SOIL Open Access Discussions EGU

Interactive comment on "Evaluating the carbon sequestration potential of volcanic soils in South Iceland after birch afforestation" *by* Matthias Hunziker et al.

Matthias Hunziker et al.

matthew_hunziker@gmx.ch

Dear Lorenzo Menichetti

we want to thank you for your valuable reviews. Some of your remarks concerning the statistics and the topic of the essential status t0 for chronosequence studies were already key points which we also discussed during the data analysis process and the writing of this manuscript. Due to the small sub-datasets per age and depth class and the not-normal distribution of these sub-datasets, we decided to apply the Wilcoxon rank-sum test (WRS), which is applied if the statistical requirements for T-test are not given.

At this point I also apologize that we have not earlier answered to your reviews. The reason is that this manuscript records one part of my PhD studies and I, as main author of the manuscript, have not been employed at any research institute for more than 1.5 years. Since that time, I work at an enterprise in the private industry. This is not an excuse to ignore your review comments, but the time and software recourses are very limited or no longer available. Nevertheless, we modified our manuscript according to your comments as thoroughly as possible, constructed replies to your comments and submit the revised manuscript and the answer of the authors hereby.

Kind regards Matthias Hunziker, main author.

Author's answers to the comments of the two referees Referee 1; Lorenzo Menichetti

General comment The study is conducted in one of the most fascinating setups ever (and this means something to me, I have to admit my bias here. I am pretty interested in these data also because of a fascination for Iceland on one side and for Andosols on another side). It also contains some pretty interesting data, and their impact can be substantial. There is a lot of work behind the data, and there are a lot of points and really long-term treatments (also extremely interesting treatments by the way! I would personally want to know more about the Barren Land treatment, but the experimental design seems in general fantastic, with really long treatments although with the problem of underlying soil variability). There seems to be a lot of valuable information here. The manuscript nevertheless falls shortly in making the most out of such data. It lacks statistical tests, and hypotheses are not discussed in a testing framework. You could and should work more on it, in my opinion. You should restructure a bit your hypotheses and conclusions. You mention in the text a bit too many times the same things, that SOC below 10 cm was surprising, it might be due to soil characteristic and it was due (you suppose) to previous accumulation and fossil soil layers. It all makes sense to me, and I understand this was unexpected for you, but try to make some logical blocks for it. After you cleared the hypothesis testing framework, then you can work on the statistics for comparison, and clarify each comparison. Compare something to something else,

always. If something increases, always states compared to what it increases. And put there the results from a statistical test for the comparison. You do not need to use necessarily fancy tests, just the basics t-test, ANOVA, linear regressions, combined in different ways, could be enough as tools. But use them extensively and ideally do not state anything without being able to offer some statistical evidence (there are of course exceptions, but in your case they look more like the rule). As a suggestion for future developments, I think you should think to some more detailed modelling, at least with some compartmental model with analytically solved steady state, and try to model the input functions (I attached a reference that might be useful as a rough start). If you get right the function of input variation, a SOC model can be calibrated on your data and give you the change of the steady state. . . and predict the future steady state, meaning the C you could accumulate in these soils. But this is clearly outside the scope of your present study, I just got excited about the idea. . . and to me such idea is exemplifying why I do believe you have a lot of useful information here.

Specific comments: Comment 1 Abstract: The idea of C "sequestered as labile" sounds a bit counterintuitive in itself to me... I'm not sure I would consider labile C as "sequestered", since it stays for very short time anyway. But this is philosophy after all, not a major comment.

Author's answer: We changed the sentence to:

Revised version: Therefore and due to absence of any increase in the tested mineral-associated SOC fractions, we assume that the afforestation process evokes a carbon deposition in the labile SOC pools. Consequently, parts of this plant-derived, labile SOC may be partly released to the atmosphere during the process of stabilization with the mineral soil phases in the future.

Comment 2: Line 11, page 3: Define "sequestration rate" better. The inputs in an afforestation follow a function variable over time, and this causes a continuous variation in the rate of change of C stored. I am sure this is what you meant, but "sequestration

rate" was not defined before and it might be ambiguous. Is the "sequestration factor", right? Yes, that would be variable over time following the function of input variation.

Author's answer: We agreed and changed the sentence to:

Revised version: The establishment of a vegetation community passes through different development stages, consequently, the sequestration rate, as a function of SOC change over time, is not linear until the new SOC stock equilibrium is reached (Smith et al., 1997; Six et al., 2002; Stewart et al., 2007).

