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Interactive comment on "CO_2 fluxes and ecosystem dynamics at five European treeless peatlands – merging data and process oriented modelling" *by* C. Metzger et al.

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We sincerely thank B. N. Sulman, for the review of our manuscript and the valuable comments (marked by "″) on our research article. We would like to respond (marked in italics) to the comments below:

"First, it would help to describe some aspects of the model in more detail. Since the manuscript is strongly focused on parameterization and whether a common parameter set can be used to simulate multiple sites, it's important to be able to evaluate the model from a process level, because differences in parameters between sites required for better matches with observation-based data could be related to missing processes. It would be very helpful if there were a conceptual figure showing model pools and fluxes, and identifying key processes. Also, since the model is being applied to wetlands, hydrology and how it interacts with the carbon cycle represents a key set of processes. It is very difficult to tell from the text how hydrology is handled in the model. The text states that water table was used explicitly, but the model equations in the supplement only include soil water content (theta). How was theta determined based on measured water table? Does the model include hydrology equations, or was this all prescribed?″

We included a description only of those processes which we modified or calibrated. We agree that it is useful to also present some of the key soil hydrology processes. We will include the most important ones and also extend the description of soil heat fluxes in the revised manuscript. Note that the water and heat flux equations are coupled and physically based (Richard and Fourier equation). An extensive description of the model including several figures is available from Jansson and Karlberg (2010). Theta represented as liquid water content, is calculated based on the water storage and temperature. However, the water table was used from measurements to better reflect the level of saturation and water table depth.

“Second, this manuscript could be improved by placing the results in the context of previous modeling work. The introduction includes a list of other ecosystem models that have been applied to peatlands, but does not include any synthesis or analysis of what this study adds to that body of knowledge. How does this model differ from previous models? What is the advantage of applying a comparatively simple model to these peatlands, while some very complex models (e.g. ecosys) also exist?″

CoupModel is a quite comprehensive model including many options and components. It was not originally developed for peatlands and therefore could be enhanced by additional processes which are specific to peatlands. The model should be generic for any hydrological conditions and it includes both saturated and unsaturated water flow equations. CoupModel itself is not one fixed model (or set of model equations),

but instead gives the user the opportunity to disable or choose between many different submodels and equations. Also, e.g. the number of soil C pools per layer or the number of different plant functional types can be increased. However, we decided to test a configuration which is relatively simple to test, whether it is capable to adequately reproduce the measurements or where a more complex representation would be necessary. Several possible improvements were identified and discussed in the manuscript, like using a different temperature response function or including a second plant layer to account for mosses. This is not only applicable for CoupModel but also for other models. Starting from a more complex model and trying to find out which processes are not relevant would be a different approach. However, our aim was to start simple and include more details when necessary.
In order to identify the main differences between site functionality as response to the climate forcing, we believe that it is important to use a model that first of all makes a proper description of abiotic conditions and secondly handle the biology in a common way for all the sites to begin with.

"Likewise, the Discussion section does not address previous modeling studies. This is a real opportunity to showcase these results: Yes, more complex models exist, but to the extent that this fairly simple model was successful in capturing dynamics at several peatlands, does this show that some of that complexity is unnecessary? Or are the failures of the model in this case helpful in identifying missing important processes that may be included in other models?″

We used the model as tool for comparing sites. The focus of this study was not to evaluate different model complexities. In fact, we work together with other modelling groups on a model comparison study including these sites and three models with different complexities (ECOSSE, Peatland-VU and CoupModel), which will be published as a separate study. The models mentioned in the introduction are like CoupModel a set of several equations which interact with each other. Most of the models did not

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invent their own equations, but instead selected one of some few existing equations for a single process. The models differ mainly in the processes they include and in which of the few existing equations were chosen for a specific process. Both are not fixed in CoupModel. Therefore the comparison with other models in the discussion section is mainly done on the level of equations. The models further differ in the number of soil pools and number of plant functional types.
Nevertheless, we might consider improving the revised manuscript by adding some more references to other modelling studies in the discussion of possible model improvements.

