Manuscript was modified to address the reviewers' comments and concerns which are listed below. Additional modifications to the manuscript were made primarily to improve clarity and readability.

Replies to reviewer 1:

Our responses to the reviewer 1 comments are provided below. The comments from the reviewer are in *italic* and our responses are in normal font. For changes to the MS text, please see the new version, in which the changes are marked in red.

The authors present a study on estimating methane emissions in northern high latitudes during the spring melting season using two different inverse modeling set-ups. The study also includes a detailed description and analysis of the estimation of the spring melting season in four different permafrost zones using SMOS F/T data. This study is a valuable contribution in reducing uncertainties in estimating the length of the spring melting season in permafrost regions, which are increasingly affected by climate change, as well as estimating the resulting CH4 emissions in those regions.
In my opinion, this study is well prepared and carefully thought out, so only minor improvements are necessary. Specifically, I believe there are three points that should be taken into account when revising the manuscript:

We thank the reviewer for the kind overview and invaluable comments to our manuscript. In the following comments we aim to answer to the given concerns.

1. There are a few details about the inverse modeling set-up and the analysis that I didn't quite understand from the description. You describe the model domain of the transport model, but are the CH4 fluxes also optimized over the whole domain (globally)? And in the results, do you evaluate the total CH4 fluxes (anthropogenic+natural) or only the natural fluxes or only the biogenic fluxes? If you evaluate all natural fluxes, what about the contribution from other natural sources, since you predominantly want to estimate wetland emission? Please add these details in your description.

We thank the reviewer for this helpful comment. The emissions were optimized globally and in this paper we only analyzed the optimized biospheric emissions. Here the biospheric emissions only include emissions from the wetlands as well as soilsink. We have added information to the chapter 2.2.3 regarding the prior fluxes in the following way:

"Biospheric and anthropogenic fluxes were optimized globally. Other fluxes were not optimized. We only analyze the optimized biospheric fluxes in this study. The biospheric prior LPX-Bern DYPTOP represents ecosystem area fractions across several land-cover types: (i) peatlands suitable for peat growth as defined by DYPTOP (Stocker et al., 2014); (ii) rice paddies coinciding with croplands and the presence of rice paddies (Spahni et al., 2011); (iii) inundated wetlands other than peatlands or rice paddies; (iv) wet mineral soils, which are not wetlands, peatlands, rice paddies, permanent freshwater bodies, or ice/sea water covered areas, but are ocassionally wet; and (v) dry mineral soils, which are areas identical to wet mineral soils but are "dry" in general. Of these categories, we did not include rice paddies in our prior as they are not present in the northern high latitude region. Soilsink was included in the biospheric prior."

We added the following to the results chapter:

"The biospheric CH_4 emissions were analyzed with both the region-based and grid-based approach. From this point onward, we will focus exclusively on biospheric emissions, regardless of whether this is explicitly mentioned. Biospheric emissions refer to optimized fluxes for which prior estimates were derived from ecosystem model simulations of wetland emissions as well as terrestrial sink fluxes (See section 2.2.3)."

2. In some cases, the figures shown are not adequately described in the accompanying text. For example, Fig. 4 has four sub-figures and displays multiple set-ups, but there is only one brief sentence describing it. Please ensure that all figures are described properly and that it is clear what they display.

We added more text to the Results chapter to make sure that all figures are described enough. We also added references to the figures to some previously existing sentences. For example Fig. 4 was referenced in the following sentence: "Using the grid-based approach, the average length of the melt period was the longest in the southernmost zone, non-permafrost (12 days), and the shortest in the northernmost zone, continuous permafrost (7 days) (Fig. 2, Fig. 3 and Fig. 4)."

We hope that all the figures are now described properly.

3. Since you are using inverse modeling, it would also be useful to include a brief comparison with the prior fluxes. So far, this has only been mentioned briefly in the discussion. For instance, you state that the CH4 fluxes are highest in the non-permafrost zone, which you attribute to it being the largest zone. However, could this also be related to the fact that the prior was possibly already estimated to be higher in these areas? The inversion can only optimize the fluxes to a certain extent. For example, if there are large areas in the continuous permafrost zone where the prior fluxes are zero, these will remain zero in the posterior state. These are some points to consider when discussing the results.

