

Author reply to Referee comments from **Anonymous Referee # 2** from 29 November 2018 (<https://doi.org/10.5194/essd-2018-98-RC2>, 2018 ) on:

## **A long-term (2002 to 2017) record of closed-path and open-path eddy covariance CO<sub>2</sub> net ecosystem exchange fluxes from the Siberian Arctic**

by David Holl et al.

### Reviewer comments (RC)

#### Author comments (AC)

#### Manuscript changes (MC)

Please note that figure numbers in this document refer to figures in this document and not to the numbering in the original draft unless denoted otherwise.

#### RC 1

Review ESSD-2018-98 Siberian Permafrost

A login barrier on Pangaea prevents me from downloading the actual .tsv files. As it turns out I have a valid Pangaea login but, once in, I still cannot access the data. THIS VIOLATES ESSD POLICIES AND PREVENT ME OR ANY OTHER USER FROM FULL EVALUATION OF THE DATA!!

This must be fixed immediately. All my comments below assume a quality effort on the part of the authors but until the data becomes fully and freely accessible, I must withhold approval of this manuscript.

The dataset is now accessible without limitations (<https://doi.pangaea.de/10.1594/PANGAEA.892751>). I addressed this issue with a short comment during the interactive discussion (SC1: 'Data access', David Holl, 14 Dec 2018).

#### RC 2

Page 3 line 15: this sentence should refer to eddy covariance “systems” because more than one instrument type was deployed on three different towers at two different locations.

I agree, “system” was changed to “systems”.

#### RC 3

Page 3 line 30: technically, “low evaporation rates” should lead to dryer than expected atmospheric humidities. Perhaps the authors refer here to soil (moisture) conditions or to a combination of low specific atmospheric water vapour contents with lower temperatures that lead to a relatively high relative humidity?

The point we wanted to make here is that even though water input by precipitation is not very high on an annual basis, water loss by evaporation is even smaller. We failed to mention that low evaporation is indeed connected to low ambient temperatures and low water vapour pressure deficits.

I changed the sentence to:

An arctic-continental climate with low mean annual temperatures prevails in the Lena River Delta. Although precipitation is low as well, the climate can be considered humid as evaporation rates are low due to low ambient temperatures and relative humidity is high.

#### RC 4

Page 4, soils: much higher resolution soil mapping documented here than in the northern circumpolar soil atlas (Jones et al. 2010) so I understand better details here. But Jones et al. soil atlas, at least for central Siberia, adopts the “Russian Soil Classification System” while this paragraph references US or FAO definitions. Why? Because permafrost carbon estimates (e.g. Hugelius et al. in ESSD 2013, not cited here but used extensively in Koven and Schuur, both of which these authors do cite) depend substantially and in fact influence soil type classifications (e.g. again see Hugelius) in permafrost regions, this paper should at least document consistency with other soil classification systems? I know this is not a description of Samoylov permafrost soils data set, but we should at least know consistency or valid reasons for inconsistencies between soil classifications systems used by flux vs soil carbon communities?

The “Site description” section collects data that has been reported from Samoylov in the past. To my knowledge, Jones et al. 2010 do in fact use the FAO WRB system for soil classification. Unfortunately, Pfeiffer and Grigoriev (2002) and Zubrzycki et al. (2013) used a different classification. Hugelius et al. 2014 (“Estimated stocks of circumpolar permafrost carbon with quantified uncertainty ranges and identified data gaps”, Biogeosciences) did use SOC data provided by Zubrzycki et al. (2013) and therefore appear to regard this dataset as consistent with the methods and resulting estimates from other authors.

RC 5

Page 4 line 20 “contributied”

I corrected this typo.

RC 6

Page 4 line 25 (and following): “wind speed” I think you actually mean wind velocity because, unlike speed, you need both magnitude and direction?

I agree. I replaced “wind speed” with “wind velocity” at the appropriate places.

RC 7

Page 4 line 28: Need to mention here that, for a period of two years, the tower location moved almost 1 km to the west-southwest?

I think this fact is already made clear by saying “three different tower structures” and referring to Figure 1 (*in the original draft*) where the different locations are illustrated. The impacts of tower relocation are discussed in more depth than in the original manuscript in a newly added “Discussion” section. See my reply to RC 14.

