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Biogeosciences Discussions

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Interactive Comment

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.

M.-T. Sebastià et al.

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Referee 1

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,

They have been clarified in the text and more information is given in a new table. See the answers to the specific comments below (specific comment n. 2 and 3).

2) prepping of the soil samples before analyses is not detailed,

Information added (see specific comment n. 4).

3) statistical analyses are not appropriate and have not been interpreted correctly,

Two types of statistical analyses have been applied to the data, both standard for the particular issue addressed: a) analysis of variance and modeling by regression analysis for problems involving one variable of interest, and 2) multivariate methods, in particular Canonical Correspondence Analysis, for problems involving many variables of interest, e.g., species frequencies or soil parameters. Authors learnt those methods at the faculties of agronomy, of biology and of statistics, and have shown ample expecience in their use in papers published in international journals during the development of their careers. Thus, we think we have applied suitable statistical methods and have used the results from those to centre the discussions. We made an effort for better clarification in the current version when describing those, to address the reviewer's concerns.

and

4) conclusions are based on correlations that are probably spurious.

Conclusions are based on standard statistical methods, including regression analysis and analysis of variance. There is always a probability associated to accepting a given hypothesis when this is false, and the other way around, but this probability is known, because it is given by the P-value provided by the analyses. If it turns out that the results are spurious (which is always a possibility that we, as scientists, never can rule out entirely), this needs to be demonstrated with further work. However, in the current case, the high significance associated to some results (in ANOVA and regression analyses), in spite of the relatively small databases, strongly suggests that those cor-

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respond with existing patterns, and not with spurious results. In those cases, we have accepted them as valid, and are the bases for our conclusions.

In other cases, we found only tendencies and, following the reviewer's concerns, have made an effort to identify those cases more clearly than in the previous version of the manuscript, and soften our conclusions. In those cases (see Fig. 2), results were based on multivariate analysis on soil parameters, which, given the low number of replicates for some land uses, was the most suitable way for comparing and understanding soil variation among land uses.

In addition, we detected a contradiction in the results, regarding species richness and date, and we modified that accordingly.

Specific comments:

1. P4108 ln18-20: 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.

We agree with the referee that the agricultural systems will (never) reach those levels of OM we find in the forest soils under the present conditions. We want just to indicate that if the climate, parent material and vegetation of the area allow a remarkable accumulation of SOM under undisturbed systems, an appropriate management of agricultural systems could probably increase their SOM, although not to that maximum level.

We changed the sentence to make it clearer:

"The soil characteristics found under the permanent forest suggest that to some extent, an improvement of the soils of the region is possible regarding both agricultural yields and as a potential carbon sink relevant to global change policies."

2. P 4112 ln 9: It needs to be detailed on how the twenty-two sites are distributed

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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 landuses?

Twenty-two sites were sampled for soil analyses, corresponding to agricultural fields with a homogeneous land use in 2003. They are detailed in new Table 2: 4 legumes (soya + orchard), 6 cereal (corn, millet and sorghum), 5 rice, 6 fallow (fallow and dam) and 1 forest. We have added the number of replications in the legend of figure 3.

3. Also, were all 22 sites on a similar soil type? The latter is necessary for a valid comparison between the land-uses?

No, the 22 sites were not located in the same soil type. If they were located in the same soil type, the results would be less representative of the whole region.

4. How was the soil prepped before analysis? 2 mm sieved?

Yes, the soil was air-dried and sieved to 2 mm in the lab. The paragraph reads now:

P 4111, l11:

“Land use was recorded. Soil was dried at room temperature and 2 mm sieved for analyses. Physico-chemical analyses ...”

5. P 4112 ln 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?

Different grasslands were sampled in the two dates, but in both dates the same variety of conditions were assessed. This has been clarified in the current version. Thus, there was not a difference in the sort of environments sampled in both dates, on the contrary, all sampled grasslands corresponded to the typical conditions found within the farm. The results can then be safely interpreted as linked to differences throughout time. We found those differences for both species richness and for species composition, as shown if figs. 3 and 4 (formerly, 4 and 5). More clarification included in the text.

