

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 very thorough evaluation of the manuscript, candid critique, and helpful suggestions for revision. Below we have provided in-text responses to each of the comments made by this reviewer.

General comments: The paper by Sturtevant et al. 2011 “Soil moisture control over autumn season methane flux, Arctic Coastal Plain of Alaska” presents some interesting and rather continuous methane flux data from the Arctic region, an area which is very likely to be particularly affected by climate change and still understudied, particularly in terms of carbon cycling and more importantly methane fluxes. Particularly continuous measurements of methane using

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the eddy covariance technique are still rare and a major challenge in remote regions. Though the topic presented is of a large interest to the scientific community, the authors did a poor job when writing this manuscript. However this might also originate from the little data available or the short period chosen. Autumn methane peaks have been shown by Mastepanov et al. 2008 and were not re-reported by previous studies. In general the authors analyze collected data trying to see if they can identify a similar pattern for the Barrow site. While this a typical approach chosen in science (though maybe not the most elegant way) I am missing a clearly hypothesis driven manuscript, specifically when having the Biocomplexity Experiment in mind. As the name already implies an experiment is commonly done to prove hypothesis.

We appreciate the reviewer recognizing the value and difficulty of obtaining the data presented in the manuscript. Although we were interested to know whether a large autumn methane pulse (Mastepanov et al. 2008 Nature) was applicable to the Barrow site (as it would help to validate for the North American Arctic both the phenomenon and revised seasonal distribution of CH₄ flux observed at the Greenland site), it was not our intent to present the data with this main purpose. The revised manuscript will maintain a more narrow focus on quantifying the importance of autumn methane emissions to the annual budget and the role of soil moisture in this regard. The results will be presented and discussed in relation to hypothesized outcomes.

In addition to this shortcoming the authors clearly advertise the BE – which is from my personal opinion great approach with lots of effort shown to gain new knowledge concerning carbon and more specifically methane fluxes – without mentioning previous water table manipulations experiments in the arctic, outside Alaska. Furthermore the authors give many statements, which seem to be based on opinion (Discussion) without being familiar with the recent literature. This assumption is based on many old citations, which were primarily done in the North American Arctic and further the lack of many studies focusing on methane

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fluxes, which were performed in Europe or the Eurasian Arctic. A list is given in the specific comments.

We thank the reviewer for pointing out the need for more recent literature citations and for providing the list of studies to compare our results with. The revised manuscript will devote more attention to presenting our results in relation to the published literature, including other Arctic water table manipulations.

Another large drawback of the study is the poor statistical analysis of the flux data, though it is common to use General Linear Models to identify variables driving fluxes, however pooling all three sites seems irrelevant, since the authors want to identify differences between the treatments and one would suspect different driving variables under different conditions. Moreover statistical significance originates from roughly 1400 data points, any relation no matter how poor (e.g. wind speed and soil temperature explaining 2-3% of the data) will be significant. The authors should be more critical with the data presented since I hardly believe that this little percentage is helpful for understanding the carbon fluxes presented. I suggest to state that besides soil moisture no single abiotic variable could be identified, that explained variations in the measured methane fluxes.

The General Linear Model with pooled data was used to identify the major factors for the entire site which explained the variation in CH₄ 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.

The authors also state, that the relate methane fluxes to abiotic and biotic vari-

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able, whereas the first is done to some point, the latter is not mentioned.

This statement will be corrected in the revised manuscript to clearly reflect the analyses done.

Last but not least I agree with the editor, Figure 3 being absolutely essential for the manuscript is difficult to understand and further comments can be found below.

In the revised manuscript, Figure 3 will be replaced with one which better presents the data.

Specific comments: Abstract, I13: What are you referring to with “as through time” – Do you mean a timeseries analysis of your data? If yes this not performed in the manuscript.

No, we did not mean a time series analysis. This statement referred to the decline in unfrozen soil moisture during the soil freezing process. The revised manuscript will state this more clearly.

