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Interactive comment on "Growing season CH_4 and N_2O fluxes from a sub-arctic landscape in northern Finland" *by* Kerry J. Dinsmore et al.

Anonymous Referee #1

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GENERAL COMMENTS

The paper you present here is a clearly written and logically constructed report on fluxes of the two important non-CO2 GHGs, CH4 and N2O, of a subarctic landscape in Northern Finland. The fluxes were measured by a static chamber technique from the main landscape elements in the region - forests and wetlands - and upscaled to an area of 4 km2 based on spectral data from a high-resolution satellite image.

The used field methods seem sound, replication of the chamber measurement is good, and the careful data analysis of the flux results is a particular strength of this study. In my opinion, some methodological issued need more detailed descriptions, and these things are specified below. The flux measurements were carried out during two relatively short campaigns in the summer and autumn season of a single year, which is

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a short data collection period compared to the similar studies published during recent years. Although neither the experimental design nor the results of this study do not include any genuinely novel aspects, this kind of regional upscaling efforts are still quite rare and very much needed to improve our ability to calculate more accurate GHG balances in a large scale. This relevance of this study should be stated much better, now it is not fully convincing. In addition to this, there are several other points that require your careful consideration and before the publication of this report can be recommended.

SPECIFIC COMMENTS

The biggest problem of this manuscript is that the relevance of this particular study is not argumented well enough. This concerns both the introduction section and conclusions, and it leaves the reader with the feeling that you were not very sure of the importance of the study yourselves. More specifically: In the introduction you base the importance of the study on large SOC pool in high-latitude soils and uncertainties of the carbon-climate feedback. Since you are not measuring CO2 fluxes that represent the most of the C gas fluxes between ecosystems and atmosphere, you should much more emphasize the importance of the non-CO2 GHGs instead (higher radiative forcing on weight-unit-basis, uncertainties in the drivers of CH4 fluxes, almost completely lacking knowledge on the distribution of N2O fluxes in high-latitude ecosystems...). The text in the abstract on lines 9-11 is a good start, but it belongs to the introduction section, since abstract should not contain ideas not mentioned in the main text of the manuscript. You should also put the CH4 and N2O emissions into context, and mention clearly enough their secondary importance relative to CO2.

Similarly, the discussion/conclusion section does not fully convince of the importance of the study. It is very good to point out the uncertainties of the presented results, but at the present state the conclusion chapter does not fully justify, why this study should be published as an important contribution to the field.

I find that the upscaling exercise is the most interesting part of the study, and should be

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more emphasized in the paper, e.g., at the expense of the discussion on the impact of the water table level on CH_4 flux that does not reach very clear conclusions. A review of similar upscaling efforts is needed. Are there many previous studies like this in the subarctic region, how about in the rest of northern Scandinavia? Are the methods used here similar or very different compared to the previous studies? What do we learn here that was not previously known?

The N_2O fluxes from the studied plots were mostly not statistically different from zero. However, the results of the N_2O fluxes are too much down-tuned in the manuscript text. Based on results from the last decade, there are surfaces in the subarctic and Arctic that have potential for N_2O emissions (Elberling et al. 2010 NGeo, Marushchak et al. 2011 GCB, Abbott et al. 2015 GCB), although N_2O is still rarely included in GHG inventories in the north. With this in view, it is important to screen various high-latitude ecosystems for N_2O fluxes and also produce base-case flux balances against which possible climate change induced changes in the fluxes can be observed. The "zero-result" is not irrelevant, but it is important knowledge, which should be much stronger stated in the manuscript.

Here are some additional minor comments:

***Abstract The abstract seems rather long to me. Could you make it more compact, concentrating just to the main outcome of the study? This would make the main message appear stronger to the reader. Page 1, line 2: Why should the ecosystems be described as consistent sinks or sources, if you can with high confidence state that the emissions are negligible? It is as important results. Now, one gets an impression that after so many flux measurements you still do not know anything about the N_2O dynamics.

***Introduction Page 2, lines 5 and 9: emissions of what? Please specify! Page 2, line 11: Here, you mention permafrost thaw as one of the secondary drivers of GHG emissions, but you do not tell in the site description if your site had permafrost or not.

Please, add this information in the site description. Page 2, lines 16-18: This is very general. How does this particular study answer to this need? What does it give that is not yet known?

