The paper "Growing season CH_4 and N_2O fluxes from a sub-arctic landscape in northern Finland" is very well structured and is written with very good, fluent language. The study based on the state of the art methods of chamber measurements (at least for CH_4 and N_2O). The topic fits well within the scope of 'Biogeosciences'. Although the CH_4 and N_2O measurements do not provide new insights, the subject of the study is very important, since reliable but simple upscaling approaches for GHG are still rare in literature, but are urgently needed.

I agree with the comments by the other referee but however, I have some additional remarks and a number of suggestions, which I believe will improve this manuscript once addressed and need to consider before publication.

Major comments:

1) I would suggest to change the title since the actual one describe insufficient the intention of the study concerning the applied modeling approach to extrapolate measured CH_4/N_2O fluxes to landscape scale.

2) In order to receive reliable mean GHG flux rates, the amount of measurements seems rather short for me, in particular when the data set is used for model building and upscaling. For upscaling to landscape scale calculated mean flux rates or emission factors should at least represent annual values. In your study, measurements were carried out during a summer and an autumn campaign with 10 and 7 to 8 single measurements during a period of 22 or 23 days. To capture the temporal variability during these periods, conducted measurements seems sufficient. Nevertheless, for the rest of the year no additional measurements were conducted, nor any estimations or literature values were given. Form several studies published in literature, it is widely known that intra and also inter annual variability can be very high which necessitate the need for long-term studies to receive reliable mean GHG flux rates. However, the harsh environment of norther Finland makes it to some point difficult to measure around the whole year. Nevertheless, measurements during springtime would have been quite useful in regard to thawing soil conditions, which perhaps resulting in a markedly different behavior of CH_4 emissions. Also a rough estimation of winter time fluxes or literature values should be given. Generally, I strongly recommend that this issue should be taken up in more detail in the introduction, discussion and the conclusion of the manuscript. Please further include a sentence in the abstract that the study based just on a few single measurements during a single year.

3) Your data analysis includes an interesting approach to consider the skewness of observed CH_4 fluxes in the calculation of means and variations. In general the issue of skewed data and the resulting error in the calculation of means and variances of those data sets is mostly disregarded in almost all studies. Therefore, I strongly support the idea to revisit this issue. Nevertheless, there are some points which have to be described in more detail or have to be considered:
a) The geometric mean is limited by the fact that variables have to be > 0. In the presented study, CH_4 and N_2O exchange include the release and uptake of both gases. To take this into account you calculate the geometric mean of all positive and all

negative flux rates independently and from this a frequency-weighted mean? Maybe it would be helpful to include the formula of the calculation approach.
b) In contrast to the arithmetic mean, the use of ± standard deviation or standard error is not meaningful for the geometric mean. Instead, the standard deviation should be given as multiplication or division factor (Lozán and Kausch, 2007). This has to be considered in the manuscript.
c) Why do you choose the geometric mean for the estimation of mean CH_4/N_2O fluxes instead of trying to apply e.g. method of moments estimators or uniformly minimum variance unbiased estimators (for this see: Parkin et al., 1988: Evaluation of statistical estimation methods for lognormally distributed variables; Parkin et al., 1990: Calculating Confidence Intervals for the Mean of a Lognormally Distributed Variable)? Can you cite any other study who calculates a geometric mean for GHG fluxes? I suggest to recalculate the mean flux rates with both methods, presented by Parkin et al., (1988) and to compare the corresponding results with the calculated geometric mean. I think this procedure will significantly contribute to reduce the uncertainty in future investigations.

Minor comments and suggestions:

1) Page 2, line 30: Vegetation also exerts a direct and indirect control on N_2O emission! Please complement.

2) Page 3, line 7: N_2O can also be produced through abiotic processes (chemodenitrification, chemical decomposition of NH_2OH, surface decomposition of NH_4NO_3; e.g. Butterbach-Bahl, 2013: Nitrous oxide emissions from soils: how well do we understand the processes and their controls?). Change the formulation of the sentence accordingly.

3) Page 5, line 12: Please ad short information's about chamber configuration: chamber height or volume, air mixing yes or no, chamber inside thermometer yes or no, rubber lip or similar to ensure air tightness during chamber placement on in situ bases, etc…

4) Page 5, line 17: How was the chamber air collected? Did you evacuated the vials previously? How do you protect the vials for air pressure differences during air transport (e.g. Glatzel and Well, 2008: Evaluation of septum-capped vials for storage of gas samples during air transport)?

5) Page 5, line 24: In the latter manuscript, you also refer to air temperature. Please describe shortly sensor type and placement, record interval, etc. Do you measure chamber inside air temperature?

6) Page 5, line 29: I recommend the term ecosystem respiration rather than soil respiration.

7) Page 5, line 30: In my point of view, the PP-Systems SCR-1 respiration chamber (150 mm height, 100 mm diameter) seems very inappropriate for measuring ecosystem respiration (or soil respiration including ground vegetation). The dimension of the chamber is by far too small to cover the predominant vegetation at your sites

investigated. Therefore, it can be assumed that this approach significantly disturbed the plants and thus markedly change the CO_2 fluxes. I strongly recommend to remove all related parts in the manuscript.