Abstract,I15: estimated

This will be corrected in the revised manuscript.

Abstract,I18: soil freezing

This will be corrected in the revised manuscript.

Abstract, I19: Define the effects? Otherwise this is confusing.

This statement will be clarified in the revised manuscript.

Introduction: The introduction needs further streamlining and state the current scientific knowledge on methane fluxes in the arctic and the relation to moisture changes – which includes, previous studies going beyond Alaska. If the large

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carbon pools are mentioned too, than one should state Tarnocai et al. 2009, GBC and additional studies. Here is a list of studies the authors should know about and have in mind during the writing process of this manuscript dealing with methane fluxes in the Arctic region in general, microtopography, water table manipulation etc. Kutzbach et al. 2004 BGC Frenzel et al 2000 BGC Bubier et al 1995 a/b Journal of Ecology /Ecology Forbrich et al. 2011 AFM Sachs et al. 2010 GCB Sachs et al 2008 JGR BG Merbold et al 2009 GCB Corradi et al 2005 GCB

As addressed in the general comments, the revised manuscript will present a more complete literature review.

P6522, I18ff: Can you also say something about the possible contribution to the annual budget of 200days per year with small efflux rates?

We are not entirely sure what the reviewer means by this comment. Our best guess is that the reviewer suggests including current estimates of how much the autumn seasonal transition period contributes to the annual methane emission budget for Arctic regions. We will include this in the revised manuscript.

P6522, I25. Is there a publication which explains the BE in general, than it should be cited here.

While there is not yet an overview publication of the BE, several papers have been published which describe the site and the experiment as an introduction to specific research (eg. Zona et al. 2009 GBC, Olivas et al. 2010 JGR, Lipson et al. 2010 JGR, Goswami et al. 2011 JGR), as the present manuscript does. Olivas et al. 2010 JGR gives the most complete description of the recent site history and is cited in the manuscript where this topic is discussed.

P6522, I27: this is not the first time of water manipulation in the arctic. The authors are supposed to give the reader an overview of the topic in the introduction, pointing to gaps in the current knowledge and show how the study intends

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to close currents gaps.

As mentioned above, this shortfall will be corrected in the revised manuscript.

P6523, I.12ff. State some clear hypothesis and see the above comment.

Please see comment above for response.

Site description: P6523, I10: from this perspective we are looking at an arctic desert. How representative are these values for the arctic?

We think the reviewer is speaking of P6524, I10. The air temperatures for this region are generally representative of coastal regions in the North American and Eurasian Arctic. The precipitation in this region is similar to other coastal areas in the North American Arctic but is low compared to much of the Eurasian Arctic. As it is common practice to simply state the climate statistics for a particular study area, we feel the manuscript appropriately addresses this topic.

3Material and Methods: in general: the information of how much water was pumped where is not that important, however the statement that the water level increase decrease was achieved proven by real data. The authors can shorten this paragraph. Purely mentioning that the change in water table was accomplished by pumping water is neccessary.

The revised manuscript will omit the pumped water volumes.

p6526, I14ff: cubicmeters of water were pumped

This will be fixed by omitting the pumped water volumes.

p6527, I3: terrain? Please explain: above the soil or the vegetation, what is the average vegetation height?

The measurement height was 1.9 m above the moss layer, which is nearly continuous. Average vegetation height is 15-30 cm above the moss layer. This statement will be

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included in the revised manuscript.

P6527, I20: replace “an” with “one”

We think the statement in question is grammatically correct.

P6527, I20ff: How comparable are these results, if the control plot is not observed permanently. This is also valid for many other comments. In an experiment one should always have the control and compare this with the different treatments.

Please see the replies to the comments concerning P6531, I5 and P6532, I.2ff. The responses address the present comment.

p.6529, I3ff: For the Li7700 is there something similar than the Burbacorrection as for the Li7500 needed?