6. P 4114 ln 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.

CCA is a standard method, and previous work by the authors showed small differences in the results when applying CCA and NMDS. In addition, in this particular CCA (fig. 2), soil parameters were used as the explanatory variables, not species composition. This constitutes a way to compare overall differences (or similarities) among samples based on soil parameters. It can be used to compare soil characteristics among land uses indendently of the number of replicates per land use. Here, given the low number of replicates for some land uses, this method is very good for comparing those. In addition, it lets obtain a comparison among land uses based on multiple soil parameters, instead of a comparison based on one parameter each time. All 22 sites were included in the analysis, but sometimes they overlap, and this why there seem to be less samples in the graphs.

7. P 4114 ln 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 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.

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For this analysis, regression techniques were used (not correlation, which is a different statistical method). The variability of environments with grassland within the farm was not very high, and the same environments were sampled in both dates. Therefore, we are fully confident that this is not a result associated to sampling different grasslands in both dates, because the same types of grasslands were included both times.

Our results do not suggest that differences in species richness over the season have a fast effect on SOC, but that the relation of species richness and SOC exists, but it depends on the time of the year in which species richness is considered. Part of the diversity effect could be related to the sort of species that appear in both dates, and in this case, this could have long term effects on SOCS. Indeed, we found that date was highly related to variation in species composition (see Fig. 5, now Fig. 4). The relationship between species richness and ecosystem services, including soil carbon storage, has been hypothesized in theoretical ecology but has been rarely demonstrated in natural, non-experimental set-ups. This is why our results are relevant. In addition to demonstrating the existence of this relationship in drylands in Africa, they point towards possible mechanisms (relation with composition), and suggest that caution must be taken when analyzing those patterns, adding a possible temporal component not previously noted and worth exploring in future work. Thus, we fully agree with the reviewer that this is a pattern to be explored in more detail in further studies, and have tried to be cautious in our interpretations in the text.

8. P 4116 ln 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.

Some of the text was misplaced, and now it should be easier to understand. In addition, indeed, as the reviewer suggests, we can only talk about tendencies when referring to Fig. 3, because of the high variability and in some cases low number of replicates.

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However, some of our conclusions were based on the multivariate analysis, based on similarities built on several soil parameters, independently of how many replicates per land use there are. Following the reviewer's suggestions, we have rephrased it to make this even more apparent than in the previous version. In addition, we substituted Fig. 3 and also Table 1 in the former version by a new Table (now Table 2) where the actual values of soil parameters are presented. We expect this to facilitate the understanding of the observed patterns.

Soil type was not taken as a source of variation, since soil carbon was quantified only for the surface horizons (0 to 15 cm), which correlated poorly with the soil types, defined according to other characteristics that did not affect surface OM contents.

In addition, the consideration of the actual values of various soil parameters in the multivariate analysis should provide a richer information about patterns than a single definition of soil type.

9. P 4116 ln 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.

The scarcity of data on similar tropical environments and the difficulties to obtain them gives an additional value to the dataset presented, and encourages continuing the data collection and interpretation. However, we consider this does not invalidate the value of the current analysis.

In any case, following the reviewer's concerns, we have reconsidered the text and tried to soften our conclusions when necessary.

Referee 2

General comments

1. The manuscript aims at elucidating relations between soil organic carbon contents and plant diversity in a demonstration farm in Northern Togo. This immediately highlights the main shortcoming of this paper: in how far is this model farm a representative

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agro-ecosystem for Western Africa and - in other words – how can the results be of any relevance to practical farming situations in the region?

The use of case studies presents examples of the wide variety of situations in which agriculture, forests, water and human processes interact. Although the case studies serve primarily to illustrate general statements, they are a powerful tool as examples of the complexity of these processes, and give importance to the fact that any generalization should be examined carefully (Thurow & Juo, 1995). In an ecosystem context all components interact in such a way that make each ecosystem unique (Knoepp et al, 2000). The small-watershed approach is a natural unit of suitable size for studies at the ecosystem level, because among other reasons it provides a method whereby many important parameters related to matter and energy budgets may be calculated, and allows us testing the effect of various land-management practices, which would be impractical at small-scale or regional surveys (Borman & Likens, 1967).

