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Biogeosciences Discussions

Interactive comment on "Soil moisture control over autumn season methane flux, Arctic Coastal Plain of Alaska" *by* C. S. Sturtevant et al.

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We thank this anonymous referee for their thorough evaluation of the manuscript and helpful comments and suggestions for improvement. Below we have provided in-text responses to each of the comments made by this reviewer.

General Comments This manuscript analyzes controls on methane fluxes during autumn at the Biocomplexity Experiment near Barrow, Alaska. Methane fluxes were measured across three experimental plots where water table height was manipulated. The authors use GLM to analyze environmental factors - such as soil moisture, temperature and wind – on methane flux rates. The work was inspired by a previous study in Greenland where a large pulse of methane was

released from soils during autumn. The authors here did not observe a similar pulse, but they do provide estimates of autumn methane fluxes, which account for 18% of cumulative fluxes during the growing season. While this topic is generally of great interest to the scientific community, the manuscript needs considerable work before it can be accepted for publication. First, the writing could be substantially improved and more concise with additional editing for clarity and accuracy. I've made a number of specific comments below, highlighting areas that need attention.

We appreciate the reviewer recognizing the value of the data presented in the manuscript. We thank the reviewer for their helpful suggestions to improve the clarity and accuracy of the writing and will focus on achieving this in the revised manuscript.

Second, it's unfortunate that the authors decided not to make thaw depth or water table height measurements during their study period. Thaw depth could be simulated using in-situ temperature measurements in association with a thermal model (e.g. GIPL). The authors might be able to gap-fill missing water table measurements by examining the relationship between soil moisture and water table during the summer season, and then use that relationship to predict fall water table height.

We recognize the need to characterize thaw depth and water table during the autumn. We are constructing models for each of these variables to extend our summer measurements into the autumn season will include these in the revised manuscript. Additionally, we have collaborated with another researcher who collected a set of thaw depth measurements at the site in mid-September, 2009, which will help validate the modeled thaw depths.

Third, the authors need to do a better job of putting this study in a broader context and supporting their findings with more recent citations from the literature. In particular, it seems relevant to discuss their findings in context of changing

surface water coverage in the Arctic, changing seasonality and growing season length, and generalizability of findings to tundra ecosystems across the circumpolar region (not just North Slope of Alaska).

The revised manuscript will better present our results in relation to the published literature and in the context of climatic and ecosystem change in the Arctic.

Fourth, the statistical analysis of flux data could be improved. See specific comments below.

This topic is addressed in the specific comment below (#35).

Specific Comments 1. Page 6520, Line 9 – I don't believe "freeze-in" is the appropriate term here. Try "freeze-up" or "period of freezing". Change throughout manuscript

We will use the suggested changes in the revised manuscript.

2. Page 6520, Line 11 – Change "liquid" to "unfrozen" soil moisture.

The suggested change will be made in the revised manuscript.

3. Page 6520, Line 22 – Ice does not have insulative properties. I believe you are referring to the effects of latent heat exchange associated with phase change. Please revisit Romanovsky and Osterkamp 2000 for a good overview of this subject with respect the ground thermal regime.

We thank the reviewer for this correction. This point will be clarified in the revised manuscript.

4. Page 6520, Line 23 – I recommend replacing "liquid" with "unfrozen" moisture content through the manuscript. This again, is more consistent with the literature on frozen ground.

The suggested change will be made in the revised manuscript.

5. Page 6521, Lines 3-4 – This statement is not entirely correct. Decomposition is a complimentary process that provides C substrate for the 2 primary methanogenesis pathways.

This statement will be corrected in the revised manuscript.

6. Page 6521, Line 9 – Whalen and Reeburgh 1992? There more recent estimates of CH_4 emissions from northern regions. See work by Zhuang et al., in particular.

This statement will be changed to give a more recent estimate of CH_4 emissions.

7. Page 6521, Line 11 – "Disproportionately" relative to what?

This statement referred to evidence of more dramatic environmental changes in the Arctic as a result of global warming relative to lower latitude ecosystems. This statement will be better clarified in the revised manuscript.

8. Page 6521, Line 23 – Replace "depth of seasonally thawed soil" with "active layer thickness".

The suggested wording change will be made throughout the revised manuscript.

9. Page 6521, Line 25-27 – I surprised that you didn't cite any of the thermokarst lake and CH_4 ebullition studies here. Seems like an important factor in tundra regions underlain by ice-rich permafrost (e.g. yedoma).

