

Reply to reviewers' comments on “A dynamic model of wetland extent and peat accumulation: results for the Holocene” by T. Kleinen et al.

We very much thank the reviewers for their constructive comments, which led to a substantial improvement of our model, as well as model results and the publication. At the same time we want to apologise to the editor and the reviewers that the submission of the revised manuscript has taken so long, though the reviewers will appreciate that the substantial changes we made to model and manuscript could not have been done overnight.

Overall the reviews showed us that our original manuscript was lacking in discussion of the assumptions our approach is based on, as well as discussion of the shortcomings of our approach. In addition, the publication by Fan & Miguez-Macho that reviewer #1 pointed out to us, which we had completely missed since model development had been finished by the time the publication actually appeared in 2011, pointed us to an improved parameterisation of wetland extent that takes permafrost soils into account and generally improves the representation of wetlands.

For the revised manuscript, we have therefore improved the parameterisation of wetland extent in our model, performed additional sensitivity experiments and extended the discussion of the assumptions we make, as well as of the limitations of our approach. In addition, we do take isostatic changes due to the melting of glacial ice sheets into account in a further sensitivity experiment.

Detailed replies to the reviewers' comments are below, with the reviewers' text in bold font and our replies in normal font.

Reviewer #1

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 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.

We are glad to hear the reviewer has high expectations of quality for modelling. Unfortunately, we fear, his or her expectations will very seldom actually be met by global scale models.

With point or local models that are driven by locally measured data, it is easily possible to reproduce measurements, even without any understanding of the processes, by correlating input and output data. Models like this can usually reproduce measurements very well, as long as all relevant input data are measured.

In global scale modelling, on the other hand, we base the model on first principles and our understanding of processes. This generally leads to a much poorer fit to point measurements.

As an example, Beer et al. (2010) compared the gross primary productivity in a number of state of the art process oriented vegetation models to observational estimates based on eddy covariance flux measurements. Their figure 1C shows how the process oriented models compare to latitudinal mean measurements. In tropical areas, modelled GPP ranges from 75% of the measured value to 200%, while outside the tropics disagreements can still be as large as 50%.

We therefore assume our results are reasonable within the context of global models, though we readily acknowledge that there is plenty of room for improvement. For the revised manuscript, we have in fact substantially improved our model and we try to make much clearer what the assumptions and shortcomings of our model are.

Specific comments

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

Quite right, an oversimplification on our part, which we removed from the revised manuscript. In fact we changed the entire line of argument of these paragraphs.

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).

We very much thank the reviewer for pointing out this paper we hadn't been aware of. When comparing their results to ours, we have to make two remarks, though. While Fan & Miguez-Macho *claim* that their approach can be applied in global scale climate models, what they actually *show* in their paper are results for North America that have been obtained at a resolution of 30", or roughly 1 km, and even at that resolution there are some wetland areas that their model doesn't capture well. If we were to increase the resolution of our model to 30" as well, we could surely obtain a much better fit to observations. Unfortunately this would also lead to a 3600-fold increase in computational requirements, which obviously is not feasible. Whether their approach really works on the global scale and at climate model resolutions therefore still remains to be demonstrated.

Nonetheless the manuscript was very helpful, since we could improve our parameterisation for wetland extent in permafrost areas based on their parameterisation.

The elevation above sea level, though, was not a variable we could include. While this would have improved the representation in the Hudson's Bay area, it would have led to an overestimate even larger than already present in Eurasian areas, especially in north-western Russia.

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.

Here we have the impression that there was a slight misunderstanding of what our figure actually showed. Our figure showed the *absolute* change in wetland area as a fraction of the grid cell, not the relative change.

Therefore a change of 0.05 might actually be a 100% change, if the total wetland area happens to be 0.05.

We apologise for not having made this point clear in the originally submitted manuscript, though we hope it will become clear in the revised version. In the revised version, we therefore show figures for peatland extent at 8 ka, with and without ice sheet, for 0 ka, and the difference.

Nonetheless the reviewer is quite right that changes are surprisingly small when compared to published estimates of peatland change. These estimates are fundamentally different from what we show, though: The published estimates show how the number of peatlands changes over time, assuming that each peatland has the same size, to come up with a peatland area change estimate.

Our model determines how wetland extent changes under climatic changes, assuming the present-day topography. The latter, of course, is a problematic assumption to make, since the accumulation of peat *changes* topography. In order to really determine peatland initiation time, as well as numbers similar to the aforementioned estimates of peatland area change, it would be necessary to use the pre-peat topography as a baseline and to determine how topography changes with peat accumulation. We almost certainly would be heavily criticised for the assumptions we'd have to make in such an approach, though it may be the only way forward.