We agree that taking the prior more into account is important. We added the prior emissions to Fig. 6, and plotted the difference between prior and posterior emissions of Fig.7 and Fig. 8 and added these figures to the Appendix. We also added more text comparing the prior and posterior emissions to the results and discussion. For example we added a chapter 3.2.1 "Comparison to prior emissions" in the results. From our results we can see that the posterior emissions follow the prior emissions quite well but the posterior decreases from the prior in the northernmost region (continuous permafrost) and increases in the southernmost region (non-permafrost). Additionally, the posterior usually increased from the prior in the Western Siberian Lowlands and Hudson Bay Lowlands regions. Overall the total melt period emissions were increased from the prior with both region- and grid-based methods. In the discussion we added analysis related to the prior and missing observations:

"The increase in emissions from prior to posterior estimates was also more pronounced in these two regions than across the northern high-latitude domain as a whole. This may be attributed to the greater extent of continuous wetlands in these regions and/or the higher density of the observational network (Fig. A1), which likely influenced the prior estimates. Additionally, there are large areas in the high northern latitudes, where the difference between the posterior and prior emissions was close to zero during the melt period (Fig. A7). In grid cells where prior emissions were zero, posterior emissions during the melt period were also zero. This likely contributed to the minimal differences observed between prior and posterior estimates in those areas."

We hope that the added details answer to the reviewer's concerns.

Specific comments

P1, L20-L22:
Could you specify "a large portion of the total soil carbon" with numbers?
We changed the sentence to:
"A large portion of the total soil carbon is stored in northern wetlands and the underlying permafrost, containing ~80% (415 ± 150 Pg C (Hugelius et al., 2020)) of the total global peatland carbon, with nearly half being permafrost affected peatland (Hugelius et al., 2020; Scharlemann et al., 2014)"

P1, L20- P2, L30:
I think the link between permafrost thaw, the carbon stock and CH4 emissions needs to be emphasized more. From the section it is not clear, how the increased near-term CH4 emissions that are concluded at the end come about.

We agree that this part of the text needs to be more clear. We changed the text to:

" Due to climate change and Arctic amplification, thawing permafrost could affect this carbon stock (Schuur et al., 2015; Knoblauch et al., 2018; Voigt et al., 2019; Turetsky et al., 2020). Even though permafrost thaw will likely lead to soil drying and increased drainage, which potentially accelerates organic matter decomposition and CO_2 emissions while suppressing CH_4 emissions (Lawrence et al., 2015), the effect of the released CH_4 might be as large due to its stronger global warming potential (Schuur and Abbott, 2011). Increasing rainfall and warming soils could increase near-term global warming, and the total annual boreal CH 4 emissions could rise 4 Tg per year Neumann et al. (2019). The study by Poulter et al. (2017) using a biochemical model concluded that boreal wetland CH_4 emissions have already increased by 1.2 Tg yr^{-1} between 2000–2012. Even if the permafrost does not fully thaw, the deepening of the active layer – the top layer of soil that thaws in summer and freezes in winter – can still release a significant amount of carbon."

P2, L36-L38:Could you give a source for the changing hydrology? Or is that sill de Vrese et al. (2023)?

We added the citation:
Rawlins, M. A. and Karmalkar, A. V.: Regime shifts in Arctic terrestrial hydrology manifested from impacts of climate warming, The Cryosphere, 18, 1033–1052, https://doi.org/10.5194/tc-18-1033-2024, 2024

P3, L68 :
"Another type of modeling is inverse modeling" Better: "Another approach to estimate fluxes is inverse modeling."
We have now changed the sentence structure to the suggested.

P3, Section 2.1:
Maybe it would be useful to give a short definition of "L-band", since the term is used repeatedly and is quite specific.

We modified the chapter 2.1 after comments from the second reviewer. We also added the following explanation of the L-band: "L-band refers to low frequency (1–2 GHz) passive microwave observations (Rautiainen et al., 2025)."

P4, L100-L101:
"Of the three categories, the thawing state of the soil is used in this study" but which one is the thawing state? The "partially frozen" or "thawed soil"? Maybe change the wording to clarify.

Modified the wording to: "Of the three categories, the thawed soil state is used in this study to define the melt period."

P4, Section 2.2:
Also put references to sections 2.2.2 and 2.2.3 to clarify, where you describe the corresponding observations and fluxes

Added the suggested references to the section 2.2.

