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.

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.

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?

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?

Page 4 line 20 "contributied"

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

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?

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?

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.

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.

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.

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.

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.

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 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 unsupportable 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).

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.

Analysis and use of this particular CO2 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, micrometeorlogical, etc. data exist, please reference those as well or instead? Tiksi?

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 winter-season measurements? Can only construct a valid annual budget for, e.g., 2016?

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 CO2 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.