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Interactive comment on "Soil organic carbon in the Sanjiang Plain of China: storage, distribution and controlling factors" *by* D. Mao et al.

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Dear editor, We have received the comments on our manuscript entitled "Soil organic carbon in the Sanjiang Plain of China: storage, distribution and controlling factors" (bgd-11-14765-2014). We are very grateful for having the opportunity to revise our paper. We thank the referee for the constructive comments and suggestions, which have improved the quality of this manuscript. We have tried our best to address these comments. Our responses to the referee's comments are attached. We hope you would be satisïňĄed with the revised manuscript. If you have any questions about this paper, please feel free to contact us.

Our responses to the comments from anonymous referee #3 are as follows: General Comments: "This manuscript reported the data of soil organic carbon in a region with

intensive agricultural activities. The SOC storage in various ecosystems and controlling factors are of importance in quantifying regional carbon budget as well as developing/validating carbon cycling model. This study is appropriate for Biogeosciences. However, some results were poorly presented, and some patterns were lack of meaningful analysis. Therefore, many parts of discussion read weak and quite arbitrary. Discussion section was poorly written. Some statements should be made very carefully, especially the implications related to climate change. This current version needs major revision before it can be published. The English in the manuscript needs more editing as well." Response: We appreciate the positive and detailed comments from the anonymous referee #3 about our manuscript. The manuscript was revised carefully following the comments, especially focusing on the results and discussion sections. Additionally, we called for an English language editing service from Elsevier WebShop. And one of co-authos, Prof. Lin Li who worked in the Indiana University - Purdue University at Indianapolis (IUPUI), USA revised this manuscript in English grammar, punctuation and diction, once again. Detailed responses were concluded as follows:

Specific comments Comment 1. "Section 1, Line 3-13, Page 14767 – although there are several references listed, it provides little information. The cited data do not look like pointing to the statement of'These estimates of SOC based on ïňĄeld samplings suggest a large difference of SOC in storage and distribution.' Since this study investigated the SOC storage in different ecosystems, a brief literature review about the SOC storage in similar ecosystems from previous studies would be helpful. With a brief picture about the SOC in various ecosystems, readers could understand better the characteristics of the target area of this study." Response: Thanks for this comment. We haven't added the literature review about the SOC storage in similar ecosystems, because these sentences in this paragraph have been rephrased to highlight that a necessity of improving SOC estimation at regional scales to achieve accurate updating of the world and national SOC budget. The highlight was used to instead of the sentence "These estimates of SOC . . .in storage and distribution". Therefore, more literature reviews about the SOC storage in similar ecosystems from previous studies

aren't needed here.

Comment 2. "Section 1, paragraph 3 and 4 can be combined and shortened. Little information was provided in paragraph 4. Line 2-4, Page 14768 was just repeating the point in paragraph1." Response: We agree and thank the referee for this kind suggestion. Paragraph 3 and 4 have been combined and sentences in the combined paragraph have been revised.

comment 3. " Line 21-23, Page 14768, delete or could go to the 'Methods' section." Response: We agree and thank this referee's comment. The sentence in line 21-23, page 14768 has been deleted.

Comment 4. "Section 2.2, Line 14-23, Page 14769, a little more details about the GIS analysis would be useful. Although the method has been published by the author in another journal, it is better to have a brief summary here." Response: Thanks for this helpful comment. Summary sentences about the GIS analysis have been added here, such as "Area for each land cover type was calculated through the ArcMap software"

Comment 5. "Section 2.2, Line 25, Page 14769, when did the second soil survey happen? Add references for it." Response: Thanks for the suggestion. The second soil survey was carried out from 1980 to 1985. A literature has been referenced here.

Comment 6. "Section 2.2. Since the authors did not present the GIS classiïňĄcation information as part of the results, you could present the results in this sectionâĂŤthe area information of each land cover type and each soil type. I noticed the area information was presented in Table 1, and Fig. 2 has both information. It is better to brieïňĆy interpret with text. Or at least have these information in the ïňĄgure caption." Response: We appreciate this kind advice. Some briefly interpretations with text have been given in section 2.2 for introducing the area information of each land cover type and soil type.

