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Comment

Interactive comment on “Revisiting factors controlling methane emissions from high-arctic tundra” by M. Mastepanov et al.

Anonymous Referee #1

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General Comments: This paper contains a large amount of data from a very important (and underrepresented) ecosystem and that this data deserves to be published. The observation of the large late-season methane fluxes, the documentation of seasonal and interannual dynamics of the methane fluxes at this site, and the importance of snow-melt date are all significant contributions. However, the manuscript would be much stronger with a substantial refocusing and revision. Other than the late season CH₄ fluxes (suggested to be caused by physical mechanisms) they could not relate the interannual variability in the fluxes to any single environmental variable. It is not clear why the authors did not then attempt any sort of multivariate explanation for the fluxes across seasons? What about a combination of temperature, WT and NEE to examine competing influences that would potentially obscure any single relationship? Most importantly, less space should be spent articulating how temperature, WT and

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NEE alone do not explain the variability observed, and much more space should be spent actually examining and testing the suggested hypotheses for what does control the observations. Fig 9 is a nice start, but how does this idea work across chambers or across years? Some of the less interesting analysis presented in this paper can then be put in supplemental information for those readers interested in all the details that brought the author's to suggest their (more interesting) hypotheses. I suggest the authors omit figures 6-8 or move to a potential supplement. Revise figure 11 to be more informative about how this plot looks relative to the emissions. I think this could be done by adding a second panel, or even just an additional column to each year showing the methane emissions during days 0-30 after snow-melt the following year. How does the magnitude compare to the sizes of the red bars and blue arrows? It is too much to expect the reader to go back and forth between figures 4 and 11. The text explaining this on page 15878, without a better figure, is difficult to follow, especially around line 22 when describing the failings of the hypothesis. A well-designed visual would really enhance the reader's understanding of how this hypothesis does or does not explain the data set. Figure 12 is fine as a starting place but then it would be great if the readers could find out how well this hypothetical scheme does in explaining your data set within and between stations and seasons.

More Specific Notes: In one or two cases, detailed in the technical notes, I was not satisfied that the relevant reference was cited, so I would recommend the author's review those cases and the references in general to ensure that they've chosen the best reference to support the claim. The second proposed idea to explain the freeze-in burst of methane, that the frozen surface layer stops methanotrophic activity, seems unlikely given the much higher solubility of O₂ in water at cold temperatures. Has this type of dynamic ever been observed (a frozen layer directly in contact with anoxic pore waters)? Also, as they mentioned, they see little influence of WT depth (a reasonable proxy for methanotrophy) and methane emissions, which makes this scenario even less plausible. Given these considerations, and the concurrent peaks in CO₂ emissions, I would suggest it is not worth mentioning this as a possibility. Lastly, although some

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editing comments are included below, quite a lot of copy-editing is needed to correct typos, grammar, and clarity of sentence structure and meaning.

Technical Details: (given the recommendation for substantial revision I did not include many technical corrections for the latter half of the paper)

p.15854 Starting an abstract with the word “Among” is quite strange. I would recommend rewriting the first sentence. A possible suggestion is: “The northern latitudes are experiencing disproportionate warming relative to the mid-latitudes, and there is growing concern about feedbacks between this warming and methane production and release from high latitude soils. However, studies of methane emissions from high-latitude sites (north of the Arctic circle), particularly those with measurements made outside the growing season, are underrepresented in the literature. Here. . . “

p.15855 Lines 7 and 8: Change “Firstly” to First and “secondly” to second. Line 10: “appeared” is not the right word here. Discovered? Line 13: lacking the capability to explain

p.15857 Line 4: “was removed (or sampled?) at a rate of approx. 0.4l min. . .” Line 19: sentence needs editing

p.15858 Line 12: For ebullition. . . based on bubble. . .

p. 15866 Line 10: organic rich

p.15872 Line 19: Those references are okay, but it might be more appropriate to cite an earlier reference, such as Conrad, 1996 for this statement. Line122: I couldn’t find anything in Christensen et al. 2003 supporting their complete dismissal of diffusion as a source of methane emissions at this site. I am aware of other studies that have demonstrated the importance of plant-mediated methane emissions as a source of methane to the atmosphere at a wide variety of environments, but that is not the theme of Christensen et al. 2003, which never mentions diffusion.

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