The area under study has been recognized as representative of the savannah area of Northern Togo, both for its natural characteristics and for its significance for the development of the whole region, since it is the only farm where the efectiveness of soil conservation measures can be implemented and assessed (Poch & Ubalde 2006, Ubalde & Poch 2000). See the answer to the specific comment n.2.

Part of these two previous paragraphs have been added to the introduction and material and methods sections to clarify the comments of the referee.

Bormann, F.H. and Likens G.E. 1967. Nutrient Cycling. Science 155, 424 - 429.

Knoepp, JD, Coleman DC, Crossley DA & Clark JS 2000. Biological indices of soil quality: an ecosystem case study of their use. Forest Ecology and Management 138, 357-368.

Poch RM, Ubalde JM. 2006. Diagnostic of degradation processes of soils from Northern Togo (West Africa) as a tool for soil and water management. New Waves in Physical Land Resources Workshop. U Gent, TIC, ICE, VUB Gent, 3-9 Sept 2006 Book of abstracts, pp 171-179

Thurow, T.L. and A.S.R. Juo. 1995. The rational for using a watershed as the basis for planning and development. In: J.M. Bartels (ed.): Agriculture and environment: bridging food production and environmental protection in developing countries. Published by ASA, CSSA, and SSSA, Madison, Wisconsin, pp. 91-114.

Ubalde JM, Poch RM. 2000. Projet de conservation des sols et des eaux dans la zone soudano-guinéenne au Centre de Formation Rurale de Tami (Togo) Bulletin du Réseau Erosión 20, 485-495

2. The title, hence, is promising more than actually produced. The subject of the work is however sufficiently relevant: soil organic matter is a crucial soil quality indicator in the depleted soils of West-Africa. The information that is critically needed, however, is on how an adequate level of organic matter can be obtained/maintained while the soil remains under production. It is not surprising to learn that higher values of organic matter are measured in the so called 'sacred forests' where neither cropping nor any other disturbance occurs. Overall, the conclusions of this study are rather predictable.

Not all changes of agricultural use to forest result necessarily in a SOM increase. Woody plants may be less effective at storing carbon in soil than perennial grasses, as it happened under a subtropical thorn steppe system (Brejda, 1997). In this case, in a grass opening without woody shrubs showed a SOC increase when grazing was eliminated, where the vegetation was shifted from a grazed grassland to an ungrazed woodland. In a wet tropical forest life zone where sugar cane fields were converted to fast-growing eucalyptus trees, input rates of eucalyptus carbon into a soil deep layer were small compared to previous sugar cane inputs (Bashkin & Binkley, 1998). Similarly, replacement of native tropical savanna with productive, deep-rooting exotic grasses resulted in SOC increases (Fisher et al. 1994). When forest is cleared for pasture establishment, considerable above-ground carbon is lost, but it does not nec-

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essarily leads to a decline in SOC. A review of several estimations of SOC change after landuse shifts shows that average rates of accumulation are similar for forest or grassland establishment (Post & Kwon, 2000).

In our case, we wanted to point out that forest soils in this region have a high potential of carbon storage, even with evidences of only a single plot. We have added part of the previous paragraph to the discussion of the new manuscript, as an answer to the concerns of the referee.

Bashkin, MA & Binkley, D 1998 Changes in soil carbon following afforestation in Hawaii. Ecology, 79, 828 833.

Brejda, JJ 1997 Soil changes following 18 years of protection from grazing in Arizona chaparral. Southwestern Naturalist, 42, 478 487.

Fisher, MJ, Rao, IM, & Ayarza, MA 1994 Carbon storage by introduced deep-rooted grasses in the South American savannas. Science, 371, 236 238.

Post WM & Kwon KC 2000 Soil carbon sequestration and land-use change: processes and potential. Global Change Biology 6, 317-327

3. It is not particularly novel to state that soil organic matter levels are highly dependent on land use and management. About the established positive relationship between soil organic carbon and plant diversity, one could say the same.