P5, Section 2.2.3:
I read in the discussion that also the sink from the soil was included in the biospheric fluxes? This should already be mentioned here for clarification.

We added a sentence to clarify that the soil sink is included in the biospheric prior.

P6, L165:
"Areas where no SMOS F/T data was available, were excluded from this study". Could you roughly estimate the proportion of excluded areas?

We added information regarding the excluded areas:
"This masking excluded areas such as southern regions below 40 ° N where almost no permafrost is located, as well as oceans, most of Greenland, and some areas close to the Great Lakes in the USA."

P7, L186-L187:
"The boundaries used in this study were similar to the ones used by Erkkilä et al. (2023) to define different seasons in the northern high latitude wetlands." It would be good if you could still briefly indicate the boundaries in this paper, because "similar to", is too vague.

We modified the sentence to:
"The boundaries used in this study were similar to the ones used by Erkkilä et al. (2023) to define different seasons in the northern high latitude wetlands, but instead of frozen or partially frozen state of the SMOS F/T data, we used the thawed soil state to define the melt period."

We did not want to add too much information regarding the boundaries defined by Erkkilä et al. (2023), so that the reader does not get confused by the difference to our boundaries. However, we added some comparison to the study in the Results chapter 3.1.1.

P7, Section 2.5.1:
In this section, you mention the word "spring" several times, e.g. L194 "the last day of spring". I'd be interested if you define this spring still based on e.g. month or exclusively by the melting season?

We took out the word spring. In this context the word spring only represented the season during the spring time when the soil turned from frozen to thaw, not any specific months.

P9, L243-L244:
These average lengths values represent the average over all permafrost zones? If so, it should be added in the sentence for clarity.

Added the suggested change for clarity in that sentence.

P10, L198-L291:
"The mean values of the length and temperature of the grid-based melting season might not have been the best to describe the relationship…" Since you state that it's not the best way, what would be a better method in your opinion?

We modified this statement to better explain the relationship between the variables:
"The mean values of the length, temperature and starting day of the grid-based melt period may be insufficient to describe their relationship because the variation between different grid cells is not seen."

P12, Fig. 3:
Could you add more spaces between the tick labels in the lower panels? It would be easier to read.
We made the font size of the ticks smaller so that it is easier to read. We also made some other modifications to the figure so that it looks better, e.g. fixed an issue with missing values in the previous figure.

P12, L317 – P13, L318:
"Hudson Bay lowlands and Western Siberian lowlands are some of the largest methane emitting wetlands in the northern high latitudes." Do you have a source for that or was it concluded from your emission dataset?

We added the following sources:
Pickett-Heaps, C. A., Jacob, D. J., Wecht, K. J., Kort, E. A., Wofsy, S. C., Diskin, G. S., Worthy, D. E. J., Kaplan, J. O., Bey, I., and Drevet, J.: Magnitude and seasonality of wetland methane emissions from the Hudson Bay Lowlands (Canada), Atmospheric Chemistry and Physics, 11, 3773–3779, https://doi.org/10.5194/acp-11-3773-2011, 2011

Umezawa, T., Machida, T., Aoki, S., and Nakazawa, T.: Contributions of natural and anthropogenic sources to atmospheric methane variations over western Siberia estimated from its carbon and hydrogen isotopes, Global Biogeochemical Cycles, 26, https://doi.org/https://doi.org/10.1029/2011GB004232, 2012.

It can also be concluded from our dataset as these 2 regions have some of the highest emissions in the whole northern high latitude region.

P14, L335:
"The average annual region-based melting season emissions" Does that include anthropogenic emissions or only natural?

This only includes the biospheric emissions. We added that to the text for clarity.

P15, L371:
"Figures 7 and A6" Is there a reason why one of the figures is in the main text

and the other in the supplements?

We only included one of the figures in the main text as the two figures are similar to each other and thus both are not needed in the main text.

P23, L506:
"as the whole area is not permafrost or wetlands" Could you please re-formulate to clarify this wording?

We reformulated the wording to:
"Within a single permafrost region, fluxes vary widely because the area does not consist entirely of permafrost or wetlands."

P24, L538-L548:
Are the high share of 31% share of the emissions in the Hudson Bay and Western Siberian lowlands also related to higher emission estimates in the prior fluxes? Also, did you adjust the scale of the NIES measurements before including them in the inverse modeling framework?

