

Responses to the comments from referees

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 like to thank the reviewers for their constructive comments and advices, which have improved the quality of this manuscript. We have tried our best to address these comments. Our responses to the reviewer’s comments are attached. We hope you would be satisfied with the revised manuscript.

If you have any questions about this paper, please feel free to contact us.

1 Responses to the comments from Prof. Ding

Comment 1: “This paper reported the storage of SOC in the Sanjiang Plain of Northeast China by averaging the data of 419 soil profiles. This study should be interesting for some readers. What I concern is how authors evaluate the influence of fertilization on SOC. In the section of Materials and methods, authors did not show detailed information. I guess that authors just compare the data of SOC with the application rate of fertilizer on the county scale. This may miss lead because the history of cropland also significantly affected the level of SOC. How authors excluded such effects?”

Response: We thank the reviewer’s comment. Generally, fertilization increase the SOC storage by enhancing the carbon input from plant productivity and crop biomass (Ren et al., 2012, Zhao et al., 2013). But, increasing fertilization may have a negative net effect on carbon sequestration because organic carbon mineralization neutralizes the carbon input (Russell et al., 2005). Influences of fertilization on SOC are complicated, and can be related to the history of cropland, vegetation types, as well as soil types and texture. As mentioned by this reviewer, we just compared the data of SOC with the application rate of fertilizer at the county scale in our manuscript. Following the suggestion by this reviewer, we have described the method about comparing the fertilization amount with SOC at the county scale in section 2.7 (Statistical analysis). In addition, new sentences have been added to discuss this comparison for the 23 counties in the revised manuscript.

Comment 2: “English grammar is poor and English native speaker should be invited to improve the text. Also please write in concise sentences.”

Response: Thanks for this suggestion. We have called for an English language editing service from Elsevier WebShop. The revised manuscript has been improved in English grammar, punctuation and diction.

Comment 3: “In the section of Abstract, authors should give some data to support the findings.”

Response: We agree this positive advice. We have added major data in the section of Abstract to support the findings.

Comment 4: “P14767, L9, "to be 70.31 Pg C (1 Pg = 10¹⁵ g)". This value is too low, please cite data from GCB paper (Xie et al., 2007).”

Response: Thanks for this comment. The SOC storage value for China reported in Xie et al. (2007) has been cited.

Comment 5: “P14767, L20-23, add references”.

Response: We thank this positive advice. A reference has been added in the revised manuscript to support this sentence.

Comment 6: “P14773, L5, "for the three depths (30, 60, and 100 cm) were". These are wrong, should be 0-30, 0-60, 0-100 cm.”

Response: We thank this reviewer’s comment. We have replaced “30, 60, and 100 cm” with “0-30, 0-60, 0-100 cm” in the revised manuscript.

Comment 7: “P14772, L5, "The SOC content at a given depth is calculated from the soil organic matter in individual layers and by use of the Bemmelen index (0.58). T_i is the thickness of the i_{th} soil layer." I cannot understand this sentence because authors measure the SOC rather than SOM. So authors do not need first converse "SOC" into "SOM" and then converse "SOM" into "SOC". Please delete it.”

Response: Thanks for the positive advice. We accept this comment and have deleted the sentence "The SOC content at a ... the Bemmelen index (0.58)" in the revised manuscript.

Comment 8: “P14775, L12-15, this paragraph should be moved to the section of Discussion.”

Response: Thanks for this kind suggestion. We accept this comment. These sentences have been moved to the section of Discussion (section 4.5) in the revised manuscript.

Comment 9: “P14776, L5-, these sentences are necessary here?”

Response: We agree. This sentence has been deleted.

Comment 10: “P14781, L7-9, please discuss about the influence of fertilizer on SOC more detailedly.”

Response: Thanks for the constructive comment. The influence of fertilizer on SOC has been discussed more detailedly in the revised manuscript.

