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Biogeosciences
Discussions

Interactive comment on "Global land-atmosphere exchange of methane and nitrous oxide: magnitude and spatiotemporal patterns" *by* H. Tian et al.

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Thanks for the reviewers' precious suggestion for improving this manuscript. We have carefully revised the text, figures and tables according to reviewers' comments. Besides, in this revised manuscript, we improved CH4 emission from rice fields, and rewrote introduction and part of discussion. Please find the detailed response below:

1st Reviewer's comments:

This study conducted a global-scale simulation of CH4 and N2O fluxes using a process-based ecosystem model. Authors discussed the global budget, spatial variation, interannual variation, and long-term trend. Topic and methodology are potentially

interesting; however, I have some concern in this manuscript. 1. First, the new finding after previous bottom-up estimates is not clear in this study. As reviewed in the introduction, many process models have already simulated global CH4 and N2O fluxes. It is needed to be clarified what is the originality of this study, and then what is the major findings and progress in this study.

Response: Thanks. The new findings of this study include: 1) concurrent estimates of CH4 and N2O in consideration of their tight linkage in both terrestrial origin and atmospheric chemistry; 2) address the impacts of multiple environmental factors, including climate variability, rising atmospheric CO2, tropospheric ozone pollution, increasing nitrogen deposition, land use and agricultural nitrogen fertilizer uses, on both gases (fewer environmental factors in previous studies); 3) interannual variations of CH4 and N2O at continental, latitudinal, and biome scales (no previous studies have done so).We reorganize the Introduction of this manuscript and emphasize the new findings in the last paragraph of Introduction (See lines 125-140).

2. The model was previously evaluated in USA and China (Tian et al., 2010, 2011; Xu et al., 2010); but the model for the global application is not well evaluated. Author compared the mean fluxes for some selected sites (Table 1), which is totally insufficient. In addition, surprisingly, no statistics was shown for Table 1 for comparison to the modeled results. Recently, much flux observations were available (e.g., Appendix A in Nicolini et al., 2013), which could be useful in model evaluation. Further comprehensive model evaluation is required before discussing global simulation data.

Response: We did not give a statistical relationship between the model-simulated and observed CH4 and N2O fluxes because most observed studies provided a range of values rather than a specific number, which make us difficult to statistically compare them. We checked the observational data from Nicolini et al. 2013 and add several data with year-wide observation in Table 1. Model evaluation at regional and global scale is added (see the response below).

3. Authors discussed the simulated global budget, spatial variation, interannual variation, and long-term trend as it was true. However, no evaluation was conducted. For example, was the simulated interannual variation of the global budget consistent with the atmospheric concentration record? Inter-comparison of bottom-up models is insufficient as discussed in Melton et al (2013). Without detailed evaluation for site, regional, and global scales, simulated results are only impractical argument.

Response: Thanks for the suggestions. We add additional evaluation work to validate the DLEM-estimated CH4 and N2O against inversion estimates at global scale (CarbonTracker model for CH4 and MOZART v4 model for N2O). The evaluations are shown in Fig. 2 and brief descriptions are added at lines: 268-276 in paragraph 2.3. The evaluation indicates that DLEM-simulated global CH4 and N2O budgets are quite close to the inverse modeling results in both magnitude and temporal variability. In addition, the DLEM-simulated regional/global budgets and spatial/temporal patterns of CH4 and N2O are also compared with other studies based on statistical extrapolation, inverse modeling and process-based modeling in the DISCUSSION sections.

4. As reported in previous studies (e.g., Melton et al al., 2013), areal extent of wetland is one of the most important in determining CH4 budget and its interannual variation. Since description for wetland extent was not available in the manuscript, I am not sure how the model treated interannual variation of the areal extent and its linkage to hydrologic cycle. At least, for discussing climate change effect to the trace gas exchange, detailed modeling for areal extent of wetland is required.

Response: In this study, we closed DLEM simulation mechanisms for dynamically simulating wetland area and extent; instead, we used a constant global wetland data set as the model input data (the global Lakes and Wetlands Database, GLWD). So our current study does not address the wetland extent dynamics but just the water dynamics within the fixed wetland extent. This is a limitation for our current study and will be addressed in future investigations. In the discussion, we did not discuss the effects of multiple environmental factors on wetland extent and area since the wetland extent is

not changed. In Melton et al. 2013 and Wania et al. (2013), we clearly described how DLEM treats wetland input data and simulates soil hydrology. So we did not add the descriptions of DLEM mechanisms in this study. The wetland data used in this study is described in Line 170.

