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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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General comments: This manuscript presents the results of a modeling exercise in which a biogeochemical model (CoupModel) was calibrated against a group of wetland sites using a Monte-Carlo approach, and simulated carbon fluxes were evaluated against eddy covariance and chamber-based measurements from the sites. The primary hypothesis tested in the study was that the model could simulate fluxes from several different sites using a single parameter set, which would imply that differences between sites were driven by environmental controls such as meteorology and hydrology rather than biological differences not included in the model.

While the experimental setup and interpretation of the results seem to be sound, there are a couple of areas where this manuscript could be strengthened. 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?

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? 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?

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.

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.

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. What are the layer depths and vertical resolution? Are there separate soil carbon and nitrogen pools in each layer? If so, how were the vertically-resolved initial values set?

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.

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 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.

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?

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

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? 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). 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.

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.

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