2 Responses to the comments from anonymous referee #1

Overview of comment: “The MS deals with an interesting issue for soil organic carbon change at the Sanjiang Plain of China. I think this article has the potential to be an interesting addition to the literature. But still needs improve huge.”

Response: We appreciate the endorsement and detailed comments from anonymous referee #1 about our manuscript. The manuscript was revised carefully following the comments and detailed responses were concluded as follows:

Comment 1: “I think most readers do not know the site of “Sanjiang Plain”. I suggest you added some sentences to explain of it in introduction. For example, the Sanjiang Plain includes the Amur River (also known as the Heilong, or literally, "Black Dragon" or River), Songhua and Ussuri (also known as the Wusuli) rivers and covers 23 counties in Heilongjiang Province, China encompassing about 109,000 km². The area has extensive wetlands (Wang et al. 2003). (1) Wang A., Zhang S., and Zhang B. A study on the change of spatial pattern of wetland in the Sanjiang Plain. *Acta Ecologica Sinica* 2003, 23(2): 237-243.”

Response: Thanks for this positive comment. New sentences have been added to introduce the Sanjiang Plain. In addition, a reference also has been added to support these introductions.

Comment 2: “Land SOC change is a global environmental problem with important political and socioeconomic ramifications. These ramifications result from complex combinations of several factors, including natural factors such as ecological and climatic variations, and anthropogenic factors such as human activities and restoration policies that lead to changes in vegetation cover (Cao et al., 2011, 2014). Given these complexities, finding solutions that are both equitable and ecologically effective is even more challenging (Wang et al. 2011)”. I believe your topic is interest. However, you should make the readers to know the significance of your research. Please download the follow references and improve your introduction and discussion.”

- (2) Shixiong Cao, Hua Ma, Wenping Yuan, Xin Wang. Interaction of ecological and social factors affects vegetation recovery in China. *Biological Conservation* 2014, DOI: 10.1016/j.biocon.2014.10.009.
- (3) Shixiong Cao, Tao Tian, Li Chen, Xiaobin Dong, Xinxiao Yu, Guosheng Wang. Damage caused to the environment by reforestation policy in arid and semi-arid areas of China. *Ambio* 2010, 39(4), 279-283.
- (4) Yafeng Wang, Shixiong Cao. Carbon Sequestration may have Negative Impacts on Ecosystem Health. *Environmental Science and Technology* 2011, 45, 1759-1760.
- (5) Shixiong Cao, Ge Sun, Zhiqiang Zhang, Liding Chen, Qi Feng, Bojie Fu, Steve McNulty, David Shankman, Jianwu Tang, Yanhui Wang, Xiaohua Wei. Greening China Naturally.

Ambio 2011, 40, 828–831.

- (6) Shixiong Cao . Impact of China’s large-scale ecological restoration program on the environment and society: achievements, problems, synthesis, and applications. *Critical Reviews in Environmental Science and Technology* 2011, 41, 317–335.
- (7) Lixin Guan, Ge Sun, Shixiong Cao. China’s bureaucracy hinders environmental recovery. *Ambio* 2011, 40, 96–99.
- (8) Shixiong Cao. Socioeconomic road in ecological restoration in China. *Environmental Science and technology*, 2010, 44(14), 5328–5329.

Response: We agree and thank this referee’s comment. Each recommended reference by referee has been downloaded and read carefully. New sentences have been added to highlight the significance of our research. And some sentences were rephrased to improve the introduction and discussion. In addition, three references recommended by the referee have been cited in the revised manuscript.

Comment 3: In my opinion, the discussion structure should different from results section and focus on the mechanism (the relation between your data and why you find different result from others’). Therefore, there are some work wait you do again. And some policy suggestion seems should be given.

Response: Thanks for this kind suggestion. These sentences in section Discussion have been rephrased. More sentences focusing on the mechanism have been added to improve this manuscript. In addition, some policy suggestions have been given, such as the more powers are needed to protect wetlands and effective agricultural managements need to be contributed to reduce the emissions of greenhouse gases.