Detailed Model input data is better summarized in a table.

Response: Except for Fig. 3, the change trends of model input data are also summarized in Table 2 and Table 3.

Quality of some figures (e.g., Figs. 5-7) are poor.

Response: The figures 5, 6, and 7 have been redrawn. Page 19820 Line 2: This is not true uncertainties, and must underestimate true uncertainties. Further comprehensive analysis is required.

Response: A modeling estimate may have multiple uncertainty sources such as input data, model structure, parameter values, and many others. It's impossible to include them all. In this study, we only examined the likely uncertainty caused by values of key parameter ensembles. We have generated over 100 parameter ensembles to drive the model, and then get 95% confidence interval for GHG emissions.

2nd Reviewer's comments:

This study considers the interactive effects of climate, land use, atmospheric CO2, nitrogen deposition and fertilizer use, and tropospheric O3 concentrations on global N2O and CH4 emissions from 1981 to 2010 through a series of simulations using the Dynamic Land Ecosystem Model. The primary goals of this paper were to (1) estimate the magnitude of CH4 and N2O fluxes and (2) explore spatiotemporal variations in terrestrial CH4 and N2O fluxes as influenced by the above-mentioned environmental factors. The topic of this study is interesting, and I appreciate that the authors attempt to investigate a variety of environmental factors that may directly/indirectly impact global N2O and CH4 emissions. However, a substantial restructuring and polishing of the manuscript is needed prior to publication. The wording is redundant in many places, and certain aspects are not clearly explained. Primary Comments: 1. The authors mention that large uncertainties may result when scaling up localized measurements to obtain regional and global budgets, as these methods do not represent the biophysical processes regulating N2O and CH4 fluxes. However, I find it interesting that the N2O budget estimates for this study are very similar (12.52 Tg N yr-1 vs. 13.31Tg N yr-1) to those reported in Xu et al (2008), which were developed by empirically extrapolating in-situ fluxes. Although the authors acknowledge this on P19828, there is no explanation as to why the use of a more detailed process based biogeochemical model that accounts for various environmental factors (i.e. climate, land use, atmospheric CO2, nitrogen deposition and fertilizer use, and tropospheric O3 concentrations) would not produce vastly different results. This needs to be addressed in the manuscript.

Response: The mean values during the study period are very similar between two studies; however, the spatial pattern is very much different (see Fig. 5 in this study and Fig. 4 in Xu et al. study). In addition, the interannual variation may be different too (no interannual variation showed in Xu et al.'s study). One of our study objectives is to identify the contributions of all environmental factors to the interannual variations of N2O, which is beyond the capability of empirical approach.

While Table 1 provides a list of the in-situ flux observations used to verify the DLEM CH4 and N2O estimates, it would be helpful to also provide figures showing the spatial locations of these sites, and the statistical relationship between the observed and estimated fluxes

Response: Thanks for the suggestion. We intend to add a spatial distribution map, but the figures in this manuscript are already too many and we have provided the latitude and longitude information in the Table 1. So we think it may be not necessary. Most of the previous studies cited in Table 1 just provide a range of CH4 or N2O emissions. We can only use the reported ranges in these studies to compare DLEM results, which make it difficult to examine the statistical relationship between observation and

DLEM results. Instead, we just mention that our simulated CH4 and N2O fluxes are in the range of the reported field observations. In the revised manuscript, we include additional model evaluation against inversion estimates with statistical matrix.

It is also not clear, in Table 1, which DLEM fluxes are from previous studies (as mentioned on P19820). It would be better to report only fluxes resulting from the simulations used in this study, unless the other studies mentioned used the same parameterization and input data.

Response: We may deliver wrong information over here. We want to say that some of the field observational data are used in our previous papers, while we don't mean that the model results are from previous studies. All the DLEM results in the Table 1 are specially obtained in this study. We rephrase the sentence to make it clear (Line 265).

