Biogeosciences Discuss., 6, C3008–C3018, 2009
www.biogeosciences-discuss.net/6/C3008/2009/

Biogeosciences
Discussions

Interactive comment on "Belowground carbon pools and dynamics in China's warm temperate and sub-tropical deciduous forests" *by* C. W. Xiao et al.

C. W. Xiao et al.

cwxiao@ibcas.ac.cn

Received and published: 13 November 2009

Responses to referee comments

We acknowledge the referees for submitting such in-depth reviews of our manuscript. Both referees pointed out flaws in the earlier version (such as omission of reporting important results and discussion of relevant issues). We further thank the referees for the numerous constructive comments.

Responses to reviewer 1

Referee #1 raised 4 general comments and many specific and technical comments.

I. We first respond to the general comments. Comments raised by the referees are italicized:

" . . . there is some mixing of the results and discussion sections (see specific comments for an enumeration of these instances)." Ask asked by the referee, we removed all discussion from the results section (see details below)

" . . .there was a lack of discussion on topics critical to the conclusions made in the manuscript, such as -insufficient discussion of C:N ratios -lack of discussion of labile vs. recalcitrant fractions " In the revised manuscript we expanded the discussion on the influence of the C:N ratio and on the differential responses of labile vs recalcitrant SOC (Paragraph 3 in section 4.1)

" . . .Additional citations from current literature, especially related to other studies of this type, would be particularly useful in improving the discussion section of the manuscript. There were a few instances were assertions were made that were not properly supported by data or citations (see specific and technical comments for further details). " We added numerous citations to support our discussion (see details below)

" . . . I could not reproduce some of the calculations made (such as fine root turnover) with the information given in tables (need to give bulk density values), and explain how loss % y-1 was calculated. " Most of the requested extra information was provided in the Figures (e.g. bulk density in Figure 2; fine root biomass in Figure 5; fine root production in Figure 6). Because we already present 9 display items, we do not give the requested data again in Tables. We did add the formula via which annual mass loss % was calculated in the legend of Table 3.

II. Specific comments ". . . The introduction briefly outlines the general methodology for determining the carbon budget of the forests, but without detail on the methodological approach. It would be useful to know whether this particular approach has been used in other ecosystems (better connection to extant literature necessary), the idea behind the methodology (i.e., density fractionation isolates soil carbon pools with different turnover times), and the caveats to this approach." We added a sentence to the introduction (middle of the third paragraph) about the density fractionation with which why we adopted this approach. We also cite one older and one recent key-note paper on this topic. However, we did not want to go into detailed discussions about the pros and cons of our approaches in the introduction because this manuscript focuses on the comparison between three forests, and we assume that caveats in any of the methods would have occurred at all three sites. Potential caveats are discussed in the manuscript, but in the Discussion section, not in the introduction.

" The steady state assumption is used for many of the turnover time calculations later on in the paper. Although a brief land use history of the sites is given, it would be useful for the reader to have some idea about tree growth rate. " We have no information about tree growth rates at the sites, but do discuss potential issues with steady state assumptions later on. (see below).

" LF-OC and HF-OC are mentioned without giving a definition of what they are" The revised manuscript now contains a detailed definition of these terms (first paragraph of section 2.2) as well as some important references.

" Please clarify whether the conversion factor referred to in p. 6344, line 18 is a value from the literature, or whether it was determined in your lab." The conversion factor referred to in p. 6344, line 18 is a value from the literature (Vance et al., 1987). This is included in the revised manuscript.

" Please add a citation to the statement "The xylem of dead roots..." (p. 6345, lines 17-19)." A citation (Janssens et al. 2002) was added to the revised manuscript.

" Please clarify the statement "The fresh soil samples were processed with a 2-mm sieve" (p.6346, lines 11-12). Were the materials used in the litter bag left on the sieve after passing the soil through? " Yes, we adapted the text to clarify this.

" ... you mention that the litter bags were collected at various sampling intervals. In

what month were they first placed (i.e. when was day 0?)" The placement dates are now explicitly given.

