Comment 1:

the study is based on the assumption that there are only N inputs introduced by the guano of seabirds (authors stated that other nutrients are not so important as the grasslands are N-limited). However, guano also contains organic and inorganic C and so the study design is biased as there is a high additional C input at seabird-sites (up to 30% of guano is organic matter!). The only way to save this study would be to measure the C content of the guano and subtract it from the SOC stocks, but it is questionable if a single value for guano-C can be used for correction of total SOC stocks of the whole soil profile (probably you could estimate the guano-C-input per year, but if this can be used to correct SOC accumulation of the last 1600 years in mature soils is questionable).

Thank you for this comment. In the submitted manuscript, we did not mention seabird C inputs as their contribution was found to be minor in comparison to the C inputs from biomass:
The annual C inputs from guano, calculated from the guano C/N ratio and the seabird-derived N input rates (47 and 67 kg ha^{-1} yr^{-1} in E_{NH} and M_{NH} respectively), amounted to 0.15 ton ha^{-1} y^{-1} on E_{NH} and 0.26 ton ha^{-1} y^{-1} on M_{NH} respectively. The annual C inputs from biomass (assuming an aboveground- and root-turnover of 1 year), amounted, however, to 12.3 and 14.4 ton ha^{-1} y^{-1} on E_{NH} (inside the seabird colony on Surtsey) and on M_{NH}, respectively. The biomass C inputs have been stable over many years, especially in M_{NH}, which is in mature successional state, but also in E_{NH}, as the succession from barrens to grassland was completed in only a few years after the initiation of seabird colonization (Magnússon et al., 2014).

In conclusion, the guano C inputs were estimated to be 1.2 and 1.8 % of the total C inputs in E_{NH} and M_{NH} respectively, which is a conservative estimate, as especially the root turnover rate is expected to be higher than 1 year. Moreover, guano C inputs are more easily decomposable than biomass C inputs. Therefore, the proportion of guano C that remains in the soil after decomposition processes will be even less compared to plant-derived C.

We do agree that it is important to better illustrate the relative importance of guano- and biomass derived C inputs in the paper, as C sequestration is central to the study. We will therefore add this information to the 'Results' section, and will discuss it briefly in the 'Discussion' section.

Comment 2:

No information on soil texture is given. Soil texture largely controls the stabilization of SOC via mineral sorption. For grassland soils Hassink (1997, Plant and Soil 191, 77-87) found are strong worldwide relationship of the maximum C storage capacity of soils with the fine mineral fraction content (medium+fine silt and clay <20 _m). Therefore, a precondition would be that the study sites are comparable in terms of soil texture. If there are differences in terms of soil texture, differences of SOC stocks could also (partly) be attributed to that.

Thank you for this valuable comment.
Indeed, soil texture affects many soil characteristics, amongst others carbon storage capacity. We, however, experienced during our sampling effort that M_{NL} and M_{NH} had comparable soil textures (based on visual and manual determination). Also according to the soil classification by Arnalds (2015), both are of Brown Andosol soil type. Also E_{NL} and E_{NH} were experienced to have similar soil textures, at least below the main root zone. In the upper layers, E_{NH} has developed an O horizon and a premature A horizon, while E_{NL}

has not developed distinct soil horizons yet. In conclusion, within successional stage (early successional vs. mature), the soils were found to have similar soil texture. The difference in the upper layers between E_{NL} and E_{NH} is part of the treatment effect (different N input rates). We did, however, not do a full soil texture analysis using standard methods. As differences in soil texture could cause differences in C storage capacity, we will perform additional soil texture measurements during the coming 3 weeks to verify our observation that the soil texture is similar within each successional stage. We will compose a supplementary table on soil texture and dedicate a paragraph in the 'Result' section to this issue. Further, we acknowledge that soil texture differs broadly between northern grasslands (the current focus of our manuscript). Therefore, we will narrow the terminology of the manuscript to 'a subarctic grassland'.

Comment 3:
Besides, the authors stated that "subplots Mnl and Mnh were protected against possible human and livestock influence prior to the measurements by enclosure cages". However, if there is livestock on Heimaey, the study sites cannot be viewed as unmanaged grasslands as on Surtsey and therefore not directly compared. Livestock grazing would not only be associated with additional manure (C and N) inputs but also with potentially enhanced decomposition of soil organic matter depending on the grazing intensity (due to animal trampling, aggregate disruption etc.).

The additional C and N inputs from livestock into the ecosystem were minimal, as the sheep are allowed to graze the year round and did not receive additional feeding. Further, there were no indications that the grazing and manure dropping activities were separated in space, so no major redistributions of C and N were assumed.
We acknowledge that grazing can have influenced C turnover rate at the mature sites. However, the grazing pressure was similar at M_{NL} and M_{NH}. Further, grazing was homogeneously distributed within the M_{NL} and M_{NH} sites, as the fertility of the sites was homogeneous (nutrient poor and nutrient rich, respectively) and therefore no preferential grazing at fertile spots took place. Consequently, we don't believe that livestock grazing did compromise comparison between these sites. Further, a large proportion of the northern grassland area is subjected to grazing. The grazing activity at the mature sites could thus increase the relevance of our findings.
Finally also the early successional sites could be called 'grazed', as graylag geese have colonized the island, feeding upon the grasslands there (Magnússon et al., 2014). In any case, the early successional site is of a very different age class as the mature sites, 26 years since grassland initiation vs ca. 1600 years, and the age/successional stage effect is expected to overrule the effect of differences in grazing pressure.

