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Discussions

Interactive comment on "Forests on drained agricultural peatland are potentially large sources of greenhouse gases – insights from a full rotation period simulation" *by* H. He et al.

Anonymous Referee #1

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I thoroughly enjoyed reading this manuscript which describes a modelling study the impact of land-use change from peatlands to forests on the GHG balance in Sweden. It is clear there has been a considerable amount of work in both the simulations and the manuscript which is well written. The figures are clear and self-explanatory with only one or two mistakes. Below are my comments and questions.

1) Figure 2c maybe it would have been better if it was a scatterplot. It will make clearer the under/over estimation of the model. My question here is that although on average the model is closer to the data how significant is the slope and intercept of the comparison. Maybe then put the scatterplot of the evaluation of the model against data (plant

& tree growth) separately and show significance of slopes/intercepts

2) Page 19683 lines 16-17. I cannot see where there is any data shown to support your suggestion that understorey layer is over predicted by the model. I see that both plant growth AND tree growth is underestimated by the model in 0-20 years (Fig 2c & 2d). Now I assume here that tree growth is included with plant growth so I don't see how understorey layer could have been overpredicted. Needs more explanation within the text and maybe more clear graphs

3) Page 19683 lines 17-18. As far as I understand the works of the CoupModel there are different ways of doing photosynthesis by either using simple light use efficiency model or the most complicated root of Farquhar model which usually comes with light attenuation mechanisms within the plant canopy. The authors have not made clear which version of the model have used. This is important since in the case of the first (i.e, light use efficiency) the relationship between LAI and NPP with radiation is stronger since photosynthesis is more directly driven by it. So the statement here does not necessarily consist of a success of the model's ability to simulation NPP.

4) Figure 3. Lines are not very clear in the graph for accumulated humus respiration and plant litter. Consider improving graph.

5) Figure 4 and Page 19684 Line 25, Page 19685 line 1. I agree the seasonality was captured by the model but I disagree that the magnitude was capture. In the case of solar radiation 2007 magnitude was not successfully simulated and for NEE 2008. In particular the maximum of NEE from observations were around 10 gC m-2 day-1 (Please check the units on the graph) where as the model peaked closer to 18 (?) gC m-2 day-1.

6) It is likely that the over-prediction of NEE is associated with underestimation of soil respiration. But if we assume that soil respiration is strongly driven by soil temperature then soil respiration should have also be overpredicted since predicted soil temperatures is higher than observed (Figure 4b). So the question is how the model has can

have higher respiration but with higher temperatures. There is a big uncertainty here which I believe is related to the decomposition parameters and how respiration is produced which I believe needs further exploration. Furthermore, data from 60km away were used to drive the model. In micro-meteorological terms, topography and climate between the site for which simulations were done using met data and the site were CO2 measurements took place with eddy covariance can not be assumed the same. All these and the fact that a fitting with a single point value of soil total C was done to represent soil processes reduces my confidence to the model. In the end the high uncertainty over soil fluxes has an impact on the final conclusion. The authors should have addressed the uncertainty arising from the lack of data with a data-model fusion such as a Bayesian calibration with MCMC or a Kalman filter.

7) By assuming a constant N deposition rate using the authors have ignored increases to global pollution levels over the recent years and the combined combination it has with increasing temperature over the higher latitude forests. It was shown that nitrogen deposition creates an extra added feedback to tree growth which should not be ignored. The authors suggest that from the sensitivity analysis any extra nutrients would have no impact on the result of the model which might be true since the relationship between nitrogen and growth as model could have reached an asymptote although some times there might be a hidden-non linear relationship only and further increase would have shown. But, by assuming a constant N deposition, failed to answer the critical questions of how N deposition will affect the balance of GHG and in this case N2O, and what feedback exist between production of carbon GHG and non-carbon GHG due to extra nitrogen. These are questions which experiments can deliver with difficulty.

8) I agree with the authors that until know models have been simulating SOM decomposition with the same rates through out a prolonged simulation period based on linear kinetics which are dependent only on soil conditions (e.g., temperature and moisture) but with no consideration both on the microbial community that drives decomposition and the quality of litter that may affect how fast decomposition is happening. Both ex-

perimental and modelling studies have shown that the fate of SOM is highly dependent on the quality of litter and how it is consumed by microbes. Good quality of litter which is easy to decompose is usually preferred by microbes thus accelerating the decomposition of fast pool to such rates that it only becomes an intermediate pool and starting to reduce faster the old, “slow” pool. Grass litter is a good example. On the other hand introducing spruce litter, which is lignin rich, will reduce decomposition of old “stable” pool by microbes since it becomes more difficult to decompose. This switch in quality of litter can associated with the change in land-use from peat to forest can make a difference to the carbon stocks and they should be included in the author’s model.

9) I agree with the other reviewer that changes to soil physical properties is important when you considering trees and how their root system changes over the years. In a peat environment there should be a bias introduced to soil dynamics and feedback because of tree growth.

Interactive comment on Biogeosciences Discuss., 12, 19673, 2015.