Comment 3: Line 2, page 6: you do not complete the Zimmermann fractionation! You skip the last step, the oxidation, right? You need to mention this in M&M. This is pretty crucial to understand many of your following statements (I initially missed this detail and I read almost 2/3rd of the manuscript before realizing it).

Author's answer: In the submitted version we already mentioned that we skipped the oxidation method ("Compared to Zimmermann et al., (2007), the present study did not conduct oxidation with sodium hypochlorite (NaOCl) to determine the resistant SOC pool."). But we added in the revised version the following sentence:

Revised version: Hence, the present study did measured the SOC in the NaOCl resistant fraction (rSOC). Comment 4: Line 5-6, page 7: I guess you mean "each soil fraction", giving an example for the fine soil fraction only, right? But the equation was the same for all fractions, right?

Author's answer: Fine soil fraction is not used in the term of any SOC fraction. It is used as a size-dependent soil material fraction. Commonly fine soil material is defined as the soil material which is smaller than 2mm in diameter and is determined by sieving with a 2mm sieve. The equation given in line 8 on page 7 was used to calculate the bulk SOC stocks based on the variables SOC concentration, bulk density of the fine earth material (< 2mm) and the sampled soil depth. The bulk density of the fine earth material (<2mm) was determined by dry sieving and water displacement of the coarse

material (> 2mm).

Revised version: No changes compared to the submitted version.

Comment 5: Line 28, page 7: this is confusing. You sampled also "Barren Land", no? Which had a high ferrhydrite content, as you say before. High compared to what? Do you mean that ferrhydrite was "high", but allophane material was higher than this latter? Or do you mean "all other samples than Barren Land"?

Author's answer: We mean that the highest clay concentrations were found at Barren Land. Further the concentrations at Barren Land are also higher than given in the literature for desert Vitrisols. The passage was changed.

Revised version: This is highlighted by their high clay contents, which comprises a mixture of allophane and ferrihydrite minerals. Compared to the concentration of the ferrihydrite clays, the allophane clay minerals predominated in all samples (Table 2). At Barren Land, we found the highest concentrations of Allophane and Ferrihydrite clay minerals (median value at 10-20 cm: 18.6 %) (Table 2). Further, these high concentrations stand in contrast to the typically low concentration (2-5 %) found in desert Vitrisol (Arnalds, 2015d). Based on these findings, our results indicate that the soils tested as part of the present study are pedogenetically developed.

Comment 6: Line 1, page 8: what do you mean with "nutrient contents"? Other nutrients than C? Please specify which nutrients. And in Table 1 you report only the C:N but not N, it is quite difficult to read this statement in the data (I would need to extract the values and do the calculation from C and C:N ratio). If also N was important, please report it with direct measurements, otherwise talk only about C.

Author's answer: The authors agreed and changed the sentence to:

Revised version: SOC concentrations varied between 0.6 and 9.8% within the whole dataset of the 84 samples (Table 1). Comment 7: Line 13, page 8: maybe you cannot generalize so much. Only the degraded volcanic soil soils you sampled showed that, I

would say you cannot say the same in general.

Author's answer: The authors agreed and rewrote the sentence as:

Revised version: Based on the analysis of the C concentrations of the soil samples, the study showed that the un-vegetated, severely degraded volcanic soils contained appreciable amounts of SOC. And further, afforestation with mountain birch increased the soil C concentration during the first 50 years of shrub establishment, predominantly in the top 10 cm.

Comment 8: line 26, page 8: "usually tested" is generic term that has pretty much no meaning. By whom? Maybe my usual tests are not the same, maybe I am personally used to something else?

Author's answer: The title was changed to

Revised version: Afforestation seems to increase the SOC stock in the top 30 cm

Comment 9: Line 3-13, page 9: all this discussion has the defect of not considering the starting point. Soil C stocks are an equilibrium defined over several decades/centuries by the inputs. A land use change represents a change from a possible equilibrium state to something else, in theory a new equilibrium after climax. If this means a loss or a gain of SOC depends on the new inputs (so the age of the plantation in this case), but also on what was there before. In all these comparisons you should indicate at least the SOC stock before and after in absolute terms (I would say that heathlands are much richer than your "Barren Land", no?).

Author's answer: The paragraph which was mentioned by the referee contains a review of already published results by different authors. We included the demanded information taken from the literature where it was available. We added the age of the tested sites as far it was given in the cited literature.

