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Discussions

Interactive comment on "Distribution, origin and cycling of carbon in the Tana River (Kenya): a dry season basin-scale survey from headwaters to the delta" *by* S. Bouillon et al.

Anonymous Referee #2

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GENERAL COMMENTS

The manuscript by Bouillon et al is an excellent study executed by a group that has consistently produced first-rate results, particularly in understudied tropical rivers and estuaries draining into the Indian Ocean. It encompasses the following key, important findings: 1) Decoupling between DOC and POC; 2) dry-season temporal decoupling of sediment dynamics between lowland mainstem and its feeder upstream mountain streams; 3) high lability of phytoplankton contribution from a reservoir; 4) convincing, multi-faceted evidence for the importance of authocthonous primary production relative to respiration in the mid-to-lower Tana river, despite the high turbidity, in parallel

with only a minor phytoplankton contribution to POC in the mid-to-lower Tana river; 5) exploration of C3 vs C4 vegetation influence as sources of organic carbon fractions and metabolism. The study is characterized by several important factors that make it distinctive: 1) Selection of a very interesting system where the absence of lowland tributary inputs means that downstream changes can be attributed largely to river corridor dynamics, making for an excellent opportunity to isolate processes; 2) focus on a river system representing a system that is poorly studied (tropical sub-arid rivers); 3) an opportunity to study reservoir effects on nutrients and carbon cycling in tropical rivers (not novel per se, but well done and an important part of the Tana). As in previous work by this group, the study is based on high-quality methods and a terrific arsenal of analytical approaches.

Despite these strenghts, it suffers from a lack of focus or set of driving questions. In fact, the Introduction itself doesn't lay out specific objectives for the paper, other than "present data from a large-scale study" from a river system that has some distinctive properties and represents a poorly studied system. As a result, the paper meanders along lots of topics and issues, without a coherent framework or perspective; for instance, on pages 5987-88, asking about whether soil 13C data are representative of the overyling biomass C3 vs C4 distribution (and decoupling this discussion from the one on river POC origin), or whether the DOC concentration data fit the soil-C:N-ratio model for DOC export control, especially when discharge or annual-scale data are not available to calculate DOC fluxes.

As a result of this diffuse focus and very broad scope, the discussion is sometimes fairly superficial and sometimes does not reflect current understanding and recent knowledge. This is despite an Introduction text that is first-rate and state-of-the-art, which could practically be published as is; and an undisputable history of excellent studies presented in excellent papers by this group. There's also substantial repetion of text and statements of data results in the Discussion sections, making the text longer and possibly more tedious to read. Finally, there are several cases where few explanations are offered to explain unusual or significant observations (eg, lack of an altitudinal gradient in soil C:N, in p. 5987; or the 13C-DIC vs. DIC pattern, discussed below).

My recommendation is that the discussion in this manuscript be re-organized and refocused before publication. For reasons already stated, the results of this study should be published in Biogeosciences, and this group can undoubtedly produce a more compelling manuscript. It's possible that asking for an overarching framework or small set of driving questions is an unwarranted demand for novel synoptic surveys like this one; I understand how difficult that can be. I suggest two possibilities: 1, Split the manuscript into two papers, with the main one focusing on organic carbon and metabolism (and the main findings summarized above and already included in the abstract), and a second one focusing on weathering and inorganic carbon cycling, including the bulk of results on 13C-DIC13. The second paper would use the major ion data that were collected but not presented; this is defensible because DIC cycling and sources appear to be dominated by weathering processes and not by biological river processes. 2, Re-arrange the discussion into fewer sections that define a set of clear but broad questions, eliminating material if necessary if it doesn't support the interpretation framework.

SPECIFIC COMMENTS

Because this manuscript attempts to cover many topics, I'm only able to comment on a subset of these topics here.

The DIC and DIC isotopes discussion is included in the section on "Indicators of aquatic metabolism" (sect. 4.2), but in reality focuses largely on the weathering origin of DIC. This focus is warranted by the nature of DIC in this system; but given that condition, the grouping of that discussion with one on metabolism is inappropriate. DIC and especially its isotopes can be used as powerful indicators of metabolism under appropriate conditions, but those conditions do not occur in this system. Also, the discussion on weathering is sometimes shallow or undermined by some misunderstandings. For example, the comparison to the 13C-DIC vs. DIC pattern found by Aucour et al (1999) in

the Rhone is not explored further to yield useful insight. The statement that carbonate weathering would result in 13C-DIC > -5 o/oo (p. 5982, line 21) is not expanded on, and is correct only under limited conditions (eg, carbonic acid derived from C4 biomass). With C3 vegetation and dominant carbonate lithologies, both Aucour et al (1999) and more recently Kanduc et al (2007) clearly demonstrated that carbonate weathering in European rivers often results in 13C-DIC in the range of -8 to -12 o/oo that roughly corresponds to the theoretical prediction under closed conditions. Finally, there are two sites with 14C-DIC values < -500 o/oo; these values are outside the common range of carbonate weathering by modern carbonic acid, and should raise the possibility of other C sources or weathering mechanisms, but these are not discussed (pp. 5982-83). Lithospheric C sources are a strong candidate in this volcanic and mountainous area (eg, Gaillardet and Galy, 2008); carbonate dissolution by sulfuric acid from pyrite oxidation can also produce such 14C-DIC ranges.

