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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.

Anonymous Referee #3

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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.

Specific comments:

1. Section 1, Ln 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.

2. Section 1, paragraph 3 and 4 can be combined and shortened. Little information was provided in paragraph 4. Ln 2-4, Page 14768 was just repeating the point in paragraph 1.

3. Ln 21-23, Page 14768, delete or could go to the 'Methods' section.

4. Section 2.2, Ln 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.

5. Section 2.2, Ln 25, Page 14769, when did the second soil survey happen? Add references for it.

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.

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?

8. Section 2.4, Ln7-9, Page 14771, the first sentence already mentioned that 12 Russian stations were included. Reorganize.

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.

10. Section 3.4, Ln 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.

11. Section 3.4, Ln 1-2, Page 14775, typo? This was opposite to what the data reflected, and also opposite to the interpretation at Ln 20-21, Page 14778.

12. Section 3.5, Ln 12-13, Page 14775, this sentence could go to the 'Methods' section, as comment 6.

13. Section 3.5, Ln 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 Table 1 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.

14. Section 3.5, Ln 24-26, Page 14775, I don't understand the objective of this relationship analysis.

15. Section 4.1, Ln 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?

16. Section 4.1, Ln 22-23, Page 14776 and Section 4.3, Ln 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?

17. Section 4.1, Ln 8-12, Page 14777, reads weird in here. Combine it with Section 4.2.

18. Section 4.2, Ln 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.

19. Section 4.3, Ln 25-26, Page 14778, over-interpretation of the pattern. See comment 10.

20. Section 4.3, Ln 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'.

21. Section 4.4, Ln 3-5, Page 14780, any references?

22. Section 4.4, Ln 18-20, Page 14780, any references?

23. Section 4.5, Ln 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?

24. Section 5, Ln 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?

Technical corrections:

1. Ln 16, Page 14768 – translation? conversion?
2. Ln 17, Page 14775 – reparable? What does this mean?
3. Ln14, Page 14780 – circle? cycle?

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