2. The introduction would benefit from extensive reorganization. If the goal of this study is to examine the impact of climate, land use, atmospheric CO2, nitrogen deposition and fertilizer use, and tropospheric O3 concentrations on global N2O and CH4 emissions, the authors need to make it clear to the reader how/why these factors are important. As currently presented, the information provided is not easy to follow and often reads as a long list (e.g. L21-29, P19814). For example, please explain the mechanisms by which increases in atmospheric CO2 and changes in tropospheric O3 concentrations might impact global N2O and CH4 emissions.

Response: Thanks for the suggestions. We rewrite the INTRODUCTION in two aspects: 1) Emphasize our study significance; 2) add more detailed mechanisms to explain the effects of multiple environmental factors on CH4 and N2O (See Line 95-124).

In section 2.2 (Model description) a couple of sentences are needed to explain the process by which changes in O3 and atmospheric CO2 can impact the simulated N2O and CH4 fluxes. This is not apparent, especially since a detailed description of the model is not provided.

Response: Several sentences are added here to elaborate how the DLEM estimate the impacts of multiple environmental factors.

The impacts of increasing atmospheric CO2 and O3 concentrations on the N2O and/or CH4 budgets were not mentioned (or were not apparent) in the discussion. Did these changes significantly influence the resulting emission budgets? If not, why? Please discuss.

Response: We are preparing another manuscript to study the impacts of individual environmental factors on CH4 and N2O fluxes. The focus of this study is to demonstrate the spatial, temporal, and biome-level variations in CH4 and N2O emission as simulated by DLEM, instead of attributing GHG fluxes to environmental changes. Therefore, we just briefly discussed the relationship between climate change and variability, and spatiotemporal pattern of GHG budget.

3. This study uses an empirical model (Davidson et al 2000) to separate N2O fluxes from NO and N2 during the denitrification process. This so-called 'leaky pipe' approach (Eq. 2) is regulated only by volumetric water content. Is it possible that changes in temperature will affect the ratio of gases converted to N2O? Do the Nnit and Ndenit rates vary with temperature, or are they treated as static parameters in the model?

Response: Yes, VWC is the major factor to determine the separation of N2O and NO/N2. The air temperature, soil moisture, soil texture, pH, available N, and other factors will influence nitrification and denitrification rates. So it is not a static parameterization approach.

4. In the discussion, the relative importance of each environmental factor (temperature, land use, atmospheric CO2, nitrogen fertilization, tropospheric O3 concentrations, etc.) in driving regional/global changes in simulated N2O and CH4 fluxes is not entirely apparent, nor are the interactions between these addressed. Instead, the focus seems to be more on bivariate trends and correlations. The authors should attempt to discuss the potential effects of climate/environmental interactions on the global N2O and CH4

budgets. For example, although increases in N2O and CH4 are attributed to air temperature (in addition to changes in rice production area and heightened fertilizer use), there is no mention of the corresponding effects of increasing precipitation (particularly for the tropics and polar climate zones, Table 3) on the DLEM emission budgets. Did this lead to significant changes in modeled volumetric water content (VWC)? If so, it seems like this would result in higher N2O and CH4 fluxes. Similarly, are the observed increases in CH4 uptake from 2000 to 2010 related only to increases in temperature, or are they also influenced by changes in VWC (e.g. P19825)? Please discuss.

Response: Thanks for the suggestions. We are preparing another manuscript to describe and discuss the effects of individual and interactions of different environmental factors on spatial and temporal change patterns of CH4 and N2O, which is beyond the scope of this manuscript. Here we want to present the estimates for global budget and spatial/temporal change patterns under multiple environmental factors. Yes, part of the increased CH4 and N2O fluxes is owed to precipitation changes during 2000-2010. We add a short discussion in the paragraph "Climate change and CH4 and N2O emissions". See lines: 535-540.

Additional Comments: P19813. L11. Is the impact of permafrost on N2O and CH4 fluxes directly relevant in this paper? If not, is it necessary to mention this here?

Response: The impact of permafrost is not directly related to this paper. Since we intend to review and summarize all the impact factors that influence CH4 fluxes, thus we hope to keep it here.

L15. Why are literature-based estimates provided for global CH4 budgets, but not for N2O?

Response: The estimation range for global N2O fluxes is added (see lines: 75-77).