Revised version: The present study found a continuous increase in median SOC stock (0-30 cm) with birch stand age (Birch15: 31; Birch20: 33; Birch25: 36; Birch50: 46 t

C ha-1) (Figure 2). In the present study, the initial state before afforestation starts is represented by the sites of Barren Land. The median SOC stock (0-30 cm) was 39 t C ha-1, which is higher than the SOC stocks at Birch15, Birch20 and Birch25. The given results of the SOC stocks (0-30 cm) might lead to the assumption that the soil acts as C source during the first 25 years of the establishment of birch. This would be in accordance with Hunziker et al., (2017) who found a decline of the SOC stock (0-30 cm) during the first 40 years of green alder encroachment on former subalpine pastures. Another finding of the present study is that after 50 years of birch growth, the SOC stock was still considerably lower than that of the old-growth woodlands of Birchnat (Δ 15 t ha-1) (Figure 2). This means that the soils at Birch50 can sequester additional organic carbon during the succession towards mature woodlands which reflects the equilibrium state. During the period between Birch15 and Birch50 (35 years), the sequestration rate is 0.42 t C ha-1 a-1 on average, without taking the SOC stock of Barren Land (as status before afforestation begins) as reference for calculation. The rate is lower than the given removal factor of 0.51 t C ha-1 a-1 for afforestation activities (Hellsing et al., 2016). Overall, The results indicate that afforestation by mountain birch, and the establishment of birch woodlands, can increase the bulk SOC stock (0-30 cm). (Birch15-Birch50), which is in accordance with Icelandic studies given in the literature. . A literature review revealed that the succession of already vegetated heathland to birch woodland in eastern Iceland shows no change in C stocks (Ritter 2007). The SOC stocks were about 40 t C ha-1 (0-20 cm) for 26 and 97 year old birch stands. . Snorrason et al., (2002) found a higher SOC stock (0-30 cm) in a 54 year old birch stand (65 t C ha-1), compared to that of grassland (54 t C ha-1) at Gunnarsholt, which leads to the assumption that the effect of afforestation is more effective than that of revegetation concerning SOC sequestration. However, Snorrason et al., (2002) and Ritter (2007) did not reported the SOC stock of the initial status before ecosystem change began. Soil development and natural vegetation succession on moraine till after glacial retreat is another typical process of land cover change in Icleand. Vilmundardóttir et al., (2015) found a SOC accumulation within the top 20 cm from 0.9 (initial status) and 13.5 t

C ha-1 at sites with a maximum age of 120 years, thereby demonstrating that the process of vegetation succession on moraine till leads to an increase in soil carbon stock. Our results indicate that the change in SOC stocks during afforestation with mountain birch on severely degraded soils (Figure 2) is comparable with those given for shrub encroachment in the cited literature.

Comment 10: Line 23-25, page 9: this is pretty well known pattern after afforestation. You can refer to fig. 1 in Goulden et al., 2011 (references at the end) (panel a), which by the way could be used as a function of production (and inputs, panlel c) for an interesting SOC modelling study of your data. Anyway, you could discuss these patterns.

Author's answer: In our opinion, the main reasons for the SOC stock patterns (decrease and after increase) is not mainly due to the change of the ecosystem production and the delivery of organic material to the soil. The main reason is that Barren Land which was assumed to be t0, can be badly taken as t0 status. Therefore, we decided to apply a depth-dependent SOC fractionation and also a physical SOC fractionation to characterize the SOC patterns during the establishment of mountain birch woodlands on severely degraded volcanic soils.

Revised version: The SOC stocks (0-30 cm) of the land cover categories indicate that the SOC pool decreases after the establishment of birch shrubs on barren land. It then increases during tree establishment to reach the level of naturally grown birch woodlands (Figure 2). However, the analysis of SOC dynamics in such a temporally dynamic landscape, which results in unequal SOC and volcanic clay concentrations patterns across land cover categories, calls for more detailed and alternative methods (Table 1, Table 2). The present study focused on the vertical distribution of the SOC and its quality, to verify whether afforestation results in the soil becoming a C source, and whether more C is sequestered during revegetation than afforestation.

Comment 11: Line 7, page 10: could you explain what is a "eratica"? If it is one identifiable organism put the taxonomic name, otherwise explain, I'm really not familiar

with the term. If it is latin, plural of "erraticus", mind you it is with double "r" and it could still be a bit obscure to many since at leats in latin it generically means something like "things that go around. . ." and not necessarily living things. If it's a discipline-specific context you might need to clarify. Author's answer: In this term it means the material of volcanic eruptions.

