

## ***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.***

### **Anonymous Referee #2**

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Review of “Impacts of a decadal drainage disturbance on surface–atmosphere fluxes of carbon dioxide in a permafrost ecosystem” 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 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 defini-

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tively 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. 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? 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. MODIS is mentioned here, but it is not indicated how it was used to support this argument. . . Page 4 line 105: is this disturbed site the same as the site operated in 2002-2005? 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. 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? 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. Page 8 line 231:”both” “both” repetition. . .remove one 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. . .

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