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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 #1

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Review Sturtevant et al. 2011

General comments: The paper by Sturtevant et al. 2011 "Soil moisture control over autumn season methane flux, Arcitic 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 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-

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Interactive Discussion

Discussion Paper

ported 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. 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 fluxes, which were performed in Europe or the Eurasian Arctic. A list is given in the specific comments. 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 authors also state, that the relate methane fluxes to abiotic and biotic variable, whereas the first is done to some point, the latter is not mentioned. 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.

Specific comments: Abstract, l13: What are you referring to with “as through time”

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8, C2091–C2097, 2011

– Do you mean a timeseries analysis of your data? If yes this not performed in the manuscript.

Abstract,l15: estimated

Abstract,l18: soil freezing

Abstract, l19: Define the effects? Otherwise this is confusing.

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 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

P6522, l18ff: Can you also say something about the possible contribution to the annual budget of 200days per year with small efflux rates?

P6522, l25. Is there a publication which explains the BE in general, than it should be cited here.

P6522, l27: 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 to close currents gaps.

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

Site description: P6523, l10: from this perspective we are looking at an arctic desert. How representative are these values for the arctic?

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8, C2091–C2097, 2011

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 nevessary.

p6526, l14ff: cubicmeters of water were pumped

p6527, l3: terrain? Please explain: above the soil or the vegetation, what is the average vegetation height?

P6527, l20: replace “an”with “one”

P6527, l20ff: 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.

p.6529, l3ff: For the Li7700 is there something similar than the Burbacorrection as for the Li7500 needed?

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

p.6529, l20: in which depth were the moisture sensors installed at 0 or at 30cm depth?

p.6530, l5ff: what about the sponge effect of the active layer? And according differences in soil surface height above permafrost?

P6530, l19: How were outliers defined?

P6530, l21: I understand the procedure, but is this helpful when analyzing your data and does it improve reliability? See also the general comments above.

P6531, l5: Why were there no differences?

P6531, l.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.

P6531, l13: shallowest. . . what? Thaw depth?

P6531, l15ff: This should be referred to in the discussion – is this a lot or normal? Missing in the discussion paragraph.

P6531, l25ff: 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.

P6532, l.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?

P6532, l12: see also the previous 2 comments

P6532, l18/19: When?

P6532, l25: give percentages where the wind originated from.

P6532, l28: What is your definition of autumn in the Arctic region?

P6533, l8-12: this is part of the discussion

P6533, l.26ff – p6534, l5: this is part of the discussion – restructure

P6534, l10: the authors state the responses of methane fluxes, why not showing simple response curves?

P6534, l.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

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8, C2091–C2097, 2011

Interactive Comment

significant? see also general comments and comments for table 1.

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

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

P6535,l.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.

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 noz aware of the results already presented in the literature.

P6537, l.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.

P6538, l4: this is contradicting the statement from before, of non-liquid soil water below 0°C.

P6538, l8-29: streamline the discussion in general.

P6539, l.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.

P6539, l15-20: 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.

P6539, l. 25ff: Shouldn’t this be avoided by the strict data filtering you applied and the 80%fethc of 135m?

P6540, l2: Reference for that, e.g. ch4 emissions form thermokarst lakes, or lakes in general (either frozen or unfrozen etc)

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.

P6542, l12: reference needed

P6542, l.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 community that new devices are tested in the field.

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