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

Naomi Geeraert et al.

naomi.geeraert@ees.kuleuven.be

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We thank C. Panagiotopoulos for the review of the paper that help us clarify the objectives of the paper and some points of the discussion.

General comments

RC: 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

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(Geeraert et al., 2015), carbon dynamics (Geeraert et al., 2017), and organic carbon decomposition in relation with d13C 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 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.

REPLY: This dataset used in this manuscript is indeed largely the same as our Biogeochemistry (2017) paper, but includes some additional data from previous research within our research group. The scope of the manuscript is, however, entirely different and what we present here could not be included in the 2017 Biogeochemistry paper as this would have made the discussion too complex and because the scope of the new paper is very different. While our previous work discussed in detail the C dynamics during three specific campaigns, here we explore the consequences of our observations (different dynamics during flooded and non-flooded seasons) on the riverine carbon fluxes over a longer time scales. There is, in our opinion, no overlap in the conclusions or take-home message of both papers.

RC: 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.

REPLY: The different parts of this manuscript are building blocks to better assess how C fluxes may change due to anthropogenic pressure. First, we want to point out that no clear contrasts between pre-dam and post-dam sediment concentrations were found, but that major differences are observed depending on whether high discharges occurred with flooding or without flooding (Geeraert et al. 2015). We referred to this as the different hydrological regimes. So for the analysis of the C dynamics, we continued focussing on the different regimes.

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Therefore, in section 3.1 and 3.2, we compare the year 2012 and 2013, because the former is an example of a year without flooding, while 2013 experienced severe flooding. Analysing the seasonal contrasts between those two years provides insights which are useful to predict the effects of human induced changes that would alter the seasonal variation of the discharge (e.g. through the implementation of a different scheme for dam releases). We use the longitudinal variation to assess how retention or release of sediment and C will change when the frequency of flooded years would change. Evidently, such predictions depend on the accuracy of the data available. To assess the potential impact of errors we would make when not including the differentiation between those two hydrological conditions, we investigated the impact of sampling frequency and timing on data quality and predictions (section 4.1).

In section 4.3, we bring all this information together with the expected hydrological changes due to dam construction and climate change so that potential future changes can be assessed.

RC: 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?).

REPLY: The good fit of the data for the period 2009-2011 is due to the fact that there was only one hydrological regime (non-flooded) during that period. If the same method would have been used for the period 2009-2013 (with the measurements from a flooded season included), we would have ended up with an underestimation of the annual fluxes in the non-flooded years (2009-2012) and an overestimation in 2013. This is represented in Figure 9 as the “Single curve” compared to the “Double curve”. This is explained on page 9, l.30-31, but maybe too briefly to be fully understandable. We will therefore adjust our explanations to make this more understandable.

RC: Moreover, it is not clear if the constructions of dams (1960-1980s; according to

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Geeraert et al., 2015) are taken into account by the authors to validate their model when estimated past C-species flux.

REPLY: First, we did not intend to reconstruct the past fluxes, we simply used the historical hydrological record to represent annual patterns in discharge as well as variations in the frequency of high discharge events and used this for our simulations.

There are no empirical data available that would allow to directly estimate C fluxes before the construction of the dams. However, we found no clear differences in the sediment fluxes before and after dam construction (fig. 4, Geeraert et al., 2015), while the pre-dam period also experienced contrasts between flooded and non-flooded conditions. Particulate OC is a relatively constant fraction of the sediment flux and is the major OC fraction transported by the river: this suggests that the conclusion with respect to the impact of different flooding regimes on C and sediment transfer will hold for pre-and post-dam OC fluxes.

RC: 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.

Specific comments

RC: 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.

REPLY: In section 4.3 (p10 L.33 –p11 L. 1), we refer to the IPCC projections for the area which show a tendency for increased precipitation, which would subsequently lead to higher annual discharge fluxes.

RC: 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

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to understand if there are interactions between the river water and the surrounding floodplain when you calculate your fluxes.

REPLY: There is indeed inundation/flooding taking place, as is explained in section 2.2 when the river experiences considerable flooding in the floodplain. High rainfall is extremely rare in the lower Tana valley (referred to in the manuscript as semi-arid environment without tributaries), while the dams are located hundreds of kilometres upstream of our research area. When looking at the fluxes in the river itself, the interaction with the floodplain is present, but it doesn't affect the interpretation of the annual fluxes. The interaction with the floodplain has been examined in more detail in Geeraert et al. (2017, Biogeochemistry).

RC: Long-term discharge dataset: It is not clear why the discharge break point was set at 500 m3 s-1?

REPLY: The break point was set at 550 m3s-1, because that worked best during our initial calculations of sediment fluxes in Geeraert et al. (2015). We decided to use the same value for consistency.

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