Nonetheless we do discuss the discrepancy between our results and these estimates in the revised text. We have to point out, though, that our statement of "changes as expected" still holds: Our expectation derives from the climatic changes over the last 8000 years, not from published estimates of peatland area change, an oversight on our part.

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 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.

There is a simple reason why we didn't take it into account: It is impossible to do in a consistent way, unless a dynamic ice sheet model is coupled into the system as well. Since we don't have ready access to such a model, we couldn't do it.

Since the reviewer is quite right about the importance of this factor for the development during the Holocene, we now do include this by using data from Peltier's ICE-5G model.

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.

Here the reviewer pointed out a severe problem in our manuscript, which we unfortunately hadn't caught ourselves. From the revised manuscript, we remove the figure and report acrotelm height instead of mass, a measure more familiar to the readers. Since acrotelm height (and also mass) is highly variable and mainly dependent on peatland type and vegetation composition, as well as local hydrology, it would be impossible for our approach that doesn't represent these details to exactly match the measurements, anyway. The mean acrotelm height across all grid cells is 39cm, with a range from 9 to 66 cm being the 5% and 95% percentile – we are quite sure the reviewer will agree that this *is* a reasonable range, though slightly biased towards fens.

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.

We'd be happy to run the model in point mode, if time series of measured temperature, precipitation and water table position were available for a substantial length of time. Since the residence time in the acrotelm is of the order of a century, we would need to run the model for a substantially longer time in order to be able to determine a meaningful acrotelm accumulation rate, which is what figure 5 is all about. Estimates of recent acrotelm accumulation also are very rare in the literature. All in all it would require a couple of centuries worth of measurement data, certainly more than is available anywhere.

Running it in point mode with climate model output to drive the model, on the other hand, doesn't allow an assessment of the shortcomings of the peat model since it would be impossible to disentangle these from the shortcomings of the climate model -- which certainly exist, but aren't the subject of the current paper.

The suggestion of larger exploration of the sensitivity space, though, is highly appreciated, and we perform such a series of tests for the revised paper by varying both the water table and the acrotelm formation rate. Based on this series of sensitivity tests we discuss this figure more extensively,

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 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).

Unfortunately this comparison is not quite as easy to make as the reviewer suggests. The reviewer is, of course, quite right that there are large numbers of peat depth measurements. But what do these actually represent? As Korhola et al (2010) discuss, there is a sampling bias in these measurements, since quite often only the deepest

part of a peatland is sampled. The peatland then grows vertically and spreads laterally, with lateral spread possibly occurring much later than peatland initiation. Such a depth measurement therefore doesn't reflect the average peat height, but only a point height. Our model, on the other hand, assumes an even vertical growth, i.e., we can determine the average peatland height, but not the actual variation with the underlying terrain. In addition, there is a second factor that complicates the matter. Since we only determine the peat *increment* since 8 ka BP, not the total peat accumulated, we would need to compare to the height increment since 8 ka BP. This cannot easily be determined from peat height measurements, since peat accumulated before 8 ka BP will have decomposed since then, actually shrinking the peat column below the 8 ka horizon, as shown by Yu (2011). While this can in principle be disentangled using estimates of the decomposition rate, the measured height increment is critically dependent on this estimate, which is also the reason why we compare our accumulation rate with LORCA estimates, which take this factor into account. We discuss these complications in the revised text and add a figure for the peat increment since 8 ka BP.

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.

Here we unfortunately have to disagree with the reviewer. The reviewer is quite right that we do not discuss the interpretation and the shortcomings of the satellite estimates sufficiently, and we improve on that in the revised manuscript. The reviewer also is quite right that the fluctuations in the water table are rather small *within* boreal peatlands. In how far the same would be true for an entire model grid cell is an entirely different question, though. Our model determines the fraction of a 0.5° x 0.5° grid cell, which corresponds to 30km x 50km at 60° latitude, for which the water table is at or above the surface. This fraction of the grid cell can vary by roughly 50% in high latitude areas, and we very much doubt that the the maximum inundated area as shown by the satellite would correspond to peatland extent. This maximum area occurs right after snowmelt and contains not just peatlands, but also numerous puddles and temporarily flooded areas that are not yet drained since water cannot infiltrate the soil that is still frozen at this time.

In the revised manuscript, we extend the discussion of what is sensed by the satellite, and as part of the sensitivity experiments we use model formulations that use the summer maximum, mean and minimum water table positions to determine peatland area. We regard the extent of peatlands as one of the main uncertainties in our model, as well as the data-based assessments of peatland carbon content, making it important to explore the uncertainty range.

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.

In the revised manuscript we extend the discussion of the shortcomings of our model. In addition, simulation results for Western Siberia, as well as HBL are significantly improved due to the modified parameterisation of wetland extent. On the other hand, as discussed in the initial paragraphs of our reply, even our initial results were quite reasonable by the standards of global scale modelling.

