Review of Mastepanov et al. “Revisiting factors controlling methane emissions from high-arctic tundra”

General comments

Good interesting paper. The authors cannot explain their observed methane fluxes by using 'conventional' relationships with environmental and biotic variables that have been related to methane fluxes in other published research – including work by the authors at other arctic wetlands. They, therefore, develop an inferential argument that attempts to explain their observations. This argument is only partially successful, largely because they lack the necessary to deductively test their ideas. This is the frustrating aspect of the manuscript but it is not an unreasonable outcome the apparent complexity of the problem. The physical dynamics related to the refreezing of the soils in the autumn, and thawing of the soils in the spring, adds a significant new factor to the seasonal methane dynamics that has not received much attention in the past. This is surprising given the importance of northern wetlands as methane sources. They use their data set well to reject what I would refer to as the easy accepted explanations of what controls the methane flux. The rejection of these explanations is very solid. The authors' conjectural alternative explanations are inductive. They also suffer from the problem of equifinality - i.e. multiple hypotheses are equally plausible and their observations does not provide a basis for confirming or rejecting alternative ideas.

The paper is generally well written but the discussion is too long. The rejection of the usual environmental variables vs methane could be much reduced. There are a lot of nice observations in this paper but I think the paper would benefit from more structure. I would suggest the authors pose a clear set of research questions and/or hypotheses in the introduction. This would set up the various tests they go through to see if the explanations stick - i.e. their process if falsifcation and rejection. This would be a clear sequence of deductive tests and it shows where convention wisdom fails. This then set up the logic of the more inductive speculation that occurs in the second half of the discussion. The authors pose several alternative explanations (hypotheses) but do not have the ability to test them directly. Right now the argument is one based on Occam’s razor. The paper would benefit from the authors describing some experiments or observational analyses that could be used to test their conjectures. Right now the paper does not really end – it kind of runs out of steam and leaves a reader hanging.

Specific comments/questions/suggestions

15855-13&14: Bit redundant - if we have a good understanding why would you do this research?

15856-1: Do you have any hypotheses or expectations that led this research?

15856-28: What are dunlin fens? I have not heard this term before.

15859-8: Were standard additions used to calibrate the effective volume of the chamber? Did the effective chamber volume change over the growing season? Was there any evidence of the ventilation of the soils due to wind shear stress or pressure changes – i.e. did the effective chamber volume change with wind shear? Were these potential effects tested for?

15866-23: Not sure I understand this? Do you mean the surface of the wetland relative to an arbitrary datum varied more than 10 cm? Further, was the movement of the surface related to changes in water table or frost table? This could be important to the methane flux if the water carries DOC. If the surface changes are not associated to water storage or active layer depths as implied in this paragraph what causes the change and how do you know it is not important in methane flux? There is something confusing about this paragraph.

15866-24 to 27: Do you the permafrost thaw or do you mean changes in active layer depth? Do you know that the permafrost thickness is actually changing?

15868-20: Does this not suggest a multivariate problem? It is not surprising that you find strong correlations between temperature and fluxes across within a single year but not across years. This only means that the initial conditions and/or other abiotic and biotic variables are involved. This is what I would expect.

In your analysis is does not look like you attempted to examine covariance among the physical variables – e.g. ALD and WTD, or do some multivariate analysis across a number of variables. Have you thought of using regression trees to try and tease out associations?

15869-3: Surprised that you do not reference to the multiyear fluxes reported for another more northern wetland such as Stordalen? All your comparisons are with boreal peatlands that do not contain permafrost.

15870-5: Did you test for relationships between the saturated zone thickness? When dealing with permafrost the active layer thickness needs to be considered along with the water table to estimate the zone of saturation – i.e. potential anaerobic conditions. Similarly a change in AL depth can change the thickness of the oxic zone without changing the thickness of the zone of production, or the thickness of the saturates zone.

15872-27: The lack of biomass information seems to be a rather large omission given the inference you are making here. Presumably you attempt to address this issue another way later on in the paper when at NEE vs CH4?

15873-2 to 4: Yes but what is relevant here is the actual exchange - NEE. You are interested in determining the productivity and the exchanges concurrent with the CH4 fluxes.

15873- 12 to 25: Fig 7 is not really necessary. It shows that the fluxes from the two measurements approaches have similar seasonal and daily variations but it also show the magnitude is systematically less for the EC measurements than the chambers. The EC flux seems less regardless of the direction of the flux but there does seem to be an asymmetry the differences? It is very likely that the biomass in the chambers is higher than the average for the footprint of the EC tower. We tend to locate chambers over healthy, good stands of vegetation.

15876- 5 to 8: If you integrate the area under the curve for the freeze back period (Table 2) does this equal a mass of methane that could be stored? What would the concentration need to be in the saturated zone? Using the data in the tables and graphs of ALD and WTD you would one to two orders magnitude difference in storage, while total growing season fluxes differ by a factor of 2 or 3?

I see you do the comparison two pages down – it might be a good idea to signal to the reader that you do this as it seems logical to raise the question here.

15876- 9 & 10: It appears you have one burst (2007), a couple puffs of 4 to 8 times smaller (2009 & 2010), and nothing in 2008. Based on this record nothing to puffs seems normal and the burst is the exceptional event - i.e. right now you can say the burst is a one in four event? Maybe it is much less?

15877-5: This is strong evidence for the physical release of the methane. I am convinced. The most interesting question is why there was so much methane stored to release in 2007 versus the other 3 years? What is difference among 2007 and the other years' freeze back? I note that 2007 has intermediate depths of both AL and WTD compared to the other three years? Large enough saturated zone to allow the storage on methane but an unsaturated surface layer in the soil that provides a pathway to allow gas to be pushed out. Two of the year the soil is saturated right to the surface. This would alter the rate of freezing and alter the zero curtain effect. It also restricts pathways for mechanical gas transport. One year is quite dry. It is unfortunate there are no measurements of methane storage in the soils.

15877-8: See comment above - in 2008 and 2009 the soil was effectively saturated but in 2010 the WTD was ~ -25. How does this fit the argument?

15877-12: Does methane dissolved in water change under pressure – what does this do for the solubility? What kinds of pressures would build up at that freezing front? When the phase change occurs with the soil water is methane exsolved? How much? Some back-of-the-envelope calculations here might put more meat into this argument.

Figure 11: Put units on the y-axis.

15878-12: I suggest the word "hypothesis" is more appropriate for the conjectural nature of this statement rather than "theory". They are not interchangeable words.

15878-22 to 24: Point raised earlier. Need connect the logic here to the points raised at 15876-5 to 8.

15880-5: Any ideas on how would you test this - examine the temporal variability in 13-C and D in the stored and flux of methane as well as 13-DOC?

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