

## Author response to reviewer's comments

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

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 in red.

### 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 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.

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.

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.

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.

**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<sub>soil</sub> 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<sub>soil</sub>-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 M&M and subsequent figure (two columns  $\pm$ SE for the average RE at a constant T<sub>soil</sub> 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<sub>C0</sub> » T<sub>C1</sub>), which they explain with shading by vegetation. It is not discussed, that(how) these differences depend on time both of the day and within season. It could well be vice versa (during evening, night or in autumn). This limitation makes the study more a qualitative than a quantitative one.

**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 added to the M&M 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.

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?

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.

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.

Does the paper address relevant scientific questions within the scope of BG? Yes

OK

Does the paper present novel concepts, ideas, tools, or data? No, no, no, yes

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 T<sub>soil</sub> 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.

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

OK

Is the overall presentation well structured and clear? yes, very much so!

OK

Is the language fluent and precise? Yes, very easy to read

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

OK

Are the number and quality of references appropriate? Yes

OK

Is the amount and quality of supplementary material appropriate? No supplementary Material

OK

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The author's wish to begin with thanking all 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 in red.

### Anonymous Referee #2

Received and published: 3 December 2009

Sigurdsson and Magnusson present a study on ecosystem respiration on a very unique substrate, a young volcanic island in Iceland. The authors measured soil respiration and vegetation cover, the density of seagull nests and took soil samples. They observed that vegetation cover, respiration i.e. ecosystem activity and nitrogen content was higher on an area that seagulls have colonised than elsewhere. They also constructed a model for vegetation cover and for ecosystem respiration using the nitrogen content as a explaining factor.

OK

The study addresses a relevant scientific question about factors affecting soil respiration and the paper presents novel data.

OK

The description of experiments and calculations is sufficiently complete and precise to allow their reproduction by fellow scientists.

OK

The authors give proper credit to related work and clearly indicate their own new/original contribution.

OK

The abstract provides a concise and complete summary.

OK

The overall presentation is well structured and clear. The language is fluent and precise. Mathematical formulae, symbols, abbreviations, and units are correctly defined and used. The number and quality of references are appropriate. There is no supplementary material but it is not needed anyway.

OK

However, information is missing in methods section as well as the title and objectives do not correspond to the content or conclusions.

This was also thoroughly commented by Reviewer's #1.

**Author actions:** The authors will add to M&M the requested issues and modify the conclusions (see author responses to Reviewer #1).

The discussion on the origin of the respiration is thin considering that it is commonly known that vegetation has higher nitrogen content as well as soil respiration rates compared with bare soil.

Yes, of course. The point of the present study was to show HOW this comes about on an 40 year old pristine volcanic island, where there was no organic/developed soil when it rose from the sea + to report actual RE of such young and emerging ecosystems. The RE:N relationship shown in the present study was only based on soil N (not including the N found in the existing vegetation).

**Author actions:** A discussion on the origin of the respiration (soil vs. aboveground vegetation) will be added.

I find that the manuscript would still need major improvements before publication.

OK

### Specific comments

The objective of the study was to investigate biological activity in different vegetation types and development stages on Surtsey, Iceland. In conclusion, the authors state that the amount of nitrogen is the most important factor for the rapid vegetation succession as well as high respiration rates. I think the conclusions correspond better to the content of the paper than the objective and title. The substantial conclusions would be reached if the authors rephrased their objective and aim to correspond to the rest of the text. The title does not clearly reflect the contents of the paper either because the paper is not about vegetation development at all, more about the vegetation cover.

Fair point: This was also thoroughly commented by Reviewer's #1.

**Author actions:** The authors will make the requested changes (also see author responses to Reviewer #1).

The scientific methods and assumptions are valid and clearly outlined except for some exceptions: How was the soil depth measured? In section 2.2 authors mention that rocks existed at the sites. What do the authors think about the rocks and do they affect the result somehow? I understood that the measurements were not carried out on a rocky surface due to practical issues.

Good point. Such patchy (rocky) plots existed in both ecosystem types and therefore the authors don't believe that

**Author actions:** The authors will add a sentence about this issue in the discussion – and include the issues mentioned in the revised M&M chapter

The exact hour of measurements should be mentioned too. The authors have performed the measurement four times at 21 measuring plots and therefore I assume that the measurements were made at different times and even on two different days. Does the measuring hour have an effect on the results? Do the results differ on different days?

Fair point: This was also thoroughly commented by Reviewer's #1.

**Author actions:** The authors will make the requested changes (also see author responses to Reviewer #1).

What would be the situation be for example in early or late state of growing season?

Fair point: This was also thoroughly commented by Reviewer's #1.

**Author actions:** The authors will make the requested changes (also see author responses to Reviewer #1).

The results are somewhat sufficient to support the interpretations and conclusions except that the authors did not actually study other factors than nitrogen and therefore I don't find it appropriate to use an expression such as "The most important factor: : :". I think the authors could also do better in describing or discussing the role of different components in Re, I mean autotrophic and heterotrophic respiration because it is more or less evident that the nitrogen content as well as the soil respiration is higher on the vegetated surface. Do the authors think that the bird dropping and other material that birds have brought play a significant role in respiration?

Fair point: This was also partly commented by Reviewer's #1.

**Author actions:** The authors will make the requested changes and add to the discussion on the origin of RE and which factors are likely to have affected it (also see author responses to Reviewer #1).

Technical comments

Table 1: I would suggest something more informative title for the column "Flux cover"

**Author actions:** will do.

Figs 1 and 2: I found it a little bit confusing that some values are missing in the series of measuring plots. Could they be renamed for the reader so that they don't have to wonder where the results from the plots number 2 and 5 are? However, this is not crucial.

**Author response:** Since these plots represent permanent study plots and their numbers are really "names" that also are used in other publications the authors are quite hesitant to change this; since it is bound to create confusion when the presented results will be used by other scholars doing studies on Surtsey in the future.

In the fig 2, I would change the places of plot numbers 9 and 8. The interval between the number is not equal in the first ones and later the numbers are not below their bars (the 23 is halfway out of the figure).

**Author actions:** Woobs – we will change this (plots 8 vs 9). The latter – the plot numbers not correctly placed compared to the bars is something that happened in the editorial office; we will see to it that it is correct in the final proofs of the paper.

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