Revised version: However, this is a typical pattern of volcanic soils which are also characterized by biologicaly active soil layers buried by ash from volcanic eruptions.

Comment 12: Line 20, 23, page 10: it is nothing too weird that you still have some C left. You can efer to the study by Barré et al., 2010, and following studies on the LTBF network for having a picture of SOC evolution in barren conditions. It takes several decades for the soil to lose the C, and several millennia to lose all of it (you can also accept the approximation of "stable" C pool of Barré, if you like, it is virtually correct at your time scales). It is nevertheless pretty interesting to me that the degradation is so faster in the upper topsoil than the lower topsoil. . . the LTBF are cultivated in the 0-20, so this stratification is not observable. You might have there also some really interesting hints about the protection of SOC exerted by depth, maybe.

Author's answer: It is right that it is quite important to recognize that it can take a long time for soils of collapsed barren ecosystems to emit all the carbon from the soils to the atmosphere, and we stress this point in the paper. We include this citation for further stressing the point.

Revised version: This implies that the soils of Barren Land contain a certain amount of SOC due to earlier soil formation processes prior to disturbance and SOC accumulation, and which occurred before the soil profile was truncated by soil erosion processes. Such pedologic conditions can ask for soil C decay studies, as it was introduced by Barré et al., (2010). However, this was not the objective of the present study.

Comment 13: Line 30, page 10: definitely agree! But "bulk SOC stocks" is not necessarily 0-30. . . you could just use bulk SOC stocks in 0-5 cm, no? It seems you mean

C9

that bulk stocks in general are not to be used, like this.

Author's answer: We used the term "bulk SOC stocks" for the unfractionated SOC stocks as given in Figure 2. However, we deleted "bulk" throughout the manuscript or changed it to "unfractionated".

Revised version: The subdivision of the studied soil columns of 30 cm in four sampling intervals explains the higher SOC stocks at Barren Land and Grass50. This is due to the higher values in the intervals "10-20 cm" and "20-30 cm" compared to the afforested birch sites (Birch15-Birch50), which constitute older buried soils (Table 1, Figure 2). The subdivision further characterizes the patterns found of SOC stock (0-30 cm) (chapter 3.2), with the high SOC stocks (0-30 cm) at Barren Land and Grass50 being caused by the carbon pool located deeper than 10 cm soil depth. Under the given site conditions, it is questionable to apply the commonly used soil depth of 30 cm for SOC stock monitoring (Aalde et al., 2006), to sampled SOC that originates from buried soils, as it distorts the effects of restoration activities in the results of SOC concentration and SOC stock. Based on this understanding, the SOC stocks (0-30 cm) do not reveal that afforestation caused a C loss during the first 25 years of mountain birch establishment at such severely degraded sites, and that the effects of revegetation is more effective than those of afforestation by mountain birch within the first 50 years.

Comment 14: Line 3, page 11: probably you mean the effects of the afforestation, rather than the afforestation itself

Author's answer: The sentence was changed to:

Revised version: Based on this understanding, the SOC stocks (0-30 cm) do not reveal that afforestation caused a C loss during the first 25 years of mountain birch establishment at such severely degraded sites, and that the effects of revegetation is more effective than those of afforestation by mountain birch within the first 50 years.

Comment 15: Line 6, page 11: in this case I would rather use a relative value for the

delta, it's more immediate

Author's answer: We added the relative value.

Revised version: However, the SOC stock (0-10 cm) of 50-year old birch woodlands is still lower (Δ 5 t C ha-1; 16 %) than the stocks identified at the Birchnat sites.

Comment 16: Line 11, page 11: "false" is not the right term here. I mean it doesn't sound right in English. A statement can be false, using something cannot, no matter how badly you're using it that's not false. It can be misleading, for example, or other similar terms.

Author's answer: We changed to word to "misleading"

Revised version: Hence, it is misleading to use the selected Barren Land site as initial status (t0) for discussing the effect of afforestation and calculating any SOC sequestration rates.

Comment 17: Line 16-17, page 11: really do you need 5 studies to say that you have higher C inputs if you have some plants compared to no plants? Just asking. . .maybe not, to me it sounds pretty obvious, although correct.

Author's answer: We changed the references.

