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Interactive comment on "A long-term (2002 to 2017) record of closed-path and open-path eddy covariance CO_2 net ecosystem exchange fluxes from the Siberian Arctic" *by* David Holl et al.

Anonymous Referee #1

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Review on the manuscript 'A long-term (2002 to 2017) record of closed-path and open-path eddy covariance CO2 net ecosystem exchange fluxes from the Siberian Arctic', submitted for publication to ESSD by David Holl et al.

This manuscript describes a long-term (16 years) record of eddy-covariance measurements of CO2 exchange fluxes between a Northern Siberian tundra ecosystem and the atmosphere. Accompanying a data publication in PANGAEA, the text provides a comprehensive background on site characteristics, data processing steps and quality assurance measures. A specific focus is placed on the use of closed-path versus open-path gas analyzers, and how related systematic errors were corrected to merge

both time series into a single dataset.

Overall, the manuscript is well structured and well written, and includes the major elements that a reader will need to understand the main characteristics of the flux dataset published in PANGAEA. Due to its unique position in Northern Siberia, the Samoylov Island observation site is of high importance for Arctic climate change research. Therefore, this 16-year time series of eddy-covariance fluxes, derived with uniform data processing and quality assessment protocols, is clearly relevant for the Arctic research community.

While I do not have any major concerns with the manuscript in its current form, I strongly recommend a number of 'medium' changes that, while probably straightforward to address, should further strengthen the text:

1.) Inclusion of a 'scientific overview' In the 'Site description' Section, the first 5 paragraphs give a comprehensive overview on the site conditions, while the last paragraph is clearly detached from this material, and in its present form does not belong there. Still, I believe it will be of use to the reader to demonstrate what has been found so far based on the flux time series presented in this manuscript. My recommendation is to move this paragraph to a new chapter 4, i.e. between methods and data availability, and extend it to a length of 3-4 paragraphs in total. This would give ample room to summarize the main findings based on Samoylov eddy-covariance (and other) data so far, therefore highlighting the value of the dataset presented herein, and the role of the site in general for Arctic climate change research.

2.) Ensure that tower locations do not disrupt continuous time series The combination of text in Section 3.1, Figure 1 and Table 1 provides a good overview on the different site setups used to form this 16-year data record. However, the material also raises the question how the shifts in tower position and sensor configuration, including sensor height, may have influenced the signal captured by the EC system, and therefore maybe biased the long-term time series. I therefore recommend moving Section 3.6

upward as a new Section 3.2, and extending the discussion of the footprint issue. You can use parts of the conclusions section for this, but more details need to be provided how the shifts in landscape element fraction in the footprints may have compromised the continuity of the flux observations. See also my comment on Section 3.6 in the 'line comments' below.

3.) Flux uncertainty description, and discussion A clear definition of data uncertainty is mandatory for publications in this journal. In Section 3.2, you briefly mention that you used the standard EddyPro feature to estimate random flux uncertainties – which is a good start, but certainly deserves more attention. So please work out in a separate paragraph what these random uncertainties consist of, and how exactly those were addressed in EddyPro. Moreover, there are also potential sources of systematic uncertainties in eddy covariance flux measurements, e.g. data-processing errors, or instrument calibration issues. These should ideally be covered directly in your uncertainty assessment of the flux data. Since you obviously decided to ignore them here, you should at least provide a convincing rationale why this simplification is justified.

I strongly recommend to consider these comments, as well as the detailed line comments below, to further strengthen this manuscript. Overall, I recommend this text for publication in ESSD after minor revisions.

Line comments: p.1, abstract & introduction: Within these sections, I'm missing data-driven insights. Having a 16-year data record at hand, I would first think about analyzing the data directly to determine long-term trends in surface-atmosphere exchange processes. Next, I would aim at generating process insights, e.g. what causes interannual and inter-seasonal variability in flux rates, Only then I would start thinking about the time series being a useful resource for calibrating and validating process models. I think these data-driven topics deserve additional attention in both sections.

p.1, l.6: FLUXNET is not restricted to CO_2 fluxes

p.2, l.7: excessive use of references for a single statement

p.2, l.16: not sure what inversion model have to do with the scope of this paper. They are trained on mixing ratio observations, not fluxes.

p.2, l.30f: this section could use a map to show location of the delta, and the island itself

p.3, l.17: there is no high-centered polygon on the entire island ..??

p.3, l.29ff: climatology information given here is certainly useful, but only based on a $\sim$20 year record from the site itself. It may be helpful to compare to longer-term climate records from the region (e.g., for Tiksi there is data starting in the 1930s).

p.4, l.1f: is there any record of snow depth, and its variability?

p.5, l.6ff: you may add the power consumption as another important difference between CP and OP systems.

p.6, l.4ff: even though you spend a few sentences to describe the WPL-approach, you fail to mention that this is about accounting for the influence of density fluctuations

p.7, Section 3.3: It's a bit odd that you start describing some elements of quality flagging already in Section 3.2, and continue with this material here, in the main quality section. This should be cleaned up. Also, you fail to reference Table 3 in the text. Moreover, you should improve the structure of this Section. You begin with a too short general overview on additional quality filters, and how they are used in the overall QC flagging scheme. You then close the section with very similar statements. This should be merged to a single introductory paragraph that clearly states that you applied 6 more quality checks, and if any of them indicated problems, the quality flag was set to 2.

p.8, l.14: The choice of 450ppm as the upper concentration limit seems rather narrow. Can you please justify?

p.9, Fig.2: Figure 2 isn't really informative, since it's hard to distinguish between corrected and uncorrected time series in such a cloud of values. Please think about a different format (box plots?), or just leave out the plots, and show the regression statistics instead in a table.

p.10, Section 3.5: I suppose Figs. 3 & 4 should belong to this section. They are not referred to in the text. Moreover, it's not necessary to show Fig.4, since given the minor absolute shifts in fluxes after Burba correction in this case, the differences between figures are not discernible. As an alternative for Fig.4, it may be interesting to show the gap-filled time series, maybe even in cumulative form?

p.11, Section 3.6: while the method applied to calculate footprints is sufficiently detailed, it is not fully clear how footprint results were combined with the land cover map. What's completely missing here is a reference to the findings, a.k.a. a bottom line. As already mentioned in the 'medium comments' above, this is an important piece of information, since (as shown in Table 1) multiple positions with multiple sensor heights were used over the 16 year data record. The authors clearly need to point out that this mixture of setups is still suitable to form a coherent, long-term time series of flux exchange for this site. It's not sufficient to just briefly mention these results in the conclusions. In particular, the results in Table 5 emphasize that the southernmost tower position, used within the years 2007-2009, featured a quite different composition of landscape elements than the northern site position. The authors need to make an effort to convince the readers that these differences did not result in a significant deviation of flux patterns, and therefore would bias the long-term trends.

p.11, Section 4: It's good to list the parameters given in the PANGAEA dataset in a separate table. However, since this dataset is obviously restricted to CO2 fluxes and their QC parameters, it would be good to also list the source for ancillary meteorological information, if available, since those will be necessary to put the flux time series into context.

p.11, Section 5: The major part of this section should be moved upwards, into a re-

vised version of the 'footprint modeling' Section. For the conclusions itself, it should be sufficient to state the value of the long-term record, its representativeness for the polygonal tundra ecosystem, and a comprehensive overview on what has been done to assure the quality of the material.

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