

## ***Interactive comment on “Greenhouse gas emissions and reactive nitrogen releases from rice production with simultaneous incorporation of wheat straw and nitrogen fertilizer” by Longlong Xia et al.***

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Dear Editors and Reviewer,

On behalf of my co-authors, thank you very much for your positive and constructive comments on our manuscript. We have carefully studied the comments and have made corrections which we hope to meet with approval. Please see the attached point-by-point responses and the tracked change version of manuscript for your further evaluation.

Response to Reviewer's comments: Reviewer 1: 1. The section on the details of the  
C1

long-term experiment (lines 145-153) is not necessary. Response: Thanks very much for your comment and suggestion. According to your suggestion, we have deleted some details of the long-term experiment and kept some to illustrate why we used equation (1) to calculate the SOCSR in this study (please see response to Question 2).

2. It was a bit confusing because the rates of residue incorporation used in this study (Table 1) were different from those in the long-term experiment (line 146). Response: Thanks for your comment and sorry for our unclear expression. Yes, the rates of residue incorporation used in this study were different from those in the long-term experiment, but we think it is appropriate to use the equation to calculate the SOCSR in this study, due to the following reasons. Generally, it takes long-term observations over years to decades before the SOC change is detectable (Yan et al., 2011). The SOC content changes of short-term field experiment couldn't be correctly measured, due to the high variability of SOC during the preliminary several years of the experiment. In this study, we used a relationship (based on the results of 22-year observation) between straw input rates and SOCSR, obtained via an on-going long-term straw application experiment in the same region, to calculate the SOCSR. Same agricultural management practices were applied to the on-going long-term experiment and the experiment of this study. Under the same agricultural managements, soil and climatic conditions, cropping systems and straw types, it is reasonable to believe that the rate of straw C stabilizing into SOC (i.e. conversion efficiency of crop residue C into SOC) are similar between these two experiments (Mandal et al., 2008). It is reported that the conversion rates of crop straw to SOC in two main wheat/maize production regions in China, which have similar soil and climatic conditions and agricultural practices, were very close, at 40.524 versus 40.607 kg SOC-C t<sup>-1</sup> dry-weight straw (Lu et al., 2009). Therefore, we hold the opinion that the above SOCSR calculation method is appropriate, although the input rates of these two experiments were different. We have to admit that this method may bring uncertainty into our results, but it unlikely affects the main conclusions of our study (please see line 152-165). References: Yan, X., Cai, Z.,

Wang, S., Smith, P.: Direct measurement of soil organic carbon content change in the croplands of China, *Global Change Biol.*, 17, 1487-1496, 2013. Mandal, B., Majumder, B., Adhya, T., Bandyopadhyay, P., Gangopadhyay, A., Sarkar, D., Kundu, M., Choudhury, S.G., Hazra, G., Kundu, S.: Potential of double-cropped rice ecology to conserve organic carbon under subtropical climate, *Global Change Biol.*, 14, 2139-2151, 2008. Lu, F., Wang, X., Han, B., Ouyang, Z., Duan, X., Zheng, H., Miao, H.: Soil carbon sequestrations by nitrogen fertilizer application, straw return and no-tillage in China's cropland, *Global Change Biol.*, 15, 281-305, 2009.

3. The relationship between CH<sub>4</sub> emission and the amount of organic matter input was not the major focus of the paper. The discussion should be simplified rather than being extended with possible explanations, some of which are speculative. Response: Thanks for your good suggestion. According to your suggestion, we have simplified the relevant discussion (please see line 261-272).

4. At a few other places in the discussion section e.g. lines 278-285 the authors presented their results, and compared the results with others', which was fine but the manuscript would be more informative if the implications of the findings could be explored. Response: Thanks for your good suggestion. According to your suggestion, we have explored the implications of the SOC sequestration of this study (please see line 292-309). We also revised somewhere else, such as line 287-289 and line 359-360, to illustrate the implications of our findings.

5. Minor comments: Line 76: delete "And" Response: Agreed and revised (please see line 76). Line 112: The scientific name of rice was provided but not for winter wheat Response: Sorry for our carelessness. We have added the scientific name of wheat in the text (please see line 112).

Lines 252-253: What does it mean by "the applied OM rates among different treatments are statistically different"? A statistical test on the independent variables (OM application rates)? Response: Sorry for our unclear expression. We have deleted this

C3

sentence.

Line 311: "was not" instead of "wasn't" Response: Agreed and revised (please see line 335).

Lines 344-346: incomplete sentence Response: Sorry for our unclear expression. We have revised this sentence (369-373).

Once again, thank you very much for your constructive comments and suggestions. Yours sincerely, XiaoyuanYan on behalf of all authors

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/bg-2015-620/bg-2015-620-AC1-supplement.pdf>

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Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2015-620, 2016.

C4