Overview of revisions

We greatly appreciate the supportive reviews and the helpful comments by both reviewers. In the following, we address each point raised by the reviewers individually. We hope that added and modified text has served to further improve our manuscript. The most relevant modification to the originally submitted manuscript is an extended description and discussion of the choice and relevance of peatland model parameters. Model simulations have not been repeated and all results remain unchanged since the first submission. Below, quoted reviewer comments are indented and in blue font. New and/or modified text is in green font.

Response to Reviewer 1

> One issue that is unclear in the manuscript is the scale the CTI is averaged over (page 4883, line 16, below equation 2). The authors write about "catchment scale", but "catchment" could in principle mean a primary catchment like the entire Mississippi catchment, a secondary catchment like the Chippewa, a tributary to the Mississippi, or a tertiary catchment, i.e., a tributary to the Chippewa. I assume the latter is the catchment scale the authors have averaged over, but this is not quite clear. This issue appears to be rather important, judging from the comments on page 4908 about the differences to previous implementations.

We averaged over primary catchments. This is a simplification in case two pixels exist where $\mathrm{CTI}_i > \mathrm{CTI}_j$, where i lies upstream from j. In this case, the relative floodability of CTI_i is affected by the fact that CTI_j has a low floodability (CTI value), when in effect there is no influence possible (except for blockage effects) as CTI_j lies downstream from CTI_i.

Operationally, this means that the catchment averaging of $\overline{\mathrm{CTI}}_\mathrm{b}$ in Eq. 2 would have to be done only over pixels that lie upstream. I.e., the averaging is different for the two pixels although they lie in the same catchment. However, CTI values generally increase downstream, hence $\mathrm{CTI}_i > \mathrm{CTI}_j$ is not frequent. This is given by the fact that the drainage area, a in Eq. 1, increases by moving downstream. Thus, CTI values increase (logarithm of a in Eq. 1 monotonically increases with a). Moreover, only the CTI distribution at the upper end is relevant for the inundated area fraction. In other words, although two pixels may exist with $\mathrm{CTI}_i > \mathrm{CTI}_j$, it will even be less frequent that $\mathrm{CTI}_i > \mathrm{CTI}^* > \mathrm{CTI}_j$.

Therefore, in our understanding, this simplification will rarely be relevant for the simulated inundation area - the variable we are interested in. We clarified this in the manuscript and provide a short explanation to justify this simplification. Added / modified text reads:

$\overline{\mathrm{CTI}}_\mathrm{b}$ is the arithmetic mean CTI value, averaged over the entire primary catchment area b in which the respective pixel is located. This is a simplification in case two pixels i and j exist where $\mathrm{CTI}_i > \mathrm{CTI}_j$, and i lies upstream from j. In this case, the relative floodability of CTI_i is affected by the fact that CTI_j has a low floodability (low CTI value), when in effect there is no influence possible as CTI_j lies downstream from CTI_i. However, CTI values generally increase downstream (drainage area a increases), hence $\mathrm{CTI}_i > \mathrm{CTI}^* > \mathrm{CTI}_j$ – is not frequent.

Furthermore, the R package also allows to the identification of the river network itself – it might be argued that the river points should be excluded from the CTI catchment scale average, so a sentence clearing up this detail would improve clarity.

The issue raised here not only applies to "river pixels" but to all permanent water bodies for which we could use additional information and omit double-counting as inundated area. In the submitted manuscript, we briefly touch upon this issue by mentioning a "conceptual difference in the nature of the observational vs. model" with respect to the inclusion of permanent water bodies in the comparison with the GIEMS data (Section 7.1.2). The land mask applied in LPX accounts for areas covered by permanent water bodies (and ice) and vegetation is only allowed to grow on a fraction $(1 - f_{\text{icewater}})$ of the gridcell. TOPMODEL predicts inundation that would first occur on areas already covered by permanent water bodies. However, reducing predicted inundation area f by f_{icewater} leads to a general underestimation of inundation. This is because TOPMODEL is imperfect in predicting constant "inundation" in areas covered by permanent water bodies. Thus, this inconsistency is not easily resolved and our approach is a pragmatic simplification leading to satisfactory results for global-scale applications.

