

Interactive comment on “Seasonal and inter-annual variations in carbon fluxes in a tropical river system (Tana River, Kenya)” by Naomi Geeraert et al.

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A. General comments This paper assesses the carbon (POC, DIC and DOC species) and sediment (TSM) fluxes of the Tana River (Kenya) from 2012 to 2014 in three distinct sides during wet and dry seasons. The authors provide results regarding the dynamics of the above species and their findings are extrapolated to the period 1942-2014. The Tana River has been well studied in previous investigations in terms of sediment mobilization (Geeraert et al., 2015), carbon dynamics (Geeraert et al., 2017), and organic carbon decomposition in relation with $\delta^{13}\text{C}$ isotopes (Geereart et al., 2016) from the same authors which makes this contribution important to the above findings. Nevertheless, I believe that an important part of the information included in this study is already

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presented in 2017 (Biogeochemistry), notably all the part regarding carbon species flux, and the only new thing is the extrapolation of these data to the period 1942-2014. This in my opinion makes the scope of this manuscript too limited and as such the MS lacks of originality. A deeper scientific objective regarding the evolution of carbon fluxes in the future is lacking considering the anthropogenic pressures (as the authors state) in studied area. The present study provided indeed more accurate flux values but as far as I can see from Table 3, it is not clear if there is a significant difference for C-species measurements using the model or not (maybe additional tests are necessary or I missed something in the text?). Moreover, it is not clear if the constructions of dams (1960-1980s; according to Geeraert et al., 2015) are taken into account by the authors to validate their model when estimated past C-species flux. The present data are definitively publishable but not to a high ranked journal as Biogeosciences. I think continental shelf research would be a more appropriate journal for the manuscript. B. Specific comments Abstract: line 21. It is not clear from the text how variations in the discharge regime are related to climate changes, this is not explained further in the MS. Please elaborate. Study area: It is not clear if inundation events occur in the study area especially under high rainfall regimes (although I believe that this is improbable due to the canalization & dams) but the authors have to comment on this. This is important in order to understand if there are interactions between the river water and the surrounding floodplain when you calculate your fluxes. Long-term discharge dataset: It is not clear why the discharge break point was set at 500 m³ s⁻¹?

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