No, there is no correction similar to the Burba correction necessary for the Li7700. The path length of the Li7700 is long enough that there is no significant difference between heat flux measured inside and outside the optical path, even under extreme sensor heating in harsh winter conditions (McDermitt et al. 2010 Appl Phys B).

p.6529, I.5ff: A graph or table showing how much data was originally available and after filtering would be very helpful.

This information will be added to the revised manuscript.

p.6529, I20: in which depth were the moisture sensors installed at 0 or at 30 cm depth?

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). This will be clarified in the revised manuscript.

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p.6530, I5ff: what about the sponge effect of the active layer? And according differences in soil surface height above permafrost?

Measuring thaw depth by inserting a metal rod into the soil until the point of resistance is established methodology in this type of ecosystem (for example, Shiklomanov et al. 2010 JGR). Additionally, calibration of a soil moisture sensor in the top 10 cm of soil by Donatella Zona using peat from a site adjacent to the one in this research proved that compaction of the moss layer only occurred at soil moisture contents below 40-50%. These values were not reached in the Central (drained) treatment after the thaw depth surpassed 10 cm and prior to refreezing of the active layer in 2009. A graduated container was used for the calibration and a section of the peat was inserted together with the moisture sensors inside this container; the container was weighted and then inserted into an oven to progressively dry the moss layer: the reading of the moisture sensor together with the weight of the water were measured under the progressive desiccation from ambient (usually water saturated, corresponding to 90% VWC) to about 30-40% water content. As mentioned, the graduated container used for the calibration procedure showed a compaction of the moss layer only when water content was below 40-50% (Zona, personal communication).

P6530, I19: How were outliers defined?

Outliers were identified with the Systat software as those having Studentized residuals with an absolute value greater than the Bonferroni-corrected critical value at an alpha level of 0.05. This will be clarified in the revised manuscript.

P6530, I21: I understand the procedure, but is this helpful when analyzing your data and does it improve reliability? See also the general comments above.

Please see above comment addressing the use of the general linear model.

P6531, I5: Why were there no differences?

We are unsure exactly why the drainage did not effectively differentiate the Central

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(drained) versus South (intermediate/control) water tables. One possibility is that the dike between the Central and South sections prohibited natural drainage of the Central section to the outlet in the southern end of the basin. Therefore, water removed from the Central section may have been similar to what would have drained naturally were no dike in place. Conversely, the hydrological isolation of South section from runoff originating from the Central and North portions of the basin may have reduced water availability in the South section and therefore resulted in a lower water table than would have occurred naturally. We decided to add water to the South section and remove it as a “control” in order to differentiate the Central and South water tables. We felt that it was more scientifically valuable to evaluate the ecosystem at different water tables than to adhere to the concept of a control, since we cannot definitively argue that it remained unaffected by the manipulation. This discussion will be added to the revised manuscript.

P6531, I.6: The North South Central naming is very confusing for a reader who does not know the site, please stick to wet/dry treatment and control.

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 preceding comment 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.

P6531, I13: shallowest. . . what? Thaw depth?

Yes, thaw depth is what this statement was referring to and is indicated as the subject in the beginning of the sentence.

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P6531, I15ff: This should be referred to in the discussion – is this a lot or normal? Missing in the discussion paragraph.

The revised manuscript will place these results in context of typical values for this region.

P6531, I25ff: remained frozen. This can hardly be seen in Figure 3. How did you prove that there was freezing, it is possible to have liquid water oin the soil below 0°C. Are you referring to your soil moisture sensors only? Did you check by drilling a hole? The latter would be the most reliable.

The reviewer is correct; it is possible to have liquid water in the soil below 0°C and we cannot be certain that the soil at 30 cm remained completely frozen throughout the study period without having drilled a hole (which was not done). We were referring to our temperature measurements only and will rephrase the statement to reflect the actual measurements.

