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Interactive comment on "Historical and future contributions of inland waters to the Congo basin carbon balance" *by* Adam Hastie et al.

Anonymous Referee #2

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Hastie and co-authors application of a sophisticated hydrological/biogeochemical model to the Congo basin is ambitious given the paucity of historical and current data, and large uncertainties in future climates and land uses for the basin. Many aspects of the model and its application raise conceptual and empirical questions. Hence, the veracity of the results would seem much more uncertain than suggested by the manuscript. Before modeling possible changes through the 21st century, it would seem necessary to rigorously evaluate the performance of the model under current conditions. In general, the manuscript would benefit from editorial work on the style, flow and focus.

Abstract The specific numbers for historical and projected fluxes of CO2 and DOC, based on modelled results, should be stated with caveats, not just uncertainty ranges,

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given the many, necessary assumptions and systematic issues with modelled results. This is a fundamental problem with extrapolating backwards and forward when the underlying algorithms and forcings all have serious caveats.

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. 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. 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. L81-82: How well are the current fluxes known? L86-92: These are rather ambitious goals, given the large uncertainties in current conditions and paucity of historical and current data.

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. 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

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the Amazon basin. 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. L124: Why is the water surface area varied diurnally? Figure 1. The figure needs latitudes and longitudes indicated. Lake Tanganyika is drawn as if a loop of rivers; redraw as a lake. 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. L177: What is the definition of swamps versus floodplains and how are they distinguished in the Congo? L178: Does inundation of the floodplains require exceedance of 'bank-full discharge'? See comment about section 2.3. L179-180: It is unclear why 'a constant proportion of river discharge is fed into the base of the soil column'. L188-190: Round the MFF to 10%. Is this value the maximum MFF or the mean maximum? L193: How are 'fens' different from swamps in the Congo? 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. L204-206: The comment 'no data is available with which to directly evaluate the simulation of DOC and CO2 leaching from the soil to the river network' is a useful caveat which makes validation of the coupling of uplands and wetlands to the rivers seriously problematic. L232: '95th percentile of water level heights (floodh95th)' would seem to require information about the topography of the area being inundated. 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

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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). 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? 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. Section 2.6: The terms in Equation 1 would all seem to be quite difficult to calculate and to validate.

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.

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.

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

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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.

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. 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. 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,

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there are publications that project hydrological and land use changes in the Amazon. 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?

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