Regarding sediment cycling and particulates, the authors state on p. 5986 (lines 5-7) that the 30-fold increase in TSM concentration between mountain and downstream rivers in the Tana is the largest of any river system they're aware of. However, in the transect presented by Aufdenkampe et al 2007 (a study from the Andes to the Amazon lowlands cited often in the work of Bouillon et al), TSM shows contrasts between mountain and downstream rivers just as large as those seen in the Tana. Given the state of understanding of mountain and tropical systems today, such observations, though still important, are no longer surprising. With respect to Section 4.7, it is very safe to assume that the annual-scale inputs of sediments from mountain streams occur episodically during storms (as clearly acknowledged in section 4.4 and cited from previous studies in Kenya), and that such sediment loading will have a different make-up (including most likely a lower OC content) than what was observed in this dry-season study (eg, Townsend-Small et al 2008, though this pattern is commonly observed across systems). I think the community has accumulated enough understanding about temporal variability of sediment and OC in mountain rivers to safely say that the simple estimate made on the first paragraph of this section is flawed (using the %POC/TSM value from mountain streams sampled during this dry-season study to estimate the loss of particle-associated OC downstream). The authors clearly acknowledge that this is a tentative assumption that needs more testing, but I think the opposite assumption or hypothesis is nowadays the more conservative or defensible one. As a compact summary of the issue of coupling of sediment load in lowland rivers vs. mountain rivers feeding them, these statements from McClain and Naiman (2008, p. 334) are very apt: "Meandering lowland rivers maintain their sediment loads by continually resuspending and depositing materials within their channels (Meade et al. 1985,Dunne et al. 1998), effectively mining sediments accumulated in the piedmont over long time scales through discrete depositional events (Aalto et al. 2003). To understand mountain-lowland linkages, one therefore needs to consider erosional processes over a broad range of timescales."

It is worth noting that the overall comparison of sediment dynamics (section 4.4) in the Tana system (120,000 km2 area) to river systems vastly larger in terms of drainage area and discharge (Mississippi, Amazon and Orinoco) seems inappropriate; this important difference in scale should be acknowledged more clearly, avoiding the reliance on ambiguous terms like "large rivers". More broadly, a weakness in this study is the absence of drainage areas and river distances for each site. It limits the interpretation of the data and comparability to other systems. Latitude and longitude should also be added, and from Fig. 1 it seems they area readily available to the authors.

The dispersed discussion on the relative role of C3 and C4 on soil OC and river metabolism would benefit from more coherence, clearer conclusions (even if they're tentative), and maybe references to more diverse, recent studies, such as Wynn & Bird (2007).

The impacts and characteritics of the Masinga Dam are an important and interesting aspect of this study. But given that authors talk about previous work on the Tana, about hydrological, geomorphological and ecological changes to downstream reaches that have emerged as a result of the dam, it'd be interesting to speculate about how those changes may be reflected in the C dynamics observed and discussed here,

downstream of the dam (beyond the impact of a labile pulse of phytoplankton POC). Are POC source and dynamics likely to be different today in the mid and lower Tana river because of the dam?

TECHNICAL CORRECTIONS OR COMMENTS

- The methods description in p. 5968-69 doesn't make it clear whether the N, P and DOC analyses were done on filtered samples. The text on this topic is confusing. From the rest of the paper, it seems clear that filtration was indeed performed, probably as described for alkalinity. But the methods text should be clarified.

- p. 5986 (line 28) - p. 5987 (line 1): Aufdenkampe et al 2007 did not present soil %OC trends, so the citation is inappropriate.

- There are a few errors in references to figures: in p. 5981 (lines 14 & 19), reference to Fig 13 should be changed to Fig. 5; in p. 5985 (line 10), reference to Fig 3 should be changed to Fig. 7

- Fig. 1 would be improved if the site markers were changed to correspond to the symbology used in most figures, distinguishing Tributaries, Masinga Reservoir, and Tana sites.

REFERENCES (except for ones already cited in Bouillon et al)

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Kanduc, T.; Szramek, K.; Ogrinc, N. & Walter, L. 2007. Origin and cycling of riverine inorganic carbon in the Sava River watershed (Slovenia) inferred from major solutes and stable carbon isotopes Biogeochemistry, 86, 137-154, doi:10.1007/s10533-007-9149-4

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