In addition we would like to point out that existing measurement-based estimates of carbon uptake by peatlands published in the peer-reviewed literature are usually based on *point measurements* interpreted as *area averages* scaled up by a *highly uncertain estimate* of peatland extent. Whether that is more reasonable than our assessment is not for us to judge, of course.

We readily acknowledge, though, that there are shortcomings in our assessment, and in the revised manuscript we discuss these more extensively than before.

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

There are no “missing” peatlands in our revised model results, though extent is still underestimated in some areas and overestimated in others. In the revised manuscript we discuss shortcomings of the model more extensively than before. In addition we now do take isostatic changes into account, though their influence really is minor.

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.

Reviewer #2

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.

We realise that discussion of our results is too short in a number of places. This has been considerably expanded in the revised manuscript, and the discussion of model shortcomings / biases is strongly expanded as well.

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

Changed to “during the Holocene”

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.

Extended this point in the abstract, as well as the main text.

p.4806, l.14: typo “decoposition”.

Corrected.

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.

Discussion in the abstract is extended.

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.

We very much thank the reviewer for pointing out this detail about the Gajewski study which we seem to have missed. We are rather surprised to see it in the submitted manuscript, anyway, since it had always been our intention to cite the Yu et al. (2010) estimate at this point. We exchanged it for that.

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

Corrected.

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

Changed.

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.

There seems to be a slight misunderstanding at this point, no doubt due to our inadequate description of model dynamics. The catotelm formation rate is not constant, but rather depends on acrotelm mass and temperature. The acrotelm mass in turn depends on water table position, which determines the fraction decomposing anaerobically, NPP and temperature. The turnover timescale of the acrotelm is on the order of decades, though it can reach century scale if much of the composition occurs under anaerobic conditions.

The large spread

And yes, we *do* define the acrotelm-catotelm boundary on a 50-year timescale a well, using a 50 year running mean of minimum water table position, a point we forgot to mention in the text.

In the revised version, we try to make these points more clear.

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?

With our new parameterisation that takes permafrost conditions into account, the underestimation of wetland areas is much reduced. So it would appear it was mainly due to this factor.

In addition, we changed the peatland definition slightly and explore minimum, mean, and maximum inundation area as peatland definitions in the sensitivity study we perform.

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.

We are sorry, we didn't understand this remark. Isn't this what we show in Fig. 3? Nonetheless, these figures are expanded, and a discussion of published estimates of peatland area change is included in the revised manuscript.

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)?

This should become much clearer in the revised manuscript, where we show wetland fraction for 8ka with and without ice sheet, as well as the extent for present day and the difference. The original plots shows the difference between the present day situation and 8 ka BP.

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.

Figure 7a shows the total carbon stored in the acrotelm for the three cases considered. The slight increase is mainly due to the increase in wetland area over the last 8 ka. A change in wetness leads to two competing effects: If the increase in water table is of a duration shorter than the 50 years averaging time, the water table within the acrotelm is raised, leading to slower decomposition due to anaerobic conditions. If the duration is longer than 50 years, the wetland area increases, leading to a lowering of the water table within the acrotelm.

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.

We have changed the paper at this point, since the acrotelm figure does not help in evaluating model performance. Instead we report the mean acrotelm height, as well as the 5% and 95% percentiles. In our approach, we do not distinguish between bogs and fens, but we rather assume a generic mean peatland which is somewhere in between a bog and a fen. In our approach, acrotelm mass or height is the balance of carbon addition from above, carbon decomposition under aerobic or anaerobic conditions and catotelm formation. Various factors that come into play in a real peatland are neglected in our model:

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!

The peat carbon tends to be around the North Atlantic, since grid cell peatland fractions there tend to be larger than in other areas, though the new parameterisation changes this slightly. In addition, there is a climatic factor involved, too, these areas tend to be wetter leading to both a larger peatland extent and a larger fraction of anaerobic decomposition.

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 from 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?

What is shown in the figure is the total sum of carbon stored in the acrotelm layer. Changes in this total can be due to either changes in peatland area or changes in acrotelm mass. The former is the case here: The overall peatland area increases slightly, leading to the slight increase in total acrotelm carbon. This does affect the rate of total catotelm formation, since the catotelm area changes as well, but due to the scale of the figure these very small fluctuations are not visible.

In the revised paper we remove Figure 7a since it seems to confuse the readers more than giving any additional insight.

References

C. Beer et al. (2010): Terrestrial Gross Carbon Dioxide Uptake: Global Distribution and Covariation with Climate. *Science*, 329, 834-837, doi:10.1126/science.1184984

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Z. Yu (2011): Holocene carbon flux histories of the world's peatlands: Global carbon-cycle implications. *The Holocene*, 21, 761-774.