Please find our response in green; Black are editor and reviewer comments, red is our previously uploaded response to those comments and green again the new comments where those responses are incorporated in the new revised manuscript.

Editor comment:

The manuscript „CO_2 fluxes and ecosystem dynamics at five European treeless peatlands – Merging data and process oriented modelling" by Metzger et al. deals with a very complex process based carbon model and is, in general, well suited for the Special Issue "Carbon and greenhouse gases in managed peatlands".
While the manuscript was considerably improved after revision, I would still recommend a further minor revision before publication. Technically, the main issue is that the authors addressed the concerns raised by the reviewers rather well, but it is, firstly, quite difficult to judge whether actual changes have been made as statements frequently conclude with phrases such as "we consider rephrasing some sentences" or "we might consider... in the revised manuscript". I would strongly suggest including page and line numbers (or quotes) of actual changes. Secondly, the authors frequently provide comprehensive answers to the reviewers' questions, but fail to incorporate the explanations (or a shorter version of them) into the manuscript. An example for this is the issue of spin-up times (section 2.2.1), which was raised by both reviewers.

Explanations are now incorporated in the new revised manuscript and notes indicating the changes, were made below each response.

Both reviewers asked for a comparison with other models or modelling studies. While it is certainly true that the aim of the study is not a model comparison, the following points should nonetheless be addressed in the introduction or discussion:

- **why was CoupModel chosen?**

  See improved introduction: p. 4, ln 21-p5 ln 7

- **what is new compared to other multi-site studies?**

  See introduction: p.3, ln 19: "the focus of this study was exploring differences between the sites while model performance was subordinate"; and the new p4, ln 9-12: "However we are not aware of any studies comparing differences in parameter distributions of CO2 related processes between treeless peatland sites, using an uncertainty based approach and a detailed process oriented model, running on site scale"

- **what are the key messages to other peatland modellers?**

  The most important message is to name the processes which were found to be site specific and which not. Those are mentioned in Abstract, Discussion and Conclusions, whereas the description of processes which did not need site specific interpretation is improved in the latest version of the manuscript (p. 2, ln 14-20, p. 16 ln 4-32, p. 24 ln 31-p. 25 ln 3). Several further messages can be found in the discussion sections:

  - The mean temperature for the lower boundary for soil temperature needs to be higher than air temperature (section 4.3)
  - Including plant cover data can help to improve soil temperature representation (section 4.4)
  - Differences between starting of senescence between sites can be eliminated / reduced by initiating it depending on day of the year instead of using a temperature sum (section 4.5)

- Storage for regrowth was identified as an important site specific parameter (section 4.6)
- The plant efficiency in using radiation cannot be explained by nutrient status or water table depth (section 4.7), the sentence p. 21 ln 4 was added
- Q10 Temperature response approach might be not sufficient (section 4.8) – this is not new, but still Q10 is widely used
- Strong interaction between soil moisture response and decomposition rates make a constraint of related parameters difficult and needs water table data in high resolution from sites with high water level dynamics. (section 4.9)

- **how much complexity is required (reviewer #1 suggests several important aspects)?**

See new first section of the discussion, p 16, ln 4-32.

It was pointed out also by both reviewers that water table depth and hydrological processes are crucial for peatland carbon dynamics. I would support a more detailed description of this part of the model, of the parameters involved and the derivation of their values (for example, of the saturated hydraulic conductivity and the parameters of the water retention curve) and a more critical discussion of the modelled soil moisture content, which will of course influence decomposition dynamics etc. For example, I am not sure whether the Brooks and Corey-function is a good choice for a peatland model, as the soil is assumed to be saturated until the air entry point. This is a strong simplification, and Brooks and Corey might not be the best choice for peat soils.

Close to saturation, Brooks-Corey was replaced by a linear expression. This is mentioned in the text now (p. 9, ln 20-23) and the eq. 45 and 46 are added to the supplement. Parameter for water retention and hydraulic conductivity were added to Table S3 in the supplement, their derivation is mentioned in 2.2.6, p. 12 ln 4-6 were rewritten. A discussion of soil moisture contents can be found now in the last section of 4.2, p. 18 ln 4-10

Reviewer #1 also proposed adding a conceptual figure showing model pools and fluxes. I would support the idea as it would help the reader to follow the manuscript without having to read the CoupModel manual.
Figure 2 is added now in the revised manuscript.

Furthermore, I would suggest adding all parameters discussed in the main text to Table 2 (for example, p□satact is missing) to improve readability.
$p_{\theta Satact}$ and k_{tot}, which was also missing are now included in table 2.

Author response to: 'Referee comment', RC C3671

``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?´´

{\it 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.}\\

The section 2.2.2 water level was replaced by two much more detailed sections about aboveground heat and water fluxes (2.2.2) and below ground heat and water fluxes (2.2.3). In table S3 the supplement, equations 32 ff. were added, to describe the most important water and heat fluxes.

``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?´´

{\it 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.}\\

See improved introduction: ”In this work the CoupModel was used, which …” (p. 4 ln 23- p. 5 ln 7) and the new first section of the discussion p. 16 ln 4-32.

``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?´´

{\it 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 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.}\\

Following references to other modelling studies were made:

- Section 4.8, p. 22, ln 25 "E.g. in the Wetland-DNDC model, the water response function depends on redox potential"
- P. 18, ln 7-8 "...so that the McGill wetland model assumes reduced photosynthesis outside a water level range of −10 to −20 cm"
- P. 24, ln 6-7: "In some models, the various SOC pools differ also in their response functions (e.g. Smith et. al, 2010)"
- P. 24, ln 3 "Therefore, many other SOC models use several different SOC pools (e.g. Franko et al., 1997; Smith et al., 1997; Cui et al., 2005; Del Grosso et al., 2005; van Huissteden et al., 2006"
- P. 17, ln 2-3: "Many models use spin-up routines of many years until SOC pools reach a steady state (e.g. Dimitrov et al., 2010; Smith et al., 2010; Thornton and Rosenbloom, 2005)."

