Response

1. Referees 1 & 2 Comments

   Referee 1*: The study is an important contribution to the ongoing efforts to evaluate the importance of nitrous oxide fluxes in the permafrost regions. The authors have considered in details the methodological aspects of the airborne EC method they applied…The footprint analyses here also indicated areas with negligible emissions and areas with high nitrous oxide emissions.*

   Referee 2*: Overall, the analytical technique and estimation of N2O flux based on EC Method are well described with plenty of details. The analytical precision of N2O mixing ratios seems satisfying for airborne measurement, and uncertainty in flux estimations has been also discussed.*

   1.1 Response to Referees: We appreciate the referees' affirmation of the techniques and uncertainty analysis on which we base our conclusions.

   1.2 Change to Manuscript: None.

2. Referees 1 & 2 Comments

   Referee 1: *To get such a high mean emission rate shown here by the EC, the emissions from the high-emitting areas have to be very high. Would be excellent if the authors could get some published or non-published data on nitrous oxide emissions in the region based on chamber measurements or determined by a gas gradient approach based on nitrous oxide content in soil. This data could then be upscaled by estimating the total coverage of the high emitting areas. If the nitrous emissions from these analyses are in the same range as the mean emission rate here, this could confirm the results obtained by EC.*

   Referee 2: *However, given the unexpectedly high N2O flux, it remains a question to us how realistic flux data we could obtain based on these airborne measurements within limited time scales. Although this approach has been tested for CH4/H2O with a near-by EC tower (Sayres et al., 2017), spatial variabilities in N2O fluxes could largely differ. Therefore, more ground-based fluxes data in the near region by EC tower or chamber measurements are necessary to confirm the applicability of this technique. Alternatively, if N2O emission factors or edaphic parameters are available in this or other similar regions, a ground-based model estimation of N2O fluxes separating landscape elements may strengthen the whole manuscript.*

   2.1 Response to Referees: Indeed the high-emitting "hot-spots" have to be quite prominent to affect the overall average. The nature of an airborne study is to provide a spatial survey of the prevalence and spatial distribution of such high-emission locations along with any other distributed sources in an area difficult of access. The reviewer characterizes the high spatial variation of N_2O flux to be well known from chamber methods. The significance of our result, in addition to confirming the spottiness, is to find such hot spots and other sources of N_2O to be sufficiently strong and/or numerous over landscape and larger scales on the North slope to add up to average N_2O emission comparable to that found in the tropics.

   To our knowledge, established emission factors do not currently exist for permafrost N_2O emissions. No significant emissions were reported from any permafrost land class until 2009, when researchers identified bare peat circles as potential emitters of significant amounts of N_2O. Several other studies (cited in the manuscript) have since come out reinforcing the notion that N_2O could be significant. However, these are mostly laboratory studies, soil N_2O observations, and metagenomic analyses, which do not translate to emission factors. The first results from this airborne approach reinforce the need for continued and enhanced examination, both spatially and temporally extensive, in the arctic. We seek to publish the significant result we've observed on a landscape-scale to help motivate future studies that can provide the confirmation sought in Referee 1's comment.

In response to Referee 2, we agree that the spatial variability for N2O could be different -- as demonstrated by our measurements. However, that does not necessitate comparative ground-based measurements catered specifically to N2O. Comparison with another measurement technique would only be necessary if there are concerns about our primary method. No concerns were given. As stated in the manuscript, our comparison with a nearby tower demonstrates that our instrument is capable of measuring airborne fluxes of gases. We also calibrated our EC system both in a wind tunnel and during the campaign. From the perspective of making trace gas flux measurements then, the only difference between CH_4, H_2O, and N_2O is a different absorption feature on our spectra. Since we demonstrate that the template of our EC system works, all that is left is making sure the N_2O sensor works properly. We have done this, both in lab and in flight, and furthermore, performed an uncertainty analysis on the N2O flux data, which includes a comprehensive ogive plot. The referee seems satisfied by this as shown in Comment 1 above.

