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. We also added information to Figure 6 (seasonal variations of inundated area for six regions). 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 $1^\circ \times 1^\circ$ 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.

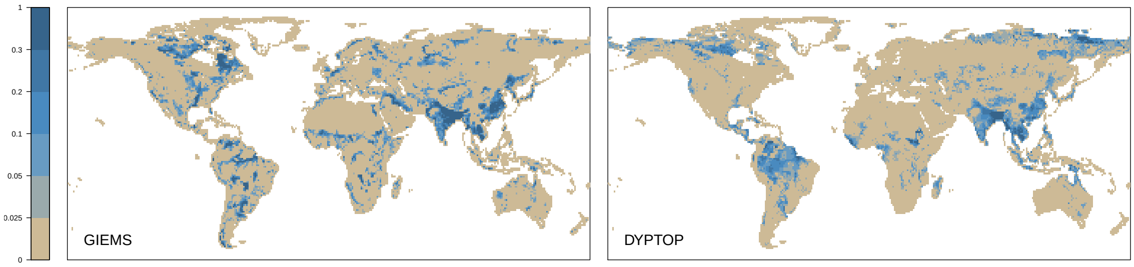

Figure 1: Annual maximum inundated area fraction. Observational data (Prigent et al., 2007) (GIEMS, left) and simulated data (DYPTOP, right). Averaged over 1993 to 2004. In contrast to Figure 5, top row shown in the main article, here we additionally masked out simulated snow covered areas (with cover 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:
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> - 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.

References

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