In the water table calculation (page 4886, eq. 8), the grid cell fraction f_{oldpeat} is considered as well. A mineral soil with high organic content, which is what f_{oldpeat} would be in the field, tends to have a rather high water holding capacity in comparison to your average mineral soil, which would tend to raise the water table, everything else being equal. Is this considered at all? To my mind itâĂŹs perfectly justifiable to treat it exactly as the mineral soil fraction, but it would be worthwhile discussing this point for completeness.

This is a good point. Indeed, this is (so far) not accounted for in the model presented here. However, we are presenting simulations of a (more or less) "equilibrium" simulation (spinup to constant pre-industrial conditions and relatively small changes from 1900-2012), where peatland retreat is not frequent and this effect does not play a large role. In transient simulations with large climatic shifts and corresponding spatial peatland shifts, this effect may be important in that the enhanced water retention capacity of "oldpeat" soils with a high organic matter content is neglected and the positive water table feedback (see Section 4.4) leads – in this case – to an accelerated *retreat*.

We did not repeat any simulation for the present revisions but are planning to account for altered soil parameters on f_{oldpeat} with higher organic matter content in the next model revision (to be applied to transiently varying climate and CO_2). Added text reads:

Future model development may account for altered soil parameters and water retention capacity on f_{oldpeat} due to an elevated soil organic matter content compared to other mineral soils on f_{mineral}. This may add to the hysteresis behaviour of peatlands when conditions become unsuitable for new establishment during transient simulations.

With regard to the minimum peatland fraction $f_{\text{peat}}^{\text{min}}$ (page 4888, line 15), the reader is left wondering how much of an impact it really has. Since the area fraction is extremely small, I assume it is negligible, but it should be easy for the authors to determine the total carbon stored in the $f_{\text{peat}}^{\text{min}}$s in all grid cells. This will likely be just a few kg of carbon in total, but it would ease the reader's mind about this implementation detail, if the authors could provide the number.

The model simulates a total of 2.9 TgC stored in peatland soils where the peatland fraction is $f_{\text{peat}} = f_{\text{peat}}^{\text{min}} = 0.001$, i.e., the peatland criteria are not satisfied (pt_{crit}=FALSE). This is 0.0005% of the global simulated peat C at 1900 (570 PgC) – indeed a neglibible amount for global C cycle studies. We added this information in modified text in Section 4:

Peatland C balance conditions are simulated for an area fraction $f_{\text{peat}}^{\text{min}} = 0.001\,\%$ in each gridcell globally. This value is small enough not to significantly affect the global C balance (0.0005% of global peat C according to results presented in Section 6), but large enough to provide an effective "seed" for peatland establishment and expansion once conditions for peatland establishment are met (It takes 1158 yr from $f_{\text{peat}}^{\text{min}} = 0.001$ to 1 at 1% yr^{-1} expansion rate, see Section 4.3).

> Finally, on page 4889, the authors introduce the criterion POAET>1 to limit the occur- rence of peatlands to areas with a positive water balance. Here it is unclear over which time frame the authors apply this criterion – I assume it's at least an annual mean, possibly a multi-year mean, since during the summer season POAET<1 over large parts of the boreal area (which contain quite a number of peatlands...).

All expansion/establishment criteria are assessed based on averages over the preceeding 31-years. This is now clarified in modified text in Section 4.1:

All criteria are computed for each gridcell (note that $f_{\text{peat}} \geq f_{\text{peat}}^{\text{min}}$ for all gridcells) for the current year by averaging the simulated C balance variables and POAET over the preceeding 31 yr to remove interannual variability in pt_{crit}.

> With regard to the model evaluation (page 4897/4898), two improvements come to mind which the authors might want to consider (I regard these as "optional"): 1) Maps of the areas of rice cultivation should be available, so it should be possible to mask these areas and thereby disregard them in the model evaluation. 2) Since GIEMS masks areas covered by snow, a similar treatment of DYPTOP results, i.e., removing all snow-covered grid points from the analysis, might improve the agreement between GIEMS data and model results.

