Biogeosciences Discuss.,
doi:10.5194/bg-2016-522-RC2, 2017

Biogeosciences
Discussions

Interactive comment on “Flooding-related increases in CO_2 and N_2O emissions from a temperate coastal grassland ecosystem” *by* Amanuel W. Gebremichael et al.

T. K. Yoon (Referee)

yoon.ecology@gmail.com

Received and published: 13 February 2017

<General comments>

The study observed that the coastal grassland soil at long duration of flooding produced higher CO2 than that at short duration of flooding, probably due the exogenous substrate and nutrient loadings by flooding. Here, the somewhat contradictory results – (1) the lower CO2 under more hydric soil at the level of environmental variability in each site (e.g., Fig 5b,c); but (2) the higher CO2 under more flooding at the level of comparison between the sites in different flooding regimes (e.g. Fig 4a) – are quite interesting with the qualified data and the reasonable discussion. Personally, I have observed similar results from a nitrogen mineralization study in a temperate forested wetland (re-

fer to dx.doi.org/10.3390/f6092941) and agree to the significance of the substrate and nutrients availability enhanced by flooding in soil biogeochemical processes as well as the general regulation of temperature and soil moisture. Therefore, the study provides a citable case regarding flooding-related GHG emissions.

However, the study design and results may not relevantly reflect the background (frequent flooding by climate change) and the implication of the study. In my understanding, the enhanced CO2 emission by flooding of the study was resulted from local hydrology and topographic factors (e.g., distance from the ditch) rather than regional climatic factors. The study design, LFS vs. SFS represented the different flooding regimes in response to topography, not climatic events. If the authors were interested in the interactions between CO2 flux and flooding in response to climate change, authors could compare interannual differences of CO2 flux. For example, at the level of interannual comparison, CO2 flux in SFS during the period C, in which a prior flooding (period A) had occurred, was lower than that during the period E without prior flooding. In SFS, 2014 was more flooded year in response to interannual climatic variability; however, CO2 emission was rather reduced in contrast to the authors' point of view. Therefore, I agree that “longer term flooding therefore increased, rather than reduced, the annual emissions by approximately 40 %” (P10 L19), in terms of the site hydrology affected by the topographical variation; whereas I reject that “any increase in freshwater flooding in response to climate change could result in a significant increase in carbon dioxide emissions from these systems” (P10 L20), according to the reduced CO2 in SFS, 2014. The flooding related increases in CO2 emission of this study could indirectly imply the relationship between frequent flooding by climate change and regional GHG budget, however, may provide little direct insights.

Moreover, as the editor and another reviewer already mentioned, lack of CH4 observations would be a critical limitation of the study which aimed to account GHG emission in especially flooding-related condition. Authors should put efforts into justifying the exclusion of CH4 with persuasive statements and/or supporting materials (references or original data). I assume that CH4 was not investigated because the gas analyzer available to the authors does not support CH4 detection. Perhaps, CH4 emission might not be critical in the studied site where dominant CO2 emission occurred during the dry, growing season. Nevertheless, authors should clarify the study without CH4 measurements could provide complete, independent, and valuable knowledge in flooding related CO2 and/or GHG fluxes. I suggest presenting preliminary CH4 data with a minimum number that address the function of CH4 fluxes in the GHG budgets of the sites, if available.

In sum, the study presents interesting and valuable findings; however, two critical concerns, 1) somewhat irrelevant interpretation in the context of climate change and 2) lack of CH4 measurements remain. I expect the authors would improve the manuscript successfully in response to my queries.

<Specific comments>

Title and Abstract

P1 L1, P1 L22, P14 L28: Was N2O emission increased by the flooding? I could not find an evidence supporting the higher N2O emission at more flooded condition.

P1 L22, P12 L2, P14 27: There is no direct evidence supporting the changes in microbial population by flooding. The difference in Q10 values is a weak evidence.

Methods

P4 L19: In my experience, at field, a hydric soil with low bulk density can be easily compacted by investigators who stand on the soil for measurement; consequently, the compaction can physically facilitate the gas evasion from the adjacent soil, resulting in biases in measurement. Have the authors considered this issue in the measurement?

P4 L36: Were there specific QA/QC procedures for the PAS analysis, such as calibration and maintenance?

P5 L9–10: Why were the annual CO2 and N2O emissions estimated for two pseudo-different, mostly overlapped periods? I know the study only covered one and half year. Authors might attempt to provide inter-annual values. However, the two periods (Feb 2014–Feb 2015 vs. May 2014–Apr 2015) overlapped too much; therefore, the values in the two periods must be analogous. Why were the values from Apr 2015 to Aug 2015 excluded in the annual estimates?

P6 L28: Could the wet soils be sieved through a 2 mm sieve? In my case, wet soils for enzyme activity analysis were sieved through an 8 mm one. I assume a 2 mm sieve seems too fine to sieve wet soils.

Results

P8 L20: If authors were interested in the seasonal variation at each hydroperiod, the averaged values of CO2, N2O, and other environmental variables for each hydroperiod (from period A to E) could be presented in a table or a figure (bar graph) with statistical tests. In addition, annual estimates of CO2 and N2O and averaged values for all periods can be included in the table or the figure.

P9 L 21: Relationships between CO2/N2O fluxes and environmental variables (e.g., soil temperature, soil water content, water depth, redox potential, and probably microbial variables) could be presented in a table with various simple linear, multiple linear, and nonlinear regressions.

P9 L29: I am not sure whether the water depth could be a relevant independent variable for CO2/N2O modeling because the water depth data are available only for flooding period in the LFS. In other words, even in the LFS, the relationship between water depth and CO2/N2O does not cover the high CO2 emission during the growing (temperature higher than 15 °C), non-flooded (water depth lower than zero) season.

Discussion

P10 L18: The ranges of annual estimates, which were calculated from the two points with pseudo interannual replicates, are meaningless.

P11 L17: I agree the substrate and nutrient loading by flooding could be a main driver of the enhanced CO_2 emission. However, I am not sure that the vertical profile of soil C and N could be an evidence for the statement. Could you provide references or another evidence supporting the higher surface soil C driven by external sources?

P11 L34: The dependence of CO_2 emissions on soil temperature is generally observed in thousands of soil CO_2 studies and unquestionable today. I think the discussion can be shortened. In addition, Q_{10} can be an indicator of the sensitivity to climate change; however, the Q_{10} values from exponential regressions with the low goodness of fit might not be reliable indicators.

P13 L15: As I already mentioned, relationship of CO_2 to water depth is only limited during the period when CO_2 emission was not intensive. In addition, "most of this variation is explained by changes in water depth alone" is it true? Soil temperature also explained 56% of variation in CO_2. The 62% of the explanatory power for temperature and soil water depth dependent model and the 45% of explanatory power for the soil water depth dependent model do not mean the small explanatory power for the temperature dependent model. I suggest that the soil water content may substitute the water depth of the model.

P14 L2: Could the growing vegetation directly uptake N_2O? Please povide an evidence or reference.

Tables 1 and 2: Present statistical differences in the variables between the two sites and standard errors, too.

Figure 1: I suggest adding photo for flooded and non-flooded period of the LFS and SFS.

Figure 4: Is the soil water content volumetric or gravimetric?

<Technical comments>

P3 L1: also be contributory factors controlling -> control

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