3 Responses to the comments from anonymous referee #2

Overview of comment: “This study presented in this paper has a great significance for quantifying the SOC storage and density over the major food production region, the Sanjiang Plain in China. On the whole, the paper was written well. However, its value of practicability is far beyond its creativity in study methods, So some necessary minor revision is needed for further publication.”

Response: We appreciate the endorsement and detailed comments from anonymous referee #2 about our manuscript. We have tried our best to address these comments. Our responses are as follows.

3.1 Comments for Data and methods

3.1.1 “Subsection 2.2, Page14769, Line 21-24: Some detailed information on HJ satellite imagery used in this study should be listed, and one classification accuracy (error matrix) should be added. Alternatively, adding a reference about the data source here is also acceptable.

Response: Thanks for this positive advice. A reference about the data source of HJ satellite imagery and land cover has been added in the revised manuscript.

3.1.2 Page14769-70, Line 26-27: Same as above, add the data source information of soil data.

Response: We agree this kind suggestion. A reference about the source information of soil data also have been added in the revised manuscript

3.1.3 Subsection 2.3. When were the soil samples collected? Which year?

Response: Thanks for this comment. The soil investigation mentioned in this paper was developed in 2012. The time has been added in the revised manuscript.

3.1.4 Subsection 2.5. Is the unit “kg hm⁻²” correct? It should be “Kg ha⁻¹”, right?

Response: We thank referee for this comment. The unit “kg hm⁻²” in subsection 2.5 have been replaced with “kg ha⁻¹”.

3.1.5 Subsection 2.6. Page14772, Line 5: After the phrase “Bemmelen index (0.58)”, one reference should be added.

Response: Thanks for this positive advice. Following the previous comment from Prof. Ding (bgd-11-C6414), this sentence including “Bemmelen index (0.58)” has been deleted. Therefore, the reference don’t need here.

3.2 Comments for Results

3.2.1 “Page14774, Line 15: After the phrase “clay content (p<0.01)”, add “(Fig. 6c1-c3)”; Line 19: After the phrase “: : 30 cm of soil”, add “(Fig. 6e1-e3)”.”

Response: We agree. Following this comment, “(Fig. 6c1-c3)” and “(Fig. 6e1-e3)” have been added after the phrase “clay content (p<0.01)” and “... 30 cm of soil”, respectively.

3.2.2 “Line22: In Table 2, what does the “SS” mean? Give its full name, please.”

Response: Thans for this comment. SS means the proportion of variances explained by variable. The full name should be “sum of squares”, which has been added in Table 2.

3.2.3 “Page14775, Line 1-2: From Table 2, how can the authors get the finding “precipitation exhibited more significant effects than temperature on SOCD”? Give some explanation, please.”

Response: We thank referee for this comment. This sentence has been rephrased to be “Temperature exhibited more significant effects than precipitation on SOCD of the top 1 m. We can get this finding according to the larger proportion explained by temperature than precipitation on variances of SOCD within the 1 m of soil (Table 2) and larger regressive coefficient (Fig. 6 A3, B3). Related explanations have been added in the revised manuscript.

3.3 Comments for Discussions

3.3.1 “Line 10-16: The authors compared the approaches of mapping SOC used in this paper with Yang et al.’s, i.e. Geostatistical Kriging interpolation vs. remote sensing VI method. In the following paragraphs, the authors also compared the estimated SOCD results in Sanjiang Plain with that published in some previous studies in Loess Plateau in China, as well as that in France. What is the objective of these comparison? What topics do the authors want to discuss here? From these comparison, what are the advantages or disadvantages in this present study?”