P19814. L24-29. This sentence is difficult to read, as it essentially is a very long list. Please keep an eye on verb usage (e.g. L25 & L29).

Response: This sentence is revised.

P19815. L4-5. I do not recall mention of elevated CO_2 effects on CH_4 seasonality in the results and discussion. If not, is this information necessary?

Response: We did not mention the effects of all environmental factors on CH_4/N_2O seasonality in the results/discussion; however, the model results implicitly reflect the seasonal impacts since our model runs at a daily time step. As we mentioned before, our in-prep manuscript aims to address the contributions of individual environmental factors on CH_4/N_2O spatial and temporal (monthly, seasonally, and decadal) patterns.

L2-9 are a little redundant. You could just mention that few studies have examined the interacting effects of multiple environmental changes on global CH_4 and N20 emissions.

Response: Sorry, we may not clearly present this sentence and make you misunderstand it. Here our purpose is to point out the unique feature of our research (i.e., our study includes more factors, and addresses the interactions of all these factors) as compared to previous studies (for all studies mentioned in previous sentences of this paragraph; including individual and interactive effects among multiple factors). We modify this sentence and move to the next paragraph, which may better express our ideas here (see Lines 125-129).

P19816. L9. A reference should be provided for the CRUNCEP climate data.

Response: The data download website is added here.

P19816. L13. Should this instead be from 1901-2010?

Response: Thanks. "1900-2010" is changed to 1901-2010.

P19816. Please explain how the daily O_3 index was derived from the monthly AOT40 dataset, and how the EDGAR-HYDE 1.3 emissions data were used to interpolate nitrogen deposition data for 1860, 1993, and 2050. This is not apparent.

Response: The interpolation of nitrogen deposition data is briefly described in Wei et al. (2013). The O_3 interpolation from monthly to daily is simply done by divide the monthly data by the day numbers in a month since we don't have auxiliary daily data for helping interpolation. We add a short sentence here and add the reference Ren et al. (2007) for O_3 data generation (see Lines 151-154).

P19818. L19. Please check to see that a reference is provided for Davidson et al (2000).

Response: Thanks. The repeated words are deleted.

P19819. L2. I could not find Lin et al (2000) in the references. Please check.

Response: The reference is added.

P19825. L14. Pay careful attention to the grammar throughout the ms. E.g. 'to increase' instead of 'increased.'

Response: Thanks. A couple of places with "increased" are changed to "to increase".

P19826. L17. Why the rapid increase in N20 emissions in Europe starting in 2000? If I recall correctly, the prescribed fertilizer use rates were held constant after 2005, due to a lack of input data.

Response: Yes, nitrogen fertilizer use is not changed after 2005. The increase of N_2O after 2000 is mostly because of climate change.

P19827. The wetland distributions used for model N_2O and CH_4 simulations in this study are not provided. This needs to be addressed. Response: Thanks. The data source for the wetland extent and a short description is added in Line 170.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C9201/2014/bgd-10-C9201-2014-supplement.pdf

Interactive comment on Biogeosciences Discuss., 10, 19811, 2013.

