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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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######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######

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.

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.

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).

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?

Line 28, page 7: this is confusing. You sampled also “Barren Land”, no? Whichhad 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 measn “all other samples than Barren Land”?

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.

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.

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?

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?).

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.

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.

Line 20, 23, page 10: it is nothing too weird that you still have some C left. You can refer 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.

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

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

Line 6, page 11: in this case I would rather use a relative value for the delta, it's more immediate

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.

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.

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...

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

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).

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

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)

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.

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...

Line 26-28, page 13: these correlations are weak, you need to state also the p-value, I'd say. For a r^2>0.8 I wouldn't be so strict, but these are rather low.

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.

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.

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?

Line 7: that most of the SOC is in the <63nm fraction is expected, that's just how SOC humification/degradation works, and it does not indicate much else. How do you think it relates to what follows?

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.

######References######

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