Separately, there is an issue regarding Referee 2's recommendation that we use chambers to confirm airborne measurements. For an airborne measurement to correspond to a fixed surface measurement their footprints must correspond. A chamber measures the same small patch (typical size for a chamber is ~1 m^2). By contrast, the minimum spatial coverage of an aircraft measurement, determined by the largest scale of atmospheric turbulence, is closer to 6 km^2. For the footprints from the two methods to correspond, the aircraft track must lie over a homogeneous surface of area 6 km^2 having the same character as that being sampled by the chamber (and the chamber's 1-square-meter footprint must accurately represent the 6-square-kilometer surface covered by the aircraft's measurements). For CH_4 or CO_2 such a surface can be identified by remote sensing. As discussed above, however, no comparable surface classification for N_2O is available to our or, presumably, to the referees' knowledge. An EC tower would sample a larger surface area than a chamber but would be subject to the same issues in establishing a common footprint with aircraft for N_2O.

Therefore, while we do agree that comparison with a tower for N2O specifically 'would be excellent' as Referee 1 states, we disagree that it's necessary as Referee 2 suggests. Having said that, we do state on Page 9/lines 20-21 that this is a stepping stone where future research would provide that ground comparison. Considering no one is currently doing this, we strongly believe publishing this information for the scientific community to see is an important step to motivate the community to look further into what's going on here. This is especially true since several chamber-based publications have already recently been published suggesting the community's assumptions about permafrost N2O emissions may need to be refined.

Finally, we agree with Referee 2 that we cover a smaller time scale than a longer-term ground-based measurement. But accordingly, we restrain our extrapolation to the month of August instead of the entire summer precisely because of our shorter time scale.

2.2 Change to Manuscript: None.

3. Referee 1 Comment

Page 2/line 1 Change the text to ". . .However, recent in situ measurement of permafrost soils in Russian tundra and northern Finland (Repo et al. 2009; Marushchak et al. 2011)"

3.1 Response to Referee: We agree with the suggestion and have changed the manuscript accordingly.

3.2 Change to Manuscript: The text was changed as Referee 1 suggested (Page 2/line 3).

4. Referee 1 Comment

Page 2/lines 10-16 The discussion on the flux data generated by chamber method could be modified to state that there are both disadvantages and benefits using chamber method for the gas fluxes. By the chambers e can catch efficiently the various functional surfaces, even very small. So, we can get knowhow on the soil and vegetation related factors affecting gas fluxes. To obtain landscape or regional fluxes by chambers for permafrost regions, accurate distribution of the functional surfaces is required. This can be done using e.g. satellite images (e.g. Treat et al. 2018. Global Change Biology, Doi: 10.1111/gcb14421).

4.1 Response to Referee: Thank you for pointing this out. We did not bring up the spatial limitation of the chamber method to suggest that method has no benefits. Rather, our goal was to point out a gap in the research that an alternative method could help fill in – our method specifically. We did point to several benefits of chamber studies on Page 2/line 10. Still, we appreciate that a reader could get the impression we might be offering airborne EC as a replacement method. That is not what we are doing. Airborne EC complements the chamber method and ground-based measurements in general. We will alter the text to better convey this sentiment.

As for the latter part of the comment, it's true that research teams perform quite spatially extensive extrapolations from chamber measurements to get a landscape-scale estimate of some trace gas emissions. Treat et al. 2018, the paper the referee cites, examines this method, and the potential pitfalls of using it, for CO_2 and CH_4. It does not for N_2O. This is probably because permafrost N_2O emissions have had significantly less research effort dedicated to them. Unlike the EC tower network that exists to varying extent for CO_2 and CH_4, there is not a single EC tower that provides consistent, long-term measurements of permafrost N_2O emissions. Chamber studies are also quite sparse. While research effort sufficient for discussion of this type of extrapolation may eventually be put forth for N_2O, such discussion feels more relevant for CO_2 and CH_4 at this time.

