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Biogeosciences Discussions

Interactive comment on "Responses of N_2O and CH_4 fluxes to fertilizer nitrogen addition rates in an irrigated wheat-maize cropping system in northern China" *by* C. Liu et al.

Anonymous Referee #3

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General comments The manuscript bg-2011-326 by Liu et al. reports the responses of soil N2O and CH4 fluxes and crop yield to increasing N fertilizer levels in a field experiment in an irrigated cinnamon soil with a clay loam texture treated with six different N fertilizer levels (urea in the range of N 0 – 850 kg/ha/yr) under a wheat-maize cropping system in northern China. The emission measurements were carried out manually by vented static chambers at least twice per week throughout the 1-yr wheat-maize rotation. The scientific questions addressed by the paper are relevant and within the scope of BG, as both N2O and CH4 are important greenhouse gases involved in global climate change and agricultural soils are known to be important sources of N2O and sinks for CH4. Worldwide, experimental field data on the impacts of increasing

N fertilization are scarce and variable in terms of the N2O emission factors and the linearity of N2O response. More data is clearly needed from different climatic and soil conditions. In this context, the data on the emissions of N2O and uptake of CH4 relative to the amount of fertilization and crop yield in a semi-humid upland soil under a highly productive irrigated double-cropping rotation in northern China provide a new and valuable contribution. The main conclusions about the linearity of N2O response to N fertilization, the low levels of N2O emission factors irrespective of N fertilizer rate and the recommended way of reducing N2O emissions by keeping fertilizer rates at a level where soil N balance is slightly positive and current yields maintained are all well based on the experimental data. It is also clear that in this study CH4 uptake by soil was not reduced by N fertilization. The overall quality of this discussion paper is good and I am happy to recommend it to be published in BG. Therefore, my comments below deal only with minor issues and technical corrections.

Specific comments Materials and method: In addition to the local soil name, the soil name should be given according to at least one of the main international systems (WRB, Soil Taxonomy). This gives a larger framework to the study. As for the way of irrigation, it is not clear what irrigation with underground water exactly means (p. 9581)? Was the groundwater just pumped up and irrigated on the soil surface. Was the irrigation water taken from the experimental field? The scientific methods and assumptions are generally valid and mostly sufficiently outlined. The description of experiments and calculations is usually sufficiently complete and precise to allow the traceability of results. The use of vented static chambers for intermittent flux measurements can be taken nowadays as a standard method. Although it may overestimate the cumulative emissions, this is unlikely to invalidate the main conclusions of the study. However, the authors could pay more attention to the description of their chamber type II: in particular, did the chamber cover the soil surface area representatively, and how much the leaking of gases through PE seal may have affected the results, as PE is well-known to be very permeable to gases and the seal was very thin? The procedure, materials , vials etc. of gas sampling, storage and the gas analysis should be described in more detail; they are lacking at present. Some auxiliary measurements, such as soil moisture content and soil bulk density, were made only in a very shallow depth (0-6 cm). This should be taken more into account when interpreting the results. The aboveground biomass results are missing, although they are mentioned in Materials and methods. The total biomass and its N content should be given in Table 1. This would allow the estimating the amount of N in the residue incorporated back into soil. Herbicide and pesticide names are lacking. The description of statistical procedures should be improved. Which nonparametric test was used? It is also not clear, why the particular nonlinear forms of regression were selected and whether the significance of all their terms were tested. The total significance of regression by F-test only tells whether the variation explained by the regression model differs from the residual variation, or if the regression can explain anything.

Results: The first section in the Results should be given another name, as the term "environmental parameters" is not suited for all variables included in it. Why not e.g. simply "Environment, soil mineral N and DOC".

Discussion: Discussion is the section of the paper that would benefit most from reformulations of text in terms of clarity, and perhaps from a somewhat deeper theoretical approach. Why should irrigation be a key factor in determining the linearity/nonlinearity of N2O response to fertilizer rates, as suggested? Or, why the increased fertilizer rates should primarily stimulate (in relative terms) the ammonia volatilization and nitrate leaching rather than N2O flux? The second paragraph of Discussion starts with a reference to the results by other researchers in a similar crop rotation as in this study ("N2O emissions were only a minor loss pathway ..."). This is very difficult to read for the first time, as it gives an impression the statement refers to this study. The authors should reformulate this by starting first with their own data and/or a more general statement, and only after this take some other results for support. As for CH4 uptake, N fertilization clearly did not decrease CH4 uptake in this study. The authors go into some detail in explaining the possible reasons for this based on current literature. They observed that CH_4 uptake was largest a few days after high precipitation or irrigation event, and speculate that this could have induced some CH_4 production in the soil. Weak production of CH_4 was indeed observed occasionally. However, usually CH_4 production indicates very reduced soil conditions that take rather long time to develop, so in my view this should not be very common. Instead, I think one explanation could as well be based on soil diffusive resistances. The impaired soil gas exchange due to increased soil moisture after irrigation or precipitation event could have slowed down the inflow of CH_4 into the soil profile, and this condition could have been relieved only after a few days. The authors use a term "chemical fertilizer" throughout the paper. This is not correct as all fertilizers are by definition chemical compounds. A correct term would be a "synthetic fertilizer". The current discussion of the magnitude of soil N_2O emission factors is based on the comparison of values to the global default for croplands, and on some previously measured local values. The discussion would benefit from more comparisons to fertilizer-induced emission values in other climatic and soil conditions with N fertilizer and irrigation treatments. The line of reasoning behind the recommendation of an optimal N fertilizer rate that would allow reducing the current high fertilizer rates, maintain the current yields and prevent large N_2O emissions is plausible, but the text should be made more fluent and the line of reasoning more clear. As the data in this study did not give a clear threshold value for an optimal fertilization (the N_2O emissions and the ratio of emissions and yield increased continuously with fertilizer rate), the authors base the fertilizer recommendations on the idea of keeping the N balance close to zero (slightly positive). They then compare the impacts on yields and gas emissions. For this reason, it would be better to first represent the aim of close-to-zero N balance and then start explaining it.

Conclusions: Conclusions should be more condense. All literature references should be removed from the Conclusions.

References: The authors give proper credit to related work and indicate their own original contribution. The number of references (30) is suitable.

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Listing of technical corrections The title clearly reflects the contents of the paper, and the abstract provides a concise and complete summary. The overall presentation is well structured and clear. The English language is mostly fluent and precise (however, see my above comments about suggested reformulation of text and choice of terms). The mathematical formulae, symbols, abbreviations, and units are correctly defined and used, except for the regression equations in Figure captions (2, 3, 5, 6, 7, 8), where “x” is used for the abscissa variable without definition. It would be better to use specific symbols for the given variables in the equations. In Figs. 2, 5, 6 and 8, the captions should be reformulated so that first are given all equations in the (a) graph, and then those in the (b) graph. As there are quite many equations, one could consider presenting their coefficients in a separate table. The reasoning behind selecting the forms of equations should be explained in the text. In my view, the figures or tables are not too many in the manuscript, but the authors could consider giving some of them as supplementary material (e.g. Table 2) . In the literature reference of Ju etal. 2009, the volume number should be 106, rather than 160.

Interactive comment on Biogeosciences Discuss., 8, 9577, 2011.