

Interactive comment on “Soil carbon and plant diversity distribution at the farm level in the savannah region of Northern Togo (West Africa)” by M.-T. Sebastià et al.

Anonymous Referee #1

Received and published: 16 December 2008

General comments:

This manuscript reports changes in soil carbon due to different land uses and relates those changes to diversity at one farm in Northern Togo. The objective of the manuscript is very interesting and worthwhile investigating, but the methodology needs to be more detailed and adapted before the manuscript can be fully considered for publication. The major issues are: 1) details on site selection and number of sites per land use are not clear, 2) prepping of the soil samples before analyses is not detailed, 3) statistical analyses are not appropriate and have not been interpreted correctly, and 4) conclusions are based on correlation that are probably spurious.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Specific comments:

P4108 In18-20: I find this conclusion to be rather weak because a forest soil is not really a realistic potential for C levels under improved agricultural management. It is a reference, but one will hardly ever reach the soil C level of a forest in an agricultural system. It is in only few places and under specific conditions that C levels of native systems can be attained in an agricultural system.

P 4112 In 9: It needs to be detailed on how the twenty-two sites are distributed across the different land-uses. It is clear from figure 3 that the forest had only 1 replicate, which is very unfortunate! But how many replicates did you have for the other land-uses? Also, were all 22 sites on a similar soil type? The latter is necessary for a valid comparison between the land-uses? How was the soil prepped before analysis? 2 mm sieved?

P 4112 In 25: why were different grasslands sampled in June versus in October? Can that be the explanation for the observed interaction between date and diversity in the effect on SOC?

P 4114 In 4: it is my understanding that species composition data is better analyzed by NMDS than by CCA. Furthermore, I do not understand why I only see 12 samples presented in figure 2a when you have 22 sites? All replicates should be considered in the CCA. The same holds for fig 2b.

P 4114 In 14-16: It does not make any sense that SOC increases with increasing species richness only in July. Differences in species richness over the season cannot have such a fast effect on a slow reacting parameter such as SOC. I do realize that it might be that the species richness of July is the only adequate richness to correlate with SOC, but it does point out the weakness of the use of correlations for this kind of assessments. The correlation can just be spurious and a controlled experiment needs to be established in order to really test the relationship between SOC and species richness. Or, at least several years of species composition measurements are needed

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



to show the link sought after. I also keep wondering if the correlation is not a result of the different sites sampled in July than in October rather than a difference in season, as is suggested in the text.

P 4116 In 2-12: given the errors presented in fig. 3, all the differences discussed in this section are not valid because none of the land-uses differ significantly in soil C according to Fig. 3. Furthermore, line 4-6 suggests that the soil type is not consistent across land use types and therefore the comparison between land-uses is confounded by soil type and hence invalid.

P 4116 In 19-20: I have to fully agree that the potential for inference with this dataset is very weak. And not only because of number of samples.

Interactive comment on Biogeosciences Discuss., 5, 4107, 2008.

BGD

5, S2442–S2444, 2008

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