We added some comparison between prior and posterior in the Hudson Bay lowlands and Western Siberian lowlands which can be seen in the reply to your comment Nr. 3. We did not adjust the NIES measurements before including them in the inverse model. We added this to the discussion chapter:

"However, the emissions in the Western Siberian lowlands could have been overestimated, because the NIES observation sites, which we did not adjust before using the measurements in the inversion, had a different calibration scale (3.0 to 5.5 ppb higher) than WMO CH_4 X2004A scale as in the (Sasakawa et al., 2025)."

We have modified the manuscript according to the following technical corrections.:

Technical corrections
P1, L5:
Missing article? "for three permafrost zones and for a seasonally frozen non-permafrost zone"
P1, L17 to L20:
Consider splitting the sentence in two sentences. Also "over a 100-year timescale"
P2, L34:
"from the increasingly dry Arctic"
P2, L57:
No article: "but reliable soil temperature data"
P3, L60:
"at a resolution of 25 km"
P3, L62:
Singular: "for the whole northern high latitude region"
P3, L66:
"spring CH_4 emissions have been studied"
P3, L71:"spring melting season and its CH_4 emissions" Better: "spring melting season and the corresponding CH_4 emissions"
P3: L76:

“the spring melting season”
P4, L104:
“the melting snow during daytime affects the descending orbit”
This sentence is deleted from the final version of this chapter.
P6, L160:
One set of parentheses too much: “(Obu et al., 2021)”
P6, L164:
“data had no values” ?
P6: L167-L168:
Please check sentence structure, it’s not clear
P8, L208:
“There were a maximum of 18 SMOS F/T pixels and a minimum of”
P9, L255:
“Other areas with a noticeably longer melting season”
P10, L183:
Missing comma: “Additionally, there was a positive correlation”
P10, L286:
“not as strong a correlation”
P15, L343:
Fig. 8 is mentioned before Fig 7, they should probably be switched?
P16, L378:
“However, there is no major difference”
P22, L465:
One comma too much “Even though methane emissions”
P24, L530 – L531
One “model” too much “with multiple process-based ecosystem models for the northern wetlands”
P24, L534:
“the mean annual emissions in the Hudson Bay lowlands and Western Siberian lowlands”
P25, L556:
No dashes “melting season lengths varied more from year to year than.”
P25, L562:
“in estimating the spring melting season”

Replies to reviewer 2:

Our responses to the reviewer 2 comments are provided below. The comments from the reviewer are in *italic* and our responses are in normal font. For changes to the MS text, please see the new version, in which the changes are marked in red.

The study attempts to technically redefine the seasons/time period of spring melting in northern high-latitudes using SMOS satellite freeze/thaw data by accurately identifying the start of melting season and examine how it relates to the total methane flux emissions. Authors have attempted two different approaches to mark the beginning and end of melting seasons using a region-based estimation and a grid cell-based estimation. Both the methods provide largely variable results but they serve different purposes, which I would suggest needs to be made clear in the manuscript. The work is solid, and would be a valuable contribution to the high-latitude methane research. However, I would suggest the authors to address the following comments which I believe would make it methodologically stronger.

We thank the reviewer for the invaluable and thoughtful comments to our manuscript. In these following comments we aim to give answers to the reviewer's concerns.

1. The Introduction section should clearly articulate the practical benefits and added value of redefining the melting season boundaries using the study's specific criteria rather than relying on the standard (MAM) period. How can redefining the spring melting season help improving the modelling (process-based or inverse) of hydrology and methane emissions in the northern high latitudes? Please include them in the introduction.

We have previously mentioned in the introduction that using the SMOS F/T data to define the melt period in the northern high latitude wetlands gives us a dynamic picture of the soil thawing including the active layer of the permafrost during the spring instead of using a static definition for the whole spring season. We added clarification to the chapter that this does not cover the spring season as a whole, by removing the word "spring" from many sentences. Melt period only covers the beginning of the spring season when the soil thaws. We also changed the term "melting season" to "melt period" to further separate the period from the whole spring season.

In our results we observe a distinct north-south difference in the timing of the melt period, suggesting that the conventional spring classification (MAM) is too strict to accurately reflect the spatial variability in this study.