RC 8

Page 4 line 30: For one year sampling rate went to 10 Hz, and later for a period of roughly 9 months sampling went to 5 Hz. Opportunity to test sampling and influence on spectral properties of flux calculations?

I agree, we missed the opportunity to report the impact of changing sampling frequencies on the spectral properties of the raw data. I added a new Discussion section to the manuscript where this topic is addressed. See my reply to RC 14.

RC 9

Page 5 line 2: “... the data set contains year-round fluxes in some years ...”. But, from Figure 3, only 2016 had anything close to full annual coverage (e.g. roughly 10k valid 0.5-hour observations out of a maximum possible of 17.5k). No other year shows anything close to full four-season data coverage. If the authors contend that 2014 and 2010 also provide “year-round” flux data then they have a very low standard/expectation for what constitutes valid year-round performance which they should share with readers.

I agree, the cited statement is a bit vague. I replaced

“Although data coverage is biased towards the growing season, the data set contains year-round fluxes in some years (see Table 1).”

with

Although data coverage is biased towards the growing season, the dataset contains considerably more shoulder season and winter fluxes in its second half from 2010 to 2017(see Table 1 (*in original draft*)). The also increasing availability of year-round ancillary meteorological data resulted in gap filled flux time series covering each half hour in the two years 2010 and 2016 (see Figure 1).

To illustrate data coverage better than only in Table 1, I added a new figure containing corrected measured and gap-filled fluxes.

