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Interactive comment on "Changes in soil carbon, nitrogen and phosphorus due to land-use changes in Brazil" *by* J. D. Groppo et al.

Anonymous Referee #1

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The objective of this ms is to compare soil carbon, nitrogen and phosphorus in 3 Brazilian biome, stressing the differences related to land use systems. It is used stoichiometry based on element concentrations and stocks in soils.

The global approach is classic and based on the survey of data, which part of them were published in a previous paper (those concerning carbon stocks).

The materials and methods are well presented. The paper is concise in the result section. The scale of sampling is the soil profile.

The paper is well presented and the argumentation is interesting for the journal.

Despite these comments, there are some fundamental questions to address and some minor corrections have to be made.

Introduction

This section presents a sequence of arguments of the interest on C, N and P stocks. Reading the ms, the impression is that the issues in this introduction were compiled and in consequence the arguments are classical and do not state the real interest of this survey, particularly regarding some originalities of the results.

Concerning phosphorus: avoid the term "fix" (page 4-line 9), prefer adsorption like line 19. In general it's not a lack of phosphorus in tropical soil (concerning Brazil, the stocks of total-P are high regarding others tropical cultivated lands, even in sandy soils developed on sandstones of the Paraná and Bahia ...), but a low concentration in phosphate-ion in the soil-solution and a hard competition of roots with the soil-solid phase which retains with a high energy the phosphate. In my comprehension, the authors are not "phosphorus specialists" and use with some misconceptions the knowledge of this topic (there are others evidences in the ms).

Concerning the C:N:P ratios: it is comprehensive to use the C:N ratio, these elements are part of the soil organic matter. But, it was showed in old works that it is not possible to use the P (as total-P determination) for soil samples (it is possible for organic materials), and the stoichiometry have to be done with the Po (organic P). And, in addition, the form of P that was compiled in the paper was a Mehlich-3 extraction, it's a double acid extraction that extracts phosphate ions and also dissolves some organic matter and then solubilise organic phosphorus.

Concerning some assumptions, in marge of research: why the increase of agricultural land is dramatic? The statement was done in relation to gas emissions, prefer to state this dramatic effect to deforestation; it was the objective of your phrase.

Page 5, lines 4 to 19, now days a part of the scientific community, which never worked with soils (?) rediscover the C:N and C:P ratios. The reality is quite different, from 100 years we used the C:N for comparisons of global conditions of capacity decomposition of organic material; it is the same for the C:Po ratio, ie the rate of decomposition of Po (organic P) in litter and excreta, depends initially on the P solubility in the residue and subsequently on the rate of decomposer growth. The rate of decomposer growth depends on substrate accessibility to the organism and its enzymes, the chemical nature and nutrient content of the substrate (extent of lignification,C:N:P ratio) and the surrounding soil water (pH . . .). I understand that it is interesting to cite recent references, but here for this journal the importance is not to write “that changes in C:N:P ratios may affect several aspects of ecosystem functioning, including carbon sequestration, and, consequently ecosystem responses to climate change”, that is a general comment with no interest, but to explain in which way your proposal of the C:N:P evaluation is original and may serve a knew knowledge in biogeochemistry of soils, and by extension its consequences for management and a few comments on global changes.

In conclusion, I think that the introduction needs a reformulation.

P3 lines 11 and 18: twice Âńon the other handÂż but the previous hands are not clear! Please refine the text.

Material and methods

- page 7, line 12, now it is preferable to use Urochloa sp for the Brachiaria genus.

- Same page, lines 18-20, this comments concern Brazil add “in Brazil” in the start of the phrase.

- Page 8, section 2.1, the reference to the ABC program is interesting in a global point of view, perhaps in another section it should be better, in particular in the discussion.

2.2 / Climatic data, it is strange to refer to a north-american database, nothing in Brazil?

2.3 / Sampling. How many replicates for the soil bulk density? Which method? When the coarse fraction is higher than 12-15 % in mass of total soil, the cylinder method is avoided . . . In the equation (1) it is not referred to the coarse soil fraction, do we understand that there is no coarse fraction? But it was written that “Air-dried soil samples were separated from plant material and stones . . . (2mm sieve ...)”. In addition, for

stocks evaluation, the coarse fractions have to be weighed in volumetric samples done for bulk density. So, as the paper is centred on stocks this section have to be performed (the real eq 1 is S=[X].z.bulk density.(1-y) were y is the mass > 2mm in g/gsoil).

2.3 / analysis. Please indicate that the elemental analysis CN by combustion includes in the C concentration a possible part of charcoals. It is important if we consider that a large part of deforestation in Brazil had the fire as a basic technique, but also the soil fine charcoal is due to past climatic changes (Holocene or pre-Holocene) (Many Brazilian soils have more or less from 0.3 to 2 kg of C-charcoal per m3 . . .).

Avoid the word “available” for the phosphate extraction, as the colorimetric method is specific to phosphate it is preferable to use extractable phosphate for Pme. For some soils from the south, we are not sure that the colorimetric method is the better one (SiO2 in solution, acid extractable effect) and the blue method is one of the methods to determine Si in solution.

- page 9 and 10, last part of the section 2.4, there is a long discussion about the conditions of sampling and the limitations. In my opinion this is interesting, but why no consolidate an uncertainty section in the discussion. In this way, there is no reference to the large diversity of soils which were sampled ...

3. Results

3.1.1

Why do you expected a decrease of concentrations with deep? Even in Brazil there are soils layers were you can have an augmentation of carbon in deep layers!

3. Preferable to use the Pme than phosphorus in this section, refer to the real analyse. Idem for the C:P, use C:Pme in the figures.

4. Discussion

4.1 page 15, there is a long section on the C:P and N:P ratios, it is written “it is difficult to further speculate about the reasons of such trends", perhaps it could be easier to refer to others papers which described and explain total-P and organic-P distribution in Brazilian soils! Concerning the Pme extractions, the authors have only a reflexion on the absolute values and ratios. But the Mehlich is an acid extraction, so the extracted phosphate is an evaluation of the potential dissolution of phosphate, mainly inorganic+organic. Then, when those values increase in deep layers compared to non cultivated lands, at the scale of this large survey, it indicates that large part of phosphate is statistically extractable in those layers, so one of the effects of the diverse cultivations is the mobilisation of this phosphate fraction. The other comment is that in CPS and pasture-R samples, the soils could similar, due to strategically survey decisions, and in consequence the effect of cumulative P-fertilisation is quite clear in figures and tables. In my point of view, not only the fertilisation had an effect, but also the cultivation in all compounds: its favours the remobilisation of phosphate soil fractions (they are some references in brazilian journals), and this point is essential to stress and comment.

In conclusion, if I agree with the discussion of the P fluctuation in the paired sites survey, I think that a more specific and detailed reflexion on the methodological approach for the P quantification is needed. In particular, in the page 19, the cited papers had differentiated methodologies, and the discussion is too general to overcome to Âńset a baseline of soil nutrients stocks and stoichiometry for future comparisonsÂż as authors wrote.

Table 2 and 3. Add the units in the title in order to simplify the table

Figure 2 and 3. Are those results means ? indicate in the title. The P is the Pme in fig 2, indicate.

Figure 4. The title of the fig is not clear, in which axis is the Assad data set?

Conclusion. I think that in a global point of view this ms need a dedicate revision of the international interest of the results issues. The quality of the results are not

questionable if some of the methodological questions are resolved, it is more the text and the way of the redaction which needs work to convince the reader that it is a ms for BG.

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