

## ***Interactive comment on “Ground cover rice production system facilitates soil carbon and nitrogen stocks at regional scale” by M. Liu et al.***

### **Anonymous Referee #4**

Received and published: 3 March 2015

#### General comments:

Liu et al. present and discuss a potentially important consideration of the ground cover rice production system (GCPRS) and its effects on soil organic carbon and nitrogen stocks. Using 49 paired sites (paddy systems and GSCPRS sites) that were analyzed after a period of 5 – 20 years following production conversion. This is a worthy study given the importance of rice production in China and in the world and the potential of this technique to increase yields in areas limited by temperature and/or impacted by water scarcity.

#### Specific comments:

1. Page 3652, lines 10-16: The table containing the site information is well-done, but

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within the manuscript it would be good to include the elevation range of the sampling sites.

2. P. 3652, l. 19: Is there any idea of the inter-annual variation in rainfall or temperature in this region? Perhaps error of some type here. Also, are there any present temperature/rainfall trends seen during this time period?

3. P. 3652, l. 22: Where did the measure of sunshine hours come from?

4. P. 3653, l. 4-8: The description of the site selection process is lacking. How did “experienced staff members” select this sites? Where the selections random? Soil type and elevation have the potential to greatly influence the outcomes of these findings, the manner in which these site characteristics were consider in selecting study sites is crucial and thus this area of the manuscript needs further explication. Is the information from interviews with farmers available?

5. P. 3653, l. 20-21: What are the soil types? Maybe an additional table could be provided or perhaps table S1 could be expanded to include more information about each site.

6. P. 3656, l. 5-13: Where all analyses conducted in SAS? Are data/code/output posted anywhere for review and reproducibility? This section is lacking on specifics and details and requires clarification.

7. P.3656-3657: The results section could be expanded to include more specific numbers. As is, the results section mostly identifies differences and points the reader to the plots without including specific numbers, significance levels, or error. Lines 5-10 on p. 3657 represent a more thorough representation of the findings. Given the thorough and well-detailed methods section, I was expecting more explicit results.

8. P. 3658, l. “Our results show that adoption of GCPRS has a positive effect . . .” This sentence in the manuscript may be overstating the findings of the results. While there is an indication of a positive trend, the findings should be placed in context of the

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region and the relatively scant time scale. Overstatements should be avoided.

9. P. 3658, l. 15-19: “. . . root biomass was found to be significantly larger under GCPRS . . .” on p. 3655, l. 22 in the methods, it is noted that root biomass was examined at only one of the paired sites. While the identified method of the increased dynamism of root systems under GCPRS influencing soil nutrient acquisition may be what is going on, the predictive ability of the outlined method does not seem to have the power to confirm this. I would reexamine this analysis and consider this a possible further area of exploration as the findings are interesting, but overarching proclamations regarding this mechanism are not necessarily supported by one site.

10. The figures for each graph/plot should mention the statistical test which the significance levels are referring too. Visually, the plots are quite nice and are nicely suited to presenting the data.

A major concern here is the confounding of findings stemming from the lack of explicit consideration for independent variables. Without considering variance in soil type and elevation among the sites, and looking for relationships among and within treatments, the findings here are constrained considerably depending on the range of soil types. And also what about time? A time range of 5-20 years is mentioned multiple times in the manuscript, but never tested explicitly to see how much of an impact time from conversion has on any variable.

It would be preferential if the data and analysis were posted publicly so that results could be verified and reproducibility could be considered.

This study is worth of publication, but does also require significant editing for language and grammar.

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Interactive comment on Biogeosciences Discuss., 12, 3647, 2015.

**BGD**

12, C464–C466, 2015

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