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.

H. He et al.

hongxing.he@gvc.gu.se

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

- *Yes a scatterplot is another way to show the data. However, we think our plotting against time shows the biases over time better. The bias is also shown by: 1) the mean error (ME) given in the plot indicates an overestimation compared to measured data. 2) The significance of a scatter plot slope is also given in the figure with a high correlation coefficient (R^2=0.86) between the model and data. We have discovered the plot can be misunderstood as a carbon stock content, where it actually is a rate, why we need to show this by the unit given on the y-axis, g C m^{-2} yr^{-1}*

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

- *We think we did not express this clear enough. The CoupModel simulates two vegetation layers, the trees (in this case Norway spruce) and an understorey vegetation (e.g. shrubs and grasses) (He et al., 2016). Figure 2c and 2d only show the Spruce growth, which we will explain better in the text. The underestimation of Spruce tree growth in the first 20 years is probably due to competition with understorey vegetation (Fig 2e), mainly grass. However after this initial period the understorey vegetation is suppressed by the spruce and in the 60 year forest the ground vegetation is very sparse composed mainly of patches with mosses. Overall the understorey vegetation has little impact on the results.*
- *We might have added some confusion between the plant growth and tree growth in the current version of the paper. We will in the revision use*

consistent and clear terminology, where the "Spruce tree growth" means Spruce tree only and "plant growth including understorey layer" the total plant growth.

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.

- *Here we use the radiation use efficiency method in CoupModel for photosynthesis simulation. Since our omission to tell this in our paper resulted in some hesitation by the reader we will make this clear in the revision, however details can be found in He et al. (2016), where all equations, parameters and methods used for modeling Skogaryd forest are reported. We there found the model able to describe the C cycling (including NPP) quite well. Also the fairly well agreement between the modelled spruce biomass obtained in this study and the tree ring data further suggest the NPP was well described.*

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

- *Yes we agree and will introduce symbols in the lines to improve this figure in the revision*

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.

- *Yes the model overestimate the radiation (shown by a positive mean error) and also the NEE compared with measured data. However, the magnitude of the overestimation is small compare to the data (10^5 compared with 10^7). The way we present the data fool the eye, since simulated data is shown as daily values and the measured data are smoothed using 5 day intervals (this makes the data visible since otherwise too many overlaps). For the discrepancies of NEE, the simulated higher NEE (hence take-up of more atmospheric CO_2 than measured) during the summer of 2008 was mainly due to an underestimated peat decomposition mainly due to a too low initial soil C content value of the current model settings (see Table 1 below). We have corrected this and rerun the model where these discrepancies become smaller. Thus we will redo and improve these figures in the revision.*
- *We also found another mistake in this figure since the unit for NEE in figure 4d should need to be changed into g C m^{-2} day^{-1}, which will be made in the revision.*

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.

- *The observation that overestimation of NEE could be due to underestimation of soil respiration, which was discovered a fault of initial soil C content (1951) used for model setting, five times too low. As shown by the Table 1 below, when it was corrected 'the updated model' compared to earlier model runs which you have reviewed showed increased soil emissions.*

Table 1 Soil C content in the soil profile during 1951 to 2011

Soil layers (cm)	*Layer thickness (cm)*	*Updated model*			*Earlier model*		
		Soil C 1951 (gC m^{-2})	*Soil C 2011 (gC m^{-2})*	*Losses in soil C (gC m^{-2})*	*Soil C 1951 (gC m^{-2})*	*Soil C 2011 (gC m^{-2})*	*Losses in soil C (gC m^{-2})*
0-5	*5*	*6268*	*7776*	*- 1508*[1]	*1343*	*938*	*405*
5-15	*10*	*12536*	*7497*	*5039*	*2686*	*468*	*2218*
15-25	*10*	*12536*	*7682*	*4854*	*2686*	*331*	*2356*
25-35	*10*	*12536*	*7943*	*4593*	*2686*	*268*	*2418*
35-50	*15*	*18804*	*14749*	*4055*	*4029*	*798*	*3231*
50-70	*20*	*25032*	*22108*	*2924*	*5333*	*2145*	*3188*
70-90	*20*	*25032*	*24299*	*733*	*5333*	*3855*	*1478*
90-100	*10*	*12516*	*12516*	*0*	*2133*	*2121*	*12*

Note: [1] negative change means an increase of soil C

- *We fully agree that micro-meteorological conditions at the climate Såtenäs station could be different with Skogaryd site. However, we needed this data for the long term simulation and we have checked this data with data from the Skogaryd site between 2006 and 2011 and the Såtenäs data showed high correlations and similar magnitude with the measured Skogaryd data.*
- *We also fully agree that it would have been be good to have more soil C data during the 60 years to validate our simulated soil C dynamics. Unfortunately that's not the case. The best we could do was to use the calibrated CoupModel, He et al., (2016,) using Generalized Likelihood Uncertainty Estimation method. In addition we in this study calibrate the model with respects to plant growth (tree ring data) over the period 1966 to 2011, plus extend with new available data, both abiotic and soil gas fluxes. One source of uncertainty is of course the soil C content in the planting year (which is*

hopelessly unknown). To overcome this we conducted a sensitivity analysis spanning a soil C variation, shown in the paper. We will further discuss the uncertainty in the revision of the paper. Taken together we believe the model is able to give a realistic description of the 60 year dynamics of soil and plant development.

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.

- *The site simulated in this study was drained peat soil, a former fen used for agriculture during a few decades before planting spruce. The soil is fertile and in our model simulation the soil N availability was mostly not limiting the forest growth. In He et al., (2016) we did a complete N budget for the forest and found the peat soil to deliver most of the N needed (118 kg N ha-1 year-1) for the forest growth and the N deposition only contribute a small amount (12 kg N ha-1 year-1). This suggests that in this type of ecosystem the N deposition is not very important. Moreover the N deposition has decreased in this area during the last decades, and is now smaller than 12 kg N ha^{-1} $year^{-1}$.*

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. 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

- *The CoupModel try to translate the real world into the main ecosystem processes, where the total soil C (SOM) is divided into different pools having separate decomposition rates. Commonly the slowly decomposing pool is called humus and the fast is called litter. However in the case of organic soils, we have assumed the peat to comprise an unknown mixture of the fast and the slow pool. And in the present study we assumed the initial values of SOM only to comprise a slow pool (humus, which may be interpreted as of peat* *origin). However over time the decomposition of added litter will add a resistant fraction into the humus pool. As shown in Table 1 (above), litter addition results in accumulation of soil C in the uppermost soil layer. The decomposition rates were obtained by the model calibration, see He et al., (2016).*

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.

- *Yes soil physical properties do not change during the rotation period, due to the model structure. However, the root system increases with tree growth. And we have shown physical properties like drainage depth, and thus GWL, to determine the GHG fluxes and also possibly the plant growth, see He et al., (2016). A more developed discussion on the model physical properties and change over time will be added in the revision.*

References mentioned

He, H., Jansson, P.-E., Svensson, M., Meyer, A., Klemedtsson, L., and Kasimir, Å.: Factors controlling Nitrous Oxide emission from a spruce forest ecosystem on drained organic soil, derived using the CoupModel, Ecological Modelling, 321, 46-63, 10.1016/j.ecolmodel.2015.10.030, 2016.