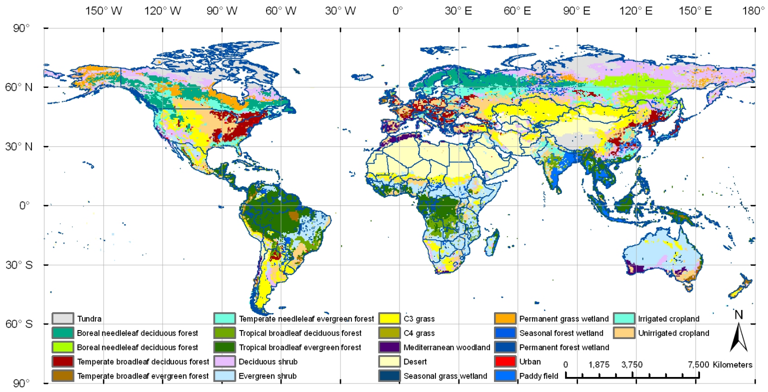

Fig. 1. Figure 1

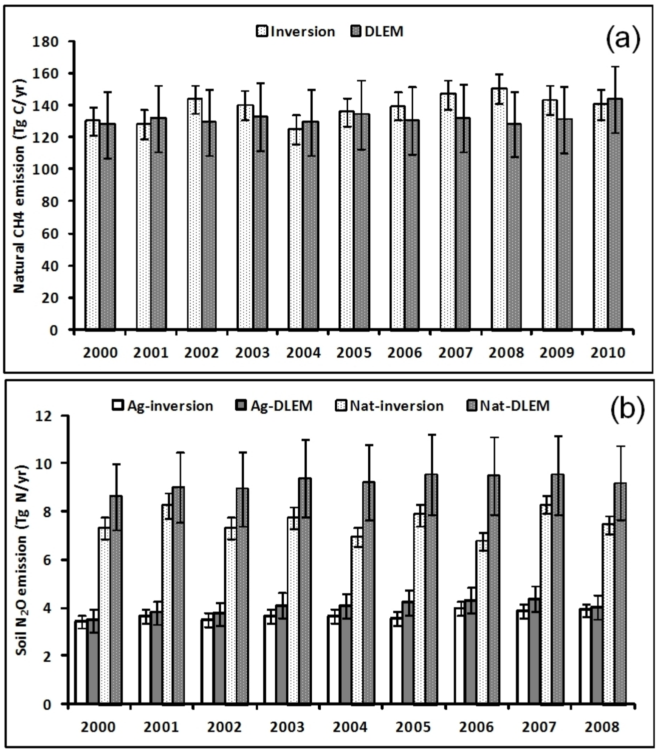

Fig. 2. Figure 2

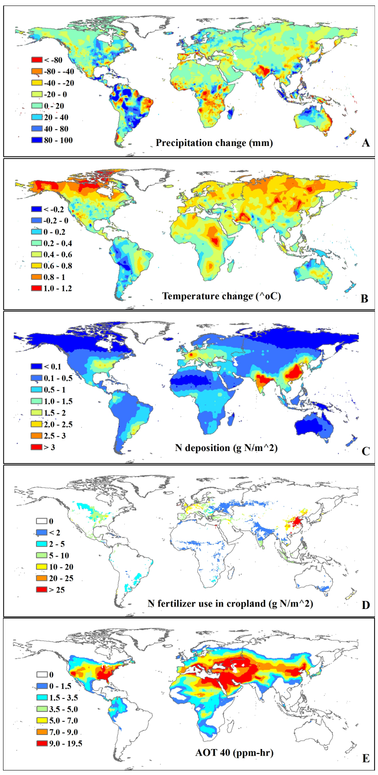

