

Interactive comment on “How methane emission from rice paddy is affected by management practices and region?” by Jinyang Wang et al.

Jinyang Wang et al.

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Answer to Referee #2

Still some language issues, e.g. title is awkward and could deter readers/interest in the paper, many other sentences have unclear meaning and/or awkward language. Paper would definitely benefit from a thorough editing for clarity and language in general.

Answer: We appreciate and followed this suggestion. We have sent our manuscript out for the language editing service (as shown in Figure 1).

Specific issues: 1. the authors already know that $\ln[\text{SOC}]$ and $\text{OMx} \ln[1 + \text{AOM}]$ will be modeled, but we don't know where that information is from.

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Answer: We appreciate the referee #2 raised this concern. In fact, the initial form of the model is an exponential relationship between emission flux and controlling factors as suggested in previous studies (Bouwman et al., 2002; Yan et al., 2005). SOC content (%) and the type and amount (t/ha) of organic amendments were factors in the above equation. It has been long recognized that CH₄ flux is proportional to both SOC content and the application rate of organic amendment. As CH₄ flux data do not fit a normal distribution, they fit a log-normal distribution. Thus, by fitting log-transformed flux data of CH₄, the above equation was revised to the Eqn (1) in this study. That's the reason why $OM \cdot \ln(1+AOM)$ is modeled was added.

2. Not sure that treating pH as categorical variable is at all justified or appropriate. Why was this done? Was pH reported from the different field sites in broad categories, or measured with crude litmus paper or similar? That might be a reason, but still. . . Authors state that the relationship of pH to emissions is 'not monotonic' but from Table 2, I don't see strong enough evidence of that, especially given the questionable shoe-horning into many ns from a ranked relationship of pH with emissions simply be error? Did the authors try converting pH to concentrations of H⁺ ions or otherwise back-log-transforming pH values, or other logical numerical ways to treat this definitely-not-categorical variable? I don't think this statement in lines 213-215, "However, soils with a pH of 5.0-5.5 showed a much higher emission than other soils", is really true. It looks to me like soils with the lowest pH values (below 4.5) had the largest effect on CH₄ emissions, and the small blips at 5- 5.5 and 7 – 7.5 are not necessarily a big deal. No other literature besides the authors' 2005 paper is cited regarding a more complicated relationship between pH and CH₄ emissions to support this idea.

Answer: We appreciate the referee #2 raised this thoughtful concern. Firstly, the reason why soil pH was treated as the categorical variable is that previous findings have been suggested the existence of optimum soil pH for CH₄ emission, albeit the inconsistency of reported values (Parashar et al., 1991; Wang et al., 1993). As shown in the below figure (Figure 2), soil pH values were broadly distributed across the listed range

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in the text in our data set. Secondly, we found that the relationship between soil pH and CH₄ flux was not monotonic. In our data set, we used pH(water) as the soil pH values for most cases. As shown in the below figure and also described in the manuscript, the largest effects of soil pH below 4.5 may not be reliable because of the limited number of observations from only two studies with large variability. The effects of soil pH above 6.0 were not significantly different from each other. Indeed, soils with a pH of 5.0-5.5 showed a much higher emission than soils with 4.5-5.0 and 5.5-6.0. Collectively, we considered the soil pH as a categorical variable which may be at least appropriate in terms of our current data sets.

3. How did the authors arrive at the weights for the organic matter additions (.2 and 1)? Not clear why this is needed or justified.

Answer: We added the explanation. There is an assumption that in cases where the amount of organic amendment is zero (i.e., no organic material added), it is the result of each type of organic material at zero application rate. By this, more data points in the analysis will have than the actual size of real observations. To ameliorate this problem, we weighted the residual of observations with organic amendments as 1 and those without as 0.2 (as the observational result was repeated five times for the five types of organic materials).

4. The authors state several times that because emissions estimates from different authors' inventory assessments, that this means the results are correct/reliable, e.g. line 70, and lines 173-175 where EDGAR estimates are similar to IPCC 2006. This is a truism, though, because doesn't EDGAR use IPCC 2006 defaults to calculate their emissions estimates?

Answer: We appreciate the referee's comment on this. In fact, the method to estimate CH₄ emission from rice fields using the IPCC methodology was different among studies. For example, in Yan et al. (2009), not only the default EF used for countries where country-specific EFs were not available but also the country-specific EF derived from

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various scaling factors were applied when estimating CH₄ emission from global rice fields. However, in the Emission Database of Global Atmospheric Research (EDGAR), only the IPCC default EF was used (EDGAR, 2017). In addition, we have revised the sentences for clarity.

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Yan, X., Akiyama, H., Yagi, K. and Akimoto, H.: Global estimations of the inventory and mitigation potential of methane emissions from rice cultivation conducted using the 2006 Intergovernmental Panel on Climate Change guidelines, *Global Biogeochem. Cycles*, 23(2), doi:10.1029/2008GB003299, 2009.

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Fig. 1. Language editing certificate

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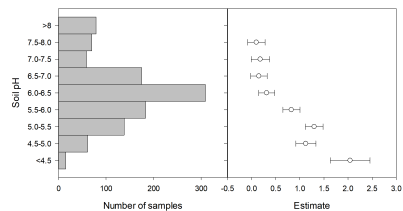


Fig. 2. Soil pH

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