Revised version: The net primary production (NPP) of a landscape is increased during afforestation. Hence, the supply of organic material to the soil is higher at shrubby sites compared to barren areas (e.g. Bjarnadottir et al., 2007).

Comment 18: Line 20-23, page 11: why do you use a median? If the distribution I skewed, as I bet it is, do the comparisons one by one. . .and use statistical tests! I mean, assess the significance of you comparisons, comparing the mean possibly. Than maybe yu can use also some more exotic things like medians, if you really like to, but for sure use p values in your comparisons. Ah, then you compare Birchnat to Birch50, without stating any number. . .

Author's answer: The sub-datasets (per age class and depth interval) consists of 3 replicates. We looked for a significance test for subsets with not-normal distributed data and only three samples. We applied the Wilcoxon rank-sum test. With this test, the lowest p-value we got was 0.05 hence the p-value was in any of the tested cases not lower than 0.05. Due to ending of the SPSS license, unfortunately I only have the p-values for the SOC stock results (Figure 2). However, we will include this data in the revised manuscript. Further, we decided to apply descriptive statistical methods to describe the patterns. In our opinion and due to a study setup with only 3 replicates, using the median value instead of the mean value is more reasonable.

As I mentioned in the introduction of the author's, doing additional statistical tests would be way beyond the practicality of such endeavour due to change of workplace. If I would repeat this study or establish another study setup, I would reduce the number of strata and increase the number of samples/replicate per strata to n=5.

Revised version: The revised version of the manuscript will contain labels for significant differences per depth interval in Figure 2 (SOC stocks 0-30cm) and also the p-values in the text concerning the SOC stocks in any case of significance.

The sites at Birchnat contained 90 mg POM g-1 soil. The lower value at Birchnat (90 mg POM g-1 soil) compared to Birch50 (174 mg POM g-1 soil) can be explained by the lower productivity of Birchnat due to the already undergone self-thinning process during the forest development at Birchnat.

Comment 19: Line 25, page 11: maybe hypothesize is better term than assume, here, or "one might hypothesize at first"

Author's answer: We agreed

Revised version: According to these results, it is hypothesized firstly that afforestation is a more effective restoration process than revegetation with grasses, in terms of supplying organic material, and hence carbon to the soil phases and secondly, this supply

increases exponentially during the establishment of afforested birch woodlands.

Comment 20: Line 25-30: the fact that you have more POM from birches but more C stocks from grassland should be related to the C found in the <63nm and HF. The fact that yiou think that such C was already there due to remnants is an explanation for what you find in the paragraph above. You have more stabile SOC here (which is desirable) compared to birch plantation because that SOC was already there due to soil chacteristics, at least this is what you suggest (you should also discuss a bit other possibilities, since you cannot be sure, such as "does grassland put C in stable fractions faster than birch"? Maybe not, but you should discuss this).

Author's answer: In this paragraph, we present the mass of the POM material, which was found after the wet sieving procedure and the density fractionation (> 63 μm and < 1.8 g cm-3). As it is written, we use it as an indicator for the supply of organic material and carbon into the soil.

Revised version: No changes were made.

Comment 21: Line 29, page 12: ok, but wasn't this belonging to the previous paragraph (ah, btw, they are paragraphs, not chapters)?

Author's answer: In our opinion, we don't see any conflict. We therefore did not change the manuscript according to this comment.

Revised version: No changes.

Comment 22: Line 30, page 12: after all the medians you showed, now I fear this median might be grouping different sites. Median between what? (and please remember my former comment about using statistical test, for which a mean might be easier. I know you might have skewed distributions, but it's pretty hard to deal with them. . . I appreciate your effort in this sense, but still you need to deal with statistics, an aggregate number itself has no real meaning without error and statistics)

Author's answer: We changed the sentence in accordance to better understanding. In

our opinion and due to a study setup with only 3 replicates, using the median value instead of the mean value is more reasonable. As I mentioned in the introduction of the author's, doing additional statistical tests would be way beyond the practicality of such endeavour due to change of workplace. For calculating a mean value of values from a dataset, the values and its distribution need to fulfill some requirements. Due to the small subdatasets in our study and distribution of the data we decided to not calculate the mean value and to show the median value. If I would repeat this study or establish another study setup, I would reduce the number of strata and increase the number of samples/replicate per strata to n=5.