We modified one sentence in the abstract to separate the melt period from the spring:

"To better understand the seasonality emissions of northern high latitude methane, we defined the melt period occurring in spring time using the remote sensing Soil Moisture and Ocean Salinity Freeze/Thaw data from 2011–2021."

We also added the following sentence to the introduction:

"This allows us to focus on the CH_4 emissions during the actual thawing of the soil instead of the whole spring season with inverse modeling."

2.Total melting-season emissions differ a lot between methods (region and grid) yet both are presented as valid "melting season" totals. This 4 times difference is large and needs a clear methodological explanation beyond just "region based vs grid based". Authors are suggested to clearly mention the purpose of each method in the abstract and conclusion.

We thank the reviewer for this important comment. In the abstract, we have mentioned the following:

"The melt period was defined for three permafrost zones and for a seasonally frozen non-permafrost region using two approaches: region-based, which considered climatological conditions of permafrost regions, and grid-based, which defines the melt period at a finer 1° x 1° scale."

as well as:

"Our dual-method approach allows for robust comparison with both large-scale regional studies and localized site-level research."

To clarify the purpose of both methods at the start, we changed the end of Introduction to:

"The spring melt period is defined for four different permafrost zones (sporadic, discontinuous and continuous permafrost and a seasonally frozen zone non-permafrost) with a region-based approach, as well as individually for each 1°x 1° grid cell with a grid-based approach. For comparison between the two methods to define the melt period, we divided the grid-based melt period into the same four permafrost zones. This allows us to estimate emissions both in climatological and local scales."

We edited one sentence in the conclusions to the following:

"The region-based method is comparable to studies with monthly emission estimates, as it focuses more on the climatological differences between large regions, while the grid-based method is more comparable to local studies."

3.The implication of the following statement in the conclusion is not clear to me. "Increasing temperatures could lead to shorter melting seasons and lower melting season methane emissions but also a longer thaw season". If total melt-season emissions directly change with melt-season length, then a shorter melt season would reduce emissions in that window, but the longer thawed period outside that window may increase annual emissions. Thus, can lead to confusions about the future total CH_4. The relations between temperature, season length and methane emission relationship needs to be explained better as it could confuse readers about whether warming increases or decreases overall emissions.

We modified the last part of conclusions to be more clear about the relationship between the variables. The modified chapter is below.

"The total melt period emissions in the northern high latitudes were only a small portion of the total annual emissions. However, future climate change and associated permafrost thawing could amplify melt period emissions, resulting in elevated CH_4 bursts. On the other hand, our results showed that a shorter melt period had smaller total emissions and a higher mean temperature on average. Increasing temperatures could lead to shorter melt periods and consequently lower melt period methane emissions. Subsequently, a longer summer thaw season could lead to higher annual emissions. However, it is still unclear how climate change feedback loops will affect emissions and shoulder season lengths in the northern high latitudes, as permafrost thawing and soil drying could lead to higher CO_2 emissions instead. To get a better look at the melt period emissions, the results from several inversions could be compared to each other. In addition, going further to the level of individual wetlands and comparing their flux measurements during the melting could bring more clarity to true wetland emissions."

4.The thresholds (e.g., +10% from min thaw, 80% of max) feel somewhat arbitrary without sensitivity tests or comparison to ground data (e.g., soil temps, snowmelt onset).

We thank the reviewer for this important comment. We agree that threshold choices should be justified. In the region-based approach, the purpose is to define a *robust,* climatologically representative transition window for each permafrost zone rather than to identify the exact onset of soil thaw at the pixel level. During early spring, SMOS L-band F/T retrievals can respond to wet snow as well as thawed soil, because wet snow produces a microwave signature similar to thawed soil; therefore the earliest changes in thawed fraction are not always uniquely attributable to soil thaw.

We therefore used a start criterion based on the thawed fraction rising 10 percentage points above the annual minimum and an end criterion at 80% of the maximum, to avoid defining season boundaries from the most noise-sensitive extremes (near 0% or near 100%) and to better capture the bulk, sustained transition at the regional scale.

Using the grid-based approach, we defined the onset of the melt period as the point at which at least one 25 km × 25 km pixel within a grid cell had thawed. This approach allows us to account for the finer spatial resolution of the original pixels. To define the end of the melt period with the grid-based approach, we wanted use a method consistent with the region-based approach.

