Authors comments to the reviewers questions

Reviewer 1

Comment 1

The reference Magnússon et al. 2014 is not given in the reference lists.

→ Added

Comment 2

It is recommended to delete the sentence that no biological N fixation is expected in the Surtsey ecosystems, at least in the non-ornitogenic soils.

→ We added a N_2 fixation by lichens and biological soil crust to the list of possible N sources to the ecosystem, but since Kristinsson and Heidmarsson (2009) showed that such species have a very limited distribution on the island, their N input to the ecosystem is assumed to be negligible for the island as a whole. Therefore our assumption that symbiotic N_2 fixation by plants and free living N_2 fixation species is probably of small importance was not completely removed.

Comment 3

Is there any information available about phosphorus concentration and stocks in soils, either ornitogenic or non-ornitogenic, or both?
Would the authors expect any differences among both types of communities?
Can you confirm the general statement that P availability is high at early stages of primary successions, either ornitogenic or not ornitogenic? (Walker and Syers, Crews et al 1995).
It is suggested to briefly discuss expectations about P availability if there is no data available.

→ Leaf analyses of five dominant plant species on Surtsey generally indicates a strong N limitation (N:P < 14), both outside and inside the colony (Thuys et al., 2014). This reference was added to strengthen the argument that the vegetation is N limited both outside and inside the colony.

Reviewer 2

Comment 1 (Introduction)

1) The premise of the paper is that the influence of gulls will be limited to the area of the colony. That strikes me as rather unlikely. First, seabirds (including gulls) tend to fly over a larger part of the island than where they nest, thereby extending guano deposition over a larger area. Second, N is often highly mobile, and given the expected low ability of the vegetation and soils to capture it rapidly, it seems like that leaching would resulted in movement of N, particularly downslope. This manuscript would really benefit from a map that illustrates the location and area of the seabird colony relative to the entire island., and includes topography and location of the 18 plots sampled. At a very minimum, the size of the colony and the relative positions of the non-seabird and seabird plots should be described (e.g., are all the seabird plots closer to each other than the non-seabird ones?). And how was "seabird colony" defined – what nest or bird density was the cut-off?

→ A topographical map was added, showing the locations of the permanent plots used in this study, as well as the outlines of the seagull colony.
The location of the plots prevent downslope leaching of N from the colony to the plots outside the seabird colony.
The seabird colony was defined visually on an infrared aerial photograph by the sharp transition from mostly non-vegetated tephra sand and lava (<5% cover) to lush grassland. Bird nests in 1000 m2 area around each permanent plot are counted every year in situ, which further backs up our spatial estimate for the limits of the colony.

Comment 2 (Introduction)

The hypotheses are not very strong:

a. Hypothesis 1 essentially says that N stocks are expected to be lower than the sum of N inputs over the history of the island. It is difficult to imagine any system in which a portion of the N added to the system is not lost over time (in a system with a lot of N-fixing plants the vegetation may drive accumulation – but leaching, denitrification, etc. will still occur). In the absence of any quantitative prediction or comparison to other systems this really doesn't say anything.

→ This was very fair comment. Therefore we made a change in the hypothesis; we hypothesize that total N stocks outside the seabird colony should approximate 75% of the total estimated accumulated atmospheric N deposition during the past 50 years This assumption is backed up by field observations in different ecosystems with slow N input rate, which have revealed an average N retention of 75% (Thomas et al., 2013).

b. Hypothesis 2 essentially says "N accumulation is what other researchers estimated it to be". Without an indication of why it might not be what was estimated in 2009 (changes in rates of accumulation over time?) this is not really a hypothesis.

→ The reviewer is quite correct; this was one of the main goals of the study, not a hypothesis. We tried to modify the text so this is clearer. Except from the coarse estimate of Magnússon (2009), which was derived from a model from the literature and nest counts on Surtsey, no data on seabird derived N input on Surtsey were available, despite the extensive list of studies that have shown drastic effects of seabird N addition on the ecosystem structure and functions on

Surtsey. Therefore the main goal of the present study was to make a more precise estimate of N accumulation (rates) and stocks in the island ecosystem.

c. Hypothesis 3 would benefit from an associated mechanism. While the presence of deep roots in deep soils would certainly reduce leaching, I could imagine that a dense net of shallow roots (most of the vascular plants are monocots) could do this quite well in shallow soils, since presumably the N is not leaching through the underlying parent material. This should be clarified.