Comment 7. "Section 2.3, unclear. Describe the design of sampling method clearly –

based on the "land-cover" and "soil types", set up "plots", collect 'replicates'. . .. . . Clarify what exactly one 'sample' means. Does a complete soil proïňĄle (i.e. 3 layers) mean one sample, or each layer of each replicate means one sample?" Response: Thanks for this suggestion. Related information for soil sampling method have been rephrased. The word 'plot' was replaced with 'site'. At a soil sampling site, there are three soil profiles, and each profile has three soil depth ranges (0 - 30 cm, 30 - 60 cm, and 60 - 100 cm). The values of SOCD in the same range of three profiles at each soil sampling site were averaged to be the SOCD for the range of the soil site. In the revised manuscript, the sentence has been revised as: "For each soil site (three soil profiles at each site), the SOC content for each depth range (i.e. 0 - 30 cm, 30 - 60 cm, and 60 - 100 cm) was represented by the average of SOC values of three spatially random profiles at the sampling site."

Comment 8. "Section 2.4, Line7-9, Page 14771, the ïňĄrst sentence already mentioned that 12 Russian stations were included. Reorganize." Response: We agree and thank the referee for this advice. This sentence has been reorganized.

Comment 9. "Section 2.5, should provide details about the fertilization. What is the difference in fertilization (amount, fertilizer) between dry farmland and paddy ïňĄeld? The effects of fertilization on the SOC storage, I think, could be the most valuable information provided by a study in such a region. However, this is the weakest part in the manuscript. This issue might not be the authors' top concern, so comments related to this point are just suggestions to the authors. But I would suggest the authors put more efforts on it." Response: We agree and thank the referee for this advice. Generally, fertilization can raise the SOC storage by enhancing the carbon input from plant productivity and crop biomass. However, over application of fertilizer can have negative net effects on carbon sequestration because organic carbon mineralization neutralizes the carbon input. Influences of fertilization on SOC are complicated, and can be related to the history of cropland, cropland types, as well as soil types and texture. Long-term field experiments for different crop types are needed to investigate

the effects of fertilization on SOC at the local scale. We accept this comment and will put more efforts on the study of fertilization on SOC in the future.

Comment 10. “Section 3.4, Line 12-13, Page 14774, this pattern might not be true. The data points did not really exhibit such a decreasing-increasing pattern. It was more likely constant at higher MAT. Choosing a polynomial equation seems quite arbitrary.” Response: We appreciate the referee for this kind suggestion. Although the pattern of data points was more likely constant at higher MAT, the polynomial equation was selected because of the largest regressive coefficient compared to other regression models between SOCD and MAT. MAT is often lower than 4.6 âĎČ in the Sanjiang Plain. The trend that a decrease in SOCD with increasing MAT was thus dominated. Yang et al. (2007) revealed that the increasing trend of SOCD from the tropical to cold-temperate zone in the eastern part of China is correlated with temperature. Therefore, the polynomial equation in our analysis could be explained.

Comment 11. “Section 3.4, Line 1-2, Page 14775, typo? This was opposite to what the data reïňĆected, and also opposite to the interpretation at Line 20-21, Page 14778.” Response: Thanks for the positive advice. These sentences in page 14775 have been revised as: “When comparing temperature with precipitation, the former exhibits more significant effects on the SOCD within the depth range 0 - 100 cm than the latter as shown by a regressive coefficient (Fig. 6 A3, B3) for temperature and a more variance of SOCD explained by temperature (Table 2).”

Comment 12. “Section 3.5, Line 12-13, Page 14775, this sentence could go to the ‘Methods’ section, as comment 6.” Response: We thank the referee for this suggestion. This sentence has been moved to the section “Data and methods” in the revised manuscript.

