

Interactive comment on “Bone char effects on soil: sequential fractionations and XANES spectroscopy” by Mohsen Morshedizad et al.

Anonymous Referee #2

Received and published: 28 July 2017

The paper is concerned with changes in phosphorus (P) chemistry following application of bone char to soil as an alternative to conventional P fertiliser. The research addresses the important need to develop strategies for increasing the sustainability of P management in agriculture, coupled with increasing demands to recycle P from renewable materials previously regarded as wastes. An important feature of the paper is the analysis of P both in the bone char particles and soil, pre- and post-amendment period. Also, the paper compares two types of bone char; one having received a novel treatment with reduced S compounds. Further pertinent factors under consideration are bone char particle size and scale of experimental approach (incubation/leaching vs plant bio-assay). Conventional chemical extraction techniques and XANES spectroscopy are used to study the soil P chemical changes, whilst XANES is also used to

C1

investigate the corresponding changes to P chemistry in the bone chars.

Specific Comments

Lines 101, 124. Can the authors explain why different soils were used for the two experiments? Ideally, the same soil should have been used for both approaches.

Line 110. What is the rationale for leaching with three pore volumes? Is this number based on conventional methodology, and/ or represent typical drainage discharge?

Line 133. I think more detail is needed on how the particles were ‘manually’ separated from the soil.

Line 148. Filtered through what pore size, or paper type?

Line 193-194. In Table 1, I don’t see any significant increases in soil P pools following the BC1-2mm additions.

Line 207-215. These detailed leachate data from the authors’ related work should not be presented here. Instead, the relevant information should be integrated within the Discussion. However, it would be useful in the current paper to see the final soil pH data, as these are directly relevant to the soil P chemistry. These additional data would require a minor amendment to the Materials and Methods section.

Line 222-224. I think it is misleading to state ‘Both BC and BCplus amendments led to enrichments of P fractions, except for the readily available and labile inorganic P concentrations in the BC treatments.’ It appears that the BC treatments fail to change any of the P fractions with any statistical significance. Similar to the comment on Lines 193-194, above, I think the authors should check through the Results section that all statements concerning treatment effects are accurately qualified in terms of their statistical significance.

Line 231-232. This reads like an afterthought. Does the statistical significance of this increase warrant an earlier mention in the section?

C2

Line 237-240. As commented previously, I think the soil pH data are too important to be omitted from the paper.

Line 362-364. Given the lack of statistical significance, I think this sentence misrepresents the data. Maybe rephrase along the lines of the data suggesting that there is more evidence for a positive effect of particle size on P fractions in the BCplus treatments...?

Line 366-377. I am not sure the data adequately support this ('largest increase...in...HCl-P') in the ryegrass experiment.

Line 367-370. The pH data must be presented in support of this discussion point. Line 399-402. As above.

Line 403-406, and further into the Discussion. The authors discuss possible acidifying effects on the P speciation. In order to develop the discussion a little more broadly, it might be useful to explore any evidence for acid producing reactions (in terms of pH, and / or P fractions and species) in the rhizosphere, per se; i.e. by comparing the control treatments between the incubation and ryegrass experiments.

Line 408-410. This sentence is confusing. I recommend removing it.

Lines 444-446. Here or earlier in the Discussion. Given the underlying challenge of enabling bone products to dissolve in neutral to alkaline soils (Lines 57-59), I think the Discussion needs to try and link briefly the current observations, obtained using acid soils, to higher soil pH scenarios.

Although the paper is well constructed, with an appropriate balance among its various components, there are several sentences where the scientific English could be improved. I would be happy to help with suggestions for these improvements using Track Changes, if a Word version of the paper was made available.

Interactive comment on SOIL Discuss., <https://doi.org/10.5194/soil-2017-16>, 2017.