→ A reference to Bowman et al. (1998) was added, who show that similar vegetation types with deeper root systems are more effective in keeping the N from being lost by leaching than their shallower rooting equivalents.
Nitrogen can leach trough the underlying parent material through the extensive network of cracks and fissures in the lava.

d. Hypothesis 5: How is "soil development" defined, beyond containing SOC? This runs the risk of being a circular argument ("High SOC, which is the product of biomass degradation, will be highest where the most biomass is produced").

→ A definition of soil development was added.

Comment 3 (Methods)

Why were "plots" included as random variables? Plots should have been the experimental unit (with soils depths nested within plot if multiple depths are included in the same analysis).
To treat multiple samples from the same plot as independent is a form of pseudoreplication, as is suggested by the idea that "plot" could be included or excluded at will.

→ Plots were taken as random variables as total nutrient additions within the seagull colony were not equal since different plots were colonized at different time points and had different breeding densities of seagulls within 1000 m2. Therefore they could not be regarded as fixed variables and a random model was used.
However during the analysis we made a comparison by treating "plots" as fixed variables and found no large differences between the two models.

Comment 4 (Methods)

I would also not have thought as "year" as a random variable; it would make more sense to explicitly test whether year has an impact, since that would certainly change the interpretation of results.

→ Thank you for this comment, year was actually regarded as fixed effect, but left out of the model as it was not significant

Comment 5 (Results)

Since C and N stock calculations were based on biomass * concentration of C or N, running correlations between C or N stock and biomass makes little sense – they are autocorrelated! It is hardly surprising then that there is a "linear positive correlation" between total ecosystem biomass and C and N stock in the habitat with substantial biomass and presumably variation in biomass(section 3.4). If this is an

incorrect interpretation, an explanation of why they are not autocorrelated needs to be added.

→ The authors feel that this comment is partly due to a misunderstanding and have changed the text to clarify this better. Ecosystem C and N stock were only partly from biomass, but actually to large extent they were located in the soil compartment which was also included in the ecosystem estimates (Fig. 2 and 5). Therefore they were not directly related to biomass: Outside the seabird colony, approximately 80% of the C and N were located in the soil. Within the seabird colony approximately 40 and 50% of the C and N respectively were located in the soil. As the C and N in the soil are not derived from biomass values, the close linear relationships of the ecosystem stocks are not simply due to autocorrelation.

Comment 6 (Results)

Second paragraph: Does the % N in above- and below-ground components differ between the deep and shallow sands? There is no figure or presentation of variance for the reader to interpret.

→ The proportion of N and C stored in the belowground compartment was added in Fig. 2 and 5.

Comment 7 (Results)

Section 3.3, lines 20-21: Given that shallow tephra sands are defined as <30 cm deep, how could roots NOT be concentrated in the top 30 cm??

→ This sentence was removed.

Comment 8 (Results)

Section 3.4: The reported R/S ratios are strange. It is difficult to image plants with 90% of their biomass in shoots (ratio of 0.1) or with 0.5% of their biomass in shoots (ratio of 194). Even the mean R/S values seem very high (18 for off-colony, 5 for on) as the authors confirm in the Discussion (yes, the values on-colony are similar to those for cool temperate grasslands – but most of those grasslands don't have massive N and P inputs!). These values suggest some methodological issues and this should be addressed.

→ Explanation is discussed (line 358-366 and 367-387)

Comment 9 (Discussion)

The authors express surprise that report that unexpectedly, N stocks are 50-60% of the N calculated to have been deposited over the island by atmospheric deposition. I think this interpretation is extremely unlikely, particularly given that very few plants would have been present during the first half of the island's history. It is much more likely that the entire island was affected by seabirds to some extent, and that the N stocks reflect significant N inputs by seabirds during the second half of the island's history (with subsequent root development and retention) everywhere (though more on-colony), not high retention of N inputs throughout the 50 year period.

→ Thank you for this comment. The first hypothesis was changed into an expected N retention of 75%, as was found in field observations in different ecosystems with slow N input rate. (Thomas et al., 2013). We added the greater leakage of N during the initial years before plant colonization as reason to explain the difference between the observed N retention (50-60%) and the expected N retention (75%).

Comment 10 (Discussion)

Section 4.2, last paragraph: is the approx. 2 mg / g DM reported in Anderson and Polis really different from the 2.5 mg /g DM reported in this paper? That study was done in an extremely dry environment, with very little plant growth in most years.

