Response to reviewer 2 (response in red).

We thank Reviewer 2 for taking the time to review the paper. We believe that the majority of the comments from reviewer 2 can be addressed relatively easily through changes to the text, namely additional caveats and discussion, combined with more tentative language (especially for sections 3.3. and 3.4).

As noted by the reviewer, the most uncertain aspect of our results is the future projections. However, it is largely very recent research (Hubau et al.,2020; Jiang et a., 2020 both in Nature and published after the submission of our paper) which brings our future projections, specifically the effect of $CO_2$ fertilization into some doubt. Indeed, these findings could lead to no less than a paradigm shift in the current generation of land-surface models. Note that until recently, field observations (Lewis et al., 2009, from 1968 and 2007) largely supported our results

Rather than removing this section altogether, we offer to keep our full results while adding further discussion and caveats, centred around these new papers. We feel that while future projections of NPP in response to rising $CO_2$ are highly uncertain in tropical forests where there is no elevated $CO_2$ experiment (none published thus far, though an Amazon FACE experiment is underway) and where nutrient limitations may decrease $CO_2$ induced enhancements, the increase in the export of carbon to the LOAC and coast (the main/ highlighted result of the future projections) remains an interesting result, and one which is also due to other drivers such as climate change. As such we still feel that these results merit inclusion and discussion, albeit with additional caveats/ discussion reiterating the uncertainty of future projections, and centred around these new publications. We also propose that we change some of the language in this section of the paper (as well as Abstract) to be more tentative, to reflect these new publications. Please see response to detailed comments below.

Introduction L44-58: Though based on published papers, the estimates of carbon stocks and fluxes in the forests and soils of the Congo would benefit from a more critical evaluation given the logistic difficulties and paucity of data for the region.

We take this point and will provide a more critical evaluation of the current published estimates

L68- 73: It is important to mention that a considerable portion of carbon being processed in the 'land-ocean aquatic continuum (LOAC)' is derived from NPP within the aquatic systems, not just carbon derived from uplands.

We will modify the manuscript in line with this comment

L73-74: To support the statement that 'The tropical region is a hotspot area for inland water C cycling' it would be more appropriate to cite results from empirical studies, rather than modelled estimates.

We will modify these lines to cite results from empirical studies over modelled where possible such as Richey et al. (2002, Nature), Rasera et al (2013, Biogeochemistry), Abril et al (2014, Nature) and the various papers by Borges et al.

L81-82: How well are the current fluxes known?

There are still considerable uncertainties associated with the current fluxes and will change wording to reflect this, adding uncertainty ranges where possible.

L86-92: These are rather ambitious goals, given the large uncertainties in current conditions and paucity of historical and current data.

We agree that these are ambitious goals given the uncertainties and paucity of data. However, we would argue that it is still better to present the full results, but with the caveats up front (including in abstract) along with uncertainty ranges.

Methods ORCHILEAK is a valuable modification to the land surface model, ORCHIDEE, and is well described in Lauerwald et al., 2017. Given that 'All of the processes represented in ORCHILEAK remain identical to those previously represented for the Amazon ORCHILEAK', the veracity of the model for the Amazon would need careful evaluation before accepting its use in the Congo. It is outside the scope of this review to revisit issues, some of which were noted by the authors, with regard the application to the Amazon. However, it is misleading to state that 'ORCHILEAK model . . . is capable of simulating both terrestrial and aquatic C fluxes in a consistent manner for the present day in the Amazon and Lena' without caveats and limitations acknowledged.

We accept the point that it is a misleading sentence without caveats, and will acknowledge the caveats and limitations within these sentences.

Moreover, the differences between the Congo and Amazon would seem to require thorough considerable before accepting identical application. As described in Borges et al. (2019): The Congo basin has a wide range of tributaries with differing lithology, soils, vegetation and rainfall in their catchments, has extensive peat deposits, and has large areas of year-round inundation. These conditions differ significantly from the Amazon basin.

We accept that the conditions in the Amazon and Congo are very different, though the Amazon also has been shown to contain significant peat deposits-( see for example Draper et al, 2014 and is expected to have larger 'undiscovered peatlands' Gumbricht et al., 2017), and also a wide range of tributaries with differing lithology soils etc, as well as a large east-west precipitation gradient. We would debate the term 'identical application' as we recalibrated the model as fully as we could with the available data, under the current model structure (admittedly with associated limitations and caveats).

L111: Camino Serrano 2015 is not listed in references. In Lauerwald et al., 2017 this reference is listed as - Camino Serrano, M.: Factors controlling dissolved organic carbon in soils: a database analysis and a model development, Universiteit Antwerpen, Belgium, 2015. This is not readily accessible.