"Specific comments: Page 9254, Lines 20-25: When listing specific objectives, it could be helpful to include specific hypotheses as well, in order to give the reader a clear path for interpreting the experimental setup and results.″

We will consider it in the revised manuscript

"Page 9257, line 25: Two years seems like a very short spinup time for peatland ecosystems, where plant growth can be slow and peat can accumulate over thousands of years. Did all of the model pools reach (approximate) steady state in that period of time? C pools were initialized using measurements. But if the pools aren't at equilibrium in the model, the mean fluxes could be more a reflection of the pools trending toward equilibrium than a real test of model structure and parameterization. I think the authors may need to be more careful with this initialization step, as it could have important impacts on how the model fluxes compare with observations.″

We agree that initial values are problematic in this type of modelling. The spinning up was just done to get the plants more independent of the initial values – otherwise they would need a site specific calibration as well. We tested also a longer period but this had only little impact on the vegetation. We checked that the C pools are

not completely changing within a few years, but we did not try to find calibrations where they are in equilibrium, because they are not in equilibrium in the real world (This concerns the upper layers, the lower ones are approximately in equilibrium also in the simulation): Four of the five sites are strongly influenced by management. At the two most intensively managed sites (FsA and FsB), the drainage ditches are still maintained – these sites lose carbon and undergo changes in substrate quality. We do not know how they were managed 20 years ago, may be even more intensive. Hor was used as agricultural crop land, fertilized and deeply drained, so it lost carbon and the soil degraded. Several years ago it was restored and started accumulation again. It was still very fertile and produced a lot of phytomass. However this will probably not last long and the accumulation rates will decrease – already now a succession to less nutrient demanding species and lower living phytomass can be observed. Amo was also drained, but then abandoned. On sites, where the management changed so drastically during the last century, running a long time simulation would require detailed information about former land use and former soil characteristics which we do not have. We agree that a correct initialisation of the pools is of high importance. Already two soil data measurements with some few years in-between could help a lot, but are not available at the studied sites. It would be interesting to look at doing long term simulations on such intensively managed sites and test different possible past land management scenarios and their effects on the pools and their stabilities. However, this would be different study.
The natural mire site is the only site for which a quite stable state could be expected and therefore a long time spin-up could be useful, but then probably some other processes need to be accounted for, like e.g. subsidence of the peat.

"Page 9258, lines 9-15: Water table is one of the most important environmental drivers of peatland carbon cycling, and this description of how it is implemented in the model is very brief and lacks detail. How was water table integrated into the model? Are there hydrological transfer equations? Does it just assume that layers below the measured

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water table are saturated? If so, how is soil moisture calculated above the water table? It's confusing that water table is mentioned here, but in the equations in the supplement only soil moisture is included.″

The ground water level was defined by assuming a continuous zone of saturation from water table down to the lower boundary of the soil profile considered: To force the water to saturation at the measured ground water table water was added or drained based on a simple drain flow equation estimating drainage level. Water flows between adjacent soil layers were calculated based on Richards equation (1931), which depends on hydraulic conductivity, water tension, depth in the profile, vapour in the soil, the diffusion coefficient for vapour in the soil and a bypass flow. Water retention was simulated according Brooks Corey (1964), unsaturated conductivity according Mualem (1976). Boundary conditions at the soil surface are given by separate subroutines accounting for snow melt and interception of precipitation by vegetation.

"What are the layer depths and vertical resolution?″

The soil profiles (2 to 4 meters total depths) were divided into 12 layers with an increasing layer thickness from 5 cm for the upper layer to 100 cm in the lowest layer. We will add this information in the revised manuscript.

"Are there separate soil carbon and nitrogen pools in each layer? If so, how were the vertically-resolved initial values set?″

There is a fast and a slow carbon pool for each layer; initial values are given in table 3. Nitrogen is calculated according to the C:N ratio of each pool (which we initialised with 10 for the slow and 27.5 for the fast pools (see section 2.2.5)). In the table we displayed them aggregated for 3 depths to be able to compare the values between the sites, as measurement depths and the depth of the profile were different between the

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sites. That is also the reason why we did not choose exactly the same layer depths for all sites.
In the revised manuscript, we make some small changes in the text for clarification.