``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.´´
{\it We will consider it in the revised manuscript}\\

Objective section was rewritten – p. 5 ln 8-18

``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.´´

{\it 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. }\\

See new section 4.1, p. 17 2-16

``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 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.´´

{\it 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?´´

{\it 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. }\\
See section 2.2.3, p. 9, ln 6-7: "The soil profiles were divided into 12 layers with an increasing layer depth from 5 cm for the upper layer to 100 cm in the lowest layer"

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

{\it 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 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. }\\
This is described in section 2.2.5, some more "two pools" and "per each layer" were added to avoid confusion.

``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.´´

{\it 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}\\

Reference was added; the decay rate value for the slow pool was already discussed in 4.9.

``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?´´

{\it 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.}\\

"maximal plant cover of 100%" was added in 2.2.4, p. 10 ln 6. Mosses have been discussed already at several points in the manuscript (e.g. sections 4.2, 4.4., 4.6, 4.10)
The discussion about mosses was further extended in the revised manuscript in 4.1., p. 17 ln 26 – p. 18 ln 8.

``"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.´´

{\it 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.}\\

In the section 2.2.4 Vegetation, references for plant adaptions to high water levels were added: (Keddy 1992, Steed et al. 2002, Jackson & Armstrong 1999).

``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?´´

{\it 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.´´

{\it 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.}\\

The second part of the corresponding sentence was deleted to avoid confusion

``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?´´

{\it 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. }\\
Added in section 2.1 p. 6 ln 6 -9

``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).´´
{\it We will consider this in the revised manuscript.}\\

See section 4.2 p. 18 ln 5-8: "Including plant stress due to high water levels and nutrient limitation might improve the performance …"

``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.´´

{\it We fully agree and will mention them explicitly also in the general part of the discussion in the revised manuscript.}\\
See 4.2: p. 17 ln 26 -29 "Especially mosses differ largely from vascular plants…."

``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.´´

{\it 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, senescence 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.}\\

See response to "messages to other modellers" and "model complexity" above. The description of processes which did not need site specific interpretation is improved in the latest version of the manuscript (p. 2, ln 14-20, p. 16 ln 4-32, p. 24 ln 31- p. 25 ln 3) and the new first section of the discussion (p. 16 ln 4-32) was added.

Response to comments from Referee #2

Received and published: 25 July 2014

Author response to: 'Referee comment', RC C3819

RC C3819: 'Review comments on "CO2 fluxes and ecosystem dynamics at five European treeless peatlands-merging data and process oriented modelling" by Metzger, C., et al.', Anonymous Referee #2, 25 Jul 2014 [reply]

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.´´

{\it 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. }\\

Aims and objectives were rephrased (p. 5, ln 8-18) and the first section in the discussion (p. 16 ln 4-32) was added.

``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.´´

{\it 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.´´

{\it 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}\\
Aims and objectives were rephrased (p. 5, ln 8-18). Also several sentences in the abstract were rephrased: p. 1 ln 22-24, p. 2 ln 3-4, p. 2 ln 14-23

``(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. ´´

{\it 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.´´

{\it We will consider giving more information on our expectations in the revised manuscript}\\

Aims and objectives were rephrased (p. 5, ln 8-18). And the first section in the discussion was added (p. 16 4-32)

``(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 key differences in the processes governing the CO_2 cycling? These key differences in the processes would be the foundation for the testable hypothesis for this study. If the differences in the CO_2 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.´´

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

This should be covered by the rephrased aims and objectives (p. 5, ln 8-18) and the new first section in the discussion (p. 16 4-32).

``(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.´´

{\it 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. }\\

Following references to other modelling studies were made:

- Section 4.8, p. 22, ln 25 "E.g. in the Wetland-DNDC model, the water response function depends on redox potential"
- P. 18, ln 7-8 "...so that the McGill wetland model assumes reduced photosynthesis outside a water level range of −10 to −20 cm"
- P. 24, ln 6-7: "In some models, the various SOC pools differ also in their response functions (e.g. Smith et. al, 2010)"
- P. 24, ln 3 "Therefore, many other SOC models use several different SOC pools (e.g. Franko et al., 1997; Smith et al., 1997; Cui et al., 2005; Del Grosso et al., 2005; van Huissteden et al., 2006"

- P. 17, ln 2-3: “Many models use spin-up routines of many years until SOC pools reach a steady state (e.g. Dimitrov et al., 2010; Smith et al., 2010; Thornton and Rosenbloom, 2005).”

``(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.´´

{\it 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.}\\
Section 2.2.2 and 2.2.3 were added, as well as eq. 36 ff in the supplement

``(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.´´

{\it Description of the symbols in the supplement will be improved in the revised manuscript}\\
The description in Table S4 in the supplement was improved.

``(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?´´

{\it 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}\\
Section 2.2.5 was rephrased to make it clearer and some more “two pools” and “per each layer” were added to avoid confusion

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

{\it 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.´´

{\it 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 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. }\\

In section 4.2, p. 19 ln 4-10 we mention now that soil moisture was not available, the good performance on soil temperature was already mentioned in the first version in the first lines of 3.1.2, the mean error was added. The contribution of this study is addressed in the revised objective sections and the first section of the discussion.

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

{\it 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. }\\

See new section 4.1, p 16 ln 4-32

``(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.´´

{\it 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 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.}

Section 2.2.2 and 2.2.3 were added, as well as eq. 36 ff in the supplement