We are very grateful to the anonymous Referee for the evaluation of our paper and for the constructive critics. Below we try to answer every comment.

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.

We appreciate the Referee recognizing the value of the presented data.

However, the manuscript would be much stronger with a substantial refocusing and revision. Other than the late season CH_4 fluxes (suggested to be caused by physical mechanisms) they could not relate the interannual variability in the fluxes to any single environmental variable.

This is a very true – in this manuscript we communicate three main messages (following this and the other Referees comments, the abstract will be changed in the revised manuscript to emphasize them more clearly):

1. We have documented the interannual variability in growing season CH_4 fluxes, that can not be explained by "traditional" environmental factors (although the seasonal variability within each specific year is quite "normal" and can be related to these environmental variables).
2. We have documented late season CH_4 and CO_2 fluxes and argue for the reasons behind their dynamics.
3. We hypothesize that CH_4 emission during late season can affect the flux during the next growing season, and show how this hypothesis can explain our interannual variability.

Message 1. could deserve a publication on its own, however such a manuscript would only then report on negative findings. Message 2. could also deserve a publication on its own, however, we try to avoid seeing the "autumn burst" only as a phenomena *per se*, but rather try to find its place in the annual and multiannual functioning of ecosystems where it happens. Message 3. is admittedly very speculative so far, but it seem reasonable to us, it and it fit our observations. Adding this message to the manuscript we turn it from negative to be pointing at challenges for the future.
Thus, we would prefer to avoid the suggested substantial refocusing and keep this manuscript oriented on the described messages. The data, presented in this manuscript, is available through the Greenland Ecosystem Monitoring database (http://dmugisweb.dmu.dk/zackenberggis/datapage.aspx), and can be used in further manuscripts of different focus.

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?

We thank the Referee for this suggestion. Following it, we have applied stepwise multi-linear regression on our data, both for individual years and for the whole dataset. The applicability of such tests (as well as linear regression) may obviously be questioned due to the strong autocorrelation in data caused by seasonality; however they could be useful as descriptive measures. The predictor

variables used (daily resolution) were active layer depth (ALD), day after snow melt (DASM), NEE, soil temperatures and water table level (WTL).
The stepwise regression generally does a good job in explaining the CH_4 flux dynamics for individual years; R^2 values range 0.88 to 0.96 (Table 1.1). However, significant coefficients selected in the test vary from year to year and there is a large variation in coefficient values; thus the results are similar to those presented in table 3 in the discussion paper.

Table 1.1. Coefficients from stepwise regression analysis (CH_4 flux independent variable) for individual years (2007-2010) and full dataset (ALL). Only significant predictors ($p<0.05$) are shown. For each test, only the most significant soil temperature was included to minimize co-linearity.

	2007	**2008**	**2009**	**2010**	**ALL**
ALD	0.32	-0.03		0.03	0.12
DASM	0.06		0.01	0.01	0.03
NEE			-0.01	-0.002	
T_5cm					
T_10cm	0.61		0.17	0.32	
T_15cm		0.09			0.63
WTL	-0.09	-0.12			-0.02
Intercept	8.64	-0.68	0.07	0.66	2.60
RMSE	0.50	0.20	0.20	0.15	0.93
Adj-R^2	0.93	0.90	0.88	0.96	0.42

When the stepwise regression is applied to the full dataset, the R^2 value is low (0.42) compared with those from individual years. The modeled CH_4 flux (CH_4 flux = 2.60 + 0.12*ALD + 0.03*DASM + 0.63*T_15cm – 0.02*WTL) fails to capture high fluxes in 2007 and overestimates fluxes in 2008 and 2009 (Figure 1.1). The decreasing trend in modeled CH_4 flux is caused by the obtained positive relationship with ALD (i.e. higher thaw depth results in lower CH_4 flux), and the rationale behind this may be questioned.

Thus, the multivariate correlation fully supports the Message 1. of our manuscript: that the seasonal dynamic of CH_4 fluxes can be quite well explained by the common environmental factors, but the interannual variability can not.
We will include a short description of this exercise in the revised version of the manuscript.