→ Thank you for this comment. We agree that the 2 mg g^{-1} DW reported by Anderson and Polis (1999) do not really differ from the 2.5 that was found at Surtsey (2.5 mg g^{-1} DW).
We changed this in the text.

Comment 11 (Discussion)

I can't follow the argument in Section 4.3. The authors state there was no effect of tephra layer depth on total N stock when biomass was included, but Table 1a shows significant differences for litter and SON, suggesting that either live biomass showed no pattern or that it was very variable. The authors then conclude that this indicates a "strong N limitation". To me, higher N in soils and litter but not biomass suggests something other than N (e.g., water) is limiting!

→ The authors have changed this argument. The lack of effect of tephra layer depth on total N stock suggests a high N retention potential in the upper centimeters of the tephra sand. Therefore, faster successional rates on deep tephra sands (Magnússon and Magnússon, 2000; Del Moral and Magnússon, 2014) can not be explained solely by increased N accumulation rates.
In general, subarctic ecosystems have been shown to be N limited. Even in situations where the ecosystem N stocks are substantial, as the largest part of it is locked up in the soil pool.

Comment 12 (Discussion)

Section 4.4: another factor that may affect a lack of increased % shoot biomass on the seabird colony is that gulls often pull up vegetation and use in in their nests, and also trample it. Was there any indication of this?

→ No, there was no visual indication in situ that the vegetation had been pulled up by gulls. The most common gull in the colony, *Larus fuscus*, only uses few straws of dead litter to ring their nests. Further, bird trampling did not appear to have any major effect on the vegetation.

Comment 13 (Tables)
Table 1a: what is the difference between the 8 leftmost (after the variable column) and the remaining 6 columns? The headers are identical. Is one set supposed to be N and one set C?

→ Thank you for this comment.
The header for columns 9-14 is changed to 'Biomass stock (ton ha^{-1})'
The header for columns 15-22 is changed to 'C stock (ton ha^{-1})'

Comment 14 (Technical corrections)

Abstract, line 8: insert “a” before “27 year old seagull colony”
→ Done

Abstract, Line 25: replace semi-colon with a comma

→ Done

Section 2.4, line 5: “trough” should be “through”

→ Done

Section 2.4, line 15: change “dictotyledons” to “eudicotyledons”

→ Done

Section 2.4, line 18: define “DM”

→ Done

Section 2.5, last paragraph: saying data was expressed per unit area is clearer than saying that “sample area was taken into account”.

→ Done

Section 2.5, line 24: “spearman” should be capitalized.

→ Done

Reviewer 3

Comment 1

Although authors had hypothesized that N accumulation rate inside the seabird colony as 30 kg N ha-1 based on the previous study (Magnússon et al. 2009), the readers would be appreciate if concise description about how Magnússon et al. (2009) derived this value were presented in this paper.

→ Done

Comment 2

In the section 4.1, N2 fixation was discussed as not important source of N because activity of free-living N2 fixing microbes would be very low due to low soil moisture and temperature. Then, are there no N2-fixing organisms other than symbiotic and free living bacteria such as lichen or biological soil crusts (BSC) in this study area? Especially BSC are well known to be an important driver for ecosystem N and C cycling especially in the dry ecosystems including arctic deserts.

→ The possibility of N input via N_2 fixation by lichens or BSC's was added to the text. However, Kristinsson and Heidmarsson (2009) showed that such species have a very limited distribution on the island. Therefore, their N input to the ecosystem is assumed to be negligible for the island as a whole.

Comment 3

I think that a shift in R/S ratio includes different two types of phenomena. The first is morphological changes in same plant species in respond to environmental change such as temperature, water availability, and nutrient status in soil. The second is changes in plant species. For example, if dominant species changed from the species with high R/S ratio to the species with relatively low R/S ratio, the plant-community level R/S ratio would also decrease. I feel that observed R/S ratio in this study would be largely explained by the latter reason because there was difference in plant community structure between inside and outside of the seabird colony as shown in section 2.1.

→ This is a good comment and we of course agree that changes in plant-community can cause changes in R/S ratio. However, the lack of significant differences in R/S ratio between the "areas" (mainly due to the high variability caused to relatively small measurement frames used for determine aboveground biomass in the patchy vegetation communities outside the seagull colony) did not allow us to discuss this in more detail.

References

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