Response: We thank the referee for this comment. In our manuscript, we made comparisons with other publications from the method and results. First, 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 VI 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. Second, 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, Laos, and France. The Loess Plateau in China located in an arid zone has a drier climate than the Sanjiang Plain. The Laos dominated in tropical monsoon climate is warmer than the Sanjiang Plain. The France has the same humid climate with the Sanjiang Plain. Different climate types induced the variances of vegetation type and distribution. Therefore, the SOCD in the Sanjiang Plain was compared with SOCD in the three regions 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, these comparisons were developed following the comments by the editor who recommend 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. And some sentences also have been rephrased to improve the discussion.

3.3.2 “Likewise, in the last paragraph of this subsection 4.1, the estimated SOC storage (2.324 Pg C) in Sanjiang Plain was compared with SOC in Northeast China and in the whole Country (26.43 Pg C and 69.1 Pg C). The acquisition time of soil data in this present study were very different from that other two studies. So in Line 6-7, how did the authors make such conclusion as “significant underestimation of SOC storage”?”

Response: We thank referee's comment. Our results revealed that the farmland have a smaller SOCD than forestland and wetland. A negative correlation of SOCD with increasing temperature and positive correlation of SOCD with increasing precipitation were observed from our analysis. Meanwhile, significant losses from forestland and wetland to farmland, obvious increase in temperature, as well as notable decrease in precipitation in the Sanjiang Plain were recognized. All those findings tend to be a loss of SOC storage. However, through concluding related publication, we found that a smaller SOCD of the Sanjiang Plain than the result observed in this study. We thus deduce the underestimation of SOC storage in previous studies in the Sanjiang Plain. In the revised manuscript, new sentences and references have been added to support this finding.

3.3.3 “How about is the SOC of forestlands? The authors didn't mention this land cover type here.”

Response: Thanks for this positive advice. Forestland covering the second largest area of Sanjiang Plain had the second largest SOCD (23.4 kg m⁻²) among the land-cover types and stocked the second largest SOC (827.5 Tg C) in the 1 m soil depth. Related information have been added in the revised manuscript.

3.3.4 “Some sentences are some descriptions on results of this study, not discussions. So, they should be moved into the corresponding subsection of “3 Results”, e.g. Page14778, Line 16-17; Page14779, Line 10-12 and Line 24-26; and others.”

Response: Thanks for this kind suggestion. We accept this comment. These sentences displaying the descriptions on results (e.g. Page14778, Line 16-17; Page14779, Line 10-12 and Line 24-26) have been moved to the section of Results in the revised manuscript.

3.4 Comments for Conclusions

3.4.1 “Page14782, Line 8-11: “Based on the comparison between our estimate and the previous studies, we demonstrated that the previous report at the Northeast China and the whole country level significantly underestimate the SOC storage in the Sanjiang Plain.” This conclusion is questionable because the soil data were acquired in different time/year.”

Response: We thank the referee’s comment. Similar to the comment 3.2, we have gave the argument. Our results revealed that the farmland have a smaller SOCD than forestland and wetland. A negative correlation of SOCD with increasing temperature and positive correlation of SOCD with increasing precipitation were observed from our analyses. Meanwhile, significant loss of forestland and wetland to farmland, obvious increase in temperature, as well as notable decrease in precipitation in the Sanjiang Plain were recognized. However, through concluding related publication, we found that a smaller SOCD of the Sanjiang Plain than the result observed in this study. Extensive soil investigation taking the land cover types and soil types into consideration in our study was developed to quantify the SOC stock. We demonstrated that the present estimation might better represent the actual SOC storage distributions in the Sanjiang Plain, and consequently that the previous report at the Northeast China and the whole country level significantly underestimate the SOC storage in the Sanjiang Plain.

4 Responses to the comments from anonymous referee #3

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-authors, 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 field 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.

Specific 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 classification information as part of the results, you could present the results in this section—the 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 briefly interpret with text. Or at least have these information in the figure 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 profile (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 first 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 field? 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 °C 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 reflected, 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 field 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 field 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 developed 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 °C 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 (Jobb ágy 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 field 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”.