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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.

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This study addresses a very interesting and important question: how do the carbon and greenhouse gas balances of a drained forested peatland change in the long run, during the whole tree stand rotation. Such long time series of GHG data are not available, which means that modeling is the only relevant method to solve the question. Thus, COUP-model was used to simulate the forest and soil development on a fertile, afforested peatland in southern Sweden. COUP-model is one of the few models that can, to my knowledge, handle organic soil processes, in which the water table dynamics, and the resultant changes in soil chemical and physical conditions, are the key-issues. In the model, soil is divided to layers, the characteristics of which can then be simulated following changes in weather, C input etc. Thus it would seem suitable for the job.

The model in these simulations, however, seems to have some very serious shortages. According to the manuscript (p. 19679), it can not simulate changes in the water table, nor changes in the soil physical characteristics. This seems odd, and since water table is an important feature controlling e.g. organic matter decomposition, it is a big lack. Even in their own tests GWL was very important (Table 1), and water table seems to have changed quite a lot during the simulation period (Fig. 5f). A question rises, how is the water level simulated in Fig. 4, if the model can not simulate water level? And still, water table level was set to constant 50 cm, but no grounds for the 50 cm were given (19679). Maybe it produced results closest to the observed ones?

The second phase was to define the initial soil C content in 1951. This was predicted using an IPCC emission factor, calculating backwards from the present soil C pool. Then the COUP-model was used to simulate the peat decomposition from 1951 to the present – producing very similar soil C stock than where started. One might ask: Does this mean that IPCC EF is as good a model as COUP for estimating peat decomposition?

But seriously, it is not told WHY the C content is needed in the first place, and HOW is the carbon in peat divided to the soil layers. For decomposition, it is not important only how much carbon there is in peat soil, what is important is that how much carbon is in AEROBIC conditions, i.e. above GWL. If 100 cm peat layer is simulated, and GWL is in 50 cm, a significant part of the C pool is anaerobic and it does not matter if that C pool is increased or not. So please explain: why is the soil C pool important for the fluxes? Are the initial soil layers realistically described? Could you describe them to the readers? It would help if the used soil layers and their physical and chemical characteristics, and their development, were shown (fig or table).

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IF the original C peat C pool was important, then the present value should be checked: It is given (19692) that the original C content in 50 cm layer in soil was 11.6 kg C m-2. This is an unbelievably small value! With bulk density of 230 kg m-3 (p. 19692) and and 85% organic matter content (Meyer et al. 2013), C concentration would have to be as low as 12% of OM (230 kg/m3 *0.5m = 115 kg m2; 115*0.85=98 kg OM m2; 98*12%=11.6 kg m-2), while it is usually close to 50% of OM. There must be an error in the C content value, it is way too low.

There are some issues with terminology of soil C fluxes. Soil C balance, which should be the most interesting variable here, is the sum of soil organic matter (peat and litter) decomposition and litter production. It is not always clear what the authors mean by "peat decomposition". Sometimes "peat decomposition" is given as if it were the soil C balance (e.g. comparison with IPCC EF-values, which are soil C balances, p. 19689). These are two different things and have to be kept separate. In results the authors state that "The GHG fluxes are composed of two important quantities, the forest carbon (C) uptake, 405 g C m-2 yr-1 and the decomposition of peat soil, 396 g C m-2 yr-1.", it is unclear to me what do they mean with the latter – the decomposition of peat, or the soil C balance. Please be more specific with the terms.

The simulation itself shows (Fig. 5) that the "peat decomposition" rate in the 100 cm soil layer continuously almost linearly decreases. I guess the authors here observe the initially 100 cm peat layer, which in the end is not anymore 100 cm (or is it?) but 50 cm. This is interesting to see, but as litter and humus are also produced (Fig. 3), and as they form a large part of the soil C balance, they should be presented with the peat decomposition fluxes.

The conclusion of this study is that more C and other GHG have been lost than has been bound to the growing tree stand and that forestry is not sustainable. This conclusion seems reasonable with the given, measured NEE, forest growth and soil efflux values (Meyer et al. 2013). Does this simulation study add our understanding of the system? Yes, I think it would, but there are still many unclear things in methods, terms

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Interactive Comment

etc. mentioned above and in the commented MS (appendix) that should be clarified before this paper be published.

More detailed comments in the MS (supplement).

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C9193/2016/bgd-12-C9193-2016-supplement.pdf

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