Regarding the relationship between land use and organic matter, see comment above.

To our knowledge, although the relationship between organic carbon and diversity has been hypothesized by ecological theory, evidence for it is really scarce, particularly in non-experimental set-ups such as those of Tilman et al. 2006. See specific comments n. 8 to the first referee.

4. It would, however, be more interesting to reveal the underlying causality in this relationship. The authors also mention that there is room for improvement by changing

the agricultural practices. How to do this in a practical and socio-economically acceptable way is not indicated and also probably not possible, based on data from a single 'model' farm.

Indeed, the model farm has a particular social structure, different from the rest of the region, since it works as a cooperative. This allows the implementation and testing of new agricultural practices to improve yields and soil quality. Unfortunately there are few possibilities to set experiments of this type.

Specific comments

1. - The first line of the abstract states that soil organic matter is 'a source of fertility for food provision'and a 'tool for climate mitigation'. Both statements are rather unsubstantiated and need reformulation.

Rephrasing:

"In western Africa, due to the lack of resources for land management, soil organic matter has a greater importance than in the rest of the world as a source of fertility..."

2. - In the same abstract, line 17, the authors mention a strong influence of human activity on soil formation and distribution. While this may be so, one can not make these statements on the basis of this study in one model farm. Or do the authors mean something else? Besides, the soil type of this farm is not even mentioned, which makes it difficult to judge the representativeness of this study. Yet, on page 4111, lines 4-7, mention is made of soil mapping, soil profiles and so on... so the information must be available.

Yes, it is available and will appear in a new table (Table 1). This farm of 100 ha has three orders of Soil Taxonomy (vertisols, mollisols and ultisols) and three general landscape units (slopes, platforms and valley bottoms), that conform the relief and main soil types of the savannah region of northern Togo (PNUD, 1991; ORSTOM, 1978)

The table contains the mapping units of the detailed 1:5000 soil map (Ubalde & Poch,

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2000).

Position		Classification		Landuse / vegetation	Parent material
		FAO 1998	SSS 1999		
Platforms		Geric Plinthosol	Haplic Plinthustult	cotton, corn, peanuts	Saprolite of syenites and gneiss
Slopes	Upslope	Orthiplinthic Acrisol or Lixisol	Plinthic Kanhaplustult	soja, corn, peanuts, cotton, millet, sorghum, pastures	Saprolite of syenites and gneiss
	Footslope	Arenic Acrisol or Lixisol	Arenic Kanhaplustult		Colluvium of quartzitic sands with lateritic gravels
	Eroded slope	Hypereutric Vertisol	Chromic Haplustert		Saprolite of syenites and gneiss
Valley bottoms	Strong accumulation	Arenic Gleysol	Aquic Quartzipsamment	Pastures, rice	Colluvium of quartzitic sands with lateritic gravels
		Pachic Phaeozem	Pachic Vermustoll	Sacred forest	Saprolite of syenites and gneiss
	Slight accumulation	Stagnic, Endoeutric Plinthosol (inc. Gleyic Phaeozem)	Oxyaquic Argiustoll (inc. Oxyaquic Haplustoll)	Pastures, rice	Saprolite of gneiss and sandy colluvium on top

These references have been added:

PNUD-Programme des Nations Unies pour le Developpement, 1991. Carte des Zones Écogéographiques du Togo. Projet PNUD/FAO/TOG/83/009. Lomé, Togo.

ORSTOM, 1978. Ressources en sols du Togo. Carte à 1/200 000 des Unités Agronomiques deduites de la carte pédologique. Paris, France.

Ubalde JM & Poch RM. 2000. Projet de conservation des sols et des eaux dans la zone soudano-guinéenne au Centre de Formation Rurale de Tami (Togo) Bulletin du Réseau Erosión 20, 485-495).

3. Page 4108, line 19: the potential carbon sink under forest is very misleading: it is not relevant to compare soils under arable farming with an undisturbed forest, as the main difference is that the former are continuously disturbed and biomass taken from them. I would argue that the forest soils are not useful as a baseline for potential fertility in the area. First of all, they can not be compared with a cropping situation

Yes, we agree. See specific comment n. 1 to referee 1.