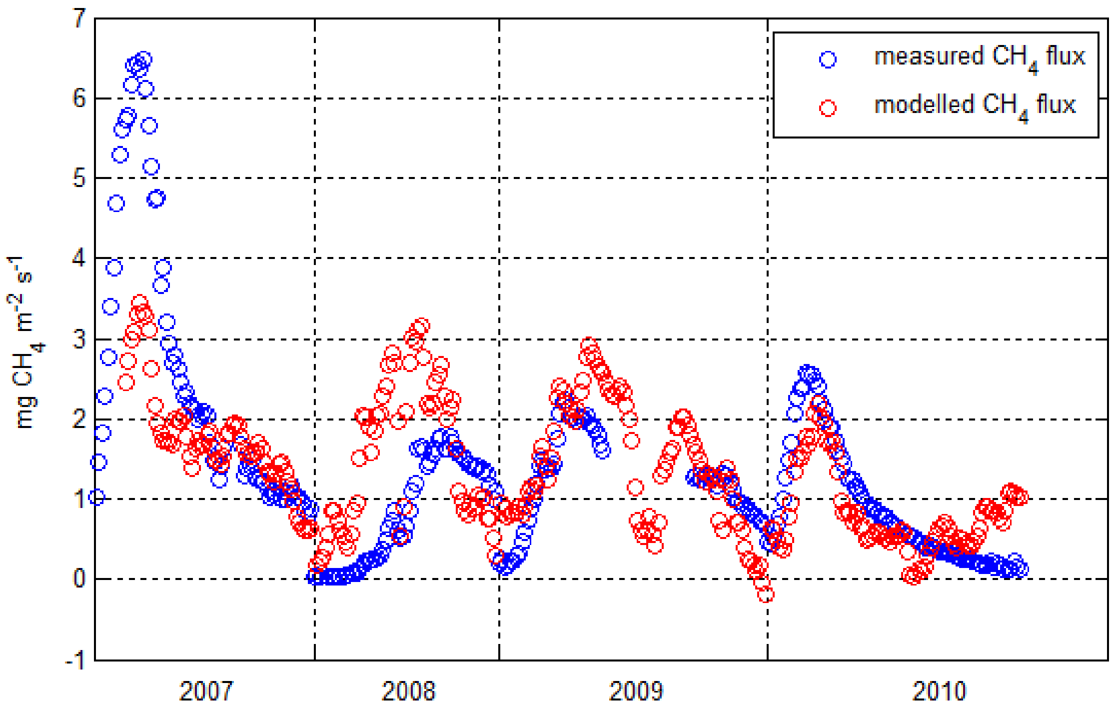

Figure 1.1. Observed (blue circles) and modelled CH_4 fluxes (red circles) based on stepwise regression using whole dataset.

Most importantly, less space should be spent articulating how temperature, WT and NEE alone do not explain the variability observed, …

We will try to further compact this part in the revised manuscript, although as this is one of the main messages we find it should be properly explained. We think that the negative results are as important for the science as the positive are, and studies where some correlations are proved not working should be published along with other, positive findings.

… and much more space should be spent actually examining and testing the suggested hypotheses for what does control the observations.

We totally agree with the Referee that our hypotheses need more tests, and we plan such tests for the future (for example, analysis of subsurface CH_4 profiles, stable isotope compositions for CH_4 fluxes and storage pools). However, there is nothing we can add right now, without extra years of studies, so we hope the existing argumentation is enough to have our hypotheses communicated and subsequently discussed.

Fig 9 is a nice start, but how does this idea work across chambers or across years?

Figure 9 is not to support the hypotheses of interannual methane pools and role of autumn bursts in their dynamics. This figure carries an argument for the possible mechanism (physical squeezing) of the autumn bursts. This mechanism was suggested in Mastepanov et al., Nature, 2008, however, the main argument we had for it at that moment was quite speculative – the mechanism looked realistic and could explain high CH_4 fluxes we observed (the same level of speculation as hypotheses of

interannual effect of subsurface CH_4 discharge and two components of growing season flux in the current manuscript). Since then, we have gathered new data and seen a synchrony between autumn CH_4 and CO_2 emissions, and a changing in time CH_4/CO_2 ratio. These arguments are explained in the current manuscript and illustrated by figures 9 and 10. In our view, now the physical mechanism is proven very likely.
For the other chambers and other years the dynamics of CH_4 and CO_2 emissions during the autumn burst are the same (may be less pronounced when fluxes are lower). This fact is to some extent illustrated by Figure 10 (two years, six chambers). This point will be clarified in the revised manuscript. If necessary, more figures similar to Figure 9 can be added to the supplementary materials.

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.

The diurnal dynamics of CH_4 fluxes might be interesting for a potential reader, however, if the manuscript must be shortened, we can move Figure 6 to a supplement. Figure 7 can also be moved to a supplement, if required.
Figure 8, illustrating the suggested (in 2008) mechanism is essential. Due to space limitations, it did not fit in Nature Letters, so this or similar illustration was never published. Our intention is to publish it in Biogeosciences, so anyone mentioning autumn fluxes and their mechanisms could use this illustration, referring to this publication.