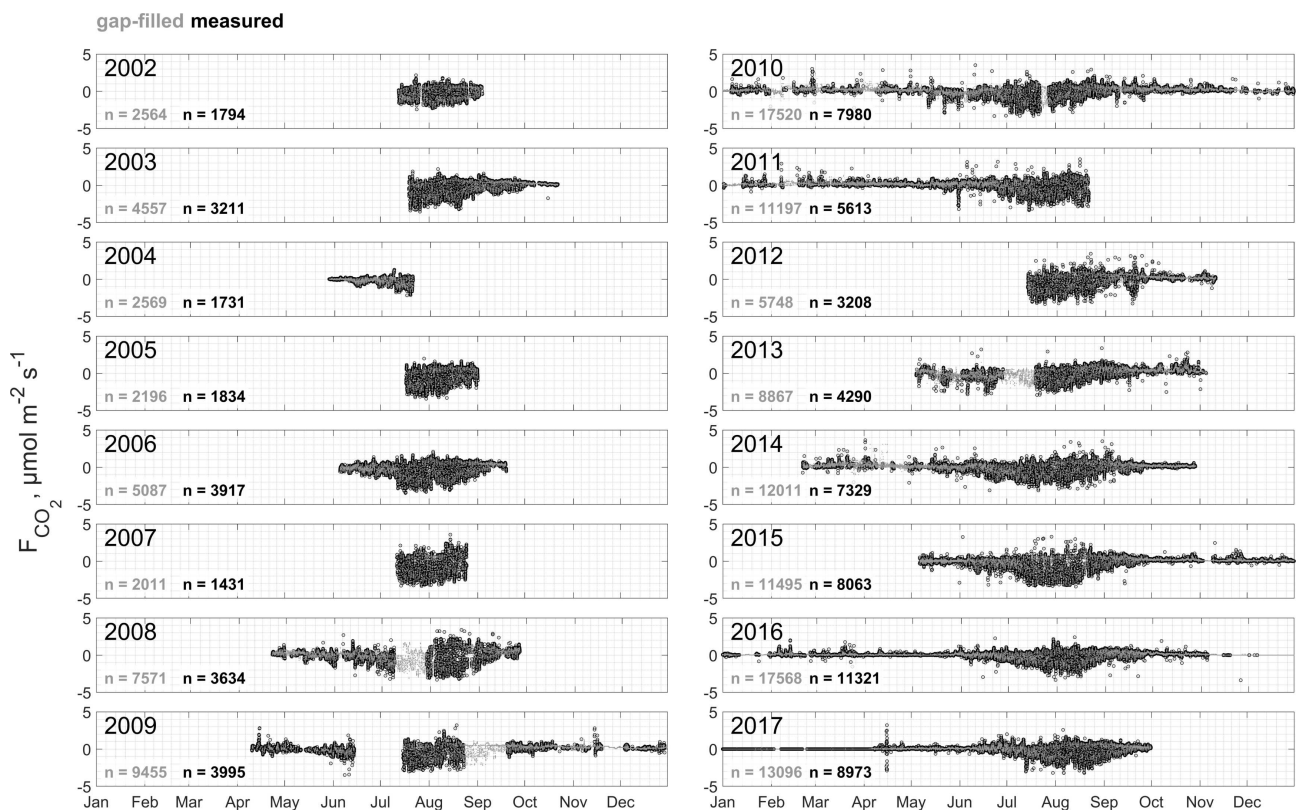


Fig. 1 Multiannual carbon dioxide flux time series compiled from fluxes measured with closed-path and open-path sensors on Samoylov Island's river terrace. Fluxes of quality class 2 are not shown. Self-heating errors in the OP dataset have been corrected for. Additionally, the result from gap filling this time series with the MDS method is shown. The given numbers of values for the gap-filled time series include measured fluxes.

RC 10

Page 6 line 4: "These Webb-Pearman-Leuning (WPL, Webb et al., 1980) terms". 'These' in this case refers to terms necessary to calculate density for OP measurements but sentence as written allows confusion. Better to specify temperature, pressure, water vapour content, etc.

RC 11

Page 6 line 7: "undisturbed heat fluxes". Reviewer may know what you mean by 'undisturbed' and why you need those, but you have not explained clearly to readers.

RC 12

Page 6 line 7: "WPL terms" By this point authors should have told readers exactly what they mean when they say 'WPL terms'. Jargon creeps in here, as well as assumption that every reader already knows the intricacies of eddy correlation measurements. Not true! Please rewrite the initial sentences of this paragraph in a clearer form and format.

Referring to comments RC 10 to RC 12: I agree, the section on the corrections for air density fluctuations are somewhat confusing and too brief. I therefore rewrote the section from page 5, line 14 (starting with "CP analyzers have the...") until page 6, line 8 (before "Major drawbacks...") and moved it to a new paragraph in an effort to include a more understandable and thorough description of the WPL approach.

Infrared gas analyzers typically measure gas densities and report the number of molecules per volume of air. To be able to refer the mass of a gas to the mass of air, gas densities are transformed to mixing ratios using air density. However, as the optical path of an OP gas analyzer is exposed to the varying temperature, pressure and humidity conditions of the atmosphere, air density in the measurement cell fluctuates mainly due to thermal expansion/contraction and water dilution/concentration. This effect, that leads to faulty concentration readings of OP instruments and thereby to incorrect flux estimates, has first been described by Webb et al. (1980). The authors proposed two flux correction terms to compensate for these density fluctuation effects that are referred to as Webb-Pearman-Leuning (WPL) terms and have since been verified experimentally and theoretically and are routinely applied in OP EC studies. Especially at times of low gas fluxes, WPL terms can become orders of magnitude larger than raw gas fluxes (Munger et al., 2012). CP analyzers have the advantage

of controlled temperature and pressure conditions in the measurement cell, allowing for the sample-wise calculation of mixing ratios rather than molar densities (Ibrom et al., 2007b) and thereby avoiding the need to apply air density fluctuation correction terms after raw flux calculation.

RC 13

Why does a reader find Figs 1 and 2 introduced at appropriate points within the text but all Tables and Figs 3 and 4 appearing at the end of the manuscript after text and references. Need to fix this now and check it again during proofreading.

Figures 3 and 4 (numbering of discussion paper) were removed. See my reply to RC 15 below.

The table placement after the references is part of the ESSD guidelines for manuscript preparation.

On the website ([https://www.earth-system-science-data.net/for\\_authors/manuscript\\_preparation.html](https://www.earth-system-science-data.net/for_authors/manuscript_preparation.html)) it says: "Any tables should appear on separate sheets after the references and should be numbered sequentially with Arabic numerals."