8) Page 6, line 18: Did you apply any transformations (or did you remove outliers) to achieve a normal distribution in the data set (e.g. for CH_4 fluxes) prior to the PCA? I think that this might be necessary since PCA based on parametric Pearson correlations!

9) Page 7, line 14 and following manuscript: Did you always mean geometric mean if you write mean?

10) Page 7, line 17: Did you mean 1.06 ± 0.44 µg N m^{-2} hr^{-1} instead of s^{-1}? (This also relates vice versa to Table 1).

11) Page 8, line, 25: Have you tested the assumptions for linear models (e.g. normal distribution of residuals, homogeneity of variances, autocorrelation etc.)? I guess that the strong skewed dataset will partly violate the assumptions of an ANOVA? Please describe your statistical procedure in the section Data analysis. Please also describe which factors (e.g. single CH_4 fluxes or mean group CH_4 fluxes, temperatures, PCA_veg, etc.) were included as fixed effects in the ANOVA. Have you tested just one factorial or also multifactorial approaches? Did you consider temporal pseudoreplication in case of chamber specific GHG fluxes?

12) Page 9, line 13: Have you tested for non-linear relationships? In case of non-normal distribution of data, Pearson correlation coefficient (r) is perhaps not the right choice as a measure for the intensity and direction of a relationship. Maybe Spearman rank correlation coefficient is more appropriate?

13) Page 9, line 27: Please mentioned that the mean CH_4 flux which you use for upscaling did not represent an annual mean CH_4 flux rate (e.g. average CH_4 flux over the growing season Page 12, Line 27). Have you tried to separate between summer and autumn CH_4 fluxes for model building and upscaling?

14) Page 10, line 1: Is the area weighting factor 61% wetland and 32% forest?

15) Page 10, line 11 to 15: Don't be too critical with the observed close to zero net N_2O fluxes and the fact that no drivers for upscaling are found. Maybe gross production of N_2O occurs at your sites investigated, but in the end it is an important result that both ecosystems actual did not significantly contribute to global warming through the release of N_2O emissions. However, this fragile balance can change very quickly in the course of e.g. climate warming, drainage, etc. and should therefore shortly be mentioned in the discussion and conclusion. Further, it would be fine to include also N_2O fluxes as an additional Figure.

Technical corrections:

Please check the entire manuscript in regard to consistency of units, citations (e.g. sometimes italic formation), fonts (sometimes times new roman, sometimes other formatting).

1) Page 2, line 9: are essential -> is essential

2) Page 3, line line 6: aerobic condition -> aerobic conditions

3) Page 4, line 14: in the area our -> in the area where our ..

4) Page 5, line 2 and 3: Formatting of the date: 12th July – 2nd August……

5) Page 5, line 14: occasions, the short -> occasions. The short …

6) Page 5, line 15: fluxes, and -> fluxes, which

7) Page 5, line 26: 5 mm instead of 5mm (maybe you mean 5 cm for dip well instead of 5 mm?)

8) 5 line 28: located equidistance -> located at equidistance …

9) Page 6, line 2 and 3: Formatting of the date…

10) Page 6, line 30: Formatting of the date…

11) Page 7, line 9: 8 and 9% instead of 9 %

12) Page 7, line 14: both units mg C m^{-2-} hr^{-1} -> mg C m^{-2} hr^{-1}

13) Page 7, line 24 and 25: P < 0.01 instead of P <0.01

14) Page 7, line 29: emissions thus -> emissions, but …

15) Page 8, line 13: emissions wert -> emissions was …

16) Page 8, line 19: correlated CH_4 -> correlated to CH_4 …

17) Page 8, line 33: Between-group differences or Between group differences; please be consistent (relates to the entire manuscript).

18) Page 9, line 23: 45%

19) Page 9, line 27: Methane can be abbreviated. This also relates to the following manuscript.

20) Page 10, line 4: -0.06 + <0.01 -> -0.06 ± <0.01

21) Page 10, line 15: Or instead of over?

22) Page 10, line 24: Turetsky et al., 2014. -> Turetsky et al., 2014).

23) Page 11, line 32: water level was -> water level were …

24) Page 12, line 3: show are -> show is …

25) Page 12, line 31: landscape scales fluxes -> landscape scale fluxes ..

26) Page 13, line 6: Hartly et al. (2015) who's study -> Hartly et al. (2015) whose study …

27) Page 13, line 22: temperature -> soil temperature

28) Page 18, Table 1: Please note that mean represent the geometric mean.

29) Page 18, Table 3: I strongly recommend the use of an adjusted r^2 instead of r^2 since $r^2_{adj.}$ considered the number of predictors in the model.

30) Page 19, Figure 1: Minus sign is missing in the unit of the X-axis

31) Page 21, Figure 3 a) and b): X and Y-axis show principle components 2 and 3 instead of 1 and 2!

32) Page 22; Figure 4: Unit of soil moisture is missing!

33) Page 24, Figure 6: The Unit of soil respiration differs from Figure 4 and Figure 9 (g m^{-2} hr^{-1}, instead of mg m^{-2} hr^{-1})! Did soil respiration represent CO_2 or CO_2-C? See also Minor comments and suggestions Nr. 7

34) Page 25, Figure 7: Units are missing!