We agree that citing recent studies of large CH_4 emissions from thermokarst features is warranted here, as it has relevance to our study. Although the Barrow site is not underlain by yedoma, we recognize the importance of placing our site and current CH_4 emission estimates in this context.

10. Page 6522, Lines 10-12 – What did the authors of the Greenland paper cite as a possible mechanism for the large autumnal methane pulse?

Mastepanov et al. (2008) suggested that the large autumn methane emissions observed at the Greenland site were a result frost action squeezing out methane which had accumulated in the active layer and the inability of the released methane to diffuse downward due to the presence of permafrost. This topic will be included in the revised manuscript where we discuss our results in comparison to previous studies.

11. Page 6522, Line 16 – What are some of the “rapidly changing environmental conditions”? Also, you seem to ignore much of the recent literature on changing growing season length (e.g. Euskirchen et al. 2006). What are the implications of changing seasonality for autumn CH_4 fluxes? In particular, how does changing growing season length influence soil moisture?

This statement was referring to the rapid decline in daily solar radiation and corresponding declines in air and soil temperatures as well as unfrozen soil moisture content as the active layer during refreezing of the active layer. This will be better clarified in the revised manuscript. We thank the reviewer for the suggestion to include discussion of changing growing season length and its influence on soil moisture, as these are certainly relevant to the present study. We will include this topic in the revised manuscript.

12. Page 6524, Line 3 – Replace “northern tip” with “North Slope”

This change will be made in the revised manuscript.

13. Page 6524, Line 4 – “Polygonized” is not a term used to describe acidic tundra. I would add a sentence here to describe polygonal ground as an indicator of the presence of ice-wedges and ice-rich permafrost.

This suggestion will be incorporated into the revised manuscript.

14. Page 6524, Line 9 – Replace “annual average” with “mean annual” with respect to temperature and precipitation. Those terms refer to two different things.

This change will be made in the revised manuscript.

Page 6524, Line 11 – Your definition of active layer depth is incomplete. Please revise to state that it’s the “maximum depth” of seasonally thawed ground.

This change will be made in the revised manuscript.

16. Page 6524, Line 24-25 – Reword sentence here. Omit “themselves are thought”. We know that they originate from local thawing of ice-rich permafrost and subsequent subsidence.

This change will be made in the revised manuscript.

17. Page 6525, Lines 16-25 – I recommend omitting this paragraph.

We think this information is important to include. It gives background on how the water table manipulation was conducted at such a large scale and gives context to the measurements presented in the manuscript.

18. Page 6526 – The site names “North”, “Central”, and “South” are not informative to the reader unfamiliar with these study sites. I recommend renaming sites to describe treatments (Control, Raised, Lowered) to improve clarity for the reader.

This naming convention was used in order to be consistent with the published studies from this site. In addition, the effect of the manipulation was not always straightforward (as discussed in the response to Anonymous Referee #1 as well as in the published studies). We feel it is important to retain the “North” / “Central” / “South” naming convention to improve the comparability and accuracy of the studies resulting from this manipulation (for example, the South was the driest site in 2007 and in this study the South section cannot be called a control because water was added in late July). However, we also recognize that it is important to improve the readability of the manuscript and will therefore use “North (flooded)” / “Central (drained)” / “South (intermediate)” in the revision.

19. Page 6526, Lines 10-11 – Did you evaluate the chemistry of the water being

pumped from the pond into the raised treatment?

The chemistry of the water pumped from the nearby lake into the drained lake basin was evaluated by David Lipson. Low nutrient content (NH_4^+ and PO_4^-) and pH similar to equilibrium with the atmosphere were found, which was similar to standing pond water in the basin (David Lipson, personal communication).

20. Page 6527, Lines 10-12, Omit sentence beginning "Our group has worked extensively with LI-COR... "

This sentence will be omitted in the revised manuscript.

21. Page 6529 – Line 13 – What do you mean by "lower frequency"? Please be specific in this section in describing the measurement interval for each parameter.

The frequency of the described measurements was given in the last sentence of this paragraph.

22. Page 6529, Line 13 – Where your temperature measurements at the soil surface exposed to radiation from the sun? Please clarify.

The temperature measurements at the soil surface were located within the moss layer and were not exposed to radiation from the sun. This will be clarified in the revised manuscript.

23. Page 6529, Lines 20-21 – Describe in more detail how you measurement soil moisture content. How did you insert probes into the soil (vertically or horizontally)? What soil horizons coincide with these depth increments? Also, how did you calibrate temperature and moisture at these sites? Please specify.

