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Discussions

Interactive comment on "Impacts of a decadal drainage disturbance on surface–atmosphere fluxes of carbon dioxide in a permafrost ecosystem" *by* F. Kittler et al.

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We thank Anonymous Referee #2 for the constructive feedback and the helpful comments/suggestions that helped improving the manuscript. In the sections below, comments from the referee are followed by our responses.

This paper is interesting and relevant to the communities. There are only few dataset from these continuous permafrost systems, so these results are extremely valuable. And there are even fewer that can span a long temporal range and compare the short term to the long term impact of drainage on the fluxes. However, while this idea is definitively relevant, the sparse nature of the data (2002-2004, and 2013-2015) is a bit too short to be able to really say something about a decadal change, especially

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considering the large interannual variability in the fluxes of these systems. I suggest the authors to be careful about overstepping the boundary of what they can say with their data. On the other hand the vegetation change is definitively something that can be safely highlighted, and potentially better quantified using remote sensing data? The signature of shrubs is very different than cotton grasses, so high resolution images should allow quantifying the vegetation change due to drainage.

The authors agree with the reviewer that observational datasets of surface-atmosphere exchange processes are often subject to pronounced interannual variability, and that long-term continuous observations are necessary to allow the identification of statistically significant trends. Accordingly, our two observation periods, i.e. the 'historic' data from 2002-05, and the 'recent' data from 2013-15, are too short to claim that each of them fully represents the average conditions within each of these timeframes; however, given the fact that we observed characteristic patterns (e.g. the maximum CO2 uptake rate in summer) where conditions could clearly be separated between historic and recent periods, resp., we believe that even this limited database can provide solid evidence that the ecosystem has undergone systematic shifts over the past ∼12 years. We highlighted the issue of data representativeness, and uncertainty induced by interannual variability, by adding new text to the discussion section of this manuscript. Regarding the analysis vegetation change over time based on remote sensing data, long-term trends can certainly be worked out, but an in-depth analysis is complicated by the fine-scale variability of ecosystem characteristics, and year-to-year variability in phenology timing. A quantitative analysis that goes beyond the description of trends in phenology therefore is beyond the scope of the presented manuscript, and is planned to be covered in a follow-up study that has a stronger focus on remote sensing methods.

Specific comments

Page 1 lines 18-19: not clear, this statement seems in contradiction with the previous one saying that "CO2 uptake to be systematically reduced within the drained area", rephrase highlighting that there was a recover? The sentence was restructured to avoid misunderstandings.

Page 1 line 24- : this is an interesting idea, unfortunately there are not enough data to really do a trend analysis, three years (2002-2004., which looking at Fig. 2 is really two summers and a half) are compared to another three (2013-2015, again as shown in Fig. 2 two summers and a half), and given the large interannual variability in fluxes of these systems, these a little too few to say something about a decadal change. MODIC is mentioned here, but it is not indicated how it was used to support this argument.

As already outlined above, we agree with the reviewer that the analysis would be stronger with higher data coverage. Since most of our analyses that target long-term shifts in flux patterns are based on qualitative evaluation of trends, and an ANOVA was applied to the peak NEE uptake only, we changed the text accordingly to avoid misunderstandings. The MODIS-based classification was applied to remove the influence of year-to-year shifts in phenology, e.g. linked to the timing of snow meld, and thus facilitate a comparison between data years. Our results demonstrate that this normalization removes a large part of the interannual variability in cumulative flux budgets, and makes possible the separation of data years into two distinct groups, the recent (2013–2015) and the historic (2002–2004) datasets.

Page 4 line 105: is this disturbed site the same as the site operated in 2002-2005? During the 'historic' observation period (2002-05), one single tower was used to monitor fluxes for control (2002-04) and drained (2005) conditions. The exact same tower (which was still in place ∼10 years later) was chosen to install the new eddy covariance instrumentation for the drainage (disturbed) tower. This fact is now clearly indicated by a new sub-sentence in Section 2.5, which describes the 'historic' experiment setup.

Page 6 line 187-190: what is this definition based on? Any reference or underlying reason to support this choice? Also, it is not clear how exactly the other periods were defined, it is only mentioned what was the start of the pre-season and the end of season, what about the other periods? Please describe in more careful details how the beginning and end of each period were defined. Their length also seem to be different depending on the year. It is relevant to understand how these dates changed among these three years and why.

The definition of our so-called 'key dates' is based on the analysis of temporal trends in MODIS time series of NDVI. The detailed method will be presented in a companion manuscript that is currently in preparation. In the revised version of this manuscript, we added information on how key dates were derived.

Page 7 lines 205-206: is this 19 gC from the drained or the control site? It should be mention the loss in both of them. Also, how much did the loss vary among these three years? Include st. deviations to the estimate for each site. Also, in 2014 the loss from the drainage site is very similar to the reference site, any reason for this?

Measurements in 2013 started mid-July, therefore only two years (2014-2015) can be analyzed for this sub-season. Adding individual standard deviations by treatment might not be useful since only two time series can be used for this analysis. Accordingly we decided to merge pre-season data across treatments (drained and references) and years (2014-2015), resulting in an average release of 19 gC m-2 during this sub-season. This was clarified in the text. Differences between drained and reference site are lower in 2014 due to smaller differences in soil temperatures.

Figure 4: the colors seem a bit off, a standardization would help to compare these pictures, maybe using the grey element (a boat?) in the right of the picture? This grey should be used to balance the colors. The colors were corrected as suggested.

Page 8 line 231:"both" "both" repetition remove one. It was corrected as the reviewer suggested.

Page 14 line 425: are really the gaps evenly distributed? I always saw much higher data coverage during summer and less early and later in the season. There certainly is year-to-year variability, and also differences in data gaps exist between towers, but the data coverage between pre-, early-, late- and post-season did not differ systematically.

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