We appreciate these suggestions and explored how additional information on snow cover and rice cultivation areas could be included into the GIEMS-DYPTOP comparison. LPX simulates snow cover in terms of water equivalents. We applied a threshold of 30 mm water (in the form of snow), which corresponds to about 100 mm snow depth, assuming a snow density of 330 kg/m^3 (old, packed snow at the end of the winter), to mask out the simulated inundated areas where a significant snow cover is present. This brings simulated and observed inundation areas (by region) to better agreement in March and April (see updated Figure 6 in the main article). However, this also reduces the simulated peak inundation area which occurs in May – June (see NA and IS) and brings it into worse agreement with observations. This is (at least partly) due to the fact that LPX simulates snow retreat somewhat too late in the season (comparison with `http://www.natice.noaa.gov/ims/`), but may also point to the fact that a mutual exclusion of inundation and snow cover presence within gridcells on a $1° \times 1°$ resolution may not be a viable assumption. To keep it as simple as possible (and not having to address model performance w.r.t. snow cover predictions) we decided to keep the Figure as presented in the original manuscript.
As to the inclusion of information on rice cultivation areas, we explored whether we can bring model and observations into better agreement by using, for each gridcell,

the maximum of observational-based rice cultivation area and the predicted inundation area ($f = \max(f_{\text{rice}}, f)$). For the region "IC" (India, China, ...), this works very well and the match in the dry season is excellent. A map of simulated and corrected (by snow masking and additional rice information) annual maximum inundation is shown in Figure 1 below.

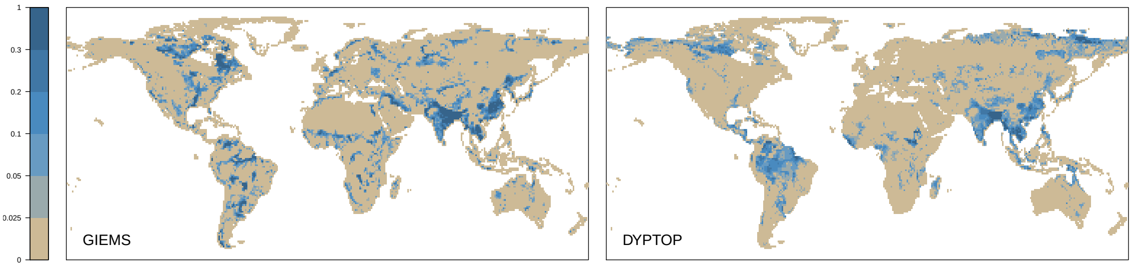

Figure 1: Annual maximum inundated area fraction. Observational data (Prigent et al., 2007) (GIEMS, left) and simulated data (DYPTOP, right). Averaged over 1993 to 2004. In contrast to Figure 5, top row shown in the main article, here we additionally masked out simulated snow covered areas (with cover depth >30 mm snow water equivalents) and added information of wet rice cultivation areas ($f = \max(f_{\text{rice}}, f)$) after Leff et al. (2004); Spahni et al. (2011).

> Figure 1: The legend seems to disagree with the main text, especially page 4883: Here, the authors write about getting the CTI values from ETOPO1, while the figure legend gives the impression the CTI from HYDRO1k is used. Maybe the authors can clarify this.

There was a typo in the figure caption. Correct is that the topography dataset is from ETOPO1, while the R library 'topmodel' was applied to derive the CTI values based on the ETOPO1 topography.

> Figure 2: "Empirical" is not entirely clear. Please clarify that this means the distribution of the original CTI based on the ETOPO1 data.

This is explained more explicitly in Section 3.1: "[...] "empirical" relationship $\hat{\Psi}$ between $\hat{f}$ and Γ. $\hat{\Psi}$ is established by evaluating $\hat{f}$ using Eqs. (2) and (3) for a sequence of Γ spanning a plausible range of values (here from −2000 mm to 1000 mm) and for each gridcell x individually." This explanation also highlights that the "empirical relationship" is *not* based on CTI values fromt he ETOPO1 dataset, as suggested by the reviewer. In the final typeset version of the manuscript, this figure will appear in Section 3.1, we therefore omit an additional reference to this section itself in the Figure caption.