The 0-30 cm soil moisture probes were inserted vertically into the soil. For the two other depth ranges (0-10 cm and 20-30 cm), the soil moisture sensors were installed diagonally within the depth range specified (the probes are 30 cm long). The organic

layer in the Biocomplexity basin is approximately 12-15 cm thick and is underlain by a silty, marine-derived mineral layer (Lipson 2010 JGR). Uncalibrated soil moisture values were converted to % saturation (% of available pore space occupied by unfrozen water) by applying a linear scaling equation which forces a representative maximum value (obtained from data at the site when the water table was known to be above the sensor level and thaw depth below the sensor level) to 100%, and forces a representative minimum value (obtained during winter at the coldest soil temperatures) to 8-14% saturation (depending on depth). Data from Hinzman et al 1991 (minimum unfrozen volumetric moisture content during winter and soil porosity) were used to calculate the minimum % saturation for the organic and mineral layers, respectively. Soil temperatures were obtained with the standard calibration applied by the type T thermocouple instruction in the CR23X datalogger. These methods will be better clarified in the revised manuscript.

24. Page 6530, Line 8-9 – It's troubling that you decided not to characterize water table or thaw depth during the study period that this manuscript is actually focused on. These seem like critical controls on the flux of CH_4 at these sites. Perhaps you could reconstruct freezing front/thaw depth dynamics during your study period using temperature profiles or zero-degree isotherms.

The response in the General Comments above addresses this concern.

25. Page 6531, Line 11 – "offsets" is not the appropriate terminology here. How about "variation across treatments... " Also, report error associated with average thaw depth values.

This will be changed in the revised manuscript.

26. Page 6531, Line 19 – The phrasing "minimum daily average for the study period" doesn't make sense in this context.

The statement will be clarified in the revised manuscript.

27. Page 6531, Line 21 – Revise sentence to state "active layer began freezing from the top-down"

This will be changed in the revised manuscript.

28. Page 6533, Line 6 – Again, study plot naming conventions need to be consistent throughout the manuscript.

The response to Comment #18 addresses this concern.

29. Page 6533, Lines 10-12 – I recommend moving this text with the citations to the Discussion section. There are actually quite a few sentences in the Results section with literature citations. These should all either be omitted or moved to the Discussion section.

The suggested changes will be made in the revised manuscript.

30. Section 4.5 Summary of key results – Omit, this is redundant with the results reported above.

This section will be removed in the revised manuscript.

31. Page 6537, Lines 3-5 – Don't you have summer data to illustrate whether or not soil moisture content in the top 30 cm is a good predictor of water table height? Analyze your data to support or reject this idea.

Modeling the water tables in the revised manuscript (discussed in the General Comments) will alleviate this concern. Although the soil moisture content in the top 30 cm of soil is generally correlated with the water table later in the growing season, correlation using summer data, especially early- to mid- season, is confounded by deepening thaw. Soil moisture contents in the top 10 cm, however, are much better predictors of water table height since thaw depth reaches 10 cm relatively early in the summer. We are currently using the 0-10 cm soil moisture sensors to model the water table in the autumn.

32. Page 6537, Lines 15-27 – This paragraph could be further supported with some literature citations (e.g. Bubier et al. 1993, 1995, etc.)

We thank the reviewer for mentioning the suggested studies to support the influence of microtopography on CH_4 emissions observed at this site. We will include these in the revised manuscript.

33. Page 6537, Lines 28-29 – How can soil moisture be an indicator of soil moisture? Re-word.

This statement will be corrected in the revised manuscript.

34. Page 6538, Line 20 – The positive correlation between wetness and temperature is due to the effect of soil moisture on thermal conductivity, which governs rates of heat conduction. This text needs to be clarified to make this point.

This point will be clarified in the revised manuscript.

35. Table 1 – Why use a GLM statistical approach? Seems like the statistical significance (but low partial R2 values) of wind speed x soil moisture, soil temperature, and radiation are driven primarily by the large sample size (>1400!) and not by an ecologically driven process.

The General Linear Model with pooled data was used to identify the major factors for the entire site which explained the variation in CH_4 emissions, and was presented mainly to show the large influence of unfrozen soil moisture. However, based on this and other reviewers' comments we agree that a more appropriate and complete statistical analysis is needed, including separate analysis of the controlling factors for each manipulation section. We also agree with the reviewer that the manuscript should be more critical of the ecological relevance of variables identified as significant but explaining a low percentage of the data. The statistical analysis in the revised manuscript will use daily averages to reduce significance simply based on a large sample size.

Interactive comment on Biogeosciences Discuss., 8, 6519, 2011.