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Biogeosciences Discussions

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Interactive Comment

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

F. Tamooh et al.

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Anonymous Referee #3 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.

REF: 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

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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 semiarid, 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. Reply: The information on organic carbon transport has already been discussed and published in an earlier article (Tamooh et al., 2012) which we refer severally in the revised version of the manuscript.

Detailed comments: REF: Pg 5184, lines 27-29: What were the light conditions during incubations for determination of P rates? Reply: The incubations were done in-situ and hence, light conditions were similar as during the sampling time. REF: 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? Reply: Our results confirm the contribution of both rock weathering and organic matter respiration as contributing to DIC concentration. We have now more extensively explored in the discussion section of the revised version of the manuscript the possible contribution of both silicate and carbonate weathering. In addition, we also observe high suspended matter in lower Tana River which we believe to be driving in-stream respiration. REF: 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? Reply: We wish to clarify that the gradual increase in d13CDIC is due to the fact that CO2 is more 13C-depleted than bicarbonate and carbonate ions, by ∼8-10‰ (temperature dependent) and not driven by carbonate weathering. Hence, assuming rapid isotopic re-equilibration between different DIC species, CO2 evasion leads to a 13C-enrichment in the remaining DIC pool (as described also in the studies referred to in the relevant section). The Sitima springs values were not reported in the results since they were meant to further support our hypothesis on rapid evasion of CO2 in headwaters. However, the values are summarized in the supplementary tables.

REF: 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. Reply: Organic matter respiration controls d13C values of DIC in lower Tana River alongside both silicate and carbonate weathering. This hypothesis has now been discussed in details in the revised version of the manuscript where we show strong correlations between TA and DSi and the sum of Ca2+ and Mg2+. However, in lower Tana River mainstream high suspended organic matter support the hypothesis on in-stream respiration as a major factor regulating the DIC dynamics. REF: 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. Reply: Nyambene Hills tributaries were characterized by a cover of high organic matter on the river bed up to 17% (Tamooh et al., 2012) and it is possible this may have driven additional in-stream respiration. The reference (Tamooh et al., 2012) to support this argument has now been cited in the revised version of the manuscript . REF: 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 accu-

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mulation 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? Reply : In the Amazon river, floodplain lakes are indeed a source of carbon and chlorophyll a for the main stream. However, we did not actually sample these systems in the present study, hence, we cannot speculate on this. REF: 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. Reply : The amplitude diurnal cycle of DO and DIC is expected to increase with an increase of P and R at community level. However, we report P and R solely in the planktonic compartment and not in the benthic compartment. In the present version of the ms we included the computation of GPP and R at community level at Chania. This shows that the periphyton (benthic) GPP and R are overwhelmingly higher than planktonic GPP and R. Hence, we concluded that the diurnal signal at Charnia is driven by periphyton. In other two sites benthic primary production is probably inexistent because of light limitation due to the turbidity (for Tana) and due to depth (for Masinga dam).

REF: 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? Reply: d18ODO signatures at Masinga dam (+25.4‰ to +28.2 ‰ showed only a moderate diurnal fluctuation but were consistently above the atmospheric equilibrium (+24.2‰ suggesting that aquatic respiration dominated over primary production. REF: 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. Reply: [0]The concentration of periphyton community in headwater streams was so negligible to make any substantial contribution to the downstream sections of the main Tana River channel as reflected by low chlorophyll a concentration, low primary production as well as low nitrate concentration suggesting planktonic community made very negligible contribution despite low turbidity in headwaters. The diurnal fluctuations observed in Chania stream was therefore attributed to be regulated by periphyton community.

REF : 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.

Reply : Gas exchange will only affect CO2 and leave HCO3- and CO3- unchanged. On average for the whole data-set, HCO3- represented 91.9% of DIC, CO3- 0.3% of DIC, and CO2 7.8% of DIC. Hence, gas exchange of CO2 is expected to have a minimal impact of DIC spatial and temporal patterns. Sources and sinks of HCO3- and CO3- are the major controlling factors of DIC, and weathering is a major source of HCO3- and CO3-. In the revised version, we provide a more detailed analysis of TA, taking into account also silicate weathering that can account for most of deviations from the 2:1 line of TA versus Ca+Mg.

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

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