Thanks for pointing out. The corrected reference is Camino Serrano et al (2018, GMD). Noted and will change accordingly

L124: Why is the water surface area varied diurnally?

This is the time step for the routing scheme of ORCHILEAK and the surface area varies with discharge.

Figure 1. The figure needs latitudes and longitudes indicated. Lake Tanganyika is drawn as if a loop of rivers; redraw as a lake.

We will add lat and long, and modify the representation of Lake Tanganyika

Figure 2 and associated text (L153-168) do not consider the veracity of these data. Though 13 plant functional groups (pft) are prescribed, how well are their ecophysiological characteristics in the conditions of the Congo known? ‘Tropical broadleaved raingreen trees’ is an odd phrase. Section 2.2: Given the importance of the wetlands to the modeling, further discussion of datasets used is warranted.

We take these points and will add some evaluation of the data veracity where possible, for example in comparison with Haensler et al., 2013.

L177: What is the definition of swamps versus floodplains and how are they distinguished in the Congo?

In terms of determining the maximum extent of swamps and floodplains forcing files, these are taken from the dataset of Gumbricht et al. (2017) and therefore follow his definitions. His definition of swamps can be found in Table 1 of his paper but the main characteristics are as follows: “Usually bound to valleys and plains; planar surfaces. Wet all year around, but not necessarily inundated. Usually tree covered.” This will be clarified in the revised manuscript.

The max floodplain is defined by aggregating all of the wetland categories in the Gumbricht dataset (including swamps).

In terms of the representation in ORCHILEAK, the difference between swamps and floodplains is outlined in section 2.2. See Lauerwald et al. (2017) for further details.

L178: Does inundation of the floodplains require exceedance of ‘bank-full discharge’? See comment about section 2.3.

Yes it does, and bank-full discharge is defined as the median stream flow over the period 1990 to 2005.

L179-180: It is unclear why ‘a constant proportion of river discharge is fed into the base of the soil column’.

ORCHILEAK does not yet explicitly represent a groundwater reservoir. This imitates how rivers and swamps are hydrologically coupled through the groundwater table. This will be clarified in revised manuscript.

Please see section 2.1.2 and Figure 3 of Lauerwald et al. (2017) for a more detailed explanation.

L188-190: Round the MFF to 10%. Is this value the maximum MFF or the mean maximum?

Mean maximum

L193: How are ‘fens’ different from swamps in the Congo?

We have merged the swamps and fens categories from Gumbricht et al. (2017) so effectively they are not different in our study. Irrespectively, according to the Gumbicht dataset there are virtually no fens in the Congo. This will be clarified in the revised manuscript.

Section 2.3: Indeed, simulating the hydrology well is critical. The description of the calibration steps is somewhat confusing. For example, line 217 states ‘Without calibration, the majority of the different climate forcing model runs performed poorly .’. However, key hydrological parameters needed calibration. Hence, it would seem issues with both forcings and model parameters are confounded.

Virtually all hydrological models require calibration through the modification of model parameters. Admittedly, the forcing datasets generally do not perform as well for the Congo as for the Amazon for example (likely a result of more climate data being available for the gridded climate forcing fields in the Amazon), which is why we tested several different climate forcing data to estimate uncertainties. The performance without calibration would not have been acceptable/ reasonable, and we feel that we stuck the right balance between improving river flow simulation and over-calibration of parameters, keeping in mind the limitations of climate forcing datasets, and that we are calibrating/ validating for 3 quite different situations (the main stem of the Congo, a much smaller tributary, and overall inundation area).

We will make the explanation of the calibration clearer in the manuscript.

L233-240: The concept of bank-full discharge as a threshold for initiation of inundation of floodplains is questionable as applied to tropical floodplain such as those in the Amazon or Congo. Studies inundation dynamics in the Amazon with detailed measurements or modeling indicate that inundation occurs more or less continuously as the rivers rise and that the water comes from both the rivers and uplands (e.g., Lesack and Melack 1995 Water Resources Res 31:329–334; Bonnet et al. 2017 Hydrol. Processes 31: 1702–1718; Rudorff et al 2014 Water Resources Res 31:329–349; Ji et al. 2019 Water Resources Res 54).

While we appreciate this point, we would respectfully debate some of your conclusions. For example, in their paper Bonnet et al. (2017) conclude that "The mainstream was the main input of water to the flooded area, accounting on average for 93% of total water inputs by the end of the water year. Direct precipitation and runoff from uplands contributed less than or equal to 5% and 10%, respectively. The seepage contribution was less than 1%". They go on to explain that in their model "Diffusive overbank flows occur where the mainstream water level is above levee crests."

Similarly, Rudorff et al. (2014) conclude that "Diffuse overbank flows represent 93% of total river to floodplain discharge"

It is true that Lesack and Melack (1995) find a much higher percentage of inflow coming from runoff (57%) but this is the results from a single case study of a small lake in the central Amazon basin.

