

Biogeosciences Discuss., referee comment RC2
<https://doi.org/10.5194/bg-2021-126-RC2>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on bg-2021-126

Peter E. Levy (Referee)

Referee comment on "Net soil carbon balance in afforested peatlands and separating autotrophic and heterotrophic soil CO₂ effluxes" by Renée Hermans et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-126-RC2>, 2021

This is a neat study, carefully designed and carried out, with important implications for modelling afforested peatland systems. For the most part, the work is clearly described and well written. Some areas which need clarification are listed below. My main comment is that the significance of the finding that the peat is a net sink for carbon 30 years after being drained and afforested is very much under-played. This is contrary to expectations and current modelling assumptions, so merits more discussion.

Specific points:

L39-44. No, the reason why we are uncertain about the effects of drainage and afforestation is because it is logistically hard to measure. Even whether drainage actually causes a loss of carbon is based more on expectation rather than hard measurement.

L97. The number of replicate plots is not given. Seems to be $n = 4$, repeated for each microform.

L135. Was the moss layer considered as "litter" and removed also? The text suggests there is only litter, peat and tree roots, but the moss layer looks non-negligible in the photo.

L160. Soil moisture can be measured and expressed in many ways. Explain what is measured here - volumetric water content (m^3/m^3) by TDR method?

L173. For clarity, it would be best to give the equation for the final fitted model. "plot as a random effect" could be either an intercept or a grouping term on one or more of the other coefficients. The former I think, from Table 2.

L179. The absolute values of AIC are very arbitrary, and there is no logic to saying that differences of less than 2 are meaningful. The relative values are meaningful, but there is no need to define such thresholds. The key thing is whether predictions differ substantially among these models - see point below.

L180+. Not sure why the weighting is mentioned, since it was not used. If there are notable differences between predictions from the different models, then using a weighted ensemble of model predictions would be sensible. Bayesian model averaging would be even better. If, however, predictions are all rather similar, that justifies the approach of choosing the single best model (minimum AIC).

L168/L185. Fitted with nlme, but predicted with lme4? I think this is an error.

L208. Only linear effects are considered here, but nonlinear effects are possible/expected, but harder to deal with and identify statistically. Can we get some justification for this?

L210. 40 % of variation was explained by the model, but this is presumably on the log scale. It needs pointing out that predictions are made in the original units, and all the uncertainties reflate.

L215. Be explicit about the interpretation & units here - I think these are intercepts and multipliers for CO₂ flux on the log scale ($\log(\mu\text{mol m}^{-2} \text{s}^{-1}) / \text{deg C}$)?

L215. There is no term for "Microform = Furrow". Maybe this is the interpretation of the intercept?

L228. The negative interaction term just means that the T coefficient decreases with SM.

L232-233. This is confusing, as it sounds like a separate step has been done. However, the whole rationale of fitting a model including soil moisture is precisely this - so that comparisons between treatments can be made, whilst accounting for differences in soil moisture.

L235. Can you show the data as well as the fitted model?

L238. For completeness, be explicit how heterotrophic fluxes are estimated - presumably

as total - autotrophic.

L243. Be explicit how the uncertainty on the annual sum is calculated. The error terms in Eqn 2 all add, and have to be transformed from log to original units.

L293. The table caption is very confusing. It reads as if this is the decay of dead roots itself, not the flux from the plot after the dead-root correction. Needs re-wording.

L378+. Of course there have to be caveats on the results, and it is the C balance over the lifespan of the forest that matters. However, the expectation in most modelling work is that drainage and afforestation causes oxidation of the peat at a rate of 50 to 300 g C m⁻² y⁻¹ (e.g. Cannell 1993). This is offset in the first few rotations by the increasing tree biomass and litter, but ultimately, the ongoing long-term degradation of the peat becomes the dominant term, and the system becomes a net carbon source after 1-5 rotations (depending on the assumed peat oxidation rate). If this study is in fact showing that the peat is a net sink of 17 to 124 g C m⁻² y⁻¹ after 30 years, this is surely the stand-out result. Worth some more discussion at least.