" In your figure captions, you mention using a least significant difference test to compare means from one-way ANOVA. Please add this information to your methods section." OK

" did you conduct statistical tests to compare the k values shown in Table 3? If I understand correctly, the p values listed in the Table 3 text are referring to the goodness of fit of the exponential decay model. How was loss% year-1 calculated? Was it derived from the exponential model, or directly from measured data?" All this information was added to the manuscript in the legend of Table 3.

" The paragraph on p. 6348 that begins on line 7 should probably be moved to the discussion section of the paper." We did not move the section but turned it into a real results paragraph without any discussion. Discussion on these results was already provided and we wanted to avoid duplication.

"In line 8 of p. 6348, "differences in LF-OC were more pronounced: : :" is there another metric, like percent increase, that could be used to make this statement more quantitative, and illustrative to the reader? I do not necessarily agree with the statement without further textual support." As requested, we made this qualitative statement quantitative by giving the actual numbers. The reader are now likely to be more convinced by these results. (2nd paragraph of 2.1)

"In the last sentence of this paragraph (lines 10-11), the manuscript states that "differences in SOC availability to microbial decay were larger than those in SOC content." This definitely seems like it belongs in the discussion section of the paper, and is not necessarily clear to the reader without a figure or explanation of the logic behind the statement." This sentence has been completely revised (see two comments above)

" it is stated that "Differences in decomposition rates were, however, significant only for leaf litter mass, fine root mass: : :". How were these differences tested (see also previous comment on section 2.7)? Were there difference for all three forest types, or just in two of three?" As can be seen in Table 3, k values were different among all three species only for litter decomposition, but only in 2 of 3 species for root decomposition and LF-OC decomposition. This is explained in general terms in section 3.2, and can be seen in detail in Table 3.3.

" In p. 6349 line 1, is the manuscript referring to fine root biomass in all the soil layers? Is that to say that there is a difference in the fine root biomass of each soil layer tested against one another, or in the sum of biomass from 0-55 cm?" It was only significantly different for the integrated total. We thus altered the text to : "As with microbial biomass, integrated fine root biomass . . ."

" . . . fine root turnover is discussed, but there are no statistics or references to a table or figure where the data is shown. . .. " Because we already have 9 display items, and fine root biomass and production data were shown in Figures 5 and 6, we decided to report these data in the text. If the referee/editor would like us to create an additional Table, we would be willing to do so. . .

". . . states that fluctuations in branch litterfall were very little. Is there some place I can find this data? " We initially had plotted these data alongside leaf litter fall in Figure 1, but this meant three lines on top of each other slightly above the intercept, which was more confusing than informative. For this reason, we decided not to show these data visually. As with the previous comment, we are willing to show these data if the referee/editor would like us to.

". . . For residence times calculated in the paragraph beginning on p. 6350, line 17, were statistical tests done?" Statistical analyses were not done here because these estimates of turnover time were only made in support of the estimates of litter layer turnover time.

" 4. Discussion; 4.1 Page 6351, line 4 refers to chemical analyses done, but there was

no information about these in the results section of the paper." We would like to thank the referee for pointing out this omission. We have now included a new section on this in the results section: last paragraph of section 3.1

" . . . The statement on lines 6-7 attributes carbon cycling rates to litter quality, but could also speculate on the role (if any) priming plays in decomposition rates. " As rightfully requested by the referee, the revised manuscript includes some speculation on the potential role of priming in this observation. (2nd paragraph in 4.1).

"Line 13 refers to possible fungal dominance in microbial community what is the relevance of this statement? Please discuss further. " This sentence has been deleted in response to a comment by referee #2

"Line 17 refers to an "expected difference in C:N ratio." What do you expect the C:N ratio of the fractions to tell you about lability/decay constants? This part of the discussion lacks important citations and needs the assumptions to be clearly spelled out for the reader." As requested, we elaborated this discussion (3rd paragraph of section 4.1) and included key-note citations.

