

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

### **Anonymous Referee #5**

Received and published: 4 March 2015

Dear Authors and Editor,

The article “Ground cover rice production system facilitates soil carbon and nitrogen stocks at regional scale” by Liu et al. is based on sophisticatedly designed soil sampling from geographically representative field sites in Central China. I found it of good value to understand local soil responses to film coverage. Its novelty in regional scale may also provide supportive information to local policy makers. However, the data obtained in this article was largely devalued by its weak argument in Introduction, lack of rationalization in Method, as well by the far-fetched interpretation in Discussion.

Here are my general comments:

1) The authors very often cite a great length of literature in Discussion, which should

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have been reviewed and argued in Introduction to build up your own argument, clarify knowledge gap and rationalize your own research question.

2) Why and how could you make a hypothesis (C, N stocks would reduce under GCRPS), but then observed completely opposite results? Do you indicate that you did sufficient literature review to guide you to such hypothesis? If yes, then how could you reject it later on with your own results? If no, then please take full use of literature review to thoroughly debate which factors could be relevant to increase or decrease C and N stocks under GCRPS.

3) Besides, if you decide to stay on the hypothesis of reducing C, N stocks under GCRPS, then it would be contradictory to use positive word such as “facilitate” in your article title.

4) The relevance of  $^{13}\text{C}$ ,  $^{15}\text{N}$ , and respiration rates should have been clarified in Introduction, i.e. why these properties are relevant, what additional information can they provide than the total C and N, what they can tell you to support your argument? Otherwise, it would be lack of ground to just bring it up in Method and Results.

5) Why did you air-dry all the soil samples before incubation? How much do you think such drying treatment will affect the mineralization potential? The community of microbes could change, I assume?

6) The Results are better reorganized to first deliver the most primary results, link them with logics, and then the secondary results. For instance, information such as soil texture, pH and bulk density could be moved below, unless you can reasonably link them to your primary results C and N stocks. On the other hand, the average C and N assimilation of aboveground biomass could be considered to be moved up directly following the C and N stocks. This may make a better reading flow.

7) In the Discussion part, authors tended to use a lot of observations from other reports to interpret the results observed in this study. This makes the Discussion less

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convincing. Peer reports should be used to compare and discuss, not to explain your own results.

8) The authors attributed the greater total C and N in GCRPS to more residues returns. Have those newly returned C and N been converted to stable form? Or they are just less decomposed litter buried or simply mixed into topsoils?

9) With respect to C stabilization/liability, what does the  $^{13}\text{C}$  and  $^{15}\text{N}$  show? What could be captured from the  $^{13}\text{C}$ ,  $^{15}\text{N}$  and mineralization rates? For instance, Figure 5 shows that Paddy soils are less depleted in  $^{15}\text{N}$  than GCRPS. This indicates that soils from GCRPS are less decomposed than that from Paddy, suggesting greater mineralization potential in GCRPS soils (I am not expert in stable isotope. Excuse me, if I am not correct here.). Then, why did Paddy soils show greater cumulative mineralization? What could be the factors? Local aeration, temperature, community or accessibility of microbes?

10) Why heavy fractions have significant differences before and after incubation, but other fractions do not. Does it have anything to do with the stabilization mechanism of SOC? And how? How does this then affect the mineralization, and SOC stocks?

11) When choosing the sampling sites, you also considered the time spans since adoption of the GCRPS technique. Then, did you do any analysis against the time variable? Any patterns of total C and N stocks over adoption time? Are the increase C and N stocks consistently observed in different adoption years? Are the increasing rates constant over different years? Could it be possible that the benefits of C and N increase only occur for the first several years and then cease when soil C and N stocks approach their maximum capacities?

12) If out of practical reasons, it is just not feasible to investigate root biomass for all field sites. Then, why did you choose this particular site? How well this site could represent all other sites of different soil types, and varying altitudes?

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13) In Conclusion part, it is better to summarize the key results first before relating to implications. The ideal case would be that the readers can get the most valuable information from just reading your conclusions.

Specific comments:

Page 3650

L13-18: lack of literature reference.

L18-20: “As with conventional paddy rice systems...as compared to Paddy systems...”: Either grammatically incorrect, or convoluted expressed.

Page 3651

L5 to 30: There should be less description on general effects of SOM on soil properties, but more related to rice system and what could possibly be the effects of GCRPS to SOM.

Page 3652

L5-7: Such detailed description should be moved to Method.

L20-22: Lack of literature reference or data source.

L23: What does “implications” mean here?

Page 3653

L10: “180kgfertilizerNha-1”: improper way to express measurement unit. At least, there should be space between numbers and measurement unit. And, is it different from the above “150kgNha-1”?

Page 3654

L1-13: It would be much more convinced if you could provide some literature references for all the methods you used here.

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Page 3657

L12: "...no differences in average potential C mineralization rates...": how did you calculate the average? You mean, averaging the mineralization rates over 200 days? Then, it seems meaningless to me. And why there are no differences in average but a higher value in cumulative mineralization rates?

L21-25: These sentences should belong to Introduction.

Page 3658

L2-5: These sentences should belong to Method.

L10-14, L19-30: They should be used in Introduction.

L15-19: These sentences are just repeating your description in Results.

Page 3659

L1-11: If these sentences are moved to Introduction, then it could be a good literature review.

L14-19: Just from "higher cumulative C loss rates", you cannot directly get the conclusion that SOM under GCRPS is more effectively persevered. Besides, you did not do aggregate fractionation, you could not simply relate your interpretation to the conceptual model of Six et al., 2004.

L20-25: Too much observations from other reports rather than your own observations. Such interpretations are far-fetched.

Page 3660

L2-15: Such discussion or information should have been either discussed in Introduction, or clarified in Method.

L17-25: Most of these sentences should be mentioned earlier in Introduction or Method.

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L29: It is not readily convinced to simply attribute “less loss of ammonia” to “the covering of soil immediately after fertilizing”. More in-depth interpretation may be needed.

Technical comments:

Figure 1: I would suggest to place SOC content above and SOC stock below, as, logically, SOC stock is calculated from SOC content.

Figure 3: What does CAGB represent here? You did not explain it in your text body. The text body and figures should be consistent.

Figure 4: Y-axis is missing.

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

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