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Interactive comment on “Effects of free-air CO₂ enrichment (FACE) and soil warming on CH₄ emission from a rice paddy field: impact assessment and stoichiometric evaluation” by T. Tokida et al.

T. Tokida et al.

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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

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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 ~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.

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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

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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.

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