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4. And secondly - as the authors themselves indicate - the reason why they are still there (under forest) may be because the intrinsic fertility of the soils is low. The authors give the example of the rice fields, where already a selection towards the more clayey soils is made (page 4115, lines 4-8).

The reason why they are under forest is probably due to religion, and not to the soil type. They are very small patches (not more than 0,5 to 1 ha, with regular outlines that do not correspond to soil boundaries) scattered on the region, that are used for religious rituals, and not restricted to a single soil type.

5. Page 4110, line 22: inconsistent: earlier the author states that fertilisation is very limited, then it is claimed that 150 kg ha^{-1} of NPK is given. In the context, this is not limited, average doses for West-Africa are a few kg's only! Points one back to the earlier raised comment about representativeness!

We agree with referee n.2. Nevertheless, NPK fertilisation is limited in the sense that for a sandy soil this fertiliser is lost very rapidly by leaching. We changed the sentence to (p 4110, line 19):

"Fertilisation is mainly organic amendments..."

- A major and often occurring problem with studies like this where multivariate analysis is applied is that the original data structure is no longer presented. I would insist on also presenting the raw data of organic matter contents, cation concentrations and other parameters in the different treatments, before they are disappearing in the different axes of the CCA.

We substituted Table 2 (former table 1) and Fig. 3 by a new Table 2 (former table 1) with the original data.

Information added as a table:

- On page 4122, table 2. I have some problems with a soil organic carbon stock that changes with the season, as seems to be the outcome of the regression model pre-

	pH H2O	pH KCl	$CE_{1:5}$	OM	CIC	Particle size analyses					N	P	K	Mg	Ca	Na	S	V	Landuse
Plot	1:2,5	1:2,5	(μs/cm)	%	cmol/kg	coarse silt	fine silt	clay	sand	texture	ppm	ppm	cmol/kg	cmol/kg	cmol/kg	Cmol/kg	cmol/kg	%	2003
1	7,4			0,57	7,4	6,4	4,1	10,2	79,3	SL	2	4	0,16	0,69	2,66	0,16	3,67	49,6	Orchard
2	6,1	4,8	30,5	0,32	6,8	8,6	6,5	30,1	54,8	SCL	6	3	0,04	1,16	3,49	0,21	4,90	72,1	Corn
3	6,3	5,2	29,6	0,34	3,7	6,9	3,5	7,7	81,9	LS	1	3	0,06	0,55	1,67	0,16	2,44	65,8	Soya
4	6	4,8	21,5	0,30	3,5	17	7,4	11,5	64,1	SL	1	3	0,02	0,30	1,24	0,16	1,71	49,0	Fallow
5	5	3,9	30,8	0,13	1,6	9,4	4,4	6,9	79,3	LS	1	6	0,03	0,13	0,33	0,14	0,63	39,5	Corn
6	7,5	6,6	95,5	0,60	5,9	7,4	6,7	23,4	62,5	SCL	2	4	0,50	0,91	3,70	0,15	5,26	89,1	Sorghum
7	5,2	4,1	17,69	0,12	1,1	3,6	2,9	6,1	87,4	LS	1	3	0,04	0,10	0,27	0,14	0,54	49,1	Soya
8	6,1	4,7	25,6	0,76	6,8	7,9	5,6	19	67,5	SL	2	4	0,05	0,88	3,13	0,17	4,22	62,1	Fallow
9	5,7	4,3	24	0,39	3,8	2,4	4	14,5	79,1	SL	1	3	0,03	0,54	1,69	0,17	2,43	64,0	Fallow
10	6	4,5	23,2	0,38	4,0	8,5	3,3	13,6	74,6	SL	1	3	0,03	0,47	1,96	0,16	2,61	65,2	Fallow
11	5,9	4,6	25,4	0,48	2,7	7,2	4,1	9,3	79,4	LS	2	5	0,16	0,33	1,20	0,18	1,87	69,1	Millet
12	6	4,8	88,7	0,35	4,1	9	8,5	9,8	72,7	SL	2	2	0,03	0,36	1,56	0,17	2,11	51,5	Fallow
13	6,8	5,8	40,4	0,47	4,3	5,9	3,1	10,8	80,2	LS	3	3	0,05	0,52	2,55	0,21	3,32	77,2	Sorghum
14	5,9	4,6	23,4	0,20	2,0	6,1	3,9	7,1	82,9	LS	1	2	0,01	0,20	1,00	0,13	1,35	67,4	Sorghum
15	6,2	4,9	27	0,51	4,0	18,5	12,9	11,5	57,1	SL	3	2	0,03	0,58	2,31	0,16	3,07	76,7	Orchard
16	6,3	5,1	17,38	0,15	1,4	6	2,5	6,2	85,3	LS	1	2	0,02	0,13	0,65	0,16	0,95	68,1	Rice
A	6,1	4,5	21,1	0,40	6,0	6,6	8,4	15,6	69,4	SL	2	2	0,04	0,88	2,88	0,21	4,01	66,8	Rice
B	6,2	4,7	42,9	1,07	14,4	17,4	29,2	35,8	17,6	SCL	1	2	0,10	2,59	7,72	0,45	10,86	75,4	Rice
C	5,9	4,5	37	1,56	16,2	9,8	26,7	32,3	31,2	CL	1	3	0,08	2,91	7,66	0,28	10,93	67,4	Rice
D	7	4,9	32,5	0,56	5,0	11,7	6,9	12,7	68,7	SL	2	2	0,02	0,68	2,53	0,54	3,77	75,3	Dam
F	6,4	4,6	33,9	1,09	12,2	15,5	24,1	24,6	35,8	LS	1	2	0,09	2,38	6,73	0,46	9,67	79,2	Rice
G	7,6	6,8	67,8	2,16	10,2	11,4	3,3	17,1	68,2	SL	3	4	0,24	1,23	8,11	0,15	9,71	95,2	Sacred Forest