Moreover, the majority of the wetlands which we represent in the Congo in ORCHILEAK are swamps, and so do not rely on overtopping at bank-full discharge.

L248-249: The algorithms used to generate the GIEMS vary in their effectiveness depending the density and extent of the inundated vegetation. Section 2.4.1: How well do the soil processes derived for Europe (Camino Serrano et al. 2018) apply to the Congo, how were the passive, slow and active pools determined and how were the decomposition rates in the flooded and non-flooded soil derived?

One of the main limitations which Camino Serrano et al. (2018) identified with the potential application of ORCHIDEE-SOM to the tropics was the lack of representation of DOC coming from throughfall, which is incorporated into ORCHILEAK (Lauerwald et al., 2017). The other main limitation in applying it to the Congo is the lack of an explicit representation of peatlands which we discuss in detail (see lines 646 to 682).

The active passive and slow pools are explained in detail on page 3832 of Lauerwald et al. (2017) but the main part of the text is as follows " The soil carbon module distinguishes 3 different pools of DOC depending on the source material: active, slow and passive (Camino Serrano et al, 2018 - GMD). The DOC derived from the active SOC pool and metabolic litter is assigned to the active DOC pool, while the DOC derived from the slow and passive SOC pools are assigned to the slow and passive DOC pools, respectively (Eqs. 43–45). A part of DOC derived from structural plant litter, which is related to the lignin structure of the litter pool (Krinner et al., 2005), is allocated to the slow DOC pool, while the remainder feeds the active DOC pool. The proportion of the decomposed litter and SOC that is transformed into DOC instead of CO2 depends on the carbon use efficiency (CUE), set here to a value of 0.5 (Manzoni et al., 2012). Taken that the same residence time for the slow and passive DOC pools is used in ORCHIDEE-SOM (Camino Serrano, 2015), we merge these two pools when computing throughfall and lateral transport of DOC. Thus, the labile pool is identical to the active pool of the soil carbon module, while the refractory pool combines the slow and passive pools. The labile (FTF,DOClab ) and refractory (FTF,DOCref ) proportions of throughfall DOC are added to the active and slow DOC pools of the first soil layer, respectively" We acknowledge the fact that these modelled SOC pools are not measurable, as in any land surface model, and there is no sufficient radiocarbon age data in Congo to accurately calibrate SOC turnovers in the model.

Moreover, note that in ORCHILEAK decomposition rates of SOC, DOC and litter in flooded soils are 3x lower that of those in non- flooded soils. This is based on the findings by Rueda-Delgado et al., (2006) but also supported by additional research such as Dos Santos & Nelson., (2013).

Section 2.4.2: What were the projected land use changes? These would seem rather difficult to prescribe, as noted in the text. The exclusion of shifting cultivation would seem a serious omission.

The main land-use changes are detailed in Figure A2 of the Appendix. We acknowledge the fact that exclusion of shifting cultivation is a major limitation, though one which would be difficult to incorporate in view of the lack of a spatially explicit dataset. The LUH1 reconstruction indicates for instance shifting cultivation affecting all the tropics with a residence time of agriculture of 15 years, whereas the review from Heininan et al. 2017 (Plos one) revised downwards the area of this type of agriculture, with generally low values in Congo, except in the North east and South East, but suggested a shorter turnover of agriculture of two years only. In view of such uncertainties, we did not include shifting agriculture in the model. But added in the discussion the possibility to improve this situation using new remote sensing datasets on high resolution land cover change (Tyukavina et al. 2018, Sci. Adv)

Results Section 3.1: In general, simulations of mean monthly discharge for large tropical river systems without large dams at downstream stations has been demonstrated as feasible with several models. Hydrological simulations can become increasingly difficult as the scale decreases, as indicated by the less successful simulations of the Ubangi River. Though the text comparing the GIEMS and simulated inundated areas makes sense, the issue of topography as a factor influencing simulated inundated area deserves mention. L358-362: These judgments should be left to the reader to make.

We accept these points and will revise and remove these sentences accordingly.

Section 3.2: What is the basis for the calculated standard deviations for the fluxes? Figure 5 would be clearer if redrafted larger with simpler graphics. Given all the uncertainties in the modeling and underlying data, Figure 6 would seem quite questionable.

The standard deviation represents the interannual variation across the relevant period (for example 1981-2010. We agree that the results depicted in Figure 6 have large associated uncertainties and therefore will either remove it or make caveats/ uncertainties clearer.

