Biogeosciences Discuss., 7, C2443–C2446, 2010
www.biogeosciences-discuss.net/7/C2443/2010/

Biogeosciences Discussions

Interactive comment on "Effects of free-air CO_2 enrichment (FACE) and soil warming on CH_4 emission from a rice paddy field: impact assessment and stoichiometric evaluation" *by* T. Tokida et al.

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Received and published: 22 August 2010

Thank you for your time and efforts with the paper. We have addressed all comments and suggestions of the reviewers in the revised manuscript. We indicate below how we have responded to each comment from the reviewer.

Referee #1 Comment 1: CH4 production in the paddy field is an microbial-involved biochemical process which is driven by substrate availability and redox potential. The authors did not find significant effect of elevated CO2 on CH4 emission, which is quite

different from that of Xu et al. as the authors mentioned in the text.

Reply 1: CH4 production is a result of carbon reduction which increases where more organic carbon (electron donor) exists and more electrons are available for the reduction of carbon itself (redox is an indicator but not the direct driver of the CH4 production process). We have therefore carefully analyzed the current results by introducing covariance analysis accounting for the electron balance to estimate the effect of FACE on CH4 emission and stated that "The stimulatory effect of FACE observed in this study (after the covariance analysis) is in agreement with the findings of previous FACE studies (without warming) conducted at the same site (Inubushi et al., 2003) and at another rice FACE site in Jiangsu Province, China (Xu et al., 2004)." (page 19 line 2-5). It is not our argument that the FACE effect differs amongst the experimental sites.

Comment 2: The SOC contents in current paper and in Xu et al. are 77.8 g kg-1 and 14.9 g kg-1, respectively. Supposing the enhancement of rice growth to elevated CO2 is the same, due to the higher SOC availability in current experiment, the enhancement of CH4 emission should be much smaller than that of Xu et al.

Reply 2: We agree with the reviewer's view that the stimulatory effect of FACE on CH4 emission will be lower as the relative importance of carbon sources other than rhizodeposition increases. However, it has well been proven that labile organic carbon, not the total carbon, is important as the substrate for methanogenesis. Andosol, which is used in this study, is rich in SOC, but labile organic C is not necessarily large. While SOC can be an important C source for CH4, fresh organic matters, such as crop residues play a crucial role. Xu et al. stated that they incorporated 3,900 kg ha-1 of wheat straw (dry weight), which is equivalent to $\sim$156 gC m–2. In our study, we removed most of the residues and incorporated left-over stubbles only (46.9 g C m-2 (p18, line 7)), the volume of which is less than one-third of Xu et al. Our argument is that the effects of FACE and other environmental factors on CH4 production are driven by multiple factors, and that we attempted to account for the major drivers to interpret the climate change effects.

Comment 3: The large CH4 emission difference between ET and NT is just the reflection that higher temperature lead to higher decomposition of SOC and then lead to higher CH4 emission. I don't think the calculation of SOM decomposition by N mineralization in situ can reflect the real SOM decomposition. As for the relevance of Fe reduction and CH4 emission, it is a worthy topic to be studied further.

Reply 3: As described in the original manuscript, the temperature dependency of SOM decomposition alone cannot explain the dramatic enhancement in CH4 emission, because i) the rate of SOM decomposition cannot fully account for the observed CH4 emission rate (Table 2), and ii) to our best knowledge, no single study has shown a 50% enhancement in SOM decomposition in response to 2 deg C warming. Instead we sought out synergistic mechanisms that may explain the large CH4 increase (refer to section 4.2). Studies about competitive reduction processes (Fe reduction vs CH4 production) began back in 1950's (at the latest). In the original manuscript we carefully cited some important papers in this field (e.g. page 6, line 7-12, page 12 14-19) and also added some review and discussion regarding the temperature sensitivity of Fe reduction (section 4.3) which we think needs further investigations.

Referee #2

Comment 1: I am wonder too which extent "FACE" type experiments can be translated to real world conditions. The measured strong increase with temperature should e.g. be seen in CH4 emission time series over longer period covering a range of ambient temperature. The manuscript certainly would gain in quality in case this aspect could also be discussed.

Reply 1: We totally agree with the reviewer's comments that we need to think to which extent the FACE-warming experiment can simulate the real world. Indeed, in the original manuscript, we did put some discussion on this point (4.2.4 Implications and future research needs: experimental warming versus real global warming). We share the reviewer's interest with regard to a potential analogy between our results (experimental

warming) and change in CH4 emissions under varying ambient temperature conditions. In the revised manuscript, we have added some more discussion to 4.2.4 as indicated below:

"The warming effect may have an analogy for inter-annual variation in CH4 emission observed under ambient temperature conditions. In a natural wetland (Hudson Bay Lowland), a large Q10 value of 7, similar to our warming effects, was observed from the relation between CH4 emission and annual air temperature. However, we are aware of no rice paddy studies which gave quantitative analysis on changes in long-term CH4 emission in response to changing ambient temperature."

Comment 2: Because the measured increase of 26% with enhanced CO2 emissions alone is not significant it has to be explicitly mentioned in the abstract, the indication of the p-value in bracket is miss leading. The same holds for the conclusion where the lack of significance is omitted.

Reply 2: We understand the reviewer's concern completely. We now note the significance level more explicitly in the revised manuscript.

Thank you again for your efforts with the paper. We hope that you will now find it acceptable for publication in Biogeosciences.

Interactive comment on Biogeosciences Discuss., 7, 1863, 2010.