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***Interactive comment on* “Trace gas fluxes of CO₂, CH₄ and N₂O in a permanent grassland soil exposed to elevated CO₂ in the Giessen FACE study” by M. K. Abbasi and C. Müller**

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Response to Reviewer's Comments

Dear Editor

We have revised the manuscript according to the suggestion of the Reviewer. Following amendments have been made in the manuscript.

Anonymous Referee #1 Received and published: 4 March 2011 General Comments

Query: These findings are interesting in their own right and should be published. The paper is logically structured and the experimental protocol and analysis of results are

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sound, but the discussion is rather protracted and can be confusing at times, especially the section on N₂O emissions where there are repetitions and no clear picture emerges. Because there was no effect of elevated CO₂ on the net N₂O emission, this section (4.2) could be streamlined and shortened, in order to better reflect the key findings of the paper, and place the emphasis on the enhancement of total denitrification and the altered N₂/N₂O ratio (Section 4.3), where clear effects of elevated CO₂ are to be found. The absence of effects on net GHG emissions was already featured in the Kammann et al paper, and need not be stressed again at length here. Rather, I would like to see a more detailed assessment of the actual mechanisms by which elevated CO₂ can affect total denitrification and N₂/N₂O ratios. There are also many typographical errors and technical corrections are needed, and the English must be checked and improved.

Reply: The discussion of the manuscript has been streamlined and is now largely rewritten. Special emphasis is now given to N₂O emission as suggested by the reviewer. In this context discussion has been added to explain the results obtained in this study. In particular an emphasis on the possible dynamics of denitrification kinetics has been added. Pages 13 Lines 326-337.

Specific Comments

Query: The stated objectives for this paper are two fold :CO₂ effect on N₂O emissions and the processes of N₂O and N₂ production. However, given the length of materials, figures, discussions devoted to CO₂ and CH₄ fluxes, the objectives should also include these, as part of a total GHG budget assessment.

Reply: The objective of the manuscript is changed; all the three GHG (CO₂, CH₄ and N₂O) have been included as suggested Page 4, line 120-123.

Query: Carbon content is given as 6.6 (with no units). One might expect a higher soil organic content from a soil that has been exposed to 9 years of elevated CO₂, which presumably would have a significant bearing on the results of the incubations. This is

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not discussed anywhere. Are there no data? predict nutrients release from organic substrates. So the statement is not correct.

Reply: Carbon contents were 4.5%. Unit is added and correction in the value has been made. The organic C content was not significantly different between the two CO₂ treatments (Lehnhart, 2008). We also refer now more extensively to data that have been published previously (e.g. by Kammann et al., 2008; Lenhart, 2008; Müller et al., 2009). Page 4, Line 133-136.

Query: The 2-pool model by Stevens et al should be described very briefly in the methods.

Reply: A more detailed description of the method is now presented in Materials and Methods; Page 6, Line 166-171.

Query: p4209, l8-13, and Figure 5: how can a contribution to total N₂O production be negative (from day 28 onwards)? N₂O production cannot be negative, or are we somehow dealing with soil N₂O consumption here? Please explain, or provide a corrected figure.

Reply: See explanations in materials and methods and the discussion; Page 6, Line 172-175 and page 13 Lines 330-335.

Query: Why is this unexpected? Shouldn't an enhanced biomass production and higher root biomass under elevated CO₂ lead to an increased soil organic matter pool, and to enhanced soil respiration and CO₂ flux? Or do you mean that it was unexpected that it was ONLY 20-25% ?

Reply: The reasons have been explained in Page 11 lines 277-280, as explained by the Reviewer in his statement that there should be a significant increase in CO₂ emissions in elevated CO₂ treatments. Although, 20% difference is not a small or ONLY difference yet the difference was non-significant; Page 11, Line 277-280.

Query: I don't understand this statement : 'The high oxidation rates by N addition in

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the present study might be that after 6–7 years of FACE establishment, it is unlikely to have any inhibitory effect by elevated CO₂. It seems as though you are formulating an hypothesis, but it is fact that elevated CO₂ did not inhibit CH₄ oxidation rates: Fig.1 shows that CH₄ was more oxidised under elevated CO₂. Further, not only did CH₄ oxidation increase in the elevated CO₂ treatment, but also in the control soil (Fig.1). So the positive effect of N addition on CH₄ oxidation cannot be attributed to the effect of elevated CO₂ alone.

Reply: We have deleted this sentence and added more discussion to explain processes that might have caused the higher CH₄ oxidation rate under elevated CO₂. Page 12, Lines 309-316.

Query: Technical Corrections / Language

Reply: All corrections have been made in the Text of the manuscript as suggested by the Reviewer; also tried to improve the Linguagistic error in the manuscript.

Query: Figure captions: in Figs 1, 2, 3 and 4, 'expose' must be changed to 'exposed.

Reply: Figure captions: in Figs 1, 2, 3 and 4, 'expose' has been changed to 'exposed.

Query: Figure 5: if the Y-axis really represents the fraction of N₂O production attributed to NO₃⁻ reduction, then the data are wrong, there can't be a negative N₂O production.

Reply: Please See explanations in Materials and Methods and discussion; Page 6, Line 172-175 and page 13 Lines 330-337.

We believe that manuscript is improved a lot and it will be now considered for publication in Atmospheric Chemistry and Physics.

Thanks in anticipation

Best wishes/Sincerely

Prof Dr M Kaleem Abbasi

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