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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 Anonymous Referee 2, 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:

"This paper presented a modelling study using an extensive calibration procedure to simulate the CO2 fluxes and ecosystem dynamics at five European treeless peatlands. A process-based model, called CoupModel, was used in this study. The authors attempted to examine if they can find a commonly acceptable value for each key parameter in the model for simulating the CO2 fluxes and ecosystem dynamics for these five distinct sites. They found that some parameters could apply common values, however, some parameters needed to be calibrated site-specifically. Some of the conclusions made from this study, for example, separate temperature responses for plant and soil heterotrophic respiration are needed for modelling improvement, were not new. I did not quite get what are the specific key contributions that this modelling exercise has made to the peatland modelling communities.″

There is a huge variety of models with differences in complexity and differences which processes are included as well as which equations are used to implement a certain process. There is an even higher number of experimental studies reporting differences in processes and responses, like for example species specific differences in emerge temperatures or plant respiration rates. It will always be possible to include additional and more detailed processes to improve the model performance and come closer to the degree of detail and of complexity in the real world. But that was not our aim. We did not expect to find any new, undiscovered processes or characteristics – this would have required a much more detailed examination. Instead we aimed to identify which processes and characteristics are needed and which do need a site specific adaption to give an acceptable (not perfect) representation of the main differences in measured CO2 fluxes at these very different treeless peatland sites. We assume that this is model independent – using a different model or method should come to the same results: which processes and characteristics are site specific and which not.
We consider rephrasing some sentences in the revised manuscript to emphasize this.

"More specific comments: (1) In the abstract, it would be better if some statistical data can be included to show how well the model performed.″

The focus should be on the differences between the sites (and therefore the parameters which needed site specific calibration). The performance of the model is subordinate. Therefore we would prefer to not include them in the abstract.

“(2) The conclusion in the abstract did not really match what were stated in the research aims. If I just read the abstract, it seemed that this study was trying to only evaluate the CoupModel for the CO_2 flux simulation of five European treeless peatlands.′′

The aim was to find out why the sites differ in their CO_2 balance. Do they differ just because of the climate and management (in particular water table), or do they also differ in their response functions which would be indicated by the need for site specific parameter values.
We will rephrase some sentences in the revised manuscript for clarification

“(3) In the introduction, you only listed what peatland models have been available. But it would be better if you can discuss the specific aspects of the models and point out what were missed in these existing models and why this modelling exercise was needed. ′′

CoupModel is also just an existing model. No new model development was done, the model was just applied. CoupModel had shown good performance in previous studies for biotic as well as abiotic processes. It has many possibilities for parameter calibration and uncertainty analysis without the need to modify the code, which was important for identifying the main differences between the sites in respect to their CO_2 fluxes. However it is surely not the only model which would have been suitable for this modelling exercise.

“(4) With your specific objectives, it would be better if you can also present the specific hypothesis that you would like to test in this study.′′

We will consider giving more information on our expectations in the revised manuscript

“(5) These five peatland sites are very different to each other. From the existing empirical studies based on chamber and EC measurements, could you please deduce some

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key differences in the processes governing the CO2 cycling? These key differences in the processes would be the foundation for the testable hypothesis for this study. If the differences in the CO2 fluxes were due to the distinct vegetation dynamics and soil processes, then it would lead to a question that a common model, such as the one used in this study, CoupModel, could be used to simulate the C cycling for these sites, although the model can be calibrated so the comparison between modelled and measured values could reach to an acceptable level.″

We wanted to find out what are the key differences in the processes by applying the model. As the sites are very different (in respect to their water table (both mean depth and dynamics), climate, actual and former management, vegetation, soil properties), it is difficult to compare them in an empiric way.
Those variables, which we used as input data are very different between the sites. Amplitude, mean values and dynamics of meteorological data as well as water table are very different. Furthermore C and N stocks, C:N ratios and their distribution in the soil was different. But can these differences explain the differences in measured CO2 fluxes? That was the question which we want to answer by applying a model. All these variables interact with each other, so it is not possible to answer that question by just looking at the data.
Apart from the input variables also the origin of the soils, soil structure and pH are different, actual and former land use, plant and plant functional types - mosses with their special characteristics are dominating on one site, at not at all present on another site. Some sites do have high productivity, dense and high vegetation, while others are sparsely vegetated. Nutrient conditions are very different between the sites. Plants are well adapted on some sites while on one the vegetation might still be under succession due to land use changes. The plant species are very different and do have different strategies. Most have aerenchyma, but not all, rooting depths are very different between the species, vegetation period length is different between the sites. Some of the species are known to emerge late, others are present only during spring, and some of the sites are species rich, others dominated by just one or two species.

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Basically all the parameters which we choose for calibration could be expected to be different between the sites.

“(6) In the discussion, it would be better if you could put this modelling exercise in the context of the existing modelling studies and discuss what the key contributions you are trying to make to the peatland modelling communities.″

The main aim of this study was not discover new processes to improve the performance on individual sites, but instead to find out what were the main differences between the sites in this study in their response to forcing data.
Most models do not invent their own equations, but instead select 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 was chosen for a specific process. Both is 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 more references to other modelling studies in the discussion of possible model improvements.

