The manuscript discussed an interesting issue that whether ground cover rice production system depletes soil carbon and nitrogen compared to traditional paddy rice system. The authors did a valuable job using a paired sampling method to examine the difference of soil carbon and nitrogen between these two systems at 49 sites on a regional scale. The results showed that the ground cover rice production system benefits soil C and N sequestration. The results are interesting and have been fully discussed in the manuscript. I think this study is worthy of publishing, but still needs improve huge.

Specific comments

1. The sampling was selected from 49 paired sites. As mentioned in the manuscript, these sites represented a wide range of different soil types. The differences in soil C and N between the contrasting systems could vary considerably across soil types or sites. Therefore, spatial heterogeneity should be considered in the manuscript.

We have included the factors “soil type” and “elevation” in our statistical model and found no significant effect of these factors on the relative change of SOC and TN stocks due to GCRPS cultivation. In the revised manuscript, we present the associated details of the statistical model.

Furthermore, the outlined mechanisms for the observed GCRPS effects on C and N storage in soils (e.g. deeper penetration of roots, decreased NH_3 losses due to coverage) are generally valid irrespective of soil type. That is precisely what we demonstrate through the sampling of the 49 paired sites.

2. The root biomass samples were sampled from two N-treatment subplots. However, in Fig. 4, root dry matter of different soil depth in two farm systems was present. What the effect of N fertilizer on root biomass? Please check the values and analysis process.

>> Many thanks for your concern and suggestion. Root biomass was significantly affected by the production system, but not by the N fertilizer rates or the interaction of production system and nitrogen fertilization rates. Therefore we have pooled data over the two N fertilizer rates. This was clarified both in the Result section and in the Figure caption in the revised version.

3. A laboratory incubation experiment was conducted to test the hypothesis that GCRPS releases more soil carbon than paddy systems. However, the same controlled incubation conditions were dissimilar to the field conditions of the two systems. It seems better to conduct a field monitoring for test the hypothesis.

>> We are fully aware of the limitations of the chosen laboratory approach under controlled conditions. The approach of choosing identical incubation conditions was followed to address changes of C mineralization due to different C substrates, different microbial communities or because of differences in the physical protection of C between the Paddy and GCRPS systems. Hence, it was our purpose to eliminate any confounding temperature or soil moisture effects. The information derived from such an experiment is an important piece of supporting information to interpret the

observed effects on SOC storage in the field. Nonetheless we agree that the results should be interpreted in the light of the limitations of such an approach under controlled conditions. This is what we do in the revised version, together with a clearer outline of the rationale behind this experiment.

4. Greater stability of soil organic matter partly contribute to higher soil C under GCRPS systems. In addition, "the more frequent oscillation in redox conditions in GCRPS may have a strong positive influence on the generation of organo-mineral complex", which implies that GCRPS may hold higher mineral-associated organic matter (s+c). However, SOC contents in various fractions were similar between the two systems before incubation experiments, as seen in Fig. 8, indicating the difference in SOM stability between the two systems were not large. The difference in potential SOM mineralization may attribute to other factors, such like microbial composition.
>> We have better clarified our argumentation in the revised version. First of all, it is important to note that s+c and HF are both organo-mineral fractions, which provide physical protection against microbial decomposition through aggregation and adsorption of SOM on mineral surfaces. It is true that the relative SOC content for the different fractions (% over the total) before incubation were similar between the two systems. However, these fractions may differ with respect to their stability. The significant change of the heavy fraction within paddy soils during the incubation may indicate lower aggregate stability compared to GCRPS (we develop this point further in our response to Reviewer 495). Furthermore, it should be noted that information on physical soil fractions is only available for the topsoil, whereas the most pronounced effects on SOC stocks were observed in the deeper horizons. This aspect is also highlighted in the discussion of the revised version. Information on physical soil fractions from deeper soil layers is unfortunately not available.
We have also added the argument of different microbial communities to the revised version. In fact we have started experiments to analyze the microbial community in Paddy and GCRPS soils using molecular tools. And indeed the first available information shows that the microbial community differs between GCRPS and Paddy soils and that microbial activity is lower in GCRPS soils than in Paddy soils, which is in line with the observed mineralization dynamics in the laboratory and with field observations on the SOC stocks. We have added this information as "personal communication" to the revised version, as the related manuscript is under preparation.

5. The manuscript requires significant language editing. Many of the paragraphs need to be tightened up and at times the sentence structure is confusing and needs to be simplified or edited carefully.
>> Dr. David Pelster, who is also a co-author, is a native English speaker who edited and polished the manuscript.

6. Sections describing the statistical analyses are poorly described.
>> This section was rewritten to improve clarity. In particular, we tested significance of treatments according to the model that included effects of soil type on soil organic

carbon/N stocks. However, we found that soil organic carbon/N stocks were not significantly affected by soil type at regional scale. So we pooled these over the different soil types in the statistical analysis.