

## ***Interactive comment on “Soil organic carbon dynamics under long-term fertilizations in arable land of northern China” by W. J. Zhang et al.***

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We appreciate the reviewers' constructive comments. We have made a major revision accordingly, including considerable rewording in all sections and rewriting of the Discussion section. For those comments that lead to rewriting of a few sentences, we only provide brief responses here to avoid massive paragraphs. On the other hand, some elements were dropped off during the major revision. Thus, some of the following comments are not applicable (N/A) anymore.

Responses to Reviewer 1#

General Comments:

This study is significant because it is a long-term, multi-site study. Because of this the  
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authors are able to make generalizations about carbon sequestration under different management regimes over large geographical areas. More studies, such as this one, are needed to truly evaluate the ability of soils to sequester carbon.

Specific comment

Comment 1: There was only one large pattern that was overlooked in this paper. Cropping (double vs mono), climate, and soil texture are all confounded. The mono-cropping systems are all in the temperate/colder climates and have 2 to 3 times the clay content of the double-cropping systems in the warm climates. They find carbon sequestration rates to be highest in the mono-cropping sites with the high clay content. They relate carbon sequestration rates to temperature and annual precipitation, but they do not mention the fact that clay content could also be driving this pattern.

R: The reviewer is correct. Clay content may also affect soil carbon sequestration. We have added a paragraph in the Discussion to address this issue.

Comment 2: They state that they have no evidence of carbon saturation (line 353), but in Figure 4(f) the manure addition treatment do look as though they asymptote at about 13 years. This seems to be mild evidence for possible carbon saturation.

R: We agree with the reviewer. Our early statement was not accurate. We have reworded accordingly.

Technical comments:

Line 113-116: Is this residue management just for wheat or also for corn?

R: We have provided details in the Materials and Methods section.

Line 146: When was manure applied?

R: We have added the information in the Materials and Methods section: 'Manure was applied before seeding once a year for all sites. For the double cropping system, manure was applied before wheat seeding'.

Line 149: There is no mention of plot replication within sites. How many plots were there for each treatment in each site?

R: We have given relevant information in Table 2, and in the text of the Materials and Methods section.

Line 172: Organic carbon inputs by roots were set at 30% of above-ground biomass. Recent publications have estimated the average root input for corn at 16% and for wheat 59%. Therefore, the average for these two crops would be 37.5%.

R: We used the percentage from a few previous studies (i.e., Chander et al., 1997; Kuzyahov and Domenski, 2000; Kundu et al., 2007). We have also tried a range of percentages, e.g., 10%, 30%, and 40%. The conclusion is robust no matter what percentage is used.

Responses to Reviewer 2#

General comments

While this paper addresses an important topic, it does not convey the new information precisely. The main shortcoming has to do with the discussion section where the authors failed to justify the reasons their several results.

R: We agree with the reviewer. Thus we have made a major revision. Particularly, we have rewritten the Discussion section to address the reviewer's comments.

Q1. Why the application of manure has no significant effect on the above-ground C biomass at most of the sites (Page 6547; Line 26-27)?

R: This is a good question. We have discussed this topic in the 4.1 section. In addition, we have compared our results with other studies.

Q2. Why there is a decreasing trend in SOC under the inorganic N fertilization at most sites (Page 6550; Lines 22-23)?

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R: We have addressed this issue in the Discussion section during the major revision.

Q3. Why results indicate that soils high in clay in mid-temperate has higher conversion rate than that in the warm temperate area with low clay content, what is the role of clay content?

R: We have added a plot (i.e., carbon conversion rate vs. clay content) in Figure 7 and relevant text in the Results section, and one paragraph in the Discussion section to address this issue.

Q4. Why for all treatments organic C contents in corn and wheat roots were taken as 39.9 g/kg and 44.4 g/kg, respectively (in the M& M Section)?

R: This is a good question. One would use different values for different treatments. However, there were no data available. Thus, we used the national average values from the NCATS (1994). We have reworded this sentence to clarify this point.

The challenge for this paper will be to show its novelty and uniqueness relative to other studies conducted in other parts of the globe.

R: We have added some discussions regarding the novelty and uniqueness of our study during the major revision.

Why significant negative relationships were found between the conversion rates and climatic conditions? This point should be discussed in the introduction and conclusion sections.

R: This is a good suggestion. We have added a paragraph to discuss this point in the Introduction, and further discussed in the Discussion section.

What are the conclusions and suggestions for future research related to the above-mentioned fact!

R: We have rewritten the Conclusion section, which includes the main conclusions of our study and suggestions for future studies. For instance, we state that further studies

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are needed to better understand the different roles and mechanisms of each factor (e.g., cropping, soil texture, and climatic conditions) on soil carbon sequestration.

Thus, I urge the author(s) to change focus and concentrate on the 4 above mentioned aspects during the write-up and their suitable explanation with clear hypotheses and objectives.

R: We have made a major revision in response to the reviewers' comments.

Please indicate/stress how this work differs from others and why it is needed in introduction.