We mentioned that Treat et al. discuss potential pitfalls of this type of extrapolation in their paper. We'd like to touch on that again as a way to further emphasize how it might be premature to discuss these types of extrapolations for permafrost N_2O. One of Treat's main messages is that satellite images can cause severe underestimations if an insufficient resolution is chosen. They've found that you need a better spatial resolution for CH_4 than for CO_2 because CH_4 emissions are more spatially variable. Without proper resolution, the CH_4 emissions are severely underestimated (by up to 65%). N_2O is considered much more variable and spotty than CH_4. We can imagine the spatial resolution therefore needs to be even better to extrapolate N_2O emissions, but it's unknown what this resolution should be or whether traditional satellite maps could properly accomplish this (the land cover map we used has the recommended 30 m x 30 m resolution for CH_4). Our presented results involved no extrapolation and, therefore, no need to make assumptions about the characteristics of a particular land class as it relates to N_2O or to worry about potential issues with the resolution of satellite images used to make the extrapolations. Instead, we obtained a landscape-scale estimate from hours of airborne measurements spanning hundreds of square kilometers of the Arctic. There are certainly advantages of the chamber method over airborne EC, as we have now more explicitly stated. However, obtaining a landscape-scale estimate is not one of them.

4.2 Change to Manuscript: Page 2/lines 14-23 changed to "The past studies on permafrost N_2O emissions have provided insight into the mechanisms of the gas's production and subsequent release into the atmosphere. The studies have been either laboratory studies or ground-based chamber studies. In general, chamber studies have the advantage of observing the same site for relatively long time periods. Additional variables (e.g. pH, water saturation) can be monitored, too, which are crucial to understanding how that environment might influence the observed extent of N_2O emissions. However, each chamber covers around 1 m^2, and a feasible chamber study can only entail a limited number of sites. Consequently, past observations have covered extremely small areas – less than 50 m^2 (Repo et al. 2009; Marushchak et al. 2011; Yang et al. 2018). Therefore, the landscape scale of this phenomenon remains unknown, let alone the regional and continental scales. Landscapes deemed vulnerable to thaw-induced N_2O emissions, permafrost and thermokarst regions, cover about one fourth of the Arctic/sub-Arctic (Voigt et al. 2017). One of those vulnerable areas is the Alaskan North Slope, which is the focus of this study. To get a landscape-scale estimate of the magnitude of permafrost N_2O emissions during late summer, we measured N_2O fluxes over the North Slope in late August 2013 using the airborne eddy covariance (EC) technique."

We also modified the text in the Conclusion (Page 9/lines 14-16), to read: "Importantly, we corroborate these findings in a complementary way: by observing fluxes on a landscape scale rather than the much smaller-scale soil plots in chamber studies, which are intended more to understand temporal representativeness and mechanisms of N_2O production."

5. Referee 2 Comment

   Page 2, Line 10-16: The authors argued that chamber measurement or lab studies cover small spatial scales. However, the airborne measurements cover only short time periods. Perhaps a little more background on spatio-temporal variabilities in N2O fluxes from permafrost?

   5.1 Response to Referee: The reported average presented in our manuscript represents, in total, a little under 10 hours of airborne measurements across the North Slope semi-randomly sampled over the span of a week. To give an estimate for the entire month of August, we therefore extrapolate our data by an order of 100. By comparison, a typical chamber study covers an area around 10,000,000 times smaller than our spatial coverage. We agree that we are temporally limited, but the spatial limitation for chambers seems more severe. Regardless, our point was not to suggest airborne EC is better than chamber studies, merely that it's more appropriate for establishing a landscape-scale estimate. We have modified the text to better convey this.

   5.2 Change to Manuscript: Please see changes in Comment Response 4

6. Referee 2 Comment

   Page 2, Line: 17-20: Much of the detailed information on flight campaign could be put in M&M.

   6.1 Response to Referee: Most of that text has been moved to the first paragraph of the Methods section.

   6.2 Change to Manuscript: Except for the last sentence, Page 2, Lines 17-20 were all moved to the first paragraph of Methods section (Page 2, Line 35-37). The text was also slightly modified to better flow with the text following its new location.

7. Referee 2 Comment

   Page 5, Line 1, equation (3): I think that running flux method (RFM, Sayres et al. 2017) was used for N2O flux estimations in this manuscript. However, Sayres et al. (2017) suggested advantage of flux fragment method (FFM) against RFM in their airborne EC CH4 study. Also, they claimed that FFM can isolate flux contributions from individual surface land classes. Please explain it.

