



EGUsphere, referee comment RC1
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Comment on egusphere-2022-306

Anonymous Referee #1

Referee comment on "Forest liming in the face of climate change: the implications of restorative liming for soil organic carbon in mature German forests" by Oliver van Straaten et al., EGU Sphere, <https://doi.org/10.5194/egusphere-2022-306-RC1>, 2022

Dear Editor,

These are my comments about the paper 'Forest liming in the face of climate change: the implications of restorative liming on soil organic carbon in mature German forests', of Oliver van Straaten et al., submitted to EGU Sphere.

It is a good paper, worth to be published. However, I have four main comments, which perhaps need a bit of attention from the authors, and also several minor complaints, mostly about small details of writing, which can be solved by authors (I assume) without special problems.

MAIN COMMENTS

Authors collect a nice set of paired plots, covering a variety of climates and situations, and thus they may explore their dataset in search for a variety of correlations and comparisons. Perhaps too much; this is evidenced by the huge amount of figures and tables, many of them are placed in an 'Annex' chapter: by the way, to me it is not clear whether this 'ANNEX' equals to a 'Supplementary Material': will these figures appear in the main core of the published paper?

The huge number of relationships studied makes difficult for authors maintain the focus. A reduction in the number of topics treated could help to make the paper easier to read. See below, for instance, my suggestion about reducing the space devoted to soil textural effects.

Four main aspects should be clarified:

a) How relevant are the changes in SOC stocks?

An important detail (very relevant to me, at least) remains unclear, after reading the paper. Liming involves applying inorganic carbon to the soil: being the soils quite acidic, the added carbonates are lost to atmosphere as CO₂. Therefore, liming involves making soils a source of C, not a sink. At least for a time. The long-term increases in SOC stocks in limed plots (relative to control plots), do they compensate the initial C losses? In other words: the inorganic C added when liming, lost in the following years, is lower or higher than the extra SOC sequestered thereafter?

b) Inorganic carbon was not analyzed

This is a detail that surprises me, to be honest. Why authors did not analyze carbonates in their samples; at least, in the plots that received lime in the past. Part of the carbon in these soils may be carbonate remaining from the added lime. Certainly, after decades, one could expect that all added carbonate has become CO₂ and released to the atmosphere; but figure 6 (and also lines 350-353) clearly show that the loss of added lime is extremely slow. On the other hand, as shown in Table 1, some sites were limed up to 3 times. The persistence of lime residues in some places (particularly when soil pH reached almost neutrality) is envisageable. I ask authors to make a rapid screening of some samples (in limed plots) to ensure the absence of inorganic carbon in their samples. Just to be sure that the slight increase in SOC stocks in limed plots (in mineral soil horizons) is due to organic carbon.

c) Using litter SOC stock as SOC stability indicator

All plots are placed in humid-temperate forest ecosystems: a large variety in climatic constraints can be discarded, therefore (If I am wrong, then consider including climatic constraints in your analysis). In these conditions, the idea of taking litter SOC (including Oh horizon) as indicator of OC decomposability in these horizons is right. The higher the decomposability, the lower SOC stock in these horizons; but with an additional condition: provided that litterfall is the same in all cases. Or, at least, very similar. This second part of the idea is problematic, for litterfall was apparently not measured in the plots (it does not appear in Table 1). Thus, SOC stock in organic horizons, alone, is a too rough approach to the decomposability of the incoming litter. Note also its problem as indicator: the higher the stock, the lower its decomposability, i.e., the sign is opposite. SOC stock in litter should be an indicator of stability, rather than decomposability.

I have no problem with the use of total SOC stock in litter for studying its relationships with SOC increase, liming, etc. Nice relationships are obtained (Fig. 4). But without

identifying it as 'decomposability': in the absence of data about litterfall, such an identification is risky, in my view. In figure 4, for instance, I suggest removing the words 'Organic matter decomposability index' from the 'X' axis. Use simply 'Forest floor C stock in the control plot (Mg C ha⁻¹)', perfectly exact and more adequate.