" On page 6352, lines 22-23 assert that SOC content differences are related only to the surface organic layer. On the contrary, data from Table 2 show that there is no difference in surface layer SOC pools. Please clarify this argument. " We acknowledge the referee for pointing this out. There was a grammatical error in this sentence that completely reversed its meaning. In the revised manuscript, this sentence has been corrected & clarified.

" In this section, please also comment on the possible role of belowground inputs on turnover rates." This was already discussed in the manuscript, but probably was unclear because of the error pointed out in the previous comment. The new text states clearly that fine root turnover correlates well with LF-OC.

"4.3 The discussion of the SMB-C and SMA variations with SOC stocks and temperature optima might be strengthened by relating it to current research, such as Bradford et al. (2008)" This section was extended, but only slightly because it remains pure speculation.

"4.4 The turnover rates reported in page 6354, lines19-22 should be reported in the results section of the paper. " These data are shown in the last paragraph of section 3.3

"The statement beginning on line 25 about the relationship between the root turnover rates in Asia white birch compare to East-Liaoning oak should be clarified and expanded. How might nutrient cycling affect root turnover? This is not obvious, and needs further explanation by the authors." This is elaborately discussed in the 3rd paragraph of 4.4.

III. Technical comments "2. Materials and Methods, 2.1 Please add a further justification and a citation for why clay-poor soils are necessary for these types of studies (p. 6341, lines 21-23)." This was a mistake: similar clay contents are needed, not low clay contents. We rephrased this sentence accordingly.

"2.2 In p.6347, line 15, a "sharp-edged metal cylinder" is mentioned. How was the cylinder inserted? Was a coring device used? (Proper bulk density sampling technique is not trivial)." This paragraph was extended to give more details

" Line 18 should read "cleaned of," not "cleaned off." Line 23 should read "after standing overnight" not "after overnight standing." Line 27 should refer to "ground soil," not "grinded soil."; 2.3 Page 6344, line 11: "cleaned of" not "cleaned off". 3. Results 3.1 In p. 6347, line 16: change "interrupted" to "until." All these minor comments were changed in the revised manuscript.

"In p. 6348, line 4 refers to Fig. 2; Table 2. This should be changed to Table 1 only. We believe the reviewer is wrong here and did not change the manuscript.

"4. Discussion 4.1 On page 6351, the statement appearing in lines 6-7 about decom-

posability of litter is awkwardly written." We rephrased the sentence

"4.2 Page 6352: the statement made in lines 2-3 needs a citation. Line 3: "SOC densities…" The word "density" is confusing in this context. Please change to "SOC content." Line 16: "positive effects of the better sub-tropical…" Please choose a different way to say this, such as "positive effects of the more favorable to decomposition sub-tropical…" Line 19: "primordial role" does not make sense. Try "critical role" or "central role." Line 28: "does not differ in mass" should be changed to "does not differ in carbon content." Page 6353, line 1: "quasi completely" should be changed to "almost completely." We adapted the manuscript accordingly. The statement in lines 2-3 was revised and a citation appears unnecessary now.

"4.4 Page 6355, line 2: please add a citation to support the statement that warmer and wetter conditions favor root production." A citation "Gill and Jackson, 2000" was added to support the statement that warmer and wetter conditions favor root production.

Responses to reviewer 2

General comments: "The manuscript would benefit from a more detailed discussion on uncertainties, particularly with respect to assumptions and methods" As suggested by the referee we added numerous sentences and paragraphs to the discussion in which we highlight potential sources of uncertainty in our manuscript (see details below).

" and from addressing possible leaks in the elemental budget" As requested, we included a paragraph and the effect of leaks in the C budget (see below for details)

Detailed comments: "Site description: I do not understand why low clay content should be a prerequisite for allowing comparisons of SOC pools. Similar clay contents are helpful, but there are too many other drivers of soil C that differ between sites. It just makes data interpretation easier. " We agree; in fact this was a grammatical mistake. Our intention was to state that similar clay contents were a prerequisite. The sentence was corrected.