We examined the daily mean thaw fraction in the four permafrost zones in relation to the region-based melt-period onset and end dates. We found that once the thaw fraction increased by approximately 10% above its minimum value, it began to rise more steadily. Conversely, after reaching about 80% of its maximum value, the rate of increase slowed considerably.

Importantly, our key conclusions rely on relative differences in melt period length and their relationship to methane emissions, and we consistently find a positive relationship between melt period length and total melt period emissions in all permafrost zones using both the region-based and grid-based definitions.

We added the following to the chapter 2.5.1:

"The selected thresholds were chosen to define a robust, zone-mean transition period rather than the exact timing of soil thaw at individual grid cells. In early spring, the SMOS F/T signal can respond to wet snow in addition to thawed soil, as liquid water in the snowpack produces a microwave signature similar to that of thawed soil (Rautiainen et al., 2025). Consequently, thawing fractions very close to 0% or 100% are more sensitive to short-term fluctuations in the microwave signal. Defining the season boundaries away from these extremes ensures that the melt period reflects a sustained, large-scale transition."

5.The original feedback suggests the title may be confusing because the definition of the season is based on the melting process, yet the term "spring melting season" might be too specific or misleading.

We agree that the title can be a little misleading as our method to define the melt period does not necessarily cover the whole spring season as the melting usually occurs in the beginning of the spring. We changed the title to:

"Melt period methane emissions in northern high latitude wetlands are governed by the length of the period and presence of permafrost"

6.Soil Moisture and Ocean Salinity Freeze/Thaw data from 2011–2021. Were there specific data quality or processing limitations that demanded ending the analysis in 2021?

The analysis period ends in 2021 because a marked increase in radio frequency interference (RFI) affecting SMOS observations occurred later, starting in late 2022, particularly over Europe and western Eurasia. This increase has been widely associated with changes in the radio-frequency environment following the escalation of the war in Ukraine. SMOS is an L-band aperture synthesis radiometer and is therefore especially sensitive to RFI contamination, which substantially reduces data availability and degrades retrieval reliability. To ensure temporal consistency and robust spatial coverage across the study domain, we therefore restricted the analysis to the period 2011–2021, during which the SMOS freeze–thaw product exhibits relatively stable data quality. This rationale is now explicitly described in Sect. 2.1.

7.Line 95 -100: who developed The SMOS F/T soil state detection algorithm? Give details

The SMOS FT soil state detection algorithm was developed by the Finnish Meteorological Institute (FMI) in collaboration with GAMMA Remote Sensing, Switzerland. The algorithm is first described in Rautiainen et al. (2016) and in more details in its latest updated and validated form in Rautiainen et al. (2025). We have clarified this in the manuscript and explicitly refer readers to the most recent algorithm description paper for full methodological details.

8.Line 98 – 100: We require additional detail regarding the methodology used for soil categorization and thawed soil identification. How are the thresholds for soil categorisation estimated? Please provide a brief but explicit explanation of the algorithm used to identify and classify thawed soil based on the observations? What is the estimated uncertainty of this spoil classification?

We agree that readers need a clear understanding of what the SMOS freeze–thaw product represents and how thawed conditions are identified. We therefore revised Sect. 2.1 to include a concise description of (i) the physical observable used (normalized polarization ratio, NPR), (ii) the use of grid-cell-specific frozen and thawed reference states, and (iii) the threshold-based classification into thawed, partially frozen, and frozen soil states. Full algorithmic details, validation, and uncertainty characterization are provided in Rautiainen et al. (2025), which is now explicitly referenced.

9.Line 102 -104: What exactly are the reasons for this? Attenuation?

We clarified the rationale for using only ascending-orbit SMOS freeze–thaw observations. Oliva et al. (2016) document the spatial structure and severity of SMOS radio frequency interference (RFI), particularly over Europe and Asia. In addition, ESA's operational RFI monitoring (ERMIT) shows orbit-dependent differences in data availability. To maximize data quality and spatial coverage in our study region, we therefore restricted the analysis to ascending-orbit observations. This orbit selection rationale is now described in Sect. 2.1.

10.Line 108: not very clear to me. what fraction of the combining pixels is considered to be taken as a thawed pixel?

