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Discussions

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

Anonymous Referee #2

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In this manuscript, Kleinen et al. describe a new model that simulates wetland extent and peat accumulation in northern high-latitude regions. The wetland module was developed on the basis of LPJ (a well-known dynamic global vegetation model) through modifying hydrology and carbon dynamics routines. The hydrology was implemented using a topography index/statistics as in the TOPOMODEL framework. The hydrology model was used to determine wetland area and water table position. The soil carbon dynamics in LPJ was modified to include the layered structure in peatlands as associated with water table and aerobic conditions. The modified LPJ model was then used to simulate wetland dynamics over the last 8000 years by coupling with CLIMBER2, a well-established EMIC.

This is indeed a welcome attempt to implement a wetland model in a global climate-

carbon cycle modeling system (CLIMBER2-LPJ) for possible long-term simulations. In general, the rationale and assumptions as presented for implementing certain aspects of wetland/peatland dynamics are reasonable, and the simulation results are in a right range by comparing with available data/information. However, the simulated wetland/peatland distribution is obviously biased in some regions. The authors discuss the underlying reasons for some of these discrepancies or biases, but more discussion would improve the manuscript. Overall I think that the model and paper represent a noticeable advance along this line of research, though many improvements could be made in the continued development of the model. I make some specific comments and suggestions below for authors' considerations.

Specific comments: Abstract Page 4806, line 3: Should it be "since the end of last glacial", or "since the last deglaciation"?

p.4806, l10-11: as the authors discuss both wetland and peatland areas, it would be useful to general readership of the journal if the authors define/clarify the difference between wetlands and peatlands. In the abstract, peatlands can be simply described as peat-accumulating wetlands with relatively stable water table position, while wetlands include marshes, etc, that accumulate little peat. More detail could be provided in the main text.

p.4806, l.14: typo "decoposition".

p.4806, l.16: More discussion is probably preferable at the end of the abstract about the simulation results: How the results compare with the observations/data or from other modeling results? Particular regions with overestimates or underestimates? What do we learn about the role of peatlands in the global carbon cycle from the simulation results? As now, only the last sentence makes a brief statement of the simulation results.

Introduction In general this well-written Introduction provides a concise background and rationale for the new model and study.

p. 4806, l. 19-20: I do not think Gajewski et al. (2001) is an appropriate reference for the stated peatland C pool of 450 PgC, as Gajewski et al. cited Gorham (1991, Ecological Applications) paper for this pool size and they provided no independent estimate in their 2001 study.

p.4807, l. 26: change "sophistication" to "details"?

p.4808, l.19: change "over the course of the Holocene" to simply " over the Holocene"

Model description p.4810, l. 25-26: I think the approach using a constant "catotelm formation rate" may introduce unrealistic acrotelm peat accumulation history. In terms of peatland dynamics process, the acrotelm is the peat layer above the lowest water table depth that peatlands often experience in late summer. So the thickness of the acrotelm is a result of peatland hydrology and is determined by water-table depth and presumably climate (along with internal feedback within the peatlands). Perhaps the time scale is more likely decadal scale because it needs more than a year to "collapse" the freshly accumulated plant tissues. So it may be more realistic to somehow define the acrotelm-catotelm boundary on decadal or bi-decadal timeframe using the lowest water table position. This is just as the authors use 50-year running mean of the results to define peatland extent. See also below for comments on Fig. 7A results.

p.4816, l. 13-14: It is a good idea in practice to distinguish peatlands (a subset of wetlands) from wetlands in general using the minimum inundation area. However, could this be the reason that underestimates peatland areas in NW North America and East Eurasia (Fig. 3), due to their climate and topographic regimes?

p.4816, l. 26: It would be interesting to show the actual areas of wetland (better yet peatland) shrinkage or expansion over time, if these results are coherent and interpretable. Where did it tend to shrink or expand during the last 8 ka and what climate conditions for these changes? We may learn something about peatland dynamics in the future by considering the location and climate conditions that cause change in peatland extent.

Results p.4818, l.25: It is unclear if the results shown in Fig. 3 are the difference in wetland fraction at two points in time between 8 ka and the present. If so, could the apparent increase in wetland area in eastern Canada be due to the presence of ice sheet there at 8 ka (so little peatlands/wetlands back then)?

p. 4819, l. 25: If the climate has been getting wetter and presumably higher water table over the last 8 ka in general, then we would expect to see that the acrotelm becomes thinner (less peat above water table). If so, then the simulated increase in acrotelm carbon over the last 8 ka as shown in Fig. 7a is opposite to this expectation. Could this be due to the approach used for carbon/peat transfer from the acrotelm to catotelm (constant catotelm peat formation rate)? See comments above on this approach.

p.4820, l.21: it would be useful to comment on the site identity for the spread of acrotelm data points as in Malmer ad Wallen (1993) as shown in Fig. 4. Do the data points with high acrotelm peat come from sites in Scotland, as these oceanic raised bogs tend to have deep water table? Or the low acrotelm peat sites are fen sites from Canada or Scandinavia, as these sites would have high water table and thin acrotelm? A bit more specific discussion may help understand why the simulated results show less spread (most of these are in a narrow range close to mean value). Again, this may have something to do with the limitation of TOPOMODEL approach or the catotelm peat formation rate approach. Or the generic peatlands in the model is more like a hybrid of fens and bogs.

p.4821: I'm curious why the simulated peat carbon tends to be around the North Atlantic Ocean, with much lower peatland carbon density in Alaska and eastern Siberia. Could this be due to the climate or topography? Why the model "doesn't like" to grow peat in these regions? Eastern Canada and NW Europe are also the regions with the large increase in wetland extent during the Holocene (Fig. 3). So it appears that change in wetland extent plays a major role also in peat carbon accumulation, and the authors identify that wetland extent is the most uncertain parameter. If so, the key is to further improve the simulations of hydrology and water table – a challenging task considering the site-specific conditions for peatland/wetland formation and the need of course resolution for global and Holocene-scale simulations!

p.4822, l.7-16: As I indicate earlier, some additional comments on the increase in acrotelm peat would be useful. As now, the model seems to generate all the variability at high and low frequencies in the top acrotelm peat (Fig. 7a) but little variability in the deep peat (Fig 7b). This is very interesting, but requires more thoughts and explanations. Oftentimes we believe that what matter for peat accumulation over the time scales of thousand years is the eventual transfer of peat form the acrotelm to the catotelm, so the variations on peat accumulation at centennial- and millennial-scales should reflect in the catotelm (deep) peat, as often observed from peat core data. Does this have anything to do with the constant catotelm formation rate approach used? In any case, why the variability in the acrotelm hasn't been transferred to the stored catotelm peat?

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