified according the the referee's comments.

Are the scientific methods and assumptions valid and clearly outlined? Yes apart from the temporal aspects of the study. Author actions: This will be dealt with – and since we have Tsoil from all measurements this can be done in an acceptable way.

Are the results sufficient to support the interpretations and conclusions? No Author actions: The conclusions will be modified according the the referee's comments.

Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Apart from the temporal setup yes. Author's response: OK

Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes. I wonder whether the Authors do not compare their findings with Mangússen's previous study (1992) Author actions: Actually the authors had written a paragraph on this in the Discussion which was omitted just before they submitted the manuscript to BG. We will add it to the Discussion chapter.

Does the title clearly reflect the contents of the paper? No the scope of the manuscript is only on respiration and not on vegetation development Author actions: Fair point – we will move “vegetation development” within the title (further back), so it is clear that it was not the main focus of the study.

Does the abstract provide a concise and complete summary? Yes Author's response: OK

C3754

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6, C3750–C3755, 2009

Interactive
Comment

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Interactive Discussion

Discussion Paper



Is the overall presentation well structured and clear? yes, very much so! Autor's response: OK

Is the language fluent and precise? Yes, very easy to read Autor's response: OK

Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes, apart from using names instead of symbols in formulas (Cover= ...) Author actions: Fair point – we will change those to single letters.

Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? As mentioned above Autor's response: OK

Are the number and quality of references appropriate? Yes Autor's response: OK

Is the amount and quality of supplementary material appropriate? No supplementary Material Autor's response: OK

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/6/C3750/2009/bgd-6-C3750-2009-supplement.pdf>

Interactive comment on Biogeosciences Discuss., 6, 8393, 2009.

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6, C3750–C3755, 2009

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