***Methods Page 4, line 17: Here, you mention that the intermediate enclosure time was 15 years, while later (page 5, line 6) you say that it was 12 years. Which one is correct? Page 5, lines 7 and 8: Even if you want to avoid subjective classification of the wetland plots, and rather rely to clustering analysis, it should be easy to distinguish between ridges and flarks. Please, mention how many of your collars were located in these different mire microforms, and does this represent the proportional coverage of these microforms. This is relevant knowledge for the later upscaling exercise (upscaling based on simple averaging within wetland and forest classes). Page 5, lines 13-15: Starting the measurements so soon after the installation of the flux collars is well enough justified here, but I am missing details on how the disturbance caused by the field workers was minimized. Did you construct boardwalks in the vicinity of study plots? Did you observe (CH4) ebullition events during the measurements, and do you think they were natural or caused by people? If yes, how large proportion of the flux measurements you had to exclude for this reason? Page 5, lines 29-30: If it includes respiration from ground vegetation, ecosystem respiration would be more accurate term than soil respiration. You can anyway determine what was included (not the respiration from taller vegetation due to the methodological limitations). Page 5, line 33: ‘vegetation coverage’ instead of just ‘vegetation’ would be more precise. Page 6, line 1: Please, add a reference on PRS and/or some specification on what they sample and by which principle? Is it just collection of soil pore water, from which nutrients are analyzed or something else? A list of the measured ions would also be good to include here. Page 6, lines 8-12: Please, specify the criteria used to include or exclude the flux data for analysis, and mention (here, or in the results) how many percent of the fluxes had to be rejected. Page 6, line 24-25: Did you try the correlations on the level of single plots to investigate the drivers of temporal variability? Sometimes there can be large variability even at small scale, and this is needed to reveal the factors of

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behind the variability. What made you think that the plots with similar flux magnitude would have similar mechanistic behavior? Page 7, line 3: It does not seem correct to state that the uncertainty of the N2O fluxes was large. On the contrary, based on the description of the field method and the data analysis, it is evident that the fluxes were near-zero with high confidence, just the sign of the small flux is uncertain. With increasing fluxes, also the absolute uncertainty usually increases, whereas it small for small fluxes. Please, revise the sentence to be more correct, for example: 'Due to low variability of N2O fluxes. . .'

***RESULTS Page 7, lines 8-9: Were these 8-9 % of the N2O fluxes that were significantly different from zero evenly distributed between study plots, or where there some that showed more often significant source or sink character? It is important to mention this, since it is well known that there is a high spatial variability of N2O emissions – where there any plots that were clearly sinks or sources? Page 8, line 3: What do you mean by soil concentration data, the concentration of nutrients in the soil pore water? Please, specify! Page 8, line 16: Do I understand this correctly, that you had higher fluxes from ridges with deep water tables than from flarks with high water tables? This is interesting. Is this a common observation from aapa mires? Page 8, line 24: Since this classification is very abstract, it would make sense to somehow relate it to wetland microforms, vegetation or similar. How were the flark and ridge collars distributed in these classes?

***DISCUSSION Page 10, line 17 onwards: The CH4 fluxes were not very well correlated with environmental factors. One explanation could be that the differences in vegetation cover were overruling the effect of other factors. The role of vegetation as a potential driver of spatio-temporal variability (vascular plant coverage/biomass/leaf area/plant number, productivity) is well acknowledged in previous literature on wetland CH4 emission. Particularly vascular plants are important due to methane transport and input of fresh carbon to the sediment. This is not adequately discussed and not very well addressed by experimental design. Please, add adequate discussion on this topic in the discussion section. Page 11, line 29-33: These citations (Tupek, Turetsky) would need some mechanistical explanation, is this water table optimum of around 20 cm related to differences in plant productivity, i.e., a side product? What is the interpretation in the cited studies? Page 12, line 5: The spatial variability in temperatures is rather small. Do you think that this is a true temperature dependence, or is it more a result of another factor that is more important for CH4 flux, such as water table level? Page 12, lines 28-32: To make this discussion meaningful, you should mention, what where the proportions of wetlands and forests in the study by Hartley et al. vs. this study. Please, add this information!

***Figures Figure 4. Please, indicate the sampling period used for this representation – are the averages for both summer and autumn campaigns used? Figure 6. The water table of the forest plots seems too high – was it really at 5 cm below the surface and not different from wetland plots? How do you explain this?

TECHNICAL CORRECTIONS

Figure 3. In the figure caption, you mention PC 1 and 2, while PC 2 and 3 are shown in the figure. Please, check this.

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