I suspect that typesetting of the final version by the publisher will result in a placement of tables closer to the references to it in the text.

RC 14

The authors' descriptions of data processing, quality filtering, self-heating corrections, temporal gap filling etc. seem appropriate and well-described. However, we find no assessment of the two-year period of tower relocation. Mentioned in the introduction and again in the conclusions, but completely absent from the data processing and data quality descriptions. If that relocation does not matter, e.g. had no effect on time series or data quality, then readers must question the footprint analysis, as mentioned in the Conclusion! Based on lack of information here, this statement from the conclusion "... ensuring that EC source area deviations are quantifiable by a potential user" seems unsupported for at least two years? One also wonders about the earlier documentation that sampling frequencies changed (e.g. 20 Hz to 10 Hz to briefly 5 Hz). Did those changes also have no effect (or no utility) on data processing. The authors seem to expect users to ignore these possibly substantial location sampling issues but, having mentioned both changes (good) they then fail to report corrections or consequences (bad).

I added a new "Discussion" section to the manuscript addressing these issues.

Although we did our best to ensure the consistency and appropriateness of the data processing workflow for the presented NEE time series, due to technical and logistical constraints during 16 years of field work, disparities in the experimental setup exist which may challenge its integrity. The EC tower was relocated twice, the measurement height was changed three times (see Figure 1 and Table 1 (*in original draft*)). These changes of tower location and measurement height affected the source area and hence the surface types sampled during flux measurements. Most notably, between July 2007 and June 2009, the EC tower was placed about 650 m south-west of its original position at the center of Samoylov Island, in an area with an increased coverage of the surface class *wet tundra*. This is revealed by the footprint analysis (Figure 2). While the EC footprint is dominated by the surface class *dry tundra* throughout the time series, during subperiods 2007, 2008 and 2009 the contributions of *wet tundra* to the measured flux are significantly higher.

To check the effect of the shifts in tower location and measurement height on cumulative CO<sub>2</sub>-C fluxes, we calculated flux sums for a period when flux time series without gaps were available in most years. The overlapping period covers days of year 200 to 234, i.e. part of the growing season in all years except for 2004 (see Figure 3). Interannual variability of cumulative C fluxes in years with constant tower location (and measurement height) appears to be large and driven by a more complex set of variables than shifts in surface class contributions only. Flux sums from the periods when EC tower relocation led to a significant shift in EC footprint composition are well within the range of the distribution of cumulated fluxes from years with a more homogeneous EC fetch area. We therefore assume that, at least with respect to budget calculations, the presented long-term time series is not disrupted and can be regarded as representative for a polygonal tundra site dominated by dry tundra. For a more in depth analysis of flux dynamics, footprint information should and can be considered by users of the data set. Recently, a comparison between surface class level NEE models based on chamber measurements with EC fluxes, using the half-hourly footprint information provided in this data set for scaling, yielded good agreement between the results obtained with both methods Eckhardt et al. (2018). We regard the availability of half-hourly footprint information in the presented NEE data set an attribute that sets it apart from other studies and holds chances for comprehensive analyses.

Apart from the changes in anemometer height, other deviations of the general instrument setup occurred due to limitations in data storage during two winter periods when the acquisition frequency was reduced to 5 Hz

and 10 Hz respectively. Rinne et al. (2008) demonstrated in a field experiment that fluxes calculated from raw data recorded at frequencies below 20 Hz compare well with fluxes derived from high frequency raw data. Differences arise as an increase of random noise and not as a systematic bias. High frequency noise removal before ensemble spectra estimation in EddyPro is effective in limiting the effect of increased noise on the quality of transfer function estimation in the process of spectral correction. Overall spectral correction in EddyPro is expressed as a spectral correction factor (SCF) which comprises the effect of all applied compensations for high and low frequency loss. Raw fluxes are multiplied with the respective SCFs during processing. We compared the SCF distributions of the two above mentioned winter periods with statistics of the remaining parts of the time series when data was recorded at 20 Hz. SCF deviations between the different acquisition frequencies are minor (see Figure 4) implying that systematic differences between fluxes calculated from raw data of different temporal resolutions are in fact small, random uncertainties increase, however.

