**General comments**

This manuscript presents atmospheric observations of CH4 from 5 new in-situ measurements sites in the Canadian Arctic and uses these (plus one other site) in atmospheric inversions to determine the land-atmosphere flux of CH4. The authors find notable inter-annual variability in the natural (wetland) flux, which may be related to variations in surface temperature. Although the study is interesting and fairly well presented, further explanations and clarifications for some of the methods are needed before being published. In addition, minor technical corrections for English language usage are required.

**Specific comments**

P1, L14: I suggest specifying the number, instead of "multiple". Also how is "inversion modelling system" defined, by the inversion algorithm or transport model used? In this study 2 different transport models were used with 3 different meteorological datasets, so I suggest the authors state this instead.

P1, L30: I suggest the authors state what the carbon is vulnerable to, i.e., conversion to CH4 and CO2 which can be emitted to the atmosphere

P2, L5: Please specify the magnitude of what, presumably CH4 emission but this should be stated

P3, L7 (and throughout): It's actually CH4 volume mixing ratio that is reported, and not concentration, so I suggest changing "concentration" to "mixing ratio" throughout.

P5, L31: By "SD of the observed time series to their fitted curves" do the authors mean the SD of the residuals, i.e., after subtracting the fitted curves? This is not clear.

P5, L34 to P6, L2: This needs some explanation why the difference between SD_PM and SD_24 gives an indication of whether the daily variability is due to local scale changes in emissions or seasonally changing atmospheric transport. I guess the authors mean that SD_24, which includes also night-time data, is more sensitive to local emissions than SD_PM, but an explanation should be provided.

P6, L6: "rectified" is not the right term here (the rectifier effect is a specific term given to the co-variation of flux and planetary boundary layer height, particularly for CO2, which doesn't apply here). Instead use "amplified".

P10, L7: The authors should change this sentence to either "Our Bayesian inversion optimizes…" or "The Bayesian inversion used here optimizes…" to make it clear that the approach used here is not the only approach.

P10, L16: The authors state that the matrix **K** is the product of **M** (the footprints) and **x** (the surface fluxes) and is a Jacobian matrix of flux sensitivities. The elements of **K** must be in mass mixing ratio units (i.e. the same units as **y**), so by definition this is not a Jacobian matrix (but **M** is a Jacobian). Also, the dimensions of **M** and **x** should be given.

P10, L22: The units of the observation uncertainty should be specified, presumably this is ppb. Also, an explanation should be given of how the value of 0.33 was derived, especially as this seems rather small. Furthermore, an estimate of the appropriateness of the uncertainty estimates should be given, e.g. the value of the reduced-chi-square statistic.

P10, L29-30: This needs a bit more explanation, do the authors mean that they have separate variables for the biomass burning and other emissions, which are optimized simultaneously. In this case, the total number of variables would be R x 2 x number of flux time steps.

Section 3.3.2: Using only 3 regions for the optimization represents a significant aggregation error, as it is assumed that both the spatial pattern and relative magnitudes of the fluxes within each region are correct. Why was the inversion performed only for these coarse regions? Other than being different territories, are they characterized by having similar ecosystems, climate or other?

Section 3.3.3: Errors in the modelled background can incur errors in the posterior fluxes. Did the authors check their global modelled CH4 mixing ratios (from NIES-TM) with independent observations in the northern high latitudes? Was an estimate of the uncertainty in the background made and included in the overall observation uncertainty?

P12, L21: The sub-region masks A to C are not defined in the text

P12, L26: I suggest the authors state that the negative biomass burning fluxes are "spurious" since the biomass burning source cannot be negative.

P15, L18-19: Do the authors mean the anomalies of the deseasonalised data? It is important to look at the anomalies in the data after the mean seasonality has been subtracted to avoid correlations with temperature between months, which would over-ride possible correlations with temperature between years.

Figure 12: It would be interesting to see the regressions for the prior wetland emissions as well. How strongly are the prior wetland emissions correlated with the meteorological variables and how does this influence the posterior correlations?

**Technical comments**

P1, L26: "stronger then" should be "stronger than"

P1, L27: add "from" before "about 722 pbb"

Generally: attention should be paid to the use of articles "the" and "a" and when no article should be used at all.

P6, L14: replace "Like" with "Similar to" as "like" in this sense is very colloquial.

P6, L15: there are words missing in this sentence, it should be "…indicates that there is a weaker local source of CH4…" and "than around the three continental sites".

P6, L19: should be "suggested that there are on-going CH4 emissions from…"

P6, L29: should be "due to the (very) short period of daylight"

P8, L27: should be "C3 is the same as used in C2, but…"

P9, L31: should be "…map of climatological termite emissions"

P12, L16: change "done" to "made"

P12, L22: should be "are shown" (not "showed")

Fig. 5: should be "same as C2"

P14, L25: Suggest changing the section heading to "Sensitivity tests" since there are more than one

P15, L7: should be "in winter compared to…" (not "against") and I think the authors mean "which might contribute to large uncertainties in the flux estimation"

P15, L9: change "done" to "made"