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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 remarks The paper reports results of methane flux measurements by eddy covariance, during the summer-fall transition, at a water table manipulation experiment near Barrow, Alaska. The authors find that the seasonal trend in methane fluxes is a function of soil moisture during fall freeze-in. No autumn methane pulse is observed – and the authors speculate that this is due to wind and/or the scale of the eddy covariance footprint; however, total methane emis-

sion during the freeze-in period adds 18% to previously observed total emissions during the growing season. The paper needs some improvement of data presentation and discussion, particularly with respect to presentation and interpretation of soil moisture and temperature data.

Specific comments Modifiers ("wet", "intermediate", and "dry") are added to the "North", "Central" and "South" designators late in the manuscript but should be applied through-out for clarity.

We will use "North (flooded)" / "Central (drained)" / "South (intermediate)" in the revision.

The offset between the timing of methane observations (August-October; Figure 3) and those of water table heights and thaw depths (June-August; Figure 2) is unfortunate given that these are controlling variables. What happens to water table heights and thaw depths during freeze-in? For example, the dry seems to be getting wetter in August (Figure 2).

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, and will include these in 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.

Figure 3 is hard to read and data are sporadic, but VWC in the top 30 cm at the "dry" location is 60% of the others (which are presumably inundated?) when it picks up in September – does this mean the water table is 12 cm below the surface in the "dry"?

In the revised manuscript Figure 3 will be replaced with one which better presents the data. Yes, the soil moisture data showed the North (flooded) and South (intermediate) sites to be inundated in early September and the Central (drained) section to be drier. However, the soil moisture data did not indicate the water table height and it cannot be assumed that the water table in the Central (drained) section was 12 cm below the surface when the data began in September. We think that the modeled values for water table and thaw depth in the revised manuscript will alleviate this confusion.

In the methods (p. 6529, line 20) there is reference to TDR logging of 1-10 cm and 20-30 cm intervals, but there is no further reference to or presentation of the resulting data. Same goes for temperature at four depths and heat flux plates (p. 6529, lines 19 and 27-28). Even if inconclusive, these data should be presented somehow or at least clearly discussed.

We will discuss or present this data in the revised manuscript.

The references to freezing to 10 and 30 cm in Figure 3d are cryptic and the description of these trends (p. 6532, lines 23-26) needs clarification. The observation that temperatures drop "consistently" and "steeply" below zero is not clear in Fig. 3d. Better and more complete presentation of the data is needed.

As mentioned, Figure 3 will be redone to better present the data and clearly show the freezing of the active layer. The temperature drop after the autumn measurement period (to which "consistently" and "steeply" were referring) was not shown in Figure 3d. These trends will be shown in a separate figure.

Discussion of temperature differences between North (wet) and South (intermediate) on p. 6532, lines 5-7, suggests that the North is warmer due to the higher water table. Would the difference of 5 cm depth above the surface (as shown in Figure 2) account for this difference? Does this imply that the difference in water table depths is maintained during the window of these observed temperature differences?

We must apologize, we have recently discovered an error in the South (intermediate)

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section 20 cm soil temperatures used in the manuscript. This will be corrected in the revised version. However, higher soil temperature due to flooding has been shown before for this site (Olivas et al. 2010 JGR). The soil temperature data does not to imply that the differences in water table depths were maintained during the window of observed temperature differences in the North (flooded) and South (intermediate) sections. We think that the modeled water tables that will be presented and discussed in the revised manuscript will alleviate this concern.

It's not clear what the reference height is for WT heights; the text says base of vegetation (p. 6530 line 6) but Fig. 2 shows heights relative to the surface (based on the caption). The surface elevation is clearly variable based on Fig. 4 (20-60 cm variation along boardwalk) and it seems the base of the vegetation should also vary. To achieve the small error bars these measures must be relative to some absolute datum, no? Where is this in Fig. 4? This looms large in the discussion of WT vs. topography on p. 6537 (lines 16-27). Illustration and clarification needed.

We treat the surface as the top of the green moss layer, which we also consider to be the base of the vegetation. This will be clarified in the revised manuscript. The reviewer is correct that this surface varies according to the elevations shown in Figure 4. As the error bars depict the standard error of the mean (Figure 2 shows the mean water table and mean thaw depth along the boardwalk for each sample date), the small error bars are a result of the large sample size (every 4 m along the 300 m boardwalk).

All of this makes it hard to understand what's going on physically with the decline in VWC through the fall (Fig 3b). To what extent to the trends indicate changing water table height (e.g., as argued on p. 6537, lines 4&27) vs. freezing (p. 6540, lines 5-12)?

We think that the modeled values of water table and thaw depth that will be presented and discussed in the revised manuscript will clarify this issue.

The discussion of variation in the duration of early winter soil moisture on p. 6536 (lines 16-20) and subsequently is not really shown in Fig. 3.

The reviewer is correct that data during this time is not shown. The revised manuscript will present another figure showing this data.

The seasonal relationship between soil moisture and methane is interesting, but the details need attention. In the end, these observations may argue for wintertime production of methane resulting in a significant springtime contribution to total emissions. This possibility should be discussed with reference to the literature. See for example the annual cycle observed by Jackowicz-Korczynski et al. (2010, JGR-Biogeosci. 115, G02009).

We thank the reviewer for this helpful suggestion to improve the discussion. This topic will be addressed in the revised manuscript.

The smaller scale (temporal) relationship between methane peaks and wind speed is also interesting. Wille et al. (2008; cited in intro but not discussed further) observed a similar correlation. How does the relationship observed here compare? The authors might consider recent ground-level observation (in automated chambers) of wind speed effects on ebullition rates (Goodrich et al., 2011, Geophys. Res. Lett. 38, p. L07404).

We appreciate that the reviewer agrees that the greater methane release during high wind speed events is interesting and noteworthy to discuss. We also agree with the reviewer that the relationship should be compared to other studies showing this trend (Wille et al. 2008 GCB; Sachs et al. 2008 JGR). As also mentioned in the replies to comments by other reviewers, the revised manuscript will devote more attention to comparing our results to previous and recent literature.

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