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Biogeosciences Discussions

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 #3

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On my first read of this paper I thought it was an excellent study. I still do but with one major caveat, which I outline below in detail.

The authors are correct that the commonly used EF by the IPCC for drainage of organic soils do not apply to the case of forested peatlands, particularly the drained forests of Finland. The forest management practices there are well done and the drainage is not excessive. Also the ditches are very clean so that also minimizes emissions. The same authors have published aspects of this story before. Their publications and this study support an argument for Tier 2 and 3 methods applied to the Finnish drained forests.

The present manuscript develops the argument further that drained mires for forest

remain sinks even in a year of extreme drought. This would be an important proof of the continued sink potential. Unfortunately, on my second read I realized that the only year where there are no eddy covariance measurements during the growing season was the year of the significant drought - 2096. The authors show the large, persistent water table drop – it is far in excess of any other year and the duration of the drought in exceptional unique in their data set. To accept the evidence that is used to support the authors' main conclusion one has to believe that the gap-filled techniques are appropriate for drought conditions. I doubt this very much. The authors evaluate their model for 'normal' years but they provide no evidence that the model applies for the drought conditions experienced in 2006. The actual test is very weak – they examine annual NEP and NECB. It is simulated reasonably well but this is largely irrelevant as the key test is to see if the model simulates GPP and Reco under drought. The reader has to know how well the model performs for extremes, particularly how well its captures the influence of high VPDs and lower water tables. Most gap-filling algorithms do well for average conditions – this is what they are designed to do. However, like most ecosystem models they do very poorly when the conditions deviate beyond the normal range of variance. The water table during 2006 was much deeper than anything experienced during the other three years. Further, the drop in water table is a persistent secular trend. The authors provide no evidence that their gap-filling algorithms can simulate GPP or Reco under these conditions. The model has no dependency on water availability. The GPP model has VPD and PPFD so the reduction in water availability has to come through the VPD function but there is no link. However, it appears that the VPDs experienced in the drought were unique – i.e. well outside the range that was used to develop the parameters in the model. I assume GPmax is derived for normal conditions not drought conditions.

For me to believe their results I would need to see the following:

1, A demonstration that the parameters in their LUE model (α, GPmax) apply to drought conditions; 2. How Reco is sensitive to changes in water storage, or how temperature reflects the effect of low water contents; 3. the range of conditions that the data was used to fit the gap-filling functions and how for outside that ranges were the conditions in 2006; and 4. an error analysis that looks at the performance of the model functions with extreme conditions – firstly using measurement and simulated data within the range of conditions observed, and hten by sensitivity analysis. The sensitivity analysis will not have any moisture effects in it so maybe an examination of the model the authors use versus what other models that incorporate moisture produce.

With these four steps the authors could place the 2006 data on solid ground or determine that their main conclusions are an artifact of the gap-filling approach. Without this the paper is simply infer untested functions for conditions they were never designed to handle and the key argument of the paper collapses.

Given the that central argument of the manuscript is dependent on the 2006 Fluxes and these are not measurements but gap-filled the authors have to provide proof the gap-filling is appropriate. If the authors can provide this proof then this manuscript would be useful addition to the literature and it would have policy relevance. Without the proof of the gap-filling validity the evidence supporting the main conclusion collapses.

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