**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.**

Anonymous Referee #1

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I. The manuscript falls within the scope of Biogeosciences, and represents an advance to the state of knowledge of soil carbon pools in forest ecosystems in east Asia. The authors attempt to quantify soil carbon pools and inputs in three forest types, representing two climatic zones (temperate and sub-tropical) and two forest types (birch forest and oak forests). These forests were chosen in order to examine the effects of climate and litter quality (as represented by chemistry of litter input across two different genera of trees). In particular, the authors attempted to (1) determine SOC pools and components in the 3 forest types, (2) to determine carbon cycling rates through SOC pool components. The work is generally successful at achieving these aims, however, there are some problems with the manuscript that prevent me from recommending the

work for publication at this time.

The data presented are quite interesting to the readers of BGD. However, at this time, the manuscript does not fully convince the reader of the conclusions made. First of all, there is some mixing of the results and discussion sections (see specific comments for an enumeration of these instances). Also, 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 (how they are defined, the meaning of the fractions, etc.) -insufficient discussion of density fractionation method

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). Moreover, 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. Overall, the presentation quality was high, with appropriate tables and use of language (except for a few minor changes, listed in technical comments section).

II. Specific comments

1. Introduction: 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.

2. Materials and Methods

2.1 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

2.2 In p. 6343, line 14, LF-OC and HF-OC are mentioned without giving a definition of what they are. There should be some attempt to familiarize the reader with the meaning of these abbreviations beforehand.

2.3 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.

2.5 Please add a citation to the statement "The xylem of dead roots…" (p. 6345, lines 17-19).

2.6 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? In line 17, 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?)

2.7 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. Also, 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?

3. Results

3.1 The paragraph on p. 6348 that begins on line 7 should probably be moved to the discussion section of the paper. 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. 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.

3.2 In page 6348, lines 24-25, 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?

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? In lines 17-10, fine root turnover is discussed, but there are no statistics or references to a table or figure where the data is shown.

3.4 Page 6349, line 23, states that fluctuations in branch litterfall were very little. Is there some place I can find this data? For residence times calculated in the paragraph beginning on p. 6350, line 17, were statistical tests done?

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. 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. Line 13 refers to possible fungal dominance in microbial communityâĂȚwhat is the relevance of this statement? Please discuss further. 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.

4.2 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. In this section, please also comment on the possible role of belowground inputs on turnover rates.

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)[1].

4.4 The turnover rates reported in page 6354, lines19-22 should be reported in the results section of the paper. 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.

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).

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). 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." In p. 6348, line 4 refers to Fig. 2; Table 2. This should be changed to Table 1 only.

4. Discussion

4.1 On page 6351, the statement appearing in lines 6-7 about decomposability of litter is awkwardly written.

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."

4.4 Page 6355, line 2: please add a citation to support the statement that warmer and wetter conditions favor root production.

1. Bradford, M.A. et al., (2008) Ecology Letters, 11: 1316-1327.

Please also note the Supplement to this comment.