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Interactive comment on "Environmental correlates of peatland carbon fluxes in a thawing landscape: do transitional thaw stages matter?" *by* A. Malhotra and N. T. Roulet

Anonymous Referee #2

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The ms explores how permafrost thaw in (sub) artic peatlands may change gaseous carbon exchange dynamics as the system moves from a frozen to fully thawed state, including several transitional stages occurring along the way. Chamber measurements of CO2 and CH4 for each transitional stage are correlated with several environmental variables to explore how the importance of these factors as controls of C exchange processes change as the system thaws. Given the potential warming of high latitudes the topic of the study is timely and important for attempts to e.g. identify and quantify feedback processes associated with ecosystem transition. It also emphasizes the complexity involved in doing so due to spatial heterogeneity. The ms is well written and, for most parts, clearly structured. Methodological approaches are generally sound, clearly

conveyed and motivated. However, I have some concern on the data evaluation and the statistical approach. Expanding from the simple bivariate correlations the authors use MSLR in order revel more complex interrelations of their variables. However, the analysis appears to be limited to additive effects while it is well known that interactions are seldom only additive. Why were other interaction terms (e.g. products) not included in the analysis? This could potentially shed more process level insights on the transition dynamics. The study finds that "elevation" is a main factor for explaining CH4 fluxes, but from a process level perspective this variable makes less sense. The authors do acknowledge that "elevation" likely integrates for other variables like WTD, nutrients etc. that are important in driving CH4 production and flux, but is there a risk that inclusion of this variable obscures correlations with other, more meaningful variables, that could be important for explaining the flux dynamics? If elevation was omitted from the MLSR the WTD would probably correlate most strongly, and it is probable from Fig 1 that the following residuals could correlate differently. Was this tested? There could be risk of strong collinear influence on potential X-variables, but this could be solved by e.g. principal component extractions and concomitant MLSR. Specific comments: P 457, L 16: strange sentence; reword. P 458, L1-10: Expand this discussion to also address the specific influence of temperature on methanogensis and methanotrophy, respectively, and the net influence on CH4 fluxes. Several studies have reported different temperature sensitivities for the two processes which are in accordance with the observations. P459, L8-11: Confusing; how can elevation/thaw depth better account for thermal regime than temperature itself? P 460, Line 15-23: Why is this observation not in the results section? As it reads it comes across as a somewhat awkward add-on. Suggest moving it to the Results and also give adequate background info in methods. You can then expand the discussion around partitioning and what controls it.

Interactive comment on Biogeosciences Discuss., 12, 445, 2015.