

## ***Interactive comment on “Effects of seabird nitrogen input on biomass and carbon accumulation after 50 years of primary succession on a young volcanic island, Surtsey” by N. I. W. Leblans et al.***

**Anonymous Referee #2**

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### General Comments:

This manuscript takes advantage of the fact that the entire history of Surtsey is known to estimate accumulation of carbon and nitrogen stocks and the potential for a seagull colony to affect this. It suffers from the usual problems of observational studies with no replication (in the absence of multiple colonies or islands it is difficult to know how much of the effects are driven by local topography, priority effects, etc.) but the situation is unique enough that there is still valuable information to be gained, especially in terms of sources of N and C loss from the system in the presence or absence of seabirds.

C2350

However, the authors simply measure stocks of N and C in various components of the system and do not measure any of the inputs or outputs. As a result, the main finding is that when a lot of N and P is added to a nutrient poor system, you get more total N, plant growth and soil development – hardly new information. In addition there were a fairly large number of issues that reduced the value of paper substantially (see below), many of them related to the fact that actual impacts of seabirds, which can include effects on vegetation such as trampling, soil compaction, and biomass removal, were not included.

### Specific Comments:

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 result 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?

2) 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. b. Hypothesis 2 essentially says “N accumulation is what other researchers estimated

C2351

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. 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. 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").

Methods: 3) I find the description of the analyses a bit difficult to follow. 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. 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.

Results: 4) 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.

5) 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.

C2352

6) 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??

7) 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.

Discussion:

8) 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.

9) 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.

10) I can't follow the argument in Section . 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!

C2353

11) 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 it in their nests, and also trample it. Was there any indication of this?

12) 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?

Technical corrections: Abstract, line 8: insert "a" before "27 year old seagull colony"  
Abstract, Line 25: replace semi-colon with a comma  
Section 2.4, line 5: "trough" should be "through"  
Section 2.4, line 15: change "dictotyledons" to "eudicotyledons"  
Section 2.4, line 18: define "DM"  
Section 2.5, last paragraph: saying data was expressed per unit area is clearer than saying that "sample area was taken into account".  
Section 2.5, line 24: "spearman" should be capitalized.

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Interactive comment on Biogeosciences Discuss., 11, 6269, 2014.