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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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Review of Hunsiker et al. 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.

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. 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.

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 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"? 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.

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. 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.

Other specific notes are listed below:

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."

please spell out sodium polytungstate

Page 14, lines 14-15 needs rewriting.

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