sented here? I read on page 4114 that the carbon stocks are related to the number of plant species and that it is this relation that is dependent on the season. This I can understand, but I fail to see a direct link between SOC and sampling time. This may need clarification.

See comment to referee 1 on this. SOCS do not change with season, what changes with season is the relationship between SOCS and species richness. As the interaction Date x No. species is significant, the P-values of either Date or No. species are not meaningful by themselves, although by tradition those are usually still given in tables.

- In the same table 2, p 4122, it is not clear to me that the factor ’No. species’ has only one df? Technical corrections

Number of species is a quantitative variable, and so has 1 d.f.. This is not an ANOVA, but a regression model. Categorical variables are entered as dummy variables in those, as described in methods. Some statistical packages including SAS (used here) do not require previous conversion of the categories into dummy variables, and do this

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directly. This explains the distribution of d.f. In any case, the caption of the table has been changed slightly, so hopefully is now less confusing.

- Page 4123, fig 1: in the x-axis: G for January?

Corrected in the figure.

- Page 4125, fig 3: no reps for the forest site, makes it hard to use it as a baseline!

The soil under the sacred forest is not used as a baseline in a quantitative way, but rather as evidence that higher OM contents can be attained given the geoecological and pedological characteristics of the region. See comment to referee 1.

Conclusions

For all the above reasons, I find insufficient merit in this paper to have it published in this journal. Not enough novelty, questions about representativeness, the lack of sound recommendations fine-tuned with socio-economic realities and a poor presentation of he data.

We disagree with the claim of reviewer number 2 about the lack of novelty and merit of this work. In the previous lines we have tried to make evident why we consider so. We would suggest for instance a review of the literature linking SOC and species richness, just as an example of how novel our work is. Or the discussion about the wide range of results found when linking SOCS and land use, which suggests that more investigation in this area is highly needed. We truly believe that our work is a contribution to these areas.

Finally, we are very grateful to the reviewers, since they pointed out some questions that were not clear in the submitted manuscript. We have tried to address the concerns shown by both of them, and modify the text, particularly results and, in part, discussion and conclusions, accordingly.

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