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Discussions

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

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We have responded to the reviewers major and minor points in order.

1. We agree with the reviewer that it is a good idea to develop the rationale of the study further. Indeed it is the first study of impacts of elevated CO2 in subarctic peatlands. We chose the species we chose because both Eriphorum and Carex are known to contribute to CH4 production in these wetlands (the relevant references from the Introduction is Prater et al., 2007; Christensen et al., 2004; Hodgkins et al., 2014). For the revisions we plan to provide more depth in this section in line with the reviewers' suggestion referring to the role of these species in the CH4 cycle. Regarding our working hypothesis for the field measurements, our hypothesis was that we would see

more CH4 emitted from plots with plants due to root exudation stimulating CH4 production and plant mediated transport. We also hypothesised more CH4 being emitted from higher biomass species. We are happy to add this to the paper and develop this aspect of the work in the discussion as suggested by the reviewer. It will also help integrate the laboratory and field data as suggested. 2. In the field we used floating chambers. As the wetlands were shallow we did not need to use boats or bridges. To reduce the impact of disturbance plots were allowed to settle for 5 minutes after the chamber was put in place before the first gas samples were taken. We assessed all of the regressions for linearity and in a few cases we had to remove a set of samples when high initial CH4 concentrations indicated ebullition during chamber installation. We'll describe experimental method including a schematic diagram of the field and laboratory set up as supplementary information together with photos to clarify the sampling conditions. In the pot experiment the peat was covered by 2-3 cm of water. 3. In response to this valid point by the reviewer we suggest addressing the limitations of the experimental set up in the discussion section where we can directly link the limitations of the respective field and laboratory conditions to the interpretation of the data. It is worth noting that the field experiment was intended to be complementary to the laboratory experiment not be used as a direct comparison due some of the factors the reviewer points out. In the discussion we can describe the limitations of comparing them directly in terms of difficulties associated with long term mesocosm experiments and the impacts on soil and water chemistry, differences associated with gaseous exchange mechanisms including lack of circulation resulting from weather factors etc. It is possible that the structure of the peat has changed during the experiment as the reviewer suggests however we don't think this will have had a major impact on methane oxidation as the peat in the laboratory experiment was always submerged. It is however possible that methane oxidation rates were affected by root growth and root oxygen release during the cause of the experiment. Specific comments Line 105. We will add the mean July temperature from the nearby Abisko weather station. Line 107. We will add information on the pH and electrical conductivity and can add in the actual moisture levels of mesic areas (i.e. peat of intermediate moisture). The pH, electrical conductivity, active layer depth and vegetation in our study mire is representative of large areas of palsa mires in northern Scandinavia. Line 126. In the field fluxes were measured after 5, 10, 20 and 30 min with a total incubation time of 30 minutes. The R2 values of the regressions was variable depending on what process dominated therefore we chose not to use a hard cut using R2 (e.g. in the instance of low CH4 production and CO2 fluxes close to zero indicating a balance between uptake and emission) instead we visually inspected the regressions to ensure they were not affected by extreme values. Line 129-130. We will report the analytical error and the specific GC settings in the manuscript in line with the recommended reference. Line 141. Peat was taken as bulk samples (several ca 20*20*20 cm blocks), and separated into pots. Peat was taken one metre below water surface in areas which were free from vegetation. Line 143. The water level was 2-3 cm above the peat surface. Due the growth form of E. vaginatum these were measured and grown as tussocks in the field and in the lab, the other species were measured as individual shoots both in the field and the laboratory. We will clarify this further in the text in the methods section as this has important implications for comparing the field and laboratory data as the reviewer points out. Line 145. Approx. 1 litre of peat, which was submerged as explained above. We did not control pH and ion concentrations but measured water chemistry at the end of the experiment and redox throughout. We suggest discussing these limitations in the discussion as mentioned above. Line 246. The site is minerotrophic and comparable to the sites referred to in the discussion both with regards to comparable basic chemical parameters and vegetation composition: The site is affected by water input from snowmelt run-off from surrounding areas, it is also influenced by influences by ground water. The pH is 4.3 $\pm$ 0.1 and the electrical conductivity is 66 $\pm$ 31 uS, extractable PO43- is 3.9 $\pm$ 1 ug g-1 and NH4+ is 0.12 $\pm$ 0.02 ug g-1. We suggest to add these data to the paper to inform the readers about the peat properties. The high emissions are comparable to nearby mires which have similar properties e.g. melting permafrost causing subsidence, flooding and establishment of graminiods which support high methane emissions (e.g. Prater et al., 2007,

Hodgkins et al., 2014). We will make this more explicit in the text of the revised ms. Line 252. All the sampling sites were underlain by peat, at relatively similar stage of decomposition (fairly shallow mire). E.vaginatum is tussock forming but not the other species. Line 252-258. We agree that root oxygenation is an important process which may impact on emissions. We will be happy to take your suggestion on board and in the resubmission we propose to outline plant impact on CH4 emissions, the direction (positive or negative) or the impact and potentially antagonistic influences. Line 262 We will make this change. Line 259-274. We have not identified any studies on elevated CO2 on the common boreal/subarctic wetland species we chose in our study which adds novelty to the study but makes comparisons with other studies more difficult. However, these Eriophrum and Carex are relatively well studied with regards to their role for plant mediated transport of CH4 (as pointed out by reviewer 2) which we plan to outline in the introduction in response to the reviewers comments. Our last sentence of that paragraph referees to a study which found that nutrient limitation impacts on the plant growth responses to elevated CO2. However, in our study we did not analyse the nitrogen demand of these species so it is difficult to assess if the response we found are driven by nitrogen demand specifically.

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