Biogeosciences Discuss.,
doi:10.5194/bg-2016-105-RC2, 2016

Biogeosciences Discussions
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Interactive comment on "Ecosystem feedbacks from subarctic wetlands: vegetative and atmospheric CO_2 controls on greenhouse gas emissions" *by* Matthew J. Bridgman et al.

Anonymous Referee #2

Received and published: 26 May 2016

The manuscript presents a combination of field measurements and a laboratory experiment aimed at discovering species-specific influences on subarctic wetland methane emissions under ambient and elevated atmospheric CO2 concentration. The study concludes that species-specific changes in biomass under elevated CO2 result in corresponding increases or decreases in CH4 emission.

The topic is of strong interest, as arctic and subarctic wetland methane exchange is known to be species-specific, and large uncertainties exist as to the response of northern latitude CH4 emissions to a changing climate. The study is well within the scope of Biogeosciences, and I think it has a lot of potential. The combination of in situ measurements and a laboratory experiment is a nice approach. The statistical approach

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is valid. It is well-structured and the results are presented in a logical fashion. However, I have some concerns about the presentation and interpretation of the results, and some recommendations to improve the clarity of experimental setup and breadth of the discussion prior to publishing the paper.

Major points:

1. The abstract (lines 20-26), results (lines 206-210), and discussion (lines 331-333) assert that changes in CH_4 emission followed changes in biomass under CO_2 fertilization treatment, going on to cite species-specific directional changes. However, at least half of the cited directional changes appear non-significant in Fig. 2a (i.e. E. angustifolium and E. vaginatum) as a result of a small mean effect relative to very large error bars. The same generally be said of the biomass results for these species (Table 1).

Perhaps the underlying concern here is that the within-species variability is so large that it stymies the reader's ability to interpret the differences both among species as well as species-specific treatment effects. This could simply be an issue with the use of bar graphs that lump all within-species variability (over time and replicates), making it appear that there were very few significant differences.

I recommend playing with different plotting formats that more clearly show the significant effects identified in the mixed model. In addition, pairwise comparison statistics could be used to support which species were actually responsible.

If the issue is not simply a matter of plotting, I recommend expanding the analysis to look deeper into the within-species variability, both for the in situ and ex situ observations. Perhaps clearer species-specific influences will emerge after accounting for other controls.

2. The intro states that vegetation is a primary control of CH_4 emission from wetlands, and lists a few example mechanisms (line 55-64). While these examples are certainly relevant to the present study, they are inadequate given that the study is centered on

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species-specific plant influences on CH_4 emission. For example, there is no mention of the widely-cited positive effect that vascular plants have on CH_4 emission by providing a conduit for CH_4 to the atmosphere (Morrissey and Livingston 1992, JGR-A; Joabsson et al. 1999, Trends in Ecology and Evolution; Joabsson and Christensen 2001, GCB; Christensen et al. 2003, Biogeochemistry; von Fischer et al. 2010, JGR; Kutzbach et al. 2004, Biogeochemistry). Many of the references above have focused on Eriophorum and Carex species. More background on species-specific CH_4 control mechanisms (if possible, specific to the species investigated) will help set up the interpretation of the results in the discussion.

3. The Methods section is lacking in important details of the site layout and measurement setup, which adds difficulty in interpreting the results. Specifically:

a. Line 123: How far apart were the plots? How were they selected?

b. Line 123-126: How were the chambers placed upon the surface? Were collars used? Did this differ between vegetated and open water plots? How was access to these plots established to minimize disturbance?

c. Line 127-128: What time of day were measurements taken? What was the total time taken to measure all plots?

d. Line 131-132: Were any measurements rejected due to non-linearity?

e. Line 137-138: What were the dimensions of these chambers?

f. Line 141-142: Are these replicates per 400 or 800 ppm treatment? Or total?

g. Line 142-144: What was the water level in the pots? Was water from the site used? What were the dimensions of the transplanted peat/plant, and were the same dimensions used across the experiment? Why were the recovered plant and soil samples separated? I would expect them to remain whole to minimize shock.

h. Line 146: If light levels were constant during daytime, this sentence should read

"...daytime light levels were constant at 400..."

i. Line 154-155: Were measurements taken under "daytime" conditions?

j. Lines 156-158: Why were only 2 air samples made to estimate flux? This would prohibit checking for linearity.

k. Lines 161-163: Please specify manufacturer and model of redox probe and rhizon samplers.

l. Line 168-170: Was initial starting biomass estimated? It does not appear so, since biomass estimates were measured destructively only at the end of the experiment. This is fine, but the manuscript interprets the biomass results (Table 1) as if the starting biomasses were equal for all same-species replicates across 400 ppm and 800 ppm treatments. This assumption and justification for making it should be explicitly stated, since the main conclusion of the paper stems from these measurements.

These missing site and experimental details are most apparent when attempting to understand the order of magnitude difference in CH4 emissions between the in situ and ex situ observations (Fig. 1 vs. Fig. 2). Reconciling these differences requires details on the dimensions of transplanted plants. I would expect the in situ and ex situ flux measurements to mimic each other as much as possible in terms of filling the floor area of each pot so that a per-area flux measurement would be comparable. If not, the reader needs to be oriented to expected differences with explanation and justification.

4. There is no mention of the plant extension growth measurements in the results. These data would be useful to support the destructive biomass sampling results performed at the end of the experiment.

5. The ex situ and in situ results are presented and discussed in isolation. Is there anything to be learned about the in situ results from the ex situ results, and vice versa?

6. The TOC, TN, TOC:TN, and E4:E6 are hard-won ancillary data that could potentially explain within-species variability as well as species-specific responses to elevated

CO_2. However, aside from a general discussion (line 304-311) on how this data "highlights the influence of species composition in these wetlands on rhizospheric carbon inputs, . . ., with implications for CH_4 production", the implications for different CH_4 production between species and associated response to elevated CO_2 is left untouched.

7. The contrasting changes in ex situ fluxes over time (line 206-210) is an important result that may shed light on species-specific plant-mediated CH_4 transport, yet it is unmentioned in the discussion.

Minor points:

8. I am unconvinced by the explanation (lines 251-257) as to why open water and vegetated CH_4 emissions were not significantly different, given that the dominant transport pathway of CH_4 to the atmosphere often differs substantially between vegetated (transport through vascular plant material) and open water areas (diffusion/ebullition). More discussion that references transport pathway is warranted here, since diffusion alone is unlikely to support the very high CH_4 fluxes observed at this site.

9. What is the future direction of this research? What are the limitations of this study and unanswered questions to be resolved?

10. The manuscript would benefit from editing to correct several typos and grammatical errors.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-105, 2016.