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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.

Anonymous Referee #3

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

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

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

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.

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.

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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.

6. Page 6521, Line 9 – Whalen and Reeburgh 1992? There more recent estimates of CH4 emissions from northern regions. See work by Zhuang et al., in particular.

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

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

9. Page 6521, Line 25-27 – I surprised that you didn't cite any of the thermokarst lake and CH4 ebullition studies here. Seems like an important factor in tundra regions underlain by ice-rich permafrost (e.g. yedoma).

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?

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 CH4 fluxes? In particular, how does changing growing season length influence soil moisture?

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

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.

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

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15. 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.

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.

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

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.

19. Page 6526, Lines 10-11 – Did you evaluate the chemistry of the water being pumped from the pond into the raised treatment?

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

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.

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

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.

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 CH4 at these sites.

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Perhaps you could reconstruct freezing front/thaw depth dynamics during your study period using temperature profiles or zero-degree isotherms.

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.

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

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

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

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.

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

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.

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

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

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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.

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.

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