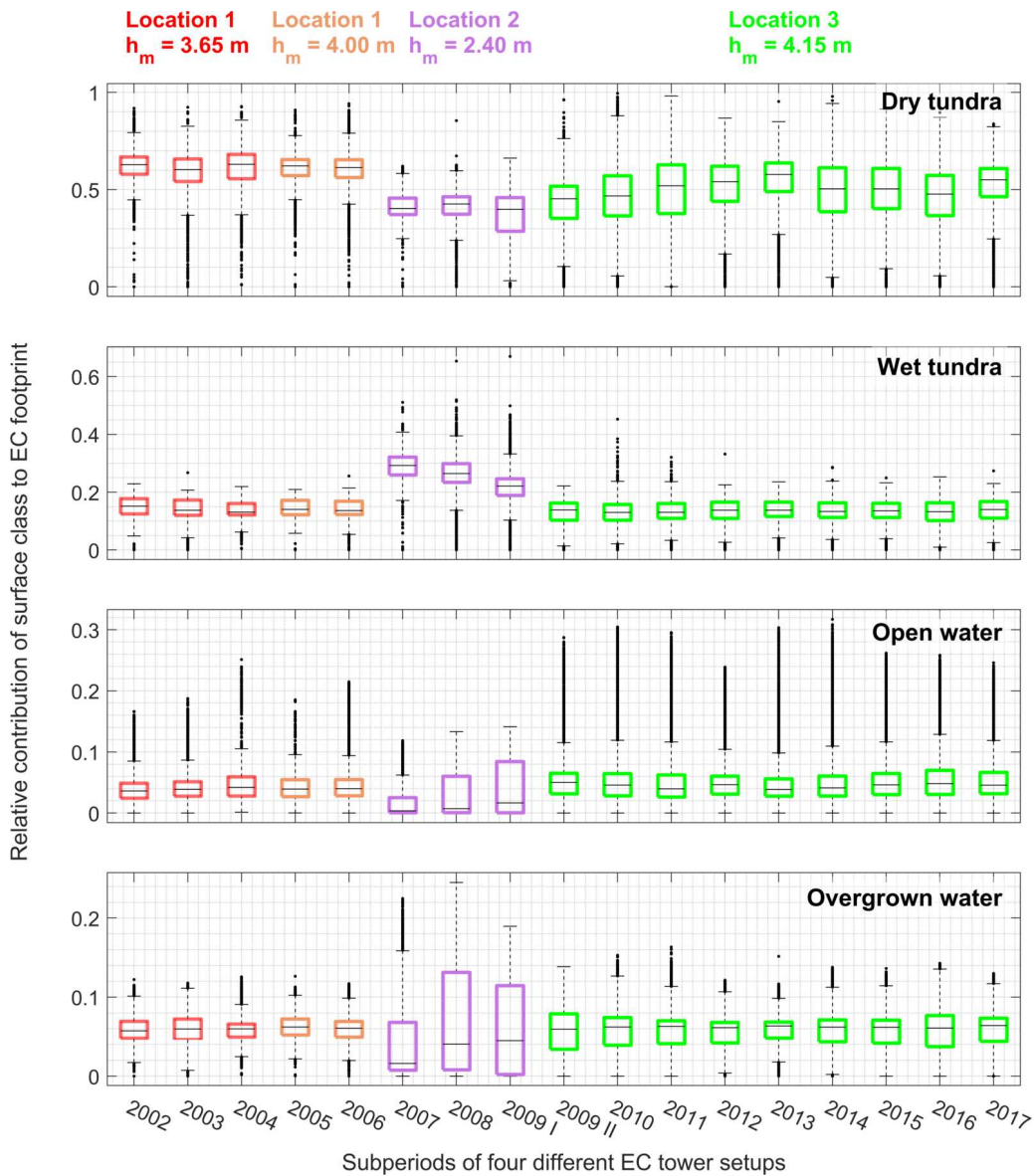


Fig. 2 Mean surface class composition of the eddy covariance footprint during 17 subperiods of four different tower setups at three locations on Samoylov Island.