We will better indicate the similarity of the grazing pressure in M_{NL} and M_{NH} in the 'Material and Methods' section, to clarify their comparability in that respect.

Comment 4:
In terms of plant analysis, characteristics of Cerastium fontanum were investigated, as it was the only species that was present in all plots. However, it is questionable if the differences in terms of aboveground biomass etc. can solely be related to different N regimes as there were different plant communities/successional stages at the study sites. Probably, the performance of Cerastium fontanum was confounded by other more dominant species?

The authors have the impression that this concern is caused by a misunderstanding. We apologize for the unclarity in the manuscript.

What is shown in Figure 3, is the 'leaf N/P stoichiometry of *Cerastium fontanum'*, the 'plant height of *Cerastium fontanum*' and 'the <u>total vascular</u> aboveground biomass'. We acknowledge that the first two parameters could be confounded by interactions with other species, but we focused on this species to avoid that differences among sites were confounded by species identity. The last parameter (total biomass), however, includes all species and is not confounded by species interactions. While the separate trends (especially of N/P ratio and plant height) might not be sufficient to show N limitation, we think that the combination of these three parameters gives a strong indication that the systems are indeed N limited.

We will better clarify in the manuscript that Figure 3.C shows <u>total</u> biomass. Further, we will rephrase the paragraph about N limitation in the discussion to clarify that it is the <u>combination</u> of the three parameters that indicates N limitation.

Comment 5:
Regarding the soil analyses and calculation of C and N stocks, it seems that the bulk density of the soils (which is necessary to calculate C and N stocks) was determined correctly, even when the authors did not use this term, but the values should be given (in g cm-3).

Thank you for this comment, we will add this term to the manuscript and will express the values in g cm^{-3}.

Comment 6:
In terms of the calculation of SOC storage rates, the millennial rates were calculated for consecutive cumulative soil ages with 200 years intervals assuming a constant soil accumulation rate in the subsoil. This assumption is too speculative: on the one hand, this is rarely the case over longer periods of time, on the other hand, translocation of C from top- to subsoils in form of DOC may be a relevant process in this environment given high precipitation. Therefore, a calculation of SOC storage rates over the 1600 years is highly speculative and thus also the conclusions regarding the long-lasting positive effect of N inputs on C sequestration. In view of the discussion, particularly in sections 4.2.2 and 4.3, it would certainly be a benefit to include literature on the C storage capacity/C saturation of soils that could also be calculated for these soils (see e.g. Hassink 1997, Wiesmeier et al. 2014, Global Change Biology 20, 653-665).

Thank you for this suggestion for improvement.
The application of the technique proposed by Weisemeier et al. ($C_{sat-pot} = 4.09 + 0.37 *$ particles ≤ 20 μm (%)) to our dataset is, however, not straightforward. The conventional techniques for soil size fractionation are not applicable to Andosols for the following reason: Allophanes and ferrihydrates, two clay minerals that are formed during the weathering of basalt, strongly bind organic material. This results in very stable silt sized aggregates that the conventional techniques for soil size fractionation are not able to break down. This leads to an over-estimation of the silt grain size and a large underestimation of the clay grainsize, including the partiles <20 μm. However, we will perform particle size measurements and calculate the C storage potential to our best potential. We will add the suggested references to the manuscript and dedicate a paragraph in the discussion on the calculated C storage potential.

The soils under investigation are young soils (<1600 years) and no C saturation was expected in the upper soil layers. However, in the deeper soil layers, both M_{NL} and M_{NH} showed C saturation in soils older than ~1000 years (Figure 5.B). It will be interesting to see whether this saturation point agrees well with the C saturation value calculated with

the technique of Weisemeier. We will allocate more attention to the C saturation and C storage potential the discussion.

We acknowledge that it is a simplification to assume a constant soil thickening rate between the 1973 ash layer to the 395 AD ash layer, which both gave a known age point within each soil profile. However, we think that, even if this assumption is rather imprecise, the ca. 200-year resolution in our results is still a justifiable approximation based on more detailed dating of soil profiles in S-Iceland that are not located close to active soil erosion areas (cf. Gisladottir et al., 2010). The temporal scale that could be reached in this 'natural gradient study', is not possible to acquire with controlled experiments. We therefore think that this assumption can be made, when it is transparently explained in the manuscript.

We will, however, dedicate more attention in the 'Discussion' section to the uncertainty of the assumption of a constant accumulation rate.

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Final comment:

We thank you for the constructive comments and suggestions, which will greatly improve the manuscript. The authors will make a major revision of the manuscript if accepted, according to the Reviewers' comments; including e.g. a new soil texture analysis on existing samples that will be ready in ca. 2-3 weeks' time. The final revision of the manuscript text is therefore pending.

References

Arnalds, Ó. 2015. *The soils of Iceland, First ed.,* Dordrecht, The Netherlands, Springer.

Magnússon, B., Magnússon, S. H., Ólafsson, E. & Sigurdsson, B. D. 2014. Plant colonization, succession and ecosystem development on Surtsey with reference to neighbouring islands. *Biogeosciences,* 11**,** 5521-5537.