Comment 13. “Section 3.5, Line 22-23, Page 14775, should the larger SOC content be SOCD? You referred to Table 1 and Fig. 8, but the two datasets look different – the SOCD in Table1 and the SOC content in Fig. 8. Clarify them. Also, the pattern

of ‘paddy ïňĄeld had a larger SOC content than dry farmland’ might not be true. If the authors only compared the mean SOCD between the two land cover types, the difference was meaningless. An ANOVA analysis at least should be done for making such conclusion.” Response: Thanks for the kind suggestion. SOC content means the ratio of soil organic carbon to soil organic matter. Based on equation 1 in section 2.6, SOCD is proportional to SOC content. Therefore, paddy ïňĄeld can be recognized to be having a larger SOC content than dry farmland in our manuscript. We thus didn’t done the ANOVA analysis. For a better discussion about the relationship between areal proportion of paddy fields to croplands and SOC content, we rephrased the sentences in section 4.5. The detailed contents are as follows: The results of this study indicate that paddy fields show a relatively larger carbon sequestration capacity as compared to other agricultural soils in the Sanjiang Plain (Table 1). As displayed in Fig. 8, the areal proportion of paddy fields to croplands is strongly correlated to the mean value of the topsoil SOC content in different counties ($P < 0.01$). Irrigation-based rice cultivation in China has significantly enriched SOC storage in paddy soils when compared with dry farmland cultivation (Pan et al., 2004).

Comment 14. “Section 3.5, Line 24-26, Page 14775, I don’t understand the objective of this relationship analysis.” Response: We thank the referee for this comment. In this study, paddy fields show larger SOCD values than dry farmlands, and the areal proportions of the two land cover types are thus related to SOC storage. As one type of typical agricultural activities, the areal proportion of paddy fields to croplands is compared to topsoil SOC content on the county scale. The analysis was deveopled to discuss the agricultural activities on the pattern of SOC.

Comment 15. “Section 4.1, Line 11-14, Page 14776, you used method different from that published earlier. What was the implication of the comparison? Any weakness of Yang’s method or any strength of your method? What is the contribution of your study?” Response: Thanks for the positive comment. In our manuscript, the method used in this study was compared to other publication. Selecting a suitable method is

essential to map the spatial distribution of SOC and quantify the SOC storage in the Sanjiang Plain. Therefore, the remote sensing vegetation index method was compared with the Geostatistical Kriging interpolation used in this manuscript. Remote sensing VI method isn't selected because of the bad correlations between SOCD and Vis induced by rich ecosystem types. In the revised manuscript, these sentences were rephrased to improve understanding the comparison. The method that was used for estimating the regional carbon pool in the present study is different from that used by Yang et al. (2008), who estimated SOC storage by correlating SOC content with a remote sensing vegetation index. Considering the rich ecosystem types of the Sanjiang Plain and coarse resolution remote sensing imagery, this study used the Kriging method to achieve more accurate estimation of SOC than those by previous studies.

Comment 16. "Section 4.1, Line 22-23, Page 14776 and Section 4.3, Line 17-18, Page 14779, the authors compared the Sanjiang Plain area with the Loess Plateau twice, but explained with different mechanisms. While it is reasonable that several reasons caused the difference, the authors should consider the context, not just treat them independently. Also, why chose the Loess Plateau to compare?" Response: We thank the referee for this comment. And the sentence in page 14776 has been revised to improve the discussion. Same mechanism has been used to explain the SOCD difference in the Sanjiang Plain and Loess Plateau. Dry climate leads to low natural vegetation cover in the Loess Plateau. Both climate and vegetation affect the SOCD in the two regions. In our manuscript, the SOC in the Sanjiang Plain with temperate continental climate was compared to that in different regions on the earth, such as Loess Plateau in China. The Loess Plateau in China located in an arid zone has a drier climate than the Sanjiang Plain. Different climate types induces the variances of vegetation type and distribution. Therefore, the SOCD in the Sanjiang Plain was compared with SOCD in the Plateau to discuss the effects of climate factors and vegetation on the pattern of SOC. This comparison also demonstrated the necessity of regional quantification of SOC. Additionally, the comparison was developed following the comments by the editor who recommended us to add comparison with results from other regions on the total

SOC amount and controlling factors obtained in this study. In our revised manuscript, we have added some sentences to display our objective or topic about those comparisons.

Comment 17. "Section 4.1, Line 8-12, Page 14777, reads weird in here. Combine it with Section 4.2." Response: We agree and thank the referee for this advice. These sentences mentioned in the comment have been combined with section 4.2.

