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Discussions

Interactive comment on "Dynamics of dissolved inorganic carbon and aquatic metabolism in the Tana River Basin, Kenya" *by* F. Tamooh et al.

Anonymous Referee #3

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This paper reports on concentrations of DIC, and many of the processes controlling DIC concentrations, along the Tana River in Kenya. Downstream increases in pCO2 and increasing chlorophyll a concentrations with increasing sediment load are reported as unexpected and distinct from results of other rivers. Other results are consistent with previous studies.

The paper certainly contains a wealth of apparently high quality data from a region of the world that is poorly studied. The analysis and interpretation are generally sound, with the exceptions noted in my detailed comments below. One important aspect I feel is lacking in the paper, however, is a discussion of the larger implications of differences observed in the Tana verses other regions of the world. For example, many references are made to the Amazon in the paper but little consideration is given to differences in

light of the vastly different environmental conditions of the basins (humid versus semi-arid, tropical rainforest versus savanna, etc.). Is there more to be noted that would raise the interest of readers in this paper?

Another issue is that the analysis and discussion is weakened by lack of information about organic carbon in transport, which is interpreted to be the source of much respired carbon. I expect these data will be treated in a separate paper, although their inclusion here would have made for a more interesting and balanced story of carbon dynamics in the Tana River.

Detailed comments:

Pg 5184, lines 27-29: What were the light conditions during incubations for determination of P rates?

Pg 5191, line 7: Why the suggestion here (in the first sentence of the discussion) of rock weathering control of downstream increases in DIC when the paper concludes that OM respiration is responsible?

Pg 5192, line 5: The reference to results from Satima springs is a bit confusing. Above reference is made to the high values of del 13C coming from highland streams and the suggestion that high values are due to carbonate weathering. The results from Satima springs are suggested to "reinforce" this conclusion by illustrating the additional increases in del 13C due to CO2 evasion. But Satima springs del 13C starts out at -20.8 per mil, clearly not carbonate weathering and increases to only -8.8 per mil after degassing... still not matching heavy values from Aberdare Range or Mt. Kenya. I don't think the point of enhancing already heavy signals is made with this example.

Additionally, were these Satima springs values reported in the results section?

Pg 5192, line 18: Here the reference to OM controls on downstream del 13C values is made, contradicting the statement in the first sentence of discussion suggesting rock weathering may control downstream DIC concentrations. Perhaps first sentence was

meant to be a suggestion later disproved, but in that case it should be presented as one of multiple possible controls.

Pg 5194, line 4: I don't believe "cover of high organic matter" was reported in results, so reference should be given for this statement.

Pg 5195, last paragraph: The downstream increase in chlorophyll a is indeed counter intuitive. The reference to samples being taken in the euphotic zone is not reassuring because I presume the river is turbulent enough to be well mixed. This may indicate, however, that even occasional (via turbulent flow) exposure to light in large rivers is sufficient to support an accumulation of chlorophyll a for autotrophs that can turn on/and off photosynthesis. The reference to the influence of residence time may support this. Another possibility not mentioned is the inflow of waters from floodplains or side channels during high flows. Could areas like this have been sources for chlorophyll a?

Pg 5197: The absence of any diurnal signal in DO, pH, etc in lower Tana is unfortunate in that it does not support the interpretation of increasing downstream P mentioned above. If residence time is invoked as an explanation for slow accumulation of chlorophyll a overtime can you estimate what the daily accumulation rate should be? Should this increment be detectable in the diurnal experiment? If so then perhaps this is evidence of another, off channel, source of chlorophyll a.

Pg 5197, line 19: Interesting that del 18O fluctuation at Masinga reservoir is nearly equivalent to Chania stream, but no diurnal process is invoked to explain it. Does exceptionally high pCO2 of Masinga somehow mask active P in shallow waters?

Pg 5199, line 18: This final statement is provocative and leaves the impression that something important may have been missed. What is the significance of periphyton dominance in headwater streams for upstream-downstream comparisons? What role might the downstream transport of "eroded" periphyton have on downstream chlorophyll a concentrations? See http://www.sciencedirect.com/science/article/pii/S1642359313000050 for an example

of the downstream transport of algal mats.

Pg 5199, line 25: I don't understand how weathering can still be invoked as the primary process controlling DIC concentrations over the entire river length. In turbulent rivers, aren't DIC concentrations ultimately controlled by saturation concentrations and gas exchange with the atmosphere? Even the TZ/Ca+Mg relationship is ultimately linked somewhat to these atmospheric exchange controls. Can you explain away the atmospheric controls? Also, Figure 5 suggests TA consistently higher than the 2:1 line.

Interactive comment on Biogeosciences Discuss., 10, 5175, 2013.