“(7) In the discussion, you have discussed the interaction between each key parameter and the input drivers. Could you please also discuss how they are specifically handled by the model? I believe that the interaction presented in the discussion should be only reflected by what has been included in the model itself. It would be better if can discuss what are the possible interaction that you can deduce from the empirical studies.″

Comparing the sites in an empirical way is another approach, which we did not do in this study. Two of the included sites were already analysed in an empirical way by Drewer et al. 2010 (included in the reference list).
We choose the approach of using a process based model which has the advantage of

taking care of overlaying effects of the different input parameters and later analysed the resulting differences between parameters and differences between model and measured data in relation to the differences in the input drivers.
The model description in respect to how soil temperature and soil moisture is calculated based on air temperature and water table will be improved in the revised manuscript.

"(8) You have included the detailed explanation of each symbol in the Supplemental materials. However, without the clear explanation for each symbol, it is very difficult to follow. I have to check back and forth to get the representation of each symbol.″

Description of the symbols in the supplement will be improved in the revised manuscript

"(9) In Section 2.2.3, how did you subdivide the whole peat profile into slow turnover C pool and fast turnover C pool? Did the water table depth play any role in the subdivision?″

The SOC was partitioned according the measured C and N stocks and the assumption of an initial C:N ratio of 10 for the slow and 27.5 for the fast turnover pool for each layer (see section 2.2.3 and 2.2.5). Water table was no division factor, however the drained sites (FsA and FsB) as well as the formerly intensively managed site (Hor) had lower C:N ratios in the upper layers (which are usually above the summer water table) and therefore a larger ratio in the slow pool compared to deeper layers, which are saturated most times during the year (see Table 3).
We rephrase some parts of the corresponding sections to emphasise that two pools were used for each layer. Also 2.2.3 was shifted with 2.2.4, so that it is followed now directly by 2.2.5

"(10) For Fig.3, it is difficult for me to see the comparison. Is it better to present them

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in a 1:1 comparison scatter plot as well? I suggest the present Fig.3 will be kept as it was. You can consider to add a new figure to present the 1:1 comparison for each component.″

The advantage of Fig. 3 compared to a 1:1 plot is, that it shows how the model produced seasonal patterns and shows when (and therefore under which conditions) we do have deviations from the measurements. The general performance of the model is good and as the model was calibrated to not be biased, we think graphs like that would not add more information. Furthermore, the main focus should be on the differences between parameters and not the model performance.

"(11) It was stated in this paper that the CoupModel was able to disable some of the modules if needed. Would it be possible for you to just simulate the CO2 cycling using this model but with disabled module of simulating the soil climate, including the soil temperature and soil water content? I believe that these data, including soil temperature and soil water content would be readily available from the biomet station of the EC measurement. By doing so, you could only need to calibrate these biotic parameters, rather than these abiotic parameters included in the model. You may be able to find out what would be the key biotic processes governing the CO2 cycling for these five distinct peatlands. If some of the key biotic processes have been missed in the present model, this would be where the real modelling improvement is needed. You may even find some of these processes were not only missed in the CoupModel, but also in other existing models. If so, this would be your great contribution to the peatland modelling community from this study.″

Water content was only available for one of wettest sites (so it was mostly saturated) and only for a short period of time. Unfortunately, it is not at all common to measure this variable at peatlands (we selected these sites as data rich sites, and measured water content was also a criterion). Even if soil water content was measured we still have the problem of interpolating it between soil layers and gapfilling when the

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sensors are not working. The same is true for soil temperature. However the model did quite well in simulating soil temperature, without the need for much calibration. Other studies reported also good performance for simulation of soil water content with the CoupModel. Those studies were mostly on mineral soils, but as no data was available (except from that mostly saturated layer which was represented well by the model), we did not try to improve the calibration of related parameters in our study.
Mean temperature of the lower boundary of the soil profile was the only abiotic parameter which was calibrated and is close to the measured mean soil temperature. All other parameters which were calibrated were related to the biotics. Identifying the key biotic processes and how they differ between the sites was exactly the aim of this study. The results were that the key processes were covered by the model, even though a relatively simple setup was used for the biotic processes, except processes which could explain the differences in decomposition rates, plant productivity and plant reserves in the mobile pool for regrowth in spring.

"(12) Could you please present more details on how you spin up the peat profile in your modelling experiment?″

We didn't do any spin up for the peat profile, but instead used the measured values as initial conditions of the soil. The first reviewer had a question on that as well, so I just repeat here our response to him:
The spinup was just done to get the plant 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 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 intensive managed sites, 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. Another site 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. The last site 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 to do long term simulations on such an intensive managed sites and test different possible past land management scenarios and their effects on the pools and their stabilities. But this would be another 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.

"(13) Water table depth is one of the key abiotic parameters in peatlands to governing the ecological functioning, and thus the CO2 cycling. Could you please present more details on how the model used the water table depth to simulate the CO2 cycling for peatlands.″

We will improve the description in the revised manuscript and supplement. Note that we do not consider water table as a parameter. Water table is a dynamic forcing variable in our modelling approach. Here the response to the first reviewer, who asked a similar question: The ground water level was defined by assuming a continuous zone

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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 assuming 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.

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