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Interactive comment on "Soil moisture control over autumn season methane flux, Arctic Coastal Plain of Alaska" *by* C. S. Sturtevant et al.

Anonymous Referee #2

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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 emission 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.

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). 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 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.

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.

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?

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.

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)? 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 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).

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).

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