P6532, I.2ff: I am surprised about the design shouldn't one always have the control site running and than compare it to the different treatments? here the north (wet treatment) is always observed and therefore moved into some sort of control. and why did one choose the timesteps as given?

We intended to have the use of three of the prototype Li7700 sensors to collect methane flux measurements in each of the three manipulation sections during the autumn (this was stated in the manuscript). However, instrument damage resulted in the immediate loss of the Central section prototype, leaving sensors at only the North and South sections. Shortly before we received a replacement instrument for the Central section, the prototype Li7700 in the South section was damaged as well. Unfortunately there was not another replacement for the South section. Since the South site was not a true control and there were only two available instruments, we felt the most worthwhile comparison would be between the wettest (North) and driest (Central) conditions. Although the data presented does not compare experimentally manipulated wet and

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dry treatments to a control, we think it still yields valuable information on ecosystem-level methane emissions under differing soil moisture conditions during a time which is particularly understudied. This discussion will be included in the revised manuscript.

P6532, I12: see also the previous 2 comments

The preceding comment applies here.

P6532, I18/19: When?

The timing of minimum winter soil temperature was approximately March 15, 2010. This will be added to the revised manuscript.

P6532, I25: give percentages where the wind originated from.

This information will be added to the revised manuscript.

P6532, I28: What is your definition of autumn in the Arctic region?

The statement in question was referring to the liquid precipitation for the entire measurement period. This will be clarified in the revised manuscript. As there is no standard definition of the autumn season in the Arctic, we loosely define the autumn season in our study region as late August to late October. This is consistent with the convention adopted by Euskirchen et al. 2006 GCB where: the transition from late summer to autumn occurs for the month in which the monthly average soil temperature at 10 cm depth is positive for that and the preceding month but at or below 0°C for the following month, and, the transition from autumn to winter occurs for the month in which the current and following months' soil temperatures are at or below 0°C but above 0°C in the preceding month. This corresponds to the months of September and October for our site. We will add this definition to the revised manuscript.

P6533, I8-12: this is part of the discussion

This will be moved to the discussion.

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P6533, I.26ff – p6534, I5: this is part of the discussion – restructure

This will be moved to the discussion.

P6534, I10: the authors state the responses of methane fluxes, why not showing simple response curves?

We agree that response curves would be helpful to illustrate the effect of the important driving variables on CH₄ emissions. We will add these to the revised manuscript.

P6534, I.18: where 65% were explained by soil moisture? and what about the 5% from 3 additional variables. Are these than really explaining variables. were there differences in explanatory variables between sites? did you check, how would the picture look a like if you were treating each side separately? less data, would it still be significant? see also general comments and comments for table 1.

As answered above, a more appropriate and complete statistical analysis will be presented in the revised manuscript. Differences in driving variables between sites were discussed in the manuscript. Treating the North (flooded) and Central (drained) sites separately resulted in only slight differences in model output of the GLM, but separate analysis of the South (intermediate) site resulted in a fairly different model where soil moisture was no longer a driving variable. However, we attribute this difference mostly to the short time frame of the South section data which did not encompass the soil freeze-up period. Another purpose of pooling the data was to alleviate the differences in measurement periods while including all data in the analysis. However, we realize that a more thorough analysis with discussion of these issues is necessary for the manuscript.

P6535, I.1: explain “interactive effects with”

“Interactive effect” referred to the observation that the increase in CH₄ emissions at higher wind speeds was magnified at higher soil moisture contents. This will be clarified

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in the revised manuscript.

P6535, I.7-I.16: I highly doubt these findings and believe these results are not needed see the statistical explanation in the general comments.

Previous responses have addressed this comment.

P6535, I.21-23: discussion

The paragraphs “Summary of results” is to many points already a discussion and otherwise only a replication of previously presented data – either shorten or remove.

This section will be removed in the revised manuscript.

Discussion: In general consider comparing your study results to previous water table manipulation experiments as well as the magnitude of your fluxes compared to other similar sites in the arctic, outside Alaska or North America. Here I do have the impressions the authors are not aware of the results already presented in the literature.