“Page 9258, lines 19-22: What is the justification for this specific number? Does it come from Whalen et al 2000? Whalen et al (2000) doesn’t seem to be in the reference list, so I can’t tell whether it provides an adequate justification, and either way this parameter is likely to be extremely important for the model results and deserves a clear justification in the text. The decomposition rate may be very small, but the pool is huge, and could potentially still add up to significant flux.″

There is no justification for this specific number. Whalen is just an example stating that the resistant pool contributes very little to the total decomposition. Even at the site FsA where the difference between fast and slow pool is the largest, the respiration from the slow pool is very low compared to the fast pool (max. 1/8000), which means it is almost inert. The reason why we did not include it in the calibration is that it is negatively correlated to the rate of the fast pool which would add another dimension of interaction between parameters, while the effect on time series dynamics is small. We discussed the effect on the relative rates of the slow pool in section 4.9. The decomposition rates are important, but even more important than the exact rate for that pool is the number of pools we used and how the measured C is partitioned to the pool in the initialisation. Raising the rate of that pool and adding a completely inert pool would be another method used in many other models. Even more important might be using an additional pool for only the fresh litter and assuming that all SOC has undergone at least a slight decomposition. Also taking care for litter quality differences between root and leaf litter could be an important improvement. However on most peatland sites in the Fluxnet database not even root biomass is sampled, nor root litter fall. There are several studies about how many and which pools should be used on mineral soils and what are possible initialisation routines, but we are not aware of any

study that gives the best solution for peat soils, especially drained ones
The missing reference will be added in the revised manuscript

"Page 9259: The peatland sites in this study are generally dominated by sedges, rushes, and shrubs, and are likely to have open canopies. Does the model take this into account? Especially in bogs, mosses can represent a large fraction of NPP, and do not appear to be represented in the model. Were mosses a significant fraction of NPP or biomass at any of the sites?″

Plant cover was simulated. Maximum plant cover was 100%. This information will be added to the revised manuscript. The rate at which it was reached was calibrated and identified as site specific (see section 4.3). Open canopies occurred on Amo and Lom which had a maximal plant cover of at least 90%, and at FsB for a very short time after harvest events. The model accounts for open canopies in respect to the absorbed radiation by the plant (affecting photosynthesis, evaporation and transpiration) and in respect to heat fluxes and evaporation from the soil. In principle the model can handle various layers that have various numbers of canopy covers, but for simplicity only one layer was considered in our setup.
We agree that mosses may be very important and this will justify a multiple layer canopy approach. However they were not simulated explicitly which might be an important model improvement as discussed in section 4.1, 4.3, 4.5 and 4.9. Shrubs did not play a major role.

""Plant stress due to high water saturation was ignored": Some previous peatland studies have shown that productivity (especially in fens) can increase during periods of low water table and decrease during periods of high water table. This is mentioned in the discussion as a potential source of error, but it might be good to support this assumption more with some references.″

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References will be added in the revised version. We agree that this may be of high importance but nothing in the current discrepancies indicated such a phenomenon in the current study when looking to the model performance at the single sites. For between site variability it might be important that the plants on one site (Hor) might not yet been adapted to the rewetted conditions. Including plant water stress due to saturated conditions would mean that this would be site specific. However, to test this, we would need a longer time series, as no extraordinary wet years appeared during the measurements period.

"Page 9260, lines 20-24: There are probably big differences in nutrient availability between sites, especially between bogs and fens. C:N of bog vegetation is probably different from that of fens, and the physical and chemical properties of peat (i.e. slow C) are probably different as well. Would the model do better if these differences were taken into account?″

We tested this in the C6 scenario (see Fig. 6 and discussion 4.9). It could be an explanation for the differences in decomposition rates between Lom and FsA and FsB, but not for Amo and Hor.