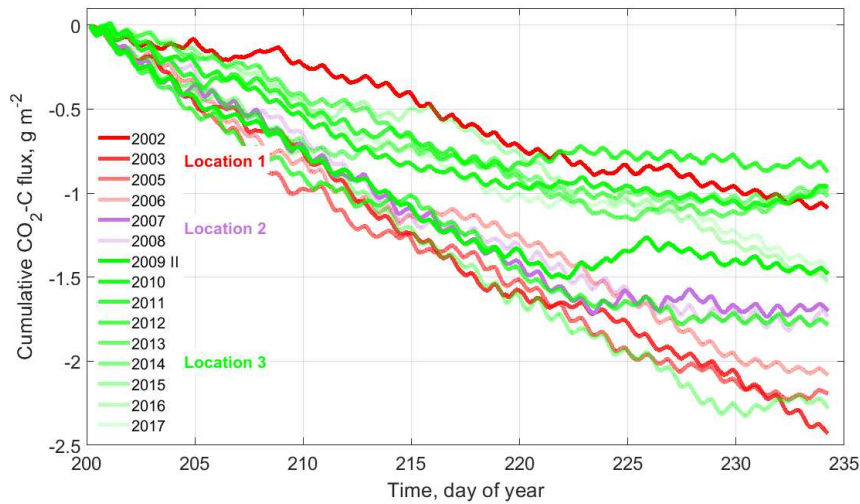


Fig. 3 Comparison of cumulative CO<sub>2</sub> flux sums of different years during the same day of year range.

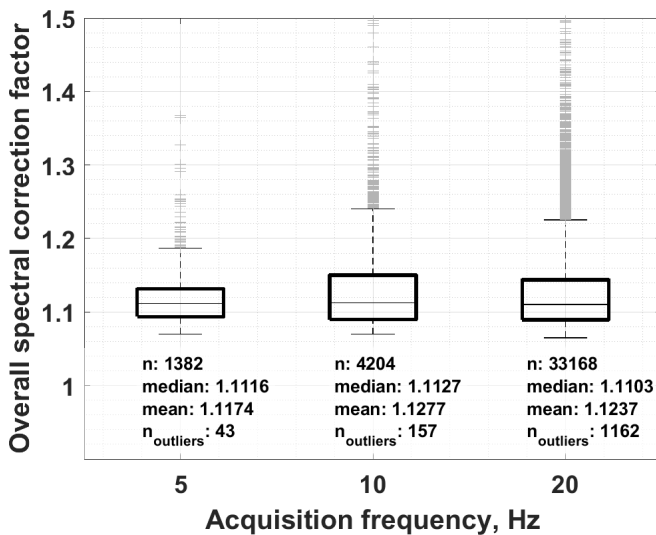


Fig. 4 Spectral correction factor statistics for periods with different acquisition frequencies.

RC 15

Fig. 4 not referenced in text? In the (barely viewable) version provided for review, Figure 3 and Figure 4 look identical, even to having identical numbers (n) of samples. The legends for the two figures differ slightly, but the figures themselves differ not at all. Wrong figure in the wrong place? Or, because we find no mention of Figure 4 in the text, one figure wrongly duplicated? Serious error, needs attention.

There is indeed a difference between Figures 3 and 4 (note that these figure numbers refer to the original draft, not the figures we see above, on this page). It is, however, fairly subtle. The difference between the plots only refers to OP fluxes, Figure 3 shows fluxes that are corrected for the self-heating effect, Figure 4 shows uncorrected fluxes, hence the number of samples are identical. My intent was for a reader to be able to flip back and forth between pages 19 and 20 in the pdf file viewed in full-screen on a computer to get a visual impression of the impact of the self-heating correction on OP fluxes. This intention clearly did not pan out, I will therefore remove Figures 3 and 4 and replace them with a new Figure (Figure 1 in this document) showing corrected measurement data and gap filled fluxes for a better overview of the final time series.