We agree that the manuscript needs a more complete comparison of our results to the published literature. This will be accomplished in the revised manuscript.

P6537, I.10ff: Underlie this with data, in this case you would expect uptake or the relation to CO₂ fluxes, which the authors mention not to have found.

We thank the reviewer for noting that this topic needs more attention in the discussion. We give an appropriate literature citation to support known microbial oxidation of methane in the oxic layer of soil above the water table. Methane consumption in the oxic soil layer would not necessarily result in net CH₄ uptake because aerenchymous plants in these ecosystems are known to provide an alternative pathway of CH₄ produced in deeper anaerobic soil to the atmosphere (Kelker & Chanton 1997 Biogeochemistry; King et al. 1998 JGR). Therefore, the positive emission of CH₄ from the

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Central section may be a result of plant-mediated release partially offset by CH₄ oxidation in the soil. Although one would expect to observe greater respiration of CO₂ with greater oxidation of CH₄, the absence of this relation with experimental drainage has been noted before (Merbold et al. 2009 GCB) as well as at this site (Zona et al. 2009 GBC). This explanation will be added to the discussion in the revised manuscript.

P6538, I4: this is contradicting the statement from before, of non-liquid soil water below 0°C.

This was addressed previously.

P6538, I8-29: streamline the discussion in general.

We believe the significant alterations to the discussion in the revised manuscript will create better streamlining.

P6539, I.4: lots of opinion with little data supporting this. focus on what you are having and moreover have a deeper insight in the literature past 2005. I see many citations, which are older than 1995 except the Zona et al. 2009 which is a very good paper.

We thank the reviewer for this suggestion to improve the discussion. The revised manuscript will more narrowly focus the discussion on the data presented in the manuscript and its comparison to recent literature.

P6539, I15-20: if you are referring to such methane pulses, you must at least cite the study that first reported this – Mastepanov. And relate your findings to it and with other studies, otherwise this is a pure replication of results.

The Mastepanov et al. 2008 Nature paper was cited several times in the manuscript in reference to an autumn CH₄ pulse. We will add the citation again here and relate our findings directly to this study (as well as other studies, as noted in previous comments).

P6539, I. 25ff: Shouldn't this be avoided by the strict data filtering you applied

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and the 80% fetch of 135m?

The statement in question was trying to highlight that the eddy covariance method measures fluxes over a large area and therefore that a few point sources may attribute little to the flux measured. A 135 m fetch for 80% flux contribution combined with variable winds means that the fluxes from each section are representative of an area conservatively approximated by a half-circle with a radius of 135 m (since the towers were located near the western edge of the basin). This amounts to greater than 28,000 m² of area measured by each tower over the course of the study period. We will better clarify this point in the revised manuscript.

P6540, I2: Reference for that, e.g. CH₄ emissions from thermokarst lakes, or lakes in general (either frozen or unfrozen etc)

Our best guess is that the reviewer is suggesting to add a reference describing ebullition as a pathway for CH₄ emission. An appropriate reference will be added here.

P6541: Why not adding a paragraph in the results section on study period budget or similar. This, after the abstract, is the first time I see this numbers.

In the revised manuscript, the results section will include calculations for the study period budget.

P6542, I12: reference needed

We do not agree that a reference is required here. Our results showed greater CH₄ emissions under higher unfrozen soil moisture contents during the autumn and that a greater amount of unfrozen soil moisture persisted into the winter season in the North (flooded) section. Therefore we speculated that wetter conditions may also substantially increase winter methane emissions. This statement will be better clarified as speculation in the revised manuscript.

P6542, I.27: this sounds a little bit like an advertisement and the information is not needed in the conclusion. It is still a great opportunity and needed for the

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community that new devices are tested in the field.

This statement was not intended to be an advertisement and will be removed from the conclusion.

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