We implied that the original resolution of the SMOS F/T data was 25 km x 25 km and we needed to change the resolution to 1º x 1º to use it in our data-analysis together with the global CarbonTrackerEurope-CH_4 inverse dataset. Each 25 km x 25 km pixel was classified as frozen, partially frozen or thawed, as is explained in the chapter 2.1. We deleted this sentence from the final version of the chapter 2.1. but added this explanation to the chapter 2.5.1:

"The resolution of the SMOS F/T data was changed from 25 km x 25 km to 1° x 1° by calculating the fraction of the thawed 25 km x 25 km pixels whose center was inside the 1° x 1° grid cell."

11. Line 184 – 185: "However, methane emissions are possible even at the very beginning of the melting season because the air temperature rises above zero and melted water can trickle into the soil". Please add references for the statement.

We added citations to the following articles:

Raz-Yaseef, N., Torn, M. S., Wu, Y., Billesbach, D. P., Liljedahl, A. K., Kneafsey, T. J., Romanovsky, V. E., Cook, D. R., and Wullschleger, S. D.: Large CO_2 and CH_4 emissions from polygonal tundra during spring thaw in northern Alaska, Geophysical Research Letters, 44, 504–513, https://doi.org//10.1002/2016GL07122, 2017.

Song, C., Xu, X., Sun, X., Tian, H., Sun, L., Miao, Y., Wang, X., and Guo, Y.: Large methane emission upon spring thaw from natural wetlands in the northern permafrost region, Environmental Research Letters, 7, 034 009, https://doi.org/10.1088/1748-9326/7/3/034009, 2012.

Rinne, J., Riutta, T., Pihlatie, M., Aurela, M., Haapanala, S., Tuovinen, J.-P., Tuittila, E.-S., and Vesala, T.: Annual cycle of methane emission from a boreal fen measured by the eddy covariance technique, Tellus B: Chemical and Physical Meteorology, 59, 449–457, https://doi.org/10.1111/j.1600-0889.2007.00261.x, 2007.

Hargreaves, K. J., Fowler, D., Pitcairn, C. E. R., and Aurela, M.: Annual methane emission from Finnish mires estimated from eddy covariance campaign measurements, Theoretical and Applied Climatology, 70, 203–213, https://doi.org/10.1007/s007040170015, 2001.

12. The second condition in region-based approach is not very clear to me. Do you mean that the spring melt must start after the day when the zone reaches its minimum annual mean thawing fraction?

Yes, this is exactly what we mean. As we explained next:

"During some years, the freezing of the soil continued past the turn of the year, which meant that the (1) boundary was reached before the maximum freezing of the soil. This meant that the additional (2) condition for the beginning of the melt period had to be defined. With the (2) condition, the melt period could be separated from the autumn freezing period. In regions with permafrost, the first (1) condition was surpassed later during the spring than the second (2) condition, but in the non-permafrost zone the second (2) condition was needed."

We changed the first sentence to be more clear. It is now written as follows:

"With the region-based approach, the melt period was set to start 1) when the mean thawing fraction of a permafrost zone had surpassed the minimum thawing fraction of that year by 0.1 (thaw(%) ≥ thawmin,year(%) + 10 %), and 2) after the day when the zone reaches its minimum annual mean thawing fraction before mid July."

13. Line 205: "The mean thawing fraction of all grid-cells in a permafrost zone was then calculated." How does this solve the problem? Do you mean an area weighted average?

We agree that this might cause some confusion so we changes this part of the text to the following: "The melt period in each grid cell was defined from the amount of thawed pixels in a grid cell, emphasizing more the small grid cells in the north. The mean thawing fraction of all grid-cells in a permafrost zone was then calculated."

Technical comments

1. Line 8 – 10: Statement not complete. Length and the mean temperature?

We fixed the sentence to say the following:

"The melt period generally occurred between March and June and was influenced by the air temperature, with a negative correlation between the length and the mean temperature of the melt period."

2. Section 2.2.2: Please mention which parameters are measured at these stations. surface variables or CH4?

We changed one of the sentences in the chapter 2.2.2 to mention CH_4:
"The data included weekly discrete air samples and hourly continuous measurements of CH_4, and the data was filtered according to the institutions' quality flags."

3. I would suggest move figure 1 to section 2.3.

We moved the figure under section 2.3.