"Page 9264, line 14: How were they constrained independently? Using other available datasets? This paragraph might fit better in the methods section.″

They were constrained by additional multiple runs, where the other parameters were set to fixed values. This is the result of calibration step II, but belongs to the method of calibration step III. Though, mentioning the correlations between the parameters is clearly a result and therefore fits better to the result section. Maybe it is sufficient to just remove the second part of the sentence, as it is already mentioned in the methods that parameters with detected covariance with other parameters were constrained by additional multiple runs.

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“Page 9265, lines 16-25: I appreciate that the authors acknowledge the empirical modeling of GPP as a source of uncertainty. It would help to have more detail about how GPP was calculated (and, in general, more detail about all the aspects of the gap-filling and flux partitioning strategies). Did it assume a function depending on light levels?”

The strategies differed between sites. The corresponding references can be found in Table S2. However at all sites function depending on light levels (Falge et al. 2001) was used for GPP calculation and a function depending on temperature (Lloyd and Taylor, 1994) for Reco calculation from either night Eddy NEE fluxes or from opaque chamber measurements at the two chamber sites. Corrections and gapfilling at Eddy sites was done according the methods described in Reichstein et al., 2005. We will add this information in the site description in the revised manuscript.

“This would be a good place to discuss whether high water table did in fact reduce plant productivity, in contrast to the assumption made in the model (I see that this is discussed below for one site, but I think it’s worth including in the more general discussion).”

We will consider this in the revised manuscript.

“Also, there is a good discussion of variability in the plant community in general, but it would be good to specifically address non-vascular plants (e.g. mosses), since they are likely to have the biggest departures from the properties of other plants in the ecosystem.”

We fully agree and will mention them explicitly also in the general part of the discussion in the revised manuscript.

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“Page 9272, line 26-page 9273, line 2: With site-specific temperature, water table, soil C+N stocks, site-specific plant productivity, and site-specific decomposition rate. . . What else remains to model? Wouldn’t this be essentially an independent model tuning for each site? Given that the model has 45 parameters, this is not very informative. The message I’m getting is that the model as constructed does not contain the processes necessary to simulate variability between sites. Maybe it would be more honest to simply state the result like that.″

Parameters regarding the dynamics were not site specific. The model has many more than the 45 parameters used in this configuration, but only 45 were calibrated – those which we expected to be in need of site specific configuration.
However only some of them showed to have an important effect: mainly the rates of plant productivity and heterotrophic respiration.
You might say that this is not new – there are many laboratory studies which stated that soils differ in their respiration rates, that substrate quality is very important, etc. but there are also studies which found that e.g. plants differ in their respiration rates, allocation factors, time / threshold temperatures when they start emerging, senescense and dormancy, etc., or that different soils show different responses to temperature and water conditions. But our study showed that the parameters representing all these characteristics do not need a site specific value to acceptably simulate the differences between the sites in this study. Only those which we discussed extensively need site specific values.
The aim of this study was to identify which of these site specific characteristics are important to simulate acceptable representations of the measured CO2 fluxes.
We think that these results apply to all models which are based on the same input variables: to simulate the between site variability of the sites in this study they need some site specific adaptions for the plant productivity and probably also for soil decomposition rates.
The light use efficiency parameter values did not follow the nutrient, water or pH gradient between the sites. That means that even if a model includes plant stress due

to nutrient limitations, pH and too much water, site specific calibration (or input data related to biomass or LAI) would be necessary to simulate the differences between the sites in this study. Of course this might be possible to simulate with a much more complex model which calculates biomass based on further input parameters. Possibly land use and land use history are very important, maybe also how well certain new species could invade the site after a land use change due to seed availability from the neighbourhood or animal dispersion – but we can only speculate about what other input parameters would be necessary for that.
For soil decomposition rates, we cannot exclude that C and N stock would be sufficient to simulate the site differences if more SOM pools and different partitioning methods were used. However we think that also here, a more complex model with additional input parameters like land use and land use history, pH and substrate quality (or a complex model simulating current and former vegetation) would be needed.

Interactive comment on Biogeosciences Discuss., 11, 9249, 2014.