Section 3.3: These results seem premature without a thorough, rigorous evaluation of the model's output under current conditions. Section 3.4: 'The dramatic increase in the concentration of atmospheric CO2 (Fig. 8 g) and subsequent fertilization effect on terrestrial NPP has the greatest overall impact on all of the fluxes across the simulation period' is a critical point and raises a fundamental question about the veracity of the projected changes. As illustrated in a recent paper (Jiang et al. 2020 Nature 580:227-231), the possible CO2 enrichment effects on mature forests are not well captured by current models and need considerably more work to be understood and properly incorporated into models. Figure 9 would be clearer if redrafted larger with simpler graphics. The colors and simple depictions of habitats are distractions.

We agree that the recent paper of Jiang et al. (2020), and in particular the recent paper by Hubau et al (2020 in Nature) (both only published after we submitted our paper) bring into question many projections of the effect of $CO_2$ fertilization using the current generation of land surface models, namely indicating that it is overestimated. Indeed, these recent results could cause no less than a paradigm shift in LSMs and will likely hasten the development of current models. However, it should be noted that the Jiang et al study was on Eucalyptus forest. Also note that until recently, field observations (Lewis et al., 2009, from 1968 and 2007) largely supported our results. Moreover, globally, ORCHIDEE is generally consistent with the global net land sink increase from historical $CO_2$ increase based on FACE experiments -see Liu et al. (2019).

We would propose keeping our full results but adding further discussion and caveats, centred around these new papers. We feel that while future projections of NPP in response to rising $CO_2$ are highly uncertain in tropical forests where there is no elevated $CO_2$ experiment and where nutrient limitations may decrease $CO_2$ induced enhancements, the increase in the export of carbon to the LOAC and coast (the main/ highlighted result of the future projections) remains an interesting result, and one which is also due to additional drivers such climate change. As such we still feel that these results merit inclusion and discussion, albeit with additional caveats/ discussion reiterating the uncertainty of future projections, and centred around these new publications. We also propose that we change some of the language in this section of the paper (and Abstract) to be more tentative, in reflection these new findings.

Discussion Section 4.1: It is not clear that CO2 enrichment effects on photosynthesis results in enhancement of NPP. Though the comparisons of modeled results with regional estimates of biomass and soil C stocks seem reasonable, the empirical estimates have considerable methodological and sampling uncertainty. L500-502: That the CO2 evasion from the water surfaces is sustained by leaching of dissolved CO2 and DOC from soils is not established. In situ C fixation by wetlands and subsequent decomposition of this material could be a significant source of the CO2 evaded as suggested by Borges, and Abril for the Amazon. Indeed, in lines 530-555, the authors discuss the likely contribution of aquatic macrophytes to the available C, and duly note the difficulty of incorporating these plants into their model. However, it is therefore odd that this possible contribution is then discounted in lines 555 to 560.

We think that the tentative language used in L500-502 “Our results suggest”, in combination with the extensive discussion which you refer to (lines 530-555) appropriately reflect the limitations in our conclusions.

We would debate the conclusion that we have “discounted” the effect of macrophytes or at least that was not our intention. We fully acknowledge the important role that macrophytes are likely to play in sustaining $CO_2$ evasion from the water surface. We only conclude that they are likely to have a limited effect on overall NEP, NBP (only these terms). However, we take the point that the language could be changed to make this clearer and the text will be modified accordingly.

Note also, that that ORCHILEAK represents floodplains as sources of $CO_2$ to the inland water network, from the decomposition of litter and SOC, but also though root respiration of plants in that area. Hence, carbon is not only coming from upland soils, but also from wetland soils and vegetation.

L537-539: It is not correct that strong currents limit the abundance of aquatic macrophytes in the Amazon since most of their growth occurs on floodplains where they can cover large areas.

Ok. This is taken from previous literature (Borges et al., 2015, Scientific Reports) but we can remove this.

L570-572: Both these estimates of the % of NPP per year transferred to inland waters are based on the same model. What are the estimates for the Amazon based on empirical data? L572-582: This discussion of differences between the Amazon and Congo is too simplistic and not representative of the relevant conditions in either system. It would best be deleted unless considerable more information is added. Section 4.2: As noted above, it seems a real stretch to be projecting through the 21st century. L610-625: As this section is written as a comparison with Lauerwald et al. (submitted), it does seem suitable to include until Lauerwald et al is available. Also, there are publications that project hydrological and land use changes in the Amazon.

We will remove the small section comparing the Congo and the Amazon but propose adding a few more limited sentences on the Amazon to the proceeding section (636-645)

L626-624: This paragraph does not seem necessary since these systems are quite different from the Congo and other examples could be selected. Section 4.3: Lines 636-645 re-enforce the issues raised above regarding the projections through the 21st century and the question of whether their inclusion in this paper is warranted.

Conclusion L692-696: Is it likely that an increase in DOC from 9.5 to 11.5 mg C/L will cause ecologically meaningful changes in pH?

It is unclear so we can remove this sentence.

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