Biogeosciences Discuss., 8, C2048–C2052, 2011
www.biogeosciences-discuss.net/8/C2048/2011/

Biogeosciences Discussions

Interactive comment on "A dynamic model of wetland extent and peat accumulation: results for the Holocene" *by* T. Kleinen et al.

Anonymous Referee #1

Received and published: 18 July 2011

This manuscript describes a new module of boreal peatland development as part of the LPJ DGVM as it is incorporated in to the CLIMBER earth system model. The idea of a fully interactive peat model is novel and necessary, particularly in the context of understanding millennial-scale and longer changes in the global carbon cycle. The description of the new peatland module is followed by a simple application to the Holocene, simulating peatland development and carbon uptake from 8 ka to the preindustrial.

Unfortunately, this study does not meet my expectations of quality for modeling. I disagree with the authors that their evaluation of the model results in light of observations is acceptable. The model-data mismatch is simply too large, and the model's inability to capture peatland development and distribution in some of the world's most important boreal peatlands is a critical flaw in the study. Observational information that could

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

have been used to perform more thorough evaluation of the model was not considered. Given the poor performance of the model, the conclusions the authors draw from the model application are so weak as to be meaningless. Thus, this model needs serious improvement before it should be applied, and the manuscript is not yet ready for publication.

Specific comments

Page 4818, lines 19-20: A reference is needed here to support the statement on draining of wetlands.

Page 4819, lines 3-6: Since the model does not do particularly well in simulating important, low-elevation wetlands, the authors could have attempted to include elevation above sea level in the parameterization of wetland area, along with topographic index, in a way similar to the methodology used by Fan & Miguez-Macho (Climate Dynamics, 2010).

Page 4819, lines 26-30: I am not particularly impressed by these results, and I disagree with the authors that the results "reflect the expected changes". This is particularly the case in the non-simulation of the Hudson's Bay Lowlands (HBL) peatland complex and peatlands along the North Slope of Alaska, and the seemingly artificial reduction in wetland area in western Siberia south of 60N, which must be an artifact of the coarse-resolution climate forcing that is a result of the CLIMBER model run. It also appears that the absolute change in wetland fraction is very small: maximum $\pm$5% in all cases, which given what we know about peatland formation from basal-date and other field syntheses (cited by the authors) seems exceedingly small. Some wetlands, including HBL, simply did not exist at 8 ka, therefore the change in wetland area must approach 100% in some regions.

The authors may also like to discuss here the role that postglacial isostatic changes played in the development of wetland area over the Holocene; this was particularly important for HBL, the northern Gulf of Bothnia and the North Sea wetland areas. In

BGD

8, C2048–C2052, 2011

fairness, this topic is touched upon at the end of the discussion section, but it is so important to the Holocene history of boreal wetlands that it needs more justification to explain why the authors did not take isostatic adjustments into account in their model runs. Other studies on Late Glacial and Holocene wetland dynamics, including those cited in the current manuscript, accounted for isostatic adjustment by e.g., using the Peltier et al. ICE-4G crustal model as a boundary condition for coastlines and topography.

Page 4820, line 21-22: Again, this the model-data comparison shown in figure 4 is not particularly convincing, the mismatch being more than 20% at both the high and low ends of the observations. If the authors feel that this is "reasonable agreement" then they need to provide some more detailed explanation as to why this is acceptable, and speculate on the processes that may be missing or not properly simulated that are causing the model-data mismatch. This is especially important for guiding future efforts to guide model development and improvement.

Page 4821, lines 16-20: Once again, I disagree with the authors that the model-data comparison in Figure 5 shows "good agreement". As the authors state, because of averaging over larger gridcells, the comparison between site-specific measurements and larger gridcells is expected to not work particularly well. Therefore I would suggest, for making these kinds of model-data comparisons meaningful, running the wetland model in "point mode", i.e., by using in-situ topographic data, meteorology and other site parameters wherever possible. Perhaps another way to quantify the potential error in the model results would be to show "uncertainty bars" on the model output shown in Figure 5, by simulating the same quantities in a series of model sensitivity tests.

Page 4821, line 26-27: Can the authors provide a reference to support the model simulation that 6m of peat accumulation in Eastern Europe is a reasonable amount? Peat depth is a very simply and quickly measured quantity, and an enormous body of literature contains measurements of peat thickness, especially from paleoecological sampling of peat bogs. A very useful analysis here would be to evaluate the model

BGD

8, C2048–C2052, 2011

simulated peat depth against observations, similar to what is done in Figure 5 for accumulation rate. It would also be helpful to see maps of the model simulated peat thickness at 8 ka and 0 ka (even as a supplementary figure).

Page 4822, line 25: This statement on the seasonality of wetland area is not so much of an issue. In boreal peatlands, fluctuations in the water table are relatively small, so we would expect the "permanent wetland area" to be close to the "maximum wetland extent". The more important distinction to be made, and what is not properly presented in the satellite inundation datasets, is that during part of the year boreal wetlands are frozen. They are still close to their seasonal maximum inundation extent, but the satellite inundation datasets sense only liquid water inundation, and not permanent wetland areas that frozen or snow covered.

Page 4824, line 21: As the model is unable to simulate the formation of the HBL, and simulates a reduction on wetland area in the southern part of Western Siberia, I disagree that the authors' results are "quite reasonable". Missing these key boreal peatland areas makes me suspicious about all of the estimates of carbon uptake that are presented. To improve the paper, I suggest turning down the rhetoric a bit, and acknowledging the severe deficiencies in the model result, perhaps by showing some back-of-the-envelope calculations to quantify the effect that missing out the simulation of the HBL has on the total estimate of C uptake into peat.

Page 4824, lines 25-27: Provide reference(s) to explain the timing of late-glacial and Holocene wetland development.

Also, in this paragraph the authors do provide an explanation for some of the model deficiencies, but I do not see how, by ignoring key land surface processes, in particular isostasty, they can put any faith in the model results. If the authors want to say that their model simulations represent a "slight underestimate" in carbon uptake over the last 8 ka, they should, at very least, provide a more empirical synthesis of peat accumulation in the key wetland regions of the world that are not properly simulated by the model,

BGD

8, C2048–C2052, 2011

including the HBL, and explain why their model results would still be reasonable in light of the "missing" peatlands not simulated in this study.

Page 4825, line 20-24: Finally, given all of the uncertainties and deficiencies in their model results, this statement about the time trend in atmospheric CO2 concentrations over the Holocene is totally unjustified. Perhaps with a more convincing model, put through a more rigorous evaluation, and with results that better reflected what is well known regarding the geographic distribution of Holocene peatlands at the present, it would be justified to draw a conclusion regarding the global carbon cycle as a whole. Otherwise, I would suggest simply leaving any statement about the Holocene CO2 trend out of this paper, and simply focus on the main result of peatland carbon accumulation without speculating on its importance for the global carbon cycle.

Interactive comment on Biogeosciences Discuss., 8, 4805, 2011.