Fig. 3. Figure 3

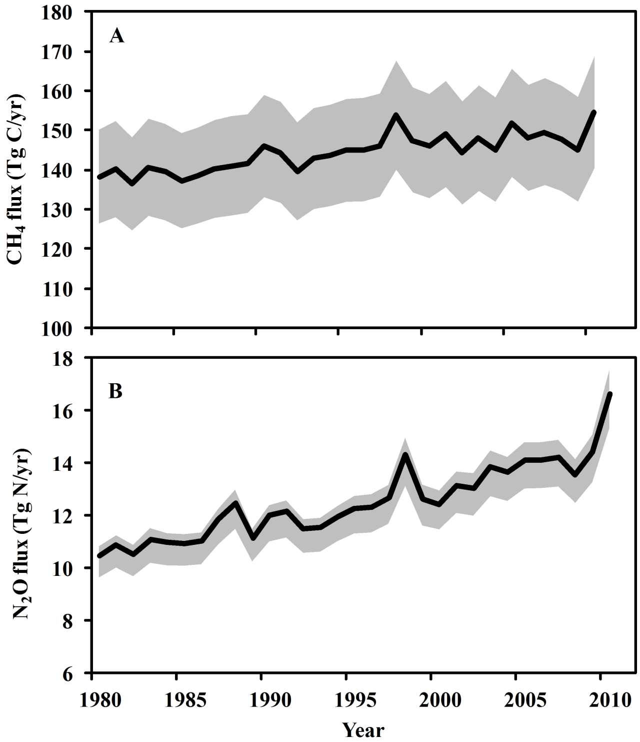

Fig. 4. Figure 4

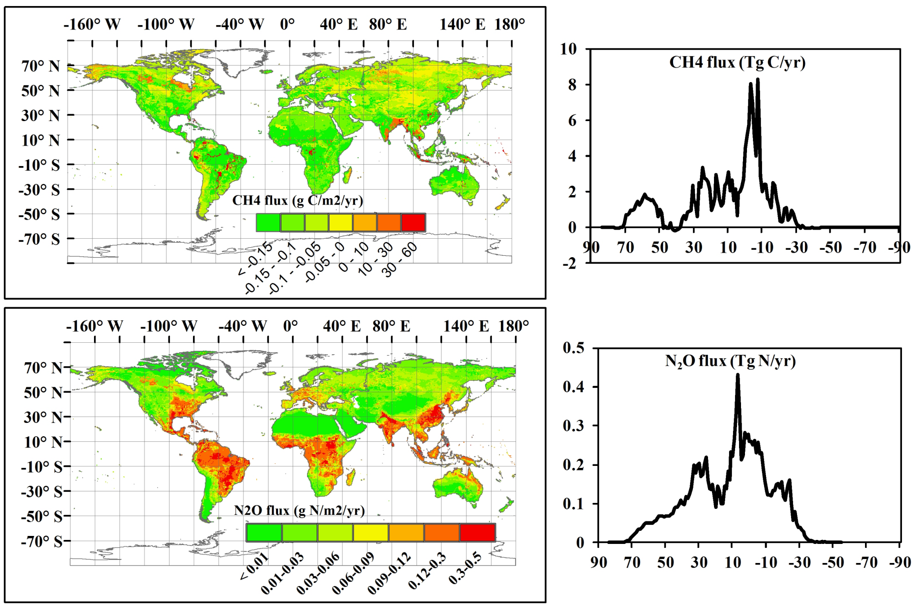

Fig. 5. Figure 5