Revised version: During afforestation, the increases between the median C stocks of the POM and '< 63 μm' fractions were 8 (+163 %) and 6 (+34 %) t C ha-1 between Birch15 and Birch50, while the SOC stock of the HF fraction seemed to stagnate at about 9 t C ha-1 during the same observation time (Table 3). These increases are explained by the increases of the '< 63 μm'-C and the POM-C concentrations during the afforested time span (Figure 3, Figure 4).

Comment 23: Line 27-28, page 12: ok, but this is a problem of your setup. You did not do the oxidation, the last step of the fractionation, so you do not have information about the stability of the material. If you did, you could relate your results to the stabilization.

Author's answer: It is true that we did not the chemical fractionation by oxidation as it is mentioned in Zimmermann et al. 2007 for all 84 "<63μm" samples. We tested the wet-oxidation with NaOCl on 42 samples (<63μm). However, the need of time and chemicals was out of scale with regard to the output due to the mineralogy of the volcanic soil material. Another reason is that according to Jagadamma et al., 2010 and Lutfalla et al., 2014, it is questionable whether the wet-oxidation by NaOCl is the proper method to quantify the resistant SOC (rSOC). Further, Zimmermann et al., (2007) did not analyze volcanic soils by the NaOCl-oxidation method. Other tests with volcanic soils from Iceland showed that oxidation with H_2O_2 do not oxidize the materials due to e.g. the Mn minerals. Based on these reasons, we, however, decided to skip the

oxidation step.

Revised version: This result can be attributed to a stabilization of the SOC due to its binding with the colloid fraction, which contains clay-sized minerals and organo-mineral complexes. However, the extraction of the material of the '< 63 μm' fraction by the physical separation technique of Zimmermann et al. (2007), and the measurement of its C concentration does not provide an indication for the stability of the SOC in the analyzed fraction due to the separation by only wet-sieving. The study skipped the chemical fractionation by wet-oxidation with NaOCl due to the mineralogy of the samples. Hence, the chosen method in this study does not give information about the location of the organic matter in the '< 63 μm' fraction and consequently, the degree of the SOC stabilization. Since the formation of organo-mineral complexes is pH dependent, the study does not give evidence whether i) the majority of the SOC is bounded to the mineral phase of the '< 63 μm' fraction, or ii) in the '< 63 μm' fraction, where the majority of the SOC is POM material that is disconnected from the mineral phases.

Comment 24: Line 2-4, page 13: as above, the Zimmermann fractionation (Zimmermann et al., 2007) is not only physical, but it includes a chemical oxidation exactly for this reason (ok, it is a rough indication, but still it is an indication of stabilization). You decided to skip this. Fine, but it is your decision, not a flaw in the method. . .

Author's answer: See comment above.

Revised version: No changes were made.

Comment 25: Line 26-28, page 13: these correlations are weak, you need to state also the p-value, I'd say. For a rĔĘ2>0.8 I wouldn't be so strict, but these are rather low.

Author's answer: The correlations were computed in excel and excel does not give any p-values for correlation tests. As I mentioned in the introduction of the author's, doing additional statistical tests would be way beyond the practicality of such endeavour due to change of workplace.

Revised version: No revision on this comment.

Comment 26: Line line 13-14, page 13: with "undetermined" you mean that you did not find any correlation? Try to be clear about these things, this sounds like a euphemism.

Author's answer: We are agree and changed the sentence.

Revised version: The stabilization of the SOC in the form of metal-humus complexes seems to be hampered due to the measured pH-values (Table 1), which were higher than the upper threshold value of 5.0 for the building of metal-humus complexes given in the literature (Figure 5; E, F).

Comment 27: Line 14-20, page 13: since this is a rather important part of your study, could you please analyze it more in detail? You could test some regressions on the different groups you indicate, and give the results (and p-values!), and try to demonstrate your hypothesis with your data. It's an interesting hypothesis, and you should find some correlation. . . instead of writing that "it is undetermined" just try to determine that stabilization, that's your job as scientist after all.

Author's answer: As I mentioned in the introduction of the author's, doing additional statistical tests would be way beyond the practicality of such endeavour due to change of workplace.

Revised version: No revision on this comment.

Comment 28: Line 29, page 13: what do you mean with "continuous"? That value is also not normalized by time, I can't understand that adjective in such context. To me "continuous" could refer here to a rate of inputs that did not change over 15 and over 50 years, but this is a (cumulative, so integrated over time and not a rate) mass. And what that increase the same for all the stands?!? What do you mean 15 t C ha-1 between 15 and 50 years?