> In addition a few wording change suggestions:
>
> - page 4876, line 4: relied on prescribed fixed peatland maps
> - page 4877, line 9: is above the surface
> - page 4881, line 8: not activated in this study
> - page 4896, line 19: lower than suggested

Done.

Response to Reviewer 2

> My only major suggestion is that the authors may want to consider a more systematic sensitivity analysis for certain parameters or values used, or at least provide further discussion about why the choices of these values would not significantly affect the results/conclusions. In particular, some parameters are not well constrained by observations, such as the minimum of fraction of 0.001%, and the relative areal change rate of 1% per year. I understand some parameter values were determined based on visual comparison of simulation results with available observations, and others by using coarse-resolution model due to computational costs. In any case, some additional justification/discussion on these and other parameters would strength the manuscript.

In our revised manuscript, we extended the discussion of model parameter choices. With regards to the minimum peatland area (seed) of 0.001% and the peatland expansion rate of 1% yr^{-1}, we also refer to our response to reviewer 1. Results presented here are only marginally affected by the choice of aforementioned parameter values. Here, we present simulations with a spinup to preindustrial quasi-equilibrium and the relatively small transient changes in climate and CO_2 throughout the 20th century imply small spatial shifts of peatlands, and hence small influence of processes affected by these parameters. We do not dwell on model predictions of spatial dynamics here. To investigate this aspect, we plan to run the model under transient and large climate shifts as suggested for the last Deglaciation (Last Glacial Maximum – Holocene) in a future study. This is mentioned in Section 7.2.1. Modified/extended text reads:

[...] This approach assumes that expansion is proportional to the peatland area and implies exponential areal growth where the potential peatland area fraction is attained on centennial to millennial time scales after initiation (pt_{crit} switched to TRUE). The choice of these parameter does not significantly affect the results presented here as shifts in the spatial peatland distribution are relatively minor throughout the 20th century. Simulated peatland C storage in gridcells not fulfilling establishment criteria (pt_{crit} =FALSE) is only 2.9 TgC (0.0005% of the global simulated peat C at 1900 (570 PgC)) and is therefore negligible for global C budgets. Further studies could be aimed at [...]

We also extended the discussion of other peatland model parameters. Added text in section 7.2.2 reads:

The mass balance criterium $\frac{dC^*_{peat}}{dt}$ determines whether conditions for long-term peat soil C accumulation are satisfied. This is relevant mostly for peatland initiation (at early stages, the criterium for C^*_{peat} is not satisfied). Additional transient long-term spinups showed that $\frac{dC^*_{peat}}{dt} = 20$ gC m^{-2} yr^{-1} would be too restrictive for North American peatlands to establish (not shown). Our choice of $\frac{dC^*_{peat}}{dt} = 10$ gC m^{-2} yr^{-1} is motivated by observational analyses that suggest that the vast majority of examined peats exhibit long-term C accumulation rates above this value (Charman et al., 2013). The C density criterium C^*_{peat} is not independent from $\frac{dC^*_{peat}}{dt}$ as it reflects a time-integration of the latter. I.e., after millennia of sustained peat C accumulation, soil properties are sufficiently altered and the land qualifies as a peatland even when $\frac{dC^*_{peat}}{dt}$ is too low. This is relevant when conditions become unfavourable for new establishment and introduces a hysteresis effect. The choice of $C^*_{peat} = 50$ kgC m^{-2} is chosen to reflect typically observed peatland soil C contents (Tarnocai et al., 2009). However, the variability is large. Again, the choice of this value is not critical for the results presented here where the vast majority of peatlands have soil C contents greater than 100 kgC m^{-2} and no large climate shifts are affecting the peatland distribution.

For a documentation of the parameter exploration of M and CTI_{min}, we now refer to Stocker (2013) in the manuscript .

> **Specific comments:**
> Title: Change "spatio-temporal" to "spatiotemporal"?

Both spellings are used. E.g. the Oxford Dictionary suggests "spatio-temporal" (`http://www.oxforddictionaries.com/definition/english/spatio-temporal`). We left it unchanged.