“Soil FAO-UNESCO classification. I use the more recent FAO/WRB 1998 but assume that ‘mountainous brown soil’ is not a regular class according to FAO.” As requested we applied the FAO claasification in the revised manuscript

“Methods 2.2: Does sampling include the litter layer? Please clarify. “ Yes, we rewrote this paragraph to be more clear on this.

“LF-OC and HF-OC must be spelled out first time and the concept behind physical fractionation to be explained in brief. “ OK, this was included

“P. 6344, L 20. Was sieved soil used for microbial activity?” Yes, this is now detailed.

“P. 6345, L. 2. Curious to see a reference from 1889; is there anything more recent (litter layer type may have changed since then)? “ The nomenclature was first introduced in 1889 so why not give credit to the person who deserves it?

“P.6346, L. 3. Beginning of sentence should be ‘Fine root turnover rate ...’ “ OK

“Some details on the lignin methods would be helpful as there is a wide array of methods to quantify lignin in soil; not all approaches are reliable.” OK, lignin measurements were described.

“P. 6350, L. 10. ‘smaller ‘ should be replaced by ‘shorter’ P. 6350, L. 14. Word ‘floor’ should be added behind ‘forest”’. OK.

“Discussion: The smaller C/N in HF-OC from my point of view just indicates a higher share of microbial derived products and I think that parameter alone does not allow making assumptions on the microbial community. “ We agree and have deleted this sentence.

“Cited LF turnover times may be realistic for temperate croplands, but data from other biomes (eg. Trumbore et al., Schulze et al, Leifeld et al., see references) indicate much longer turnover times for similar fractions. This section needs further elaboration and a closer connection of the measured data with the literature. HF is stabilized no only

through microaggregation, but, more importantly, through surface interactions and its turnover time is in the range of decades to millennia. “ As requested, we elaborated this part of the discussion. (paragraph 3 in 4.1)

“P.6352, L. 2. Stabilization potential is not only driven by the clay content, but equally by mineralogy (e.g. Six et al.).” We agree and have included this assumption explicitly in the revised manuscript. We refer to the Six et al paper. Earlier in the manuscript.

“P. 6352, L. 18. Authors argue that site climate for two of the sites is similar. Even under similar regional climate, the stand climate is affected by differences in ET, biomass, shading et.c. I suggest arguing more carefully here.” As requested, we softened the strength of our statement.

General: “C-budget approach I. The authors assume steady-state conditions in their turnover calculations. The stand age is between 55 and 60 years (secondary forest?) and steady-state is unlikely, particularly for the C stock. A discussion on uncertainties regarding steady-state assumptions is missing.” The revised manuscript now clearly indicates where results are uncertain because of steady state assumptions (1st paragraph in section 4.1; second paragraph in 4.2; and 3rd paragraph of 4.2.). However, a direct discussion of these uncertainties was not possible because the uncertainties are too uncertain.

“C-budget approach II. Sites have in-clinations of between 28 and 32_ which may induce erosion particularly of forest litter after heavy rainfalls. Is there any C-export by erosion? We observed no indications of forest litter export by heavy rains, but did nonetheless explicitly state this potential source of uncertainty twice in the discussion.

“C-budget approach III. Input estimates are based on litter fall and fine root turnover using in-growth cores. Without being an expert in that latter technique I suppose that root dynamics in root-free soil will differ from that of an undisturbed soil. How reliable is this technique and how does its results compare to other techniques such as 14C? The paper would benefit from some discussion on that.” We have included an entire paragraph on uncertainties related to the in-growth bag technique (3rd paragraph in 4.4), but did not expand to other techniques because this would dilute our paper too much, we believe. After all, we apply so many techniques that if we would go deep into each of them we would end up with too long a paper.

“C-budget approach IV. Litter bags were used to derive decomposition rates of several carbon fractions. Since some of the C leaves the bag as dissolved or even particulate matter, mass loss rates may be overestimated.” We allocated the last section of the first paragraph of section 4.1 on the uncertainty introduced by using this technique, without elaborating further on methodology.

Interactive comment on Biogeosciences Discuss., 6, 6339, 2009.