   7.1 Response to Referee: The running flux method was not used for N_2O flux estimations presented in Table 2 of this manuscript. Equation (3) is the general equation for airborne EC fluxes, not an equation specific to RFM. We don't use FFM either. We opted instead for the more robust approach of averaging over entire flights in order to focus on the overarching landscape and to present simpler, more statistically sound results.

   We do, however, use RFM to determine the values for the data in Figure 6. RFM is an application of equation (3), as described in Page 7/lines 9-10 of the ACPD manuscript (note we avoid using the term 'RFM' in an effort to minimize jargon in the manuscript). Regarding FFM versus RFM, Sayres et al. 2017 only suggests advantage of FFM over RFM under certain circumstances. They do not assert FFM has an overall advantage (some of the authors of that publication also author this manuscript). The goal of Figure 6 is to illustrate the spottiness of N_2O emissions, and RFM is better suited for that purpose.

7.2 Change to Manuscript: On Page 5/line 2, we changed the equation (3) description from 'standard equation' to 'general equation' to better clarify this is not an equation specific to RFM or FFM.

8. Referee 2 Comment

Page 7, Line 29 and Line 32: Could you give a more quantitative description of land classes (in % or area size) for Table 1?

8.1 Response to Referee: We have provided percent of observed footprints that fall into those land classes. In order to quantify based on land type, we more strictly followed the NSSI Landclass map; lakes and rivers have been combined to open water as they are according to NSSI.

8.2 Change to Manuscript: See imbedded table below.

Flight date DD.HH	Start time UTC - 10	End time UTC - 10	Temperature (°C)	Dominant land classes
25.18	17:43	19:49	5	Sedge (33%), Mesic sedge (19%), FWM (8%), Open Water (7%)
27.11	09:40	13:00	6	Open Water (31%), Sedge (26%), FWM (17%), Tussock Tundra (14%)
27.19	16:46	20:02	10	Sedge (44%), Open Water (16%) Mesic sedge (15%), FWM (15%)
28.10	08:39	11:39	11	Tussock tundra (46%), Open Water (26%), Sedge (19%), FWM (7%)
28.15	13:59	15:44	16	Sedge (47%), Mesic sedge (26%), FWM (8%), Open Water (7%)

9. Referee 2 Comment

Page 8: Line 27-30: Gene abundance does not directly refer to denitrification and N2O reduction rates. It still needs to be expressed so that N2O can be reduced. Better focus on the O2 inhibition effect on N2O reductase.

9.1 Response to Referee: Recognizing that gene abundance alone does not account for the fraction of the genes that are expressed in a population, the connection is at some level indicative of the population of the microbial community present. This argument is presented as one found in the literature that supports the possibility of N2O production instead of N2 production. Here's the quote from the microbiology review paper we cite for that assertion:

"In the Arctic permafrost metagenomes that have been analysed so far, most of the genes that are involved in the denitrification pathway have been detected, but the relative gene abundances for the last steps in the pathway were too low to lead to N2 production."

The review then goes on to posit that a possible consequence of this is the accumulation of N2O. Because this is not our argument, but an argument from a published review paper in the field of microbiology, we will keep the sentence as is.

9.2 Change to Manuscript: None.

10. Referee 2 Comment

Abstract: Some explanation of the high N2O fluxes needs to be implemented. Also, please indicate the site location in the abstract.

10.1 Response to Referee: We agree the site location should be in the abstract. We will add that along with the date. However, we feel that an explanation of the mechanism of the high N2O fluxes would be inappropriate since our results do not provide that insight, and explanations are largely based on insights provided by past chamber and laboratory studies. Having said that, we do provide more detail to our findings to clarify that our average represents observations with high variability.

10.2 Change to Manuscript: Page 1, Lines 15-17 has been altered as follows: "In late August 2013, we used the airborne eddy covariance technique to make *in situ* N_2O flux measurements over the North Slope of Alaska from a low-flying aircraft spanning a much larger area: around 310 km^2. We observed large variability of N_2O fluxes with many areas exhibiting negligible emissions."