

Interactive comment on “Carbon accumulation in a drained boreal bog was decreased but not stopped by seasonal drought” by Kari Minkkinen et al.

Anonymous Referee #2

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GENERAL COMMENTS This is a well written, clear paper that tackles the important yet controversial topic of peat accumulation of peatlands drained for silviculture. The results of the experiments presented in the manuscript are interesting in that they highlight that the ecosystem is still a sink, in spite of having been drained. The authors approach the problem with rigour, and measure gas exchange via two means, eddy covariance and chambers. They report the results of their experiments in a clear manner and discuss possible implications. I especially thought the last section in the discussion “Can the carbon sink last?” was valuable. In this section they discuss the ultimate fate of the carbon and how important the carbon accumulation in the soil (rather than in the forest biomass) is for sustainable practices. The authors present an important

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topic at a time where peatland ecosystem services are being valued more than ever before and management practices need to be evidence-driven.

SPECIFIC COMMENTS My main criticism of the manuscript is the issue of DOC losses and actual long-term carbon accumulation (as measured in the peat profile using dating and carbon measurements).

1. Is DOC loss really a minor component – due to ineffective ditching and high transpiration? All peatlands lose some carbon via DOC, the losses in pristine peatlands are often non-negligible. Drained peatlands may experience even larger (or sometimes smaller) DOC losses. Are the authors suggesting there is no water that comes off their site due to evapotranspiration? Can the authors show evidence of studies on forested drained peatlands that have measured DOC losses during high precipitation events after a dry period, for example? Evidence from non-forested drained peatlands suggests losses are substantial (see for example papers from Strack et al., 2008 and others).

1. Page 10 Line 24: Again, this issue is related to DOC loss. A large Rh_{et} is originated in the top part of peatlands when these are pristine (as was the case for the study site in the reference the authors cite, Chimner and Cooper, 2003). Once a peatland has been drained for a long time, I am not sure this necessarily holds true for every peatland. Of course, labile carbon from the top of the peat is preferentially decomposed always, but the older peat might also get decomposed if exposed for a peatland long time and this may be lost via DOC preferentially (not completely oxidised as CO₂ and therefore not detected by any of the means the authors deployed). See for example Evans, C. D., et al. (2014), Contrasting vulnerability of drained tropical and high-latitude peatlands to fluvial loss of stored carbon, *Global Biogeochem. Cycles*, 28, doi:10.1002/2013GB004782.

2. An important methodological question is, considering the uncertainty in the measurements (NEE has an error between 35-114 g CO₂ m⁻² yr⁻¹) and the fact that the authors did not measure DOC losses, I wonder if coring the peat, and measuring recent accumulation of peat directly by dating the peat would have been a good way

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of validating the idea of the carbon sink. It is a shame that this has not been done, since the authors even cored the peat at the site. Have any of those cores been analysed for carbon and dated? Is peat actually accumulating (and shrinking) at the same time? The authors present a very interesting result, but with the uncertainty in NEE and the possibility of DOC losses, one cannot be absolutely certain that this site is actually accumulating peat in the long term. Additionally, the authors need to be careful of estimating actual peat accumulation via modern fluxes alone. Experiments in which both a) fluxes and b) peat C accumulation measured via dating of the peat profile have been carried out, have found a large disparity between the two measurements. In fact, experiments demonstrate long term peat accumulation is much smaller than contemporary fluxes suggest, see: "Contemporary carbon fluxes do not reflect the long-term carbon balance for an Atlantic blanket bog", Joshua Rattcliffe, Roxane Andersen, Russell Anderson, Anthony Newton, David Campbell Holocene Vol 28, Issue 1, pp. 140 – 149, First Published June 30, 2017, <https://doi.org/10.1177/0959683617715689> I'd be really interested in seeing some of those collected cores dated from Kalevansuo.

I am not expecting the authors to expand their methodology to measure DOC or C accumulation in the peat profile, but I perhaps expect them to discuss these two main issues a little more in their manuscript. This is important since the title suggests that C accumulation is the main topic of the article, but actually peat C accumulation has not been directly measured.

Additionally. I have some suggestions for clarifying the methodology used: - How long before the measurements were the chambers installed? I suggest this is included in the methods. - Why are the chambers 30 cm deep? Did the authors know roots were not significant lower down? Other studies have used 50 or 60 cm deep chambers. - Repeated clipping – did you clip before every measurement? Or if not, then how often? Please consider including this in the methods. - Was the biomass of the understorey measured? I thought it was according to the methods, but then in Page 11 line 11

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the authors write that they assume biomass to not be increasing. Was the biomass only measured once? If so, this needs to be made explicit in the methods section. "Thus the method should not be overestimating soil C pool increase, more likely 15 underestimating it. " It is unclear to the reader why this should be so, could the authors explain more explicitly in the text? - Why are the errors estimated (a very nice treatment of uncertainty in Appendix 2) but not reported in the main text? (What figure is (Fig. xx in chapter 3.2) in Appendix 2?)

TECHNICAL CORRECTIONS Page 1: Line 10: turn into (instead of turn to) Line 11: We measured the carbon ("the" is missing) Line 11: NEE of CO₂ was measured with an eddy covariance method... Line 14: Biomass (in singular, not plural) and litter production... Line 14-16: Consider changing to: Soil balance was estimated...from NEE, and this showed that the soil itself was a carbon sink as well. Page 6: Line 34: Consider changing to: WT varied also spatially ("quite much" should be deleted)... Page 10: Line 33: Consider changing to: "The value is considerably smaller than that reported for agricultural fields."

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