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

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Dear Robert Qualls

we want to thank you for your valuable review. 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. Further responses on this topic are listed below.

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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.

Referee 2; Robert Qualls

General comment This manuscript describes a very interesting study of the accumulation of carbon, particle density fractions and the clay fraction that would be relevant to adsorption of carbon in volcanic soils. It would be relevant to the literature on soil development during primary succession on volcanic soils, and perhaps to secondary succession on volcanic soils. One thing that is unique is that unlike in many studies of soil development during succession, there is only one species of tree involved, with one "variable" removed (with the exception of the grassland which provides and interesting contrast with deposition of carbon at different depths. As the authors acknowledge, there is unfortunately no "time zero" for the afforestation of the birch since the barren plots seem to have organic matter left from a previous era when it must have been vegetated, as indicated by C contents that are greater, even at depth than the young birch plots. Perhaps some initial state can be inferred by extrapolation to zero time in the birch time sequence. The methods used were very pertinent to a study of soil development on volcanic substrates. The analyses of allophane and Fe and Al oxyhydroxides are just what this reviewer used in comparable studies. The separation of carbon by density fractions are also what Sollins et al. (see reference below) recommended to monitor the deposition of root detritus vs. the adsorbed or occluded carbon that might be expected with allophane and Fe/Al oxyhydroxides interactions.

Author's answer An exponential function based on the time-dependent SOC stocks (0-30 cm) of B15-B50 as input data (computed in Excel) showed an SOC stock as initial status (t0) of 26.25 t C ha-1 (y=26.246e0.0111x, R2=0.44). This is a quite smaller SOC stock value than found at Barren Land (39 t C ha-1). According to these, it seems that at the sites of B15, B20, B25 and B50 the initial SOC stock before any afforestation activities starts is distinct lower than the used initial status of severely degraded land (Barren Land) in the present study.

Revised version The authors suggest to include the reviewer's input by inserting this finding in the section concerning the SOC stock (0-30 cm) similarly.

Comment 1 There are a few things that I might suggest could be made clearer to the readers. In the description of the history of the sites, I was not able to follow which plots actually used for the study were associated with each history. Perhaps it would help to have a table listing each group of plots (barren, planted birch, natural birch, grassland) and relevant elements of history (previous land use, eroded, volcanic desert, volcanic sand deposition, etc.). In many comparable studies of chronosquences, a key question is the degree to which all vegetation/age types originated from the same parent material. Obviously they are all of volcanic origin, but some had different histories and there is no true "initial state" since there appears to be a buried A horizon.

Author's answer: In our opinion, the description is good enough and a table would overload the section which already contains Figure 1 about the location and the setup of the soil sampling. In the revised version, we labeled the different tested categories.

Revised version: However, we can create a short table in the revised version of the manuscript.

Comment 2 Perhaps clarify the discussion as to which sites can be considered subsets of "vegetation/age" classes can be considered as having the same initial states that differ by age or vegetation.

Author's answer: In the discussion, we considered this comment.

Revised version: The results indicate that spatial variability must be taken into account when analyzing SOC of volcanic soils, especially when deeper than 10 cm, between the sampled sites and the land cover categories (i.e. grassland, barren, etc). This is even more relevant in landscapes with past or recent erosion processes as soil forming process. Thus, the equality or comparability of the sites, except for the studied variable, is not ensured for space-for-time substitution sampling approaches under such circumstances as performed in the present study (Walker et al., 2010). Hence, it is misleading to use the selected Barren Land sites, which were selected at 4 km distance from the afforested sites (Birch15, Birch20, Birch25 and Birch50) and 15 km from Birchnat, as initial status (t0) for discussing the effect of afforestation and calculating any SOC sequestration rates.

Comment 3 Study design and replication. The following paragraph makes it difficult to figure out the experimental design and replication: "Each of the land cover types and age categories described above was represented by three test sites, resulting in a total of 21 sampling sites (Figure 1; E). . . ..r. At each site, five soil pits were randomly placed. At the woody sites, sampling occurred within one half of the crown diameter of a dominant mountain birch (Betula pubescens Ehrh. ssp. czerepanovii) tree. The soil was sampled with a cylindric metal core (Eijkelkamp Soil & Water, Giesbeek) of 100 cm3 volume and 5 cm in diameter at given soil intervals (0- 5, 5-10, 10-20 and 20-30 cm). The five subsamples per depth interval were immediately mixed in order to form one composite sample. Thus, each depth interval per category was represented by three composite samples (Figure 1), resulting in a total of 84 composite samples."