RC 16

Analysis and use of this particular CO<sub>2</sub> data will require simultaneous access to observation time series from boreholes, river gauges, water sampling, long-term meteorology, e.g. Boike et al. 2018. Very important to connect these two data sets. If Boike et al. emerges successfully from ESSD, then we need a close explicit link described here. Until Boike et al. appear in ESSD or elsewhere, release of this data seems premature at best. If other sources of necessary soil, radiation, micrometeorological, etc. data exist, please reference those as well or instead? Tiksi?

I agree, the fact that long-term meteorological and soil records are available makes our NEE time series much more valuable. Boike et al. (2018) archived their data in two long-term repositories (Pangaea and Zenodo). Links are added in the following paragraph that I added to the end of the conclusion.

Furthermore, analysis of this NEE time series is not limited to the gas flux data only. An extensive data stream of meteorological and soil variables between 2002 and 2017 has recently been published by Boike et al. (2018). The authors made their records publicly accessible on the two long-term repositories Pangaea (<https://doi.pangaea.de/10.1594/PANGAEA.891142>) and Zenodo (<https://zenodo.org/record/2223709>). The fact of simultaneously available ancillary ecosystem variables enables a potential user to put the gas flux dynamics reported in this publication into context with the variability of other ecosystem properties and potential flux drivers. We regard this type of analysis as vital to understand inter-annual variability of CO<sub>2</sub> fluxes on Samoylov Island and are working on it ourselves (Kutzbach, unpublished).

RC 17

Note reference to methane measurements. Do these authors consider that they have now have sufficient information, biogeochemical and ecological, to construct an annual carbon budget? If so, they should at least assure readers of forthcoming analyses. If not, why not? Lack of winterseason measurements? Can only construct a valid annual budget for, e.g., 2016?

We regard this dataset publication as a starting point for analysis of flux dynamics done by us and other members of the scientific community. Yes, we think the literature record of information necessary to conduct carbon budget calculations for Samoylov Island has certainly grown over the last years. Especially the manuscript at hand contributes an important part of the information necessary to tackle the task of budget calculations successfully. As mentioned before, we are aiming at publishing those types of results in the future (Kutzbach, unpublished). In this paper, however, we wanted to focus on the methods we used to process the data rather than its interpretation. To our understanding, this proceeding is in line with the "Aims and scope" of ESSD, which is one reason why we selected this journal.

*"Articles in the data section may pertain to the planning, instrumentation, and execution of experiments or collection of data. Any interpretation of data is outside the scope of regular articles. Articles on methods describe nontrivial statistical and other methods employed (e.g. to filter, normalize, or convert raw data to primary published data) as well as nontrivial instrumentation or operational methods. Any comparison to other methods is beyond the scope of regular articles."* ([https://www.earth-system-science-data.net/about/aims\\_and\\_scope.html](https://www.earth-system-science-data.net/about/aims_and_scope.html))

RC 18

What makes this time series interesting? Why not simply download from FLUXNET2015?

Presumably this data serves as important piece of the tundra carbon analysis presented by Zono et al.? For this reader, the authors have not made an adequate case about potential importance and utility. Readers might consider it potentially very important but this statement, again from the conclusion - "a valuable addition to the already existing data base of CO<sub>2</sub> net ecosystem exchange observations from the Arctic" - seems weak and vague. Do the authors claim to have produced a unique high-quality data set or just another contribution to FLUXNET. If the former, then ESSD seems an appropriate venue. If the latter, why bother? Publish the entire FLUXNET data set instead? Again, this reader favours the former but the authors have not made a strong case.

We are currently talking with FLUXNET staff about how to submit our updated time series. Some of the information we provide is beyond what one could find in a typical FLUXNET release. At this time, I am not sure if our parallel data stream of open and closed-path sensors from a single site conforms to the FLUXNET data format. For the same reason, the EC footprint data will most likely stay exclusive to the Pangaea release of this data set. While shifts in EC footprint composition are important to understand flux dynamics they are not routinely included in FLUXNET data sets. Besides the latter fact that emphasizes the uniqueness of the presented data set, I think a publication in ESSD is further justified by the revised analysis of the overall time

series consistency with respect to instrumentation and location changes as well as by the thorough description of data processing/quality filtering. Furthermore, a detailed description of site characteristics including the relevant literature put the data set into context beyond of what is possible within a FLUXNET release.

#### New References

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