Apply this to the rest of the text: I suggest authors avoiding (or at least refining) the 'decomposability' in the discussion about the effect of liming in SOC stocks.

d) Textural effects

Figure 5 is nice in order to suggest a role for soil texture in the changes of SOC stock related to lime addition. But the conclusions are unclear to me. If the total number of points is $n = 26$, given the natural variability, perhaps this is unavoidable. Note also that fine soil textures (particularly: high clay content) result often in higher SOC stocks. In figure 5 all points are considered altogether for the regression; Figure 3d is perhaps better in order to suggest a role for soil texture. May I suggest you perform a figure similar to 3d, but with clay contents (say, $< 14\%$ and $\geq 14\%$). Note that the high number of sites for which soil texture was not available (11 out of 26) suggest being prudent about spending a big effort on this point.

SPECIFIC COMMENTS

Line 21 (in abstract). 'Liming however largely offsets this organic layer buildup'. What does it mean, exactly? That the amount of carbon accumulated in forest floor is lower than the C released (as CO₂?) upon liming? Or precisely the contrary? Do you refer to losses of organic C or of inorganic C (carbonates)?

Line 21. '...which means that nutrients remain mobile and are not bound in soil organic matter complexes'. I do not see how this statement arises from the previous one.

[Actually, note that if you remove lines 20-22, and start directly with 'Results from the paired plot analysis showed...', the paragraph runs perfectly.

Line 37. Even though?

Line 53. '...stand age), (3) the application of lime...'. Better?

Line 54. '...and (4) the ongoing acidification from...'. Better, again?

Line 58. 'While it is broadly reported that...'

Line 65. 'Liming-induced changes in nutrient stoichiometry...'

Line 95. '...meaning that these sites...'

Line 115. I understand, therefore, that you obtained at each plot four (4) composite samples per depth, each of these obtained by pooling three (3) samples. Ok?

Lines 117-119. I am surprised by the fact that authors did not analyze carbonates in the limed samples. Apparently they are confident that all added lime has disappeared. To me, a verification of this point would have been welcome. Otherwise, the risk of overestimations of organic C in the limed plots can not be discarded.

Line 162. '...three beech forest sites: Dassel 4227 (DAS 4227)...' (etc).

Line 245 (caption of figure 245). This is (to me) a very relevant detail: in some cases, liming causes the disparition of the Oh horizon. Can you write a couple of lines about this detail? In how many cases does this happen? Could be another explanation, besides the pH increase?

Line 253. Why is this not shown? This is extremely interesting!

Lines 259-261. I accept that the difference between control and limed plots did not reach significance, at any depth. Nevertheless figure 2 shows that, even though the variability was very high, SOC sequestration values were on average consistently higher in limed plots. Could this be mentioned somehow?

Line 307. Perhaps 'partly compensated' rather than 'offset'. If the SOC losses in the forest floor were lower than the gains in mineral soil, then the net balance is still positive, not? If you want, you can write 'partly offset'.

Line 364. 'Because the biochemical environmental plays...' (better?)

Lines 466-470. The importance of calcium for stabilizing soil organic matter, widely known in South Europe (where calcareous environments are common, and calcium-rich soils, too), has been largely underestimated in studies about Central Europe. I acknowledge the mention of Ca^{2+} as a stabilizer of organic matter in these lines; but I rather regret such a small space given by authors to this explanation, which is to me the most obvious one to account for the increases in SOC stocks after liming. Carbonates are a huge source of calcium for soils, and Calcium (rather than carbonates themselves) the probable main reason for the accumulation of SOC in carbonate-rich soils.

Line 477. The very minor contribution of lime-derived CO_2 to the total CO_2 efflux should be taken as a sign of the relative resistance of lime to disappear in a context of acidic soils. The absence of inorganic carbon in soil samples should be verified. Otherwise, the data about SOC stocks may have been overestimated, at least in the limed plots.

Lines 484-486. If such an adsorption of lime to SOM complexes did effectively occur, then my complaints about the need of analyzing inorganic carbon in the soil samples makes even more sense.

FIGURES

Must be improved, definitively. In my computer I put the image at 150 % size, and even at such scale I had difficulties in seeing some figures.

Figure 3 will become almost illegible, for instance. Putting the four sub-figures in line makes all four very small, and letters are almost illegible. I suggest you reconsider the design: instead of four figures in a single line, make a set of four figures, placed in square (two in the upper line, two in the lower line).

Figure 4 will suffer the same problem. The four small figures at the top are barely legible in its current form, and in the printed version of the paper can not be read, quite simply (I verified it). Again, reconsider the design. Also, note that the Figure 4e is different from the others (4a to 4d), and could well be a separate figure.