Authors answers to the reviewers questions – Anonymous Referee #2

Comment 1:

The main emphasis of this study should simply be on the potential effects of bird droppings (guano) on net changes in soil C storage across different Iceland grasslands. The current emphasis on the "necessity for a better understanding of the N-induced stimulation of long-term C storage in northern ecosystems" is honestly outside the limits and possibilities of this study. The authors have not addressed the potential C sink ability of northern ecosystems under increasing anthropogenic N emissions but the long-term effects of guano deposition on net changes in SOC. The same authors state this in the discussion: "the N status was clearly more closely related to the annual seabird-derived N input than to ecosystem maturation", thus I would change the 'global change' emphasis from anthropogenic N inputs and ecosystem C sinks to how long term organic N inputs might influence changes in soil C accumulation rates. The introduction should set the stage of how long-term organic C and N inputs were found to be influencing C accumulation rates from previous literature studies. I find strange that 'organic N inputs' or "N inputs from guano' are expressions never used in the manuscript.

We thank the referee for this comment.
We agree that the present focus of the manuscript is ambitious on the issue of mineral vs. organic N inputs. However, the need for long-term N input studies on ecosystem C storage is pressing. This gap cannot be achieved with controlled mineral-N addition experiments. Natural gradients in N inputs, irrespective of N-form, such as seabird nesting areas, are one way to get a better understanding of the effects of long-term increased N inputs. Even though the design of our study is not perfect, we think that the results are still valuable in this respect.

However, we acknowledge that the assumption that our results are applicable for all 'northern grasslands' (which the current title suggest) is too ambitious. Therefore we narrow the focus of the manuscript to 'a subarctic grassland'. We will also include more discussion on the generalizability of our results regarding effects of anthropogenic mineral N inputs.

Comment 2:

If the authors aim to compare total ecosystem C and N stocks they need to clearly set a soil depth range, 0-20 or 0-30 cm for example, where total ecosystem stocks are compared across the different sites. Currently there are too many differences in ecosystem age, soil depths, and successional development stages to be able to compare main C and N stocks in a meaningful way. I think authors have to clarify what 'topsoil' means in terms of soil depth range (could this be 0-30 cm for all sites?).

At the mature study sites, the authors separated between topsoil and subsoil using the 1973 ash layer in each soil profile. This ash layer was located at 6.4 ± 0.4 (SE) and 11.4 ± 1.7 (SE) cm soil depth at M_{NL} and M_{NH}, respectively. The soil on top of this layer (which had been accumulating over the last 40 years) contained over 70% of the roots and was the layer with the highest biological activity. At the early developmental study sites on Surtsey, the age of the whole soil profile was comparable to the topsoil of the older islands (45 years since the eruption ended in 1967). The authors therefore made no subdivision between topsoil and subsoil there.

We will rephrase the 'Material and Method' section about the topsoil-subsoil separation to avoid misunderstandings.

Comment 3:

In terms of mechanisms, a key missing factor here, which could be mainly responsible for changes in soil C accumulation rates across sites is actually the rate of C addition to soils through guano deposition. Bird droppings return both C and N to the soil with significant consequences for the formation of SOM. If the authors do not have information on rates of C additions per hectare per year, they might be missing a critical factor, which could explain as much variability as N inputs in long-term changes of soil SOC.

Thank you for this comment. We already addressed this issue in our answers to Referee 1. We do agree that it is important to show the relative importance of guano- and plant derived C inputs in the paper (guano annual C-input being ca. 1.5% of the annual plant C inputs). We will add this information to the 'Results' section, and will discuss it briefly in the 'Discussion' section.

Comment 4:
There is some confusion in relation to the species composition of the plant communities studied here. For example, on Page 6, lines 1-2: The authors state that Cerastium fontanum was the only plant species found in all experimental plots but on page 4, lines 29-30 they also state that "The MNL site hosts a species-rich grassland community, typical for low nutrient conditions (Magnússon et al., 2014)", which is contradictory and create confusion later when interpreting the results.

The authors have the impression that this concerns is caused by a misunderstanding. We apologize for the unclarity in the manuscript. With the expression "*Cerastium fontanum* was the only plant species found in all experimental plots", we did not mean that *Cerastium fontanum* was the only species that occurred in all experimental plots, but that this was the only species in common between all the plots. It is true that M_{NL} was species rich, but E_{NL} and M_{NH} were species poor. The only overlapping species (over all treatments: E_{NL}, E_{NH}, M_{NL} and M_{NH}) was *Cerastium fontanum*.

We will rephrase this sentence to "*Cerastium fontanum* was the only species that that was common between all the experimental plots" to avoid misunderstanding.

Comment 5:
Page 5, lines 12-15: I don't understand why sampling was done 'outside' the main long-term experimental plots (10x10 m) in "Adjacent to each permanent plot, three 0.2x0.5 m subplots were placed for destructive measurements". What is then the meaning of the permanent plots to this particular study?

The 'permanent survey plots' in this study were important because an extensive amount of other data has been published for these plots. They were established between 1990 and 1995 and have been followed closely ever since. To keep these 'permanent plots' undisturbed for future research, destructive soil- and vegetation sampling (as was necessary for our research) is always done outside the confines of the main plot. Placing our sampling sites close to these plots provided background information on vegetation development, seabird nesting density, soil parameters, gas exchange and many other variables.

To make the value of the 'permanent plots' more clear, we will dedicate a small paragraph in the 'Material and Methods' section to their long history. Further, we will change the terminology from 'permanent plots' to 'permanent survey plots'.

Comment 6:
Page 12, Lines 21-23: The study does not show in any way that " the decadal net SOC storage rate of mature Icelandic grasslands was greatly stimulated by chronically elevated N inputs, which supported the theory that the increasing northern terrestrial C sink during the past decades could be (partly) caused by increasing anthropogenic N inputs". The SOC storage effect is likely due by long-term deposition of bird droppings.

We are sorry that the reasoning behind this conclusion was not clear. As discussed earlier, the droppings themselves are only adding <2% of the annual C-inputs to the ecosystem. We made this conclusion because of the higher SOC accumulation rate in the topsoil (above the 1973 ash layer) of M_{NH} compared to M_{NL}: During the past 40 years (1973 – 2013), almost twice as much SOC had been stored in M_{NH} compared to M_{NL}. We will reword this reasoning in the 'Discussion' section, so that it is more clear why this conclusion is made:

Previous wording:
The strong positive effect of chronically elevated N inputs on decadal net SOC storage in our study is in line with the theory that the recent northern C sink is at least partly caused by increasing N deposition (Hudson et al., 1994; Lloyd, 1999; Schlesinger, 2009).

New wording:
The strong positive effect of chronically elevated N inputs on decadal net SOC storage (i.e. SOC storage in the soil layer that was accumulated after 1973), is in line with the theory that the recent northern C sink is at least partly caused by increasing N deposition (Hudson et al., 1994; Lloyd, 1999; Schlesinger, 2009).

Comment 7:
Remove Fig. 6 because the positive relationship between soil C and soil N (either net changes or content %) is already well known and does not add any new insight into the main findings of this study.

Thank you, this is a valid point.

We will move the figure to the supplementary and only discuss it briefly in the 'Discussion' section.

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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. This includes e.g. a new soil texture analysis requested by Reviewer 1 that will be ready in ca. 2-3 weeks' time; which is why the final revision is now pending.