1. Page 3652, lines 10-16: The table containing the site information is well-done, but within the manuscript it would be good to include the elevation range of the sampling sites.

>> Revised as suggested - the elevation of all sampling sites was added to the table. The elevation ranges from 169 m to 661m above sea level.

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?

>> Yes, we have added the inter-annual variation and presented rainfall and air temperature in this region based on seven meteorological stations located in the respective counties in this region (Shiyan City, Danjiangkou City, Yun County, Yunxi County, Fang County, Zhuxi County and Zhushan County) from 1961 to 2009. This information is given in the publication of Zhu et al., 2010. The data show that there is little interannual variation in rainfall and temperature for these sites (coefficient of variations of 5% and 1%). Present temperature / rainfall trends were (not) observed in the experiment year. All this information was added to the M+M section of the revised version.

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

>> It comes from the reference of Zhu et al., 2010. We added this citation to the reference list.

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?

>> The mentioned "experienced staff members" have been working in the Department of Agriculture in Shiyan with close interaction with the farmers in the individual villages since more than 20 years, also overseeing the introduction of GCRPS in the region. The site selection process was as follows: Information on topography, geology, soil type, and land use was collected from Shiyan Agricultural Bureau to identify a large set of potential villages and sites. Then, villages and potentially suitable paired sites were visited and information on agronomic parameters (e. g., transplanting data) and the time since conversion from Paddy to GCRPS cultivation as provided by the local extension staff was compared with the related information collected from farmers interviews. In case of sites were selected that provided unambiguous information on site history. Otherwise, we continued the site search until a representative set of paired sites with respect to elevation and geology was gained for the target region (i. e., 49 paired sites). Farmer interviews are available in form of Table S2. We have added this information to the revised version.

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.

>> Revised as suggested - we have added the soil types in Table S1 of the revised version for each single sample site. The soil types are: Dystric Cambisols, Haplic Luvisols, Dystric Regosols, Calcaric Regosols and Eutric Gleysols.

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.

>> Yes, all analyses have been conducted in SAS 8.2. The section on statistical analyses in the revised version has been rephrased and extended to improve clarity. We have also added Tables S3, S4.

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.

>> Many thanks for your constructive comments. We added Table S4 in which the well-detailed results of statistical analyses were listed. In order to comply with these suggestions, we have rephrased the results section describing in more detail the differences observed between GCRPS and Paddy systems. This explicitly includes the addition of numbers, error ranges and significance levels.

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

>> We do not agree with the term "trend" proposed by the reviewer, because increased SOC concentrations were statistically significant over the entire soil profile and increased SOC stocks were significant in 3 out of 4 sampled soil depths. While the term "trend" suggests statistical insignificance, we found significant results based on an extremely robust dataset with 49 replicated sites. However, in order to avoid overstatement, we limit the statement that GCRPS has a positive effect on SOC to the investigated region in Central China in the revised manuscript. We had included the factor "time since conversion to GCRPS" in our statistical analyses; however we found that this factor was insignificant (Table S3 in the revised version). This may be due to an insufficient number of sites in some of the investigated age classes due to the short timespan since the introduction of this technique and the generally slow changes in SOC and TN stocks. A significant time effect, and thus the calculation of a change rate per year may indeed be possible in another 10-20 years from now.

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.

>> It is actually true that, unfortunately, and due to logistic reasons it is just not possible to sample root biomass at all investigated sites. However, the observed effects of GCRPS cultivation on the root system at one of the sites was consistent with earlier independent publications (e.g. Li et al., 2007; Thakur et al., 2011; Uga et al., 2013). Furthermore,, we intensively sampled 22 plots at a well-managed site with a well-known land management history that can be considered as representative for the rest of sites.

Overall, the positive effect of GCRPS cultivation on root biomass and rooting depth is well acknowledged from previous studies, and the following reasons may explain this effect:

1) higher translocation of photosynthetic product into the root; 2) reduced anaerobiosis favoring root development and 3) relocation of nutrients such as NO_3 in deeper soil depth, requiring more vigorous root development.

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.

>> In the revised, we have added information on the statistical test used to the caption of each figure in the Table S4.

We have included the variables "soil type" and "time since conversion" in our statistical model, but neither these factors nor their interaction were significant. We have added this information to the revised version. For potential explanations of the insignificance of the factors time and soil type, see responses above.

We are willing to add our database on soil C and N content (and further data if desired such as soil texture) at the level of single fields and soil layers as supplementary data to the revised version (although this will be a very large table).

In the context of the comments on potential biases of different elevation or soil type

(this information was also added for each site), we feel that it is important to state that it is exactly the consideration of 49 replicated paired sites with different soil types and elevation, including a good spatial replication at each single field and the sampling down to 90 cm depth which makes our findings on GCRPS effects on soil C and N stocks extremely robust. We are not aware of any study with a comparable site replication.

Language quality was checked by a native speaker.