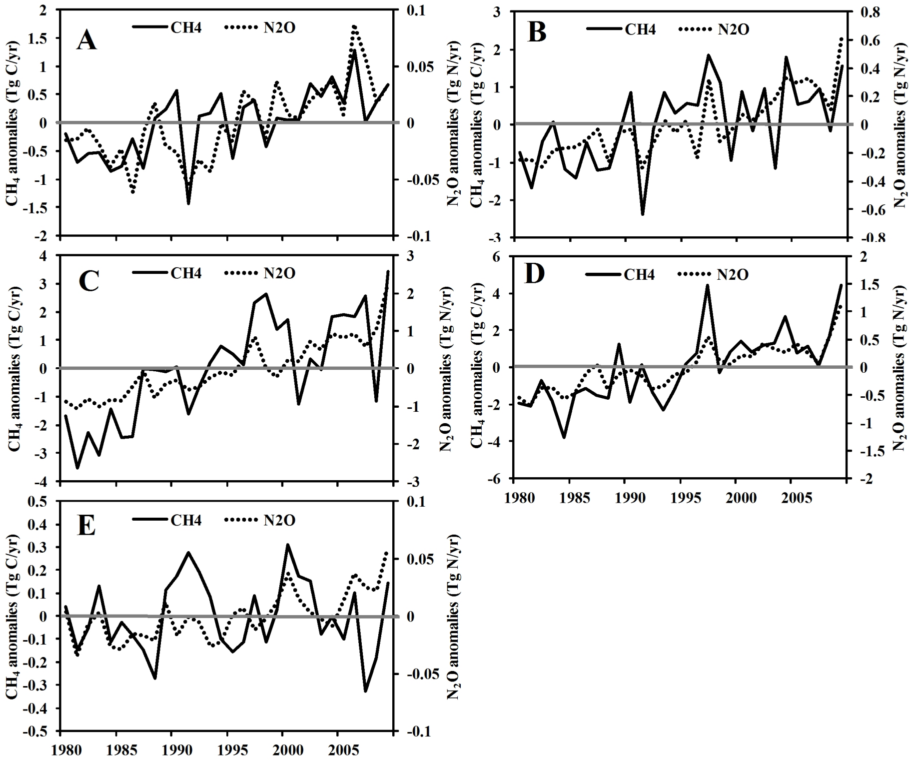

Fig. 6. Figure 6

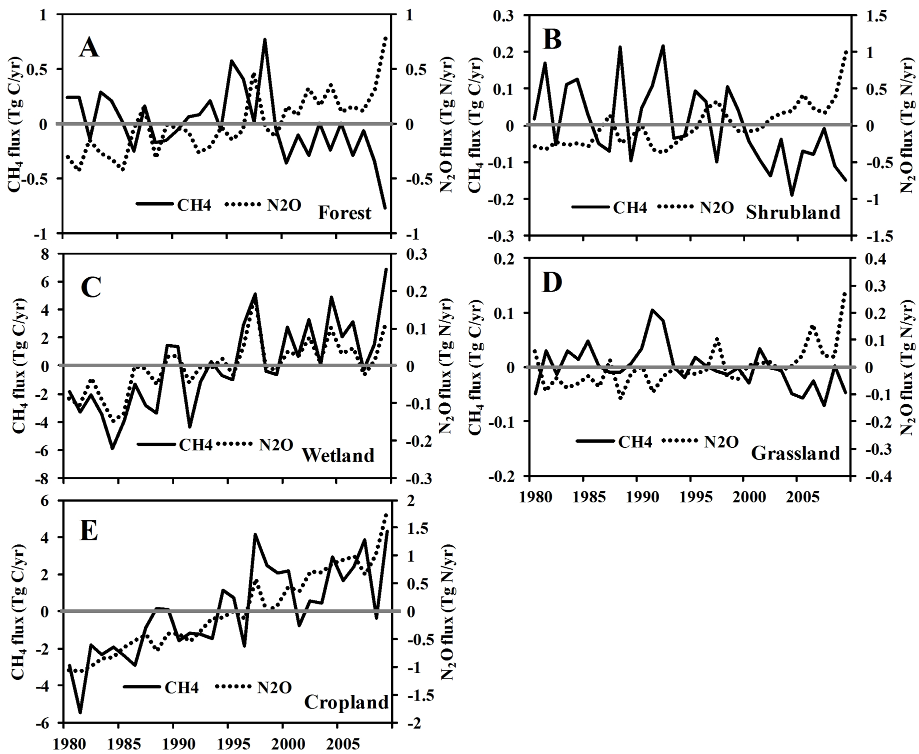

Fig. 7. Figure 7

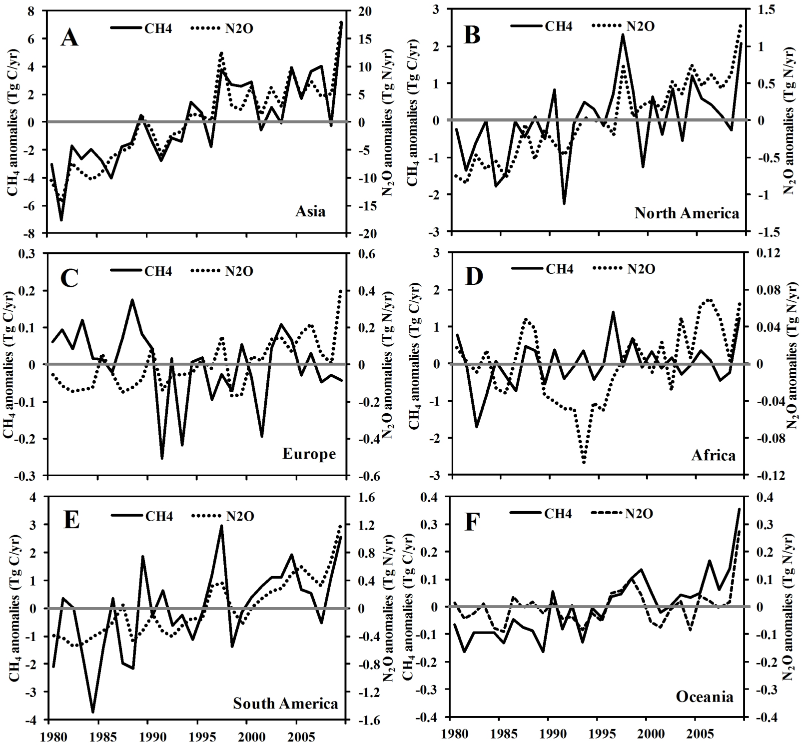

Fig. 8. Figure 8