R: Done.

The overall language (English) of this paper is not strong.

R: We have made a major revision, including improvement in English.

Specific comments

Abstract.

Please start with a single sentence: Why this study was needed?

R: We have rewritten the abstract accordingly.

Line 1, long-term experimental sites. Line 9 and elsewhere, all sites instead of all the sites

R: Done

Introduction Page 6541, lines 8-10, lines 20-23 and elsewhere) the references are not in a proper sequence.

R: We will follow the Biogeosciences style.

Page 6542, line 5, topsoil instead of top soil.

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R: Done

Why did not you mention about the role of climatic conditions on SOC sequestration in China?

R: We have discussed the role of climatic conditions on SOC sequestration in China in the Introduction section during the revision.

M&M.

I suggest not using abbreviations to denote several places. It is very difficult for the readers to remember!

R: Done.

Information related to depth of sowing, spacing and seed rate is needed.

R: We have added the relevant information in the Materials and Methods section.

The experiments were conducted earlier. Then why did you use present tense in this section?

R: Corrected.

Page 6543, line 11, continuous instead of continues.

R: Done

Why CK was used to denote control treatment? Why not simply control?

R: Corrected.

The plot sizes are huge? How did you sample the representative soil sample? In which year the samples were taken? Were they not in the same time?

R: We have added all relevant information.

Available K estimation by Shi (1976) looks odd!

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R: The measurement of available K was in the initial year (i.e., 1981-1990) of the experiments. The method of Shi (1976) was later modified and embodied in Soil Science Society of China (2000). We have added the latter reference.

I cannot understand the logic behind Eq.(1)! Any ref.?

R: Eq (1) was actually a simple unit conversion. We have reworded relevant sentences to clarify this point.

Results.

Page 6547, line 1 and elsewhere, decreasing trend instead of decline trend!

R: Done

What are the properties of the desert soil?

R: We have added relevant information in this section.

Page 6548, lines 3-5, revise the sentence.

R: These two sentences were reworded.

Page 6548, after table 4, reference of table 7 has come in the text. Where are the contents related to the other tables?

R: N/A.

The sentence: 'Table 7 illustrates that' reads very monotonous. No need of all these words.

R: We have reworded this sentence.

Page 6549, lines 16-20, these lines would better suit in the discussion section.

R: Done.

Page 6548, lines 1-3, pl. be consistent in using either C or carbon. So many 'there are'

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and 'there is' in this section.

R: We have rewritten this section.

Discussion.

In some instances the statistical inferences were ignored. Only discuss the similarities or lack thereof based on statistical inferences. Restructure the discussion section, refocus it so as to address the objectives. The discussion section should integrate all results to explain concept; hypotheses, and objectives.

R: We agree. We have restructured and rewritten the Discussion section.

I suggest to use mineral fertilization instead of inorganic fertilization throughout the ms.

R: Done.

Pl. avoid we, our etc: Punctuations should be properly given when 'whereas' is used.

R: Revised as suggested

Page 6552, line 1, what is recommendable? Lines 19-20: Is there any doubt?

R: N/A

Page 6553, line 1, the conversion rate of 31% is really very high? Why so? Did you measure C data twice to double check it?

R: Soil sample analyses were conducted with three replicates. We have discussed this issue in the Discussion section 4.2

Conclusions.

Pl. give stress on the novelty as suggested.

R: We have rewritten the Conclusions section.

Table 3. Footnotes can go to the text.

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R: Done

Why not SI units were used?

R: We have used the unit of t ha<sup>-1</sup> for most carbon calculations (e.g., above-ground carbon biomass, carbon input and carbon sequestration rate).

Table 4, column 5, is it organic C content in manure or C addition by manure? I think the first one is correct. If so, it is not clear. I suggest all columns should be clearly written.

R: The reviewer is correct. We have made changes accordingly.

Table 5. SOC indicates soil organic carbon.

R: Done.

Table 6. Did you test significant differences across sites?

R: No, we did not conduct such a test. The main reason was that there were considerable differences in experiment designs across these sites, e.g., cropping system and fertilization rate.

Table 7. Why negative correlation in NPK treated plots? Title is not clear! What is SOC vs. fertilization time?

R: We have reworded the title and footnote to clarify these points.

Table 8. Surface means at what depth? When these samples were taken?

R: We have reworded the title of Table 8, and provided details of sampling in the text to clarify these two points.

Fig. 2 and 4. These figures may be enlarged. SI units should be used.

R: Done.

Fig. 3. Estimated average annual C input instead of averaged annual C input.

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R: Done

Fig. 6. What is the 'n' in these figures?

R: We have given the 'n' in the plots.

Fig. 7. Why a and b are in bold?

R: We have made changes in Figure 7.

How did you calculate conversion rate of C input and annual accumulative temp.? Please mention these in M&M section.

R: We have provided details of the calculations in the Materials and Methods section.

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Interactive comment on Biogeosciences Discuss., 6, 6539, 2009.

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