

Interactive comment on “Drainage reduces CO₂ uptake and increases CO₂ efflux by a Siberian floodplain due to shifts in vegetation community and soil thermal characteristics” by M. J. Kwon et al.

Anonymous Referee #2

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The topic of the submission is undoubtedly of interest to the readership of Biogeosciences Discussions as it presents some interesting data arising from an experimental manipulation and its impacts upon ecosystem functioning plus gas exchange in a floodplain. Of particular note is the attention to phenomena during the frozen season as well as the thaw season. However, my greatest criticism of the overall project is why investigate CO₂ exchange in such temporal and spatial detail and yet completely ignore methane, and for that matter nitrous oxide exchange and changes both spatially and temporally. It is surely the balance of changes between these contrasting greenhouse gases, of differing radiative forcing strength and atmospheric concentrations that

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is key here? I miss this vital context entirely within the paper as currently submitted. The abstract is of a suitable length and is reasonably informative, but I do question the concluding remark. It is not sufficiently explanative in relation to the findings of the experiment. The remainder of the text is rather lengthy and could be substantially consolidated without losing any impact (in fact making it much more impactful). The number of figures is verging on excessive. They could be perhaps consolidated and some relegated to appendix/supplementary materials, providing the key focus the manuscript currently lacks? Page 2 line 25 onwards (and subsequent incidences) -the use and comparison of cumulative figures for 20 days in year 1 and 66 days in year 2 does not seem to me to be sufficiently clear to the reader. The premise and importance of the study are generally both well explained within the introduction section of the manuscript. Line 114: ‘Reliable Prognosis’? What is this – it requires more careful explanation. Line 118: what is the magnitude of such fluctuations? Line 120: the depth of the drainage ditch was? Line 129 ‘affected’ not ‘effected’. How do you know that the drainage ditch had no effect at 600 m away – how did you ascertain this? Line 135: It is not clear to the reader what the rationale was for the 3 weeks sampling in 2013 and then 10 weeks in 2014. This should be made clear. Line 136: you introduce the term transects but have not done so before m- this is very confusing and should be addressed fully. Likewise, the labelling of transects is poorly defined. Line 141: The PVC collar was installed permanently, but how so and when, and to what depth was the soil isolated? For how long was the collar installed prior to sampling beginning? i.e. how long was there for recovery of the vegetation? Did cutting-in of the collar lead, as in many cases reported elsewhere, and in my own considerable experience, lead to vegetation damage/death in any case? Line 154: What evidence do you have that using ice packs effectively worked to keep the temperature constant as you claim? Line 158: the units quoted need attention. Line 165 onwards: Your phrasing is not sufficiently clear here regarding the ‘conflict’ between choosing core sites based on WTD category Line 170: define and quantify ‘nearby’ accurately please. Line 174: It is not clear how the data from 2003 form a reference! Lines 175-181: I find the explanation

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here insufficiently clear. You need to provide the reader with a much clearer explanation of what you did and why! Lines 188-189: Aerobic incubation of soils – insufficient detail is provided here for this to be repeated. Lines 206-207: There is insufficient detail regarding why you chose August as your point of reference. Line 223: You do not describe the term -1. GPP. This should be corrected. Line 269-70: how exactly did you add this error range in each case? Section 3.3.1. I find this, as currently written, to be overly complicated in structure and terminology. The authors need to simplify their scheme substantially and really pull-out the key take-home points from the data. A key issue for me in this respect is that you are comparing data with different time periods of coverage between years. Surely a direct comparison between same time periods each year would be more helpful to the reader? In addition, your choice of use of cumulative data seems at odds with your choice of units. This requires correction. Line 396 onwards: low variability of what exactly? In the discussion section, I find the terminology again confusing – clearing this up substantially earlier-on in the paper and then following this through to the discussion would really help the reader comprehend the key messages much better than at present! Line 428: this is rather a weak initial statement – be more specific. Line 440: What direct evidence do you have to back-up this clear statement? Figures: (1) A useful conceptual figure, but much better reproduction is required. There seems to be a mis-match between continued drainage in the experiment and ‘events’ stated in the legend. This must be addressed. Figure legend terms do not match directly those used in the text of the manuscript. (2) You need to include the measurements taken at each observation site. (3) OK (4) Grey points are not sufficiently clear on this figure. SD error bars – n = ? (5) Sub-seasons of 2014 are unclear – see my comments on this issue in the text of the manuscript also. (6) OK – but why is this Figure 6 – surely this information warrants being at the start of the explanations!! (7) Clouds of grey points are hard to distinguish – rethink...such as boxplots for example. These would, I think be much clearer for the reader. (8) Ditto (9) OK (10) OK (11) Comments as (7) Appendix figures OK. There are numerous points in the text where small edits of the correct word are required for clarity of the narrative.

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There are too many to list here, but a native English speaker should be consulted to address these shortfalls. This is the worst such manuscript in this respect that I have read in some years.

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