Comment 18. "Section 4.2, Line 11-14, Page 14778, rough. If root distribution is the primary driver of both the vertical pattern of SOC storage and the relationship between SOCD and environmental factors, make the interpretation clear. Reorganize the discussion." Response: Thanks for this kind advice. The correlations of SOCD with the examined environmental factors decrease with the soil depth. This could be related to the change of vegetation types. Vegetation affects the lateral and vertical patterns of SOC through the distribution and production of above- or below-ground biomass. Related sentences in page 14778 have been reorganized in the section of Discussion "4.2" in the revised manuscript.

Comment 19. "Section 4.3, Line 25-26, Page 14778, over-interpretation of the pattern. See comment 10." Response: As response to comment 10, although the pattern of data points was more likely constant at higher MAT, the polynomial equation was selected because of the largest regressive coefficient compared to other regression models between SOCD and MAT. MAT is often lower than 4.6 âĎČ in the Sanjiang Plain. The trend that a decrease in SOCD with increasing MAT was thus dominated. Related sentences in page 14778 have been rephrased in the revised manuscript.

Comment 20. "Section 4.3, Line 20-22, Page 14779, not clear. I don't understand how 'improved NPP induced by increasing MAP' caused 'less carbon input in deep soil layer'." Response: We appreciate the referee for the positive advice. This sentence has been rephrased to explain the decreased correlation with SOCD. MAP decreasingly explained the variation of SOCD with increasing soil depth (Table 2) and displayed

a decreased correlation with SOCD (Table 3). This can be attributed to relative low soil moisture to deep soil depth layers which affects the root vertical distribution with increasing soil depth (Jobbágy and Jackson, 2000).

Comment 21. "Section 4.4, Line 3-5, Page 14780, any references?" Response: Thanks for this helpful comment. One literature has been added.

Comment 22. "Section 4.4, Line 18-20, Page 14780, any references?" Response: Thanks for this kind suggestion. One literature has been added.

Comment 23. "Section 4.5, Line 17-20, Page 14781, this statement has to be carefully made. Paddy rice ïňĄeld might have less CO2 emission, but it is one of the main sources of CH4. Did Chinese government really make such a policy because of this?" Response: We appreciate the referee for this kind suggestion. We accept this comment and rephrase related sentences to give the statement. Revised sentences are as follows: "The conversion of dry farmlands into paddy fields in the Sanjiang Plain, which is enforced by governmental policy and stimulated by economic benefit, has fostered the local carbon accumulation and mitigated climate change by reducing CO2 emission. Additionally, one literature has been added to support this sentence."

Comment 24. "Section 5, Line 8-11, Page 14782, although your estimates were higher than the literature values, there was no discussion in the manuscript to support this conclusion. Why your method is better? Could I say your results overestimated the SOC storage?" Response: Thanks for the positive comment. This conclusion has been rephrased and more discussion have been given to support this conclusion in the revised manuscript. This study resulted in the total estimated SOC storage 2.32 Pg C within the soil depth range 0 - 100 cm in the Sanjiang Plain. Similar estimations yielded 26.43 Pg C for the Northeast China (Wang et al., 2003) and 69.10 Pg C for the whole China (Wu et al., 2003). Converting these two SOC storage values to SOCD based on related publications would give rise to SOCD values of the Sanjiang Plain, which are smaller than the SOCD result observed in this study. Our results reveal that

the farmland has a SOCD value smaller than those for the forestland and wetland. Fig. 6 show negative correlation of SOCD with temperature and positive correlation with precipitation. Additionally, the Sanjiang Plain experienced significant losses of both forestland and wetland to farmland, obvious increases in temperature, and notable decreases in precipitation (Wang et al., 2011; Song et al., 2014). All these factors should contribute to the loss of SOC storage. Therefore, we are confident that the present SOCD estimation is more close to the actual SOC storage in the Sanjiang Plain, and the previous reported SOCD for the Northeast China and the whole country level underestimated the SOC storage.

Technical corrections: Correction 1. Line 16, Page 14768 – translation? conversion? Response: The word "translation" has been replaced with "conversion".

Correction 2. Line 17, Page 14775 – reparable? What does this mean? Response: The word "reparable" has been replaced with "remarkable".

Correction 3. Line 14, Page 14780 – circle? cycle? Response: The word "circle" has been replaced with "cycle".

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/11/C7329/2014/bgd-11-C7329-2014-supplement.pdf

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