> Page 4876, line 11: Delete "Here" or change to other wordings, as you use "Here" earlier in the abstract already.

Done. We simply dropped the word "here" on l.11.

> P4877, l15: change to "40-50 Tg CH4" if that is the case, as the later part of sentence refers to C

Done. In fact, it's Tg CH_4-C (only the weight of the element C is counted).

> P4878, l11: change "area specific fluxes" to "flux rate per unit area" or "flux intensity"?

Done.

> P4879, l7: define f here in its first use (="inundation area fraction") to increase readability.

To clarify this, we reformulated this sentence to "the area at maximum soil water content is used as a surrogate for the inundated area fraction, thereafter referred to as f."

> L9-11: rephrase this 2 sentences, such as "Here we present an implementation of TOPMODEL by ... We term our module/implementation ... DYPTOP." The readability can be improved with rephrasing.

To improve redability, we write the meaning behind the acronym (DYPTOP) in italics: "Here, we present the *DYnamical Peatland model based on TOPmodel* (short DYPTOP)."

> P4881, l20: change "sphagnum" to "Sphagnum" (also italic). L25: do you mean "deciduous" by "raingreen"?

Done. Yes, this refers to "deciduous".

> P4885, l2: change to "...in R (R Code Team, 2012)" (similar typo in Figure 1 caption) L24: change "Sect." to "Subsection"? Distinguishing Sections and Subsections in the text may help increase the clarity.

It is actually correct to refer to "R Core Team" (see function `citation()` in R).

> P4885, L24: change "Sect." to "Subsection"? Distinguishing Sections and Subsections in the text may help increase the clarity.

Copernicus requires references to sections and subsections to be referred to as "Sect.".

> P4888, l15: minimum fraction of peat at 0.001%: How sensitive would it be if using different value other than 0.001%? What is the impact on the global C balance using different values? Is it the smallest fraction to make "peatland seeding" effective? Some discussion on how this prescribed values would affect results/conclusions may be useful.

See above and response to reviewer 1.

> P4889, l10-15: Again, it would be useful if additional sensitivity analysis is done to make sure that the selection of specific values for these parameters, such as 10 gC/m2/yr, 50 kg/m2, or 31 years, would not affect the results/conclusions. I don't think we have empirical observations on the limits for peatland persistence, so sensitivity analysis or additional discussion would increase the readers' confidence and strengthen the manuscript.

We added text to justify and discuss our parameter value choices for C^*_{peat} and $\frac{\mathrm{d}C^*_{\mathrm{peat}}}{\mathrm{d}t}$ (see above). The choice of the averaging period being 31 years is twofold: First, 30 years (1961-1990) is the common time period over which meteorological variables are averaged to derive mean modern climatologies (`http://www.wmo.int/datastat/wmodata_en.html`). Second, during spinup, the climate is held constant (but including stationary inter-annual variability) at preindustrial levels whereby 31 years are recycled. These 31 years correspond to 1901-1931 in the CRU TS 3.21 data set, see section 5.2. By averaging over this period (also relevant during the spinup, see Section 5.1), we eliminate inter-annual climate variability and largely eliminate variations in $\mathrm{pt}_{\mathrm{crit}}$ within individual gridcells during the spinup. This is now made clearer by added/modified text:

[...] by averaging the simulated C balance variables and POAET over the preceeding 31 years. This is to reduce interannual variability in $\mathrm{pt}_{\mathrm{crit}}$, which is driven by interannual variability in climate (a 31-years time series is repeatedly prescribed during the spinup, see Sect. 5.2).

> P4890, l17-18: change to "...allocated to f_{oldpeat}, and re-expanding peatlands first expand into f_{oldpeat}." (add a "," between two phrases separated by "and").

Done.

> P4896, l7: change "but are biased low..." to "but are underestimated..."

Done.

> P4897, l119-26: Good point about human modification on rice paddies. The authors may want to point out more explicitly that some rice paddies fields were constructed on the mountain slopes as terraces, so certainly topographic analysis (as modeled here) would not consider these slopes (some times quite steep) as wetlands, but satellites as in inundation dataset can still see.