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.

We apologize for the possible misinterpretation of Figure 11. In this figure we try to draw the hypothetical storage of CH_4 in the subsurface pool, however, we do not have any measures of this storage. As we tried to explain in the text (this part will be rewritten to be more clear in the revised manuscript), we make an assumption, that the peak growing season emission reflects the storage in the peat matrix; so we use peak emissions as a proxy for storage. Red bars in Figure 11 are numerically equal to peak CH_4 emission values (shown in Table 1 and Figure 4); however, in Figure 11 they represent storage. The Y scale is relative, as we have no measurements of this storage.
So the suggested second panel or additional column would be the same as the existing (red bars).

How does the magnitude compare to the sizes of the red bars and blue arrows?

Blue arrows show the total CH_4 discharge (decrease of stored CH_4) during the autumn burst. Strictly speaking, we also do not have these numbers, as we never were able to monitor the late emissions until their end. So instead of total discharge the arrows are sized to known discharge – amount of CH_4 emitted during our monitoring (Table 1, g C m^{-2}).
Very roughly, the longest blue arrow should be about 4-5 g C m^{-2}, so the highest red bar – about 5-6 g C m^{-2} and the lowest – about 1-2 g C m^{-2}. The magnitude seems realistic – for example, Strack and Waddington (2008, JGR 113, G02010) report the total peat profile bubble CH_4 stock of 0.3 - 1.0 mol m^{-2} (3.6 - 12 g C m^{-2}) for a boreal Canadian fen.

It is too much to expect the reader to go back and forth between figures 4 and 11.

We are very sorry for this inconvenience, but do not see a way to avoid it. Figure 4 (and Table 1) shows the real data, while Figure 11 shows the concept. We hope that readers scrolling through the text will agree with our concept, and readers who want to examine it will take the trouble to look into Table 1 and Figure 4.

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.

The text will be changed in the revised manuscript. We will also change the style of Figure 11 – it should look more like a sketch (as Figure 12) to avoid misinterpreting the bars as reflecting real numbers.
We will rephrase line 22 to avoid possible misunderstanding of failure in the hypothesis. There is nothing contradictory (at least from what we know so far) in the hypothesis of autumn bursts affecting the following growing season peak. It just needs an additional assumption – of bicomponental growing season emission – to explain the similarity in fluxes after 30-60 DASM.

A well-designed visual would really enhance the reader's understanding of how this hypothesis does or does not explain the data set.

We totally agree with the Referee and will do our best to improve Figure 11 and the corresponding text.

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.

The corresponding text will be refined and expanded in the revised manuscript.

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.

This will be improved, also according to the hints given by other Referees.

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 O_2 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 CO_2 emissions, I would suggest it is not worth mentioning this as a possibility.

This part will be removed from the revised manuscript. We thank the Referee for supporting us in our opinion that this idea is plausible. It was born as a main criticism for our Nature 2008 paper, but now seems obsolete.

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

We will do our best to improve the situation in the revised manuscript.

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.

We will use another phrasing in the revised manuscript.

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 highlatitude sites (north of the Arctic circle), particularly those with measurements made outside the growing season, are underrepresented in the literature. Here. . . "

We appreciate this suggestion and will use it in the revised manuscript.

p.15855 Lines 7 and 8: Change "Firstly" to First and "secondly" to second.

The suggested changes will be made in the revised manuscript.

Line 10: "appeared" is not the right word here. Discovered?

The suggested changes will be made in the revised manuscript.

Line 13: lacking the capability to explain

The suggested changes will be made in the revised manuscript.

p.15857 Line 4: "was removed (or sampled?) at a rate of approx. 0.4l min. . ."

The suggested changes will be made in the revised manuscript.

Line 19: sentence needs editing

This line will be changed in the revised manuscript.

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

The suggested changes will be made in the revised manuscript.

p. 15866 Line 10: organic rich

The suggested changes will be made in the revised manuscript.

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.

Here we cited just two recent review papers, which in turn classify and cite a lot of earlier references. In the revised manuscript we will add direct citations to earlier studies, including Conrad, 1996.

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.

This reference was simply a mistake. It was meant to refer to Christensen et al, Biotic controls on CO_2 and CH_4 exchange in wetlands - a closed environment study, Biogeochemistry, 64, 337-354, 2003. However this reference accidentally fell out of the reference list.
In the revised manuscript this reference will be fixed.