Author's answer: The sentence was corrected.

Revised version: Afforestation with mountain birch leads to anincrease of the SOC stock (15 t C ha-1) for birch stands between 15 and 50 years.

Comment 29: Line 14-15, page 13: you wanted to "evaluate the SOC sequestration potential of afforestation on severely degraded soils in southern Iceland." and your key message is a recommendation about caution in choosing the sampling depth for soil surveys?!? I think you should focus a bit more on your main aims, you have some information there. And try to be consistent with such aims, write down your hypotheses, test them (also statistically) and tell me more about how it went. I wouldn't use the last line for a recommendation that just points out some shortcomings of your study, actually.

Author's answer: We improved the conclusion chapter by adding more essential results and place three key messages which are the findings of the study.

Revised version: The study aimed to evaluate the SOC sequestration potential of afforestation on severely degraded soils in southern Iceland. For this, we measured the SOC stocks of differently-aged afforested birch stands and compared them with those of eroded and degraded soils, re-vegetated grasslands and non-degraded woodlands which have escaped the soil erosion, respectively. In addition, the SOC quality of all sites was analyzed by physical soil fractionation. The present study differentiated between the physically separated SOC pools, which allowed for the evaluation of the success of afforestation by mountain birch on a landscape with highly diverse soil patterns and SOC distributions. The results of the present study also clearly show that undertaking research on soil organic carbon patterns on severely degraded soils within this area is challenging, owing to the high SOC stocks (0-30 cm) of these degraded soils. Nevertheless, afforestation with mountain birch leads to anincrease of the SOC stock (+15 t C ha-1; +48 %) for birch stands between 15 and 50 years. Afforested birch stands can still potentially accumulate SOC after 50 years of growth, due to their lower SOC stock (+13 t C ha-1; +28 %) compared to naturally, old growth birch woodlands. During this time, the POM mass (+131 mg g-1 soil; +300 %) and POM-C concentrations (+35

mg g-1 soil; +285 %) increase during the succession of the mountain birch ecosystem. These increases were mainly observed in the top 10 cm of the mineral soil. Further, at least 56 % of the total SOC stock (0-30 cm) was found in the HF- and '< 63 μm' fractions and at all tested sites most of the carbon was stored in the < 63 μm fraction. Even severely degraded soils contain considerable amounts of the SOC stocks. Due to the increased amount of POM-C stock and the doubling of the DOC stock, it, however, seems that afforestation leads to SOC pools which are more vulnerable to release C to the atmosphere. The first key message is that severely degraded, un-vegetated soils can sequester considerable amounts of SOC and there is still a potential of SOC sequestration after 50 years of plant growth. Second, the standardized soil sampling depth of 30 cm needs to be vertically subdivided for evaluating the success of restoration regarding SOC sequestration on severely degraded soils. Third, the interaction of the organic material with the mineral phase of such volcanic soils needs to be studied in more detail. . Regarding the chosen setup approach, the applied space-for-time substitution approach showed limited success by reason of the heterogeneity of the parent material and its SOC properties at greater soil depths, . In such cases, it would be more effective to use permanent plots and a long-term monitoring approaches to assess soil development during vegetation restoration, as initially suggested by Johnson and Miyanishi (2008), carried out by Arnalds et al., (2013) and Thorsson (in prep.), and further developed by Bárcena et al., (2014). Hence, the fourth key message of the study is that the establishment of chronosequence plots on severely degraded soils needs to be applied with caution.

######References###### Goulden ML, Mcmillan AMS, Winston GC, Rocha A V., Manies KL, Harden JW, et al. Patterns of NPP, GPP, respiration, and NEP during boreal forest succession. Glob Chang Biol. 2011;17: 855–871. doi:10.1111/j.1365-2486.2010.02274.x

Barré P, Eglin T, Christensen B, Ciais P, Houot S, Kätterer T, et al. Quantifying and isolating stable soil organic carbon using long-term bare fallow experiments. Biogeo-

sciences.2010;7: 3839–3850. doi:10.5194/bg-7-3839-2010

Zimmermann M, Leifeld J, Schmidt MWI, Smith P, Fuhrer J. Measured soil organic matter fractions can be related to pools in the RothC model. Eur J Soil Sci. 2007;58: 658–667. doi:10.1111/j.1365- 2389.2006.00855.x

Basel, 26 December 2018, M. Hunziker