It is difficult to figure out the experimental design from paragraph and figure (Figure 1) seems to have some contradictions. There were 5 pits in each site. Part of the problem is the use of the words "land cover types" and "sites". Many authors use "site" to indicate the "treatment" and "plot" as the unit that serves as a replicate. I realized these were not randomly allocated treatments, but the nomenclature is confusing making it

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difficult to tell that there are 3 replicates per “vegetation/age” class. What is “category” in “depth interval per category, is this the same as site? Could site be referred to as “plot”?

Author’s answer: We see the problem which is mentioned by the reviewer. During the writing of the manuscript we intensively thought about the most appropriate terminology. Throughout the manuscript, we keep the terminology constant. It is correct that there were 5 pits per site. "Category" in "depth interval per category" is land cover category in combination with the age of vegetation growth e.g. "Barren Land", "Grass50, "Birch15". And the term "category" is not the same as "site" because we tested three sites per category. In our study setup the term "site" is referred to as "plot" which serves as a replicate according to the reviewer.

Revised version: Each of the land cover types (e.g. Barren Land, afforested birch stands) and age categories (e.g. 15, 20, 50 yrs old birch stand) described above was represented by three test sites (3 replicates) (Figure 1; E).

Thus, each depth interval per category was represented by three composite samples (3 replicates per depth interval) (Figure 1), resulting in a dataset of total of 84 composite samples.

Comment 4 In Figure 1, the map is useful. But, in the maps B, C, and D I do not see asterisks, triangles, etc. as it says in the caption. The list of sites, profiles, and composite samples is only confusing. Perhaps you could list “vegetation/age” classes, “number of plots or sites within each class”, “subsamples composited within each plot”. . . to make the number of true replicates apparent.

Author’s answer: The points of the test sites were categorized as it is mentioned in the caption. However, we keep the list with the numbers of test sites, soil pits, collected samples and composite samples.

Revised version: Figure 1 was changed.

Comment 5 In the discussion, there are a couple of very relevant references that are comparable in terms of (1) the rate of carbon accumulation over time on volcanic soils, (2) the development of allophane and iron and aluminum oxyhydroxides and the role of adsorption of carbon, and (3) the use of density fractionation to examine the role of association of C with volcanic minerals and its refractory nature.

Author's answer: As I mentioned in the introduction of the author's, due to the change of the workplace I have no longer access to the scientific literature and a request for renewing my university account was declined. Nevertheless, Sollins et al. 1983 is cited in the revised version of the manuscript.

These are listed below: Sollins, P., Spycher, G., Topik, C., 1983. Processes of soil organic matter accretion at a mudflow chronosequence, Mt Shasta, California. Ecology 64, 1273– 1282.

Lilienfein*, J., Qualls, R.G, Uselman*, S.M, and Bridgham S.D. 2003. Soil formation and organic matter accretion in a young andesitic chronosequence at Mt. Shasta, California. Geoderma 116:249-264.

Lilienfein J, Qualls R.G, Uselman* S.M.and Bridgham S.D. 2004. Adsorption of dissolved organic carbon and nitrogen in soils of a weathering chronosequence. Soil Science Society of America Journal 68 292-305.

Revised version: Our findings are confirmed by the results of Sollins et al., (1983), who studied C dynamics at four mudflow chronosequences at Mt. Shasta in California and hence stated that the heavy fraction is an important C sink (37-72% of total C).

At these sites, the SOC stock (Figure 2) consisted mostly of carbon which was stored in the '< 63 μm' (65 %) and HF (28 %) fractions, respectively (Table 3) which is in accordance with Sollins et al., (1983).

Specific comments: Comment 6 Abstract lines 26 through 29. The cause and effect does not seem clear. Suggested revision: "After 50 years of birch growth, the SOC

stock is lower than that of a naturally growing birch woodland. Suggesting that afforested stands could sequester additional SOC beyond 50 years of growth."

Author's answer: Reviewer's suggestion was accepted.

Revised version: After 50 years of birch growth, the SOC stock is lower than that of naturally growing birch woodland. Hence, afforested stands can sequester additional SOC after 50 years of birch growth.

Comment 7 please spell out sodium polytungstate

Author's answer: The suggestion was accepted and the sentence was changed.

Revised version: The particulate organic material (POM) was separated from the denser organic material in the mineral-associated sand and aggregate fraction (heavy fraction; HF) by density fractionation (1.8 g cm-3, sodium polytungstate from Sometu) on the soil material (> 63 microns).

Comment 8 Page 14, lines 14-15 needs rewriting.

Author's answer: The sentence was changed.

Revised version: The pattern that the upper most sampling intervals of the vegetated sites (dotted circle) are decoupled from the nested scatters, was also observed at the relationship between the selected SOC pools (bulk SOC concentration, < 63 μm SOC concentration) and the organo-mineral complexes (Figure 5; E, F).

Basel, 26 December 2018, M. Hunziker

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