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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 critique of the manuscript and suggestion for improvement. Below we provide in-text responses to the major issues identified by this reviewer.

The manuscript by Sturtevant et al. is reporting on methane fluxes from the coastal plain of Alaska in the geographical area subject to a large scale "Biocomplexity Experiment" hosting water table manipulations and a range of different associated experiments and measurements. The methane flux measurements have been conducted by the eddy covariance technique using two early versions of the now commercially available LICOR-7700.

The data presented are interesting from an instrument evaluation point-of-view and they clearly reflect the problems and consequently many holes in the data series that arise from child-diseases etc of any new instrument.

We appreciate the reviewer recognizing the importance of testing new instruments in the field. However, we think the data presented in the manuscript are also very interesting and important from a scientific point of view, as this is the only study besides Mastepanov et al. (2008) to focus on autumn CH_4 fluxes in the Arctic (and the only one using eddy covariance). The shorter measurement ranges for the South (intermediate) and Central (drained) sections relative to the North (flooded) section are certainly due to "child-diseases" of a new instrument. However, during the instrument ranges indicated in the manuscript, the many small holes in the data are mainly due to the effects of poor weather, wind direction, and other meteorological conditions. A table breaking down the amounts and reasons for data removal will be given in the revised manuscript.

The manuscript also presents some interesting albeit rather predictable responses with respect to the relationship between the manipulated water table depth and active layer development, i.e. more water in the system -> more energy conducted -> deeper active layer.

There are many details and interesting parts that may be discussed as already seen in the interactive correspondence but a main and serious problem this reviewer has with the manuscript is the fundamental direction it takes in targeting the issue of the autumn methane burst in tundra environments. The authors have apparently many other interesting data that will be published elsewhere but this particular communication is brought forward as providing what is needed for a comparison with the first observations of this phenomena reported by Mastepanov et al. (Nature, 2008). The fundamental problem is that this manuscript does not present any data that can provide a conclusive answer as to whether or not there could also have been an autumn burst in the year of presented mea-

surements at Barrow. The data coverage is simply too sparse and doesn't extent deep enough into the frozen season to say anything about the issue.

From Figure 1 in Mastepanov et al. (2008) it is clear that at a comparable site to Barrow the burst does not start before the frozen top horizon is at 10 cm and peaks when it reaches 15 cm. The current manuscript does not show any data from when the freeze has progressed to this point. A final clump of 2-3 days worth a data around the time of the freezing front reaching 10 cm (Figure 3) is showing still sizeable emissions (surprising by conventional standards) indicating that there may well have been a burst starting but it was just not recorded. Based on the data presented in this manuscript we simply cannot draw any conclusion in relation to the possible presence of an autumn burst at Barrow.

In the manuscript, we acknowledged the possibility that our observations ended prior to a methane pulse. However, the pattern in autumn CH_4 emissions that we observed for the Barrow site is inconsistent with those at the Greenland site where the burst was reported (Mastepanov et al. 2008 Nature). Late summer/early autumn CH_4 emissions for the Barrow and Greenland sites were similar ($\sim$1 mg CH_4 m^{-2} hr^{-1}). However, methane emissions at the Greenland site began increasing as soon as the active layer started to refreeze, whereas emissions at the Barrow site generally declined from this point. By the time the freezing front reached 10 cm, emissions at the Greenland site were approximately triple those prior to freeze-up. Our data extends past this point, yet with no increase in emissions above pre-freeze levels. The small rise in emissions during the final few days of Barrow data did not exceed pre-freeze levels and occurred during high winds, which was consistent with other observed peaks in methane efflux during the autumn. Finally, the maximum active layer depth at the Zackenberg (Greenland) study site was greater than that for Barrow in the respective study years (50-56 cm for Zackenberg compared to an average $<$ 40 cm for the Biocomplexity site). Therefore, we would expect that a burst should be evident sooner (at a shallower freezing depth) at the Barrow site since the mechanism suggested by Mastepanov et al. ex-

plaining the autumn burst was that methane trapped in the soil was squeezed out by frost action and was unable to diffuse downward due to the presence of permafrost. This discussion will be added to the revised manuscript.

Judging from the above mentioned and the fact that a lot of volume in this manuscript is directed towards the autumn issue this reviewer does not find it worth communicating any further. With a shift of emphasis, however, towards issues that the manuscript do document (water table controls etc) it could be worth reconsidering. But it seems it may then just as well be merged with other ongoing publication preparations based on the same data gathering efforts.

Based on the reviews from this and the other referees, we agree that the manuscript should be better and more thoroughly focused on evaluating the methane emissions observed at this site in relation to environmental controls, and with less emphasis on the autumn burst issue. We think that the items we discuss in our responses to other reviews to improve the manuscript (modeled water table and thaw depth measurements during the autumn, more appropriate and extensive statistical analysis, and more thorough comparison to previous research) will achieve this.

We think that a manuscript devoted to the autumn methane fluxes at this site has merit given that it contains a scientifically relevant message (beyond the autumn burst issue). Although annual methane emission budgets for Arctic regions in the context of global change are of great interest to the scientific community, there are very few studies which present ecosystem-level measurements outside the summer season or under manipulated moisture conditions. To our knowledge, this work is unique in its focus on the effects of an experimentally manipulated water table on ecosystem-level methane emissions during the summer-winter seasonal transition. Our results show that the autumn is important to the annual methane budget and we think our data gives insight into how altered tundra moistures during the autumn may affect the methane budget in the future.

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