We included this suggestion. Modified/added text in Sect. 6.1 reads

[...] This has to be interpreted with regard to the fact that anthropogenic modifications of the land surface in areas of wet rice cultivation increase the flooded area beyond naturally inundated regions (e.g., rice paddies constructed on slopes). This anthropogenic extension of flooded areas is most relevant in the wet season, while in the dry season, rice paddies are commonly drained, resulting in an amplification of the seasonal amplitude. Additionally accounting for information on rice cultivation areas improves the agreement between modelled and observed inundation areas in region IC (dashed line in Figure 6).

> P4899, l6-10: maybe reword the sentence(s) here to use either phrases or full sentences, rather than a mixture of both for items (i), (ii) and (iii). Something like: "the following three criteria/steps: (i). . .."

We re-formulated this sentence. Modified text reads:

As outlined in Sect. 4, the distribution of the peatland area fraction f_{peat} is simulated as the combination of (i) the suitability of climate and peatland vegetation growth conditions for long-term C accumulation in soils, (ii) the flooding persistency, and (iii) the effect of peatland presence on the regional-scale hydrology by imposing a positive feedback on the extent of peatlands.

> P4903, l7: change to "Ringeval et al. (2012)" (misplaced "(") L13: change to "Note, however, that M. . ." (add a "," before "however")

Done.

> P4904, l10: change "E.g., " to "For example,". Similar change can be applied in several other places in the text, such as page 4907, line 14.

Done, we changed some of these "e.g."s.

> P4909, l10-11: any reference to the values used (for example, 1% per year)? Or further comments on the sensitivity of the simulation results/conclusions to the specific value used. See comments above on sensitivity analysis.

See discussions above.

> P4910, l17-18: maybe indicate here which three additional PFTs to represent peatland beyond boreal regions. Also, it would be useful to readers if the authors also briefly state the PFTs used for boreal peatlands in Spahni et al. (2013).

This is described in Sect. 2: "Peatland vegetation is represented by *Sphagnum* (moss) and *Graminoids* (sedges). [...] In contrast to earlier studies of Spahni et al. (2011, 2013), we include three additional PFTs on peatlands. These inherit properties of the tropical evergreen and tropical deciduous tree PFTs and the C4 grass PFT (see Sitch et al., 2003), but are adapted for flood tolerance (Ringeval et al., 2014). Additionally, we removed the upper temperature limitation of the other peatland-specific PFTs, already used in previous studies (*Graminoids, Sphagnum*) to permit their growth outside the boreal region."

> Comments on figures: Figure 1. -Figure caption about Step 3 is a little unclear. If "$(v, k, q, f^{\max})$ are prescribed to LPX-Bern" (used as input as indicated on page 4885 line 9), then would it be useful to add a line/arrow in the flowchart to link "fit parameters" box with "LPX" diamond? -in figure caption, state what the brown double-lined arrow stands for "feedback of peat growth on water table". The authors mention this in the text, but not here in figure caption.

We added a reference (number 7) to the red (brown) arrow that represents the peatland-water table position feedback. With regards to the additional arrow to the "LPX" diamond: We decided to illustrate all steps related to the TOPMODEL implementation separate from LPX because no direct feedbacks (except via f_{peat}) exist between f and any state variable within LPX. f is simply calculated as a function of Γ and fit parameters $(v, k, q, f^{\max})$. Figure 1 also visualises that the TOPMODEL implementation presented here may be treated as a diagnostic that can also be applied offline to the outputs of LPX or any other model that predicts Γ (after the simulation is completed) – with no difference to the results so long as peatlands are not included in model simulations (see Sect. 7.1.5). The fact that the read-in of $(v, k, q, f^{\max})$ and the calculation of f is actually implemented within the LPX framework is only a technical aspect. Conceptually, we think it's better to represent this as is and we decided not to change the figure.

> Figure 2 -caption: change “industrial period” to “instrumental period”? -is that also for the 31-year period (1982-2012)? If so, indicate so.

We modified this sentence for clarification: “Vertical blue lines illustrate Γ for each month as simulated by LPX for the period 1901–2012 (see Sect. 5.2).”

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