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Biogeosciences 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 #1

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Comments to bg-2013-101:

I think these are important data from a poorly represented landscape that will be a valuable addition to the literature. The use of mass balance, isotopes and attention to seasonality make this a high quality piece of work. The basin scale patterns shown by the authors that differ from the temperate zone are especially intriguing and highlight major differences in the tropics. I do however have a few suggestions that will need to be addressed in order to answer remaining questions regarding metabolism and dynamics, and gas transfer physics. These issues can be addressed with the available data and should not require major reformulations of the manuscript.

General Comments: The attempt to balance gas emissions from metabolism is noteworthy here, but I found the discussion and interpretation to be somewhat problematic.

Since CO2 emissions were measured during the day, P and R should contribute to the drawdown and simultaneous production of pCO2. However, the authors attempt to balance the CO2 flux estimates using R alone. I'd really like to see CO2 emissions compared with net production, and some reference as to the bias in daytime flux estimates could be noted or cited. The discussion of source strength and metabolism (i.e., page 5199) should be framed in terms of net ecosystem production, not just P or R. There is emerging evidence that lakes can be both net autotrophic and significant sources of CO2 to the atmosphere, pointing to the large role of groundwater, which is probably important for streams too (see McDonald et al., 2013; Global Biogeochemical Cycles). CO2 flux to the atmosphere does not necessarily equate to net heterotrophy.

The second main criticism I have concerns the lack of observed diurnal patterns in the lower reaches of the basin. Given that P and R increase downstream, I might expect that diurnal patterns would become more pronounced, not disappear. More discussion and exploration of this phenomenon is needed. If possible, I also suggest that the diurnal dissolved oxygen data from the 24 hour samplings be used to calculate whole ecosystem metabolism to supplement the incubation values for P and R.

There is currently no discussion of physical gas exchange. Like pCO2, there are emerging basin scale patterns such as decreasing gas transfer velocity with increasing stream order (or in this case elevation as a proxy). Given that the authors have CO2 concentrations and fluxes, the gas transfer velocity can be calculated with little extra effort. A presentation and discussion of the gas transfer velocity will help readers determine the potential bias of the chamber technique (which is not yet well understood) and the broad scale patterns of flux in this basin. Where are the physical hotspots? The headwaters as CO2 emission hotspots might be driven by a combination of high transfer rates and/or high pCO2, but here, the headwaters seemed to have much lower pCO2 relative to the main river. A comparison of gas exchange potential from the different systems would add important detail to this variability. If the authors attempt to calculate whole ecosystem metabolism using diurnal dissolved oxygen data (or even better, both DO and CO_2) these gas transfer values can be used to constrain the atmospheric flux in the model structure.

Finally, while the units are generally properly presented, the attention to metabolic mass balance requires compatible units for oxygen and CO_2. I generally think pCO_2 should be presented in micro-atmospheres (or the SI units of pascals which aren't widely used) instead of parts per million by volume (as gases move across pressure gradients), but when comparing with oxygen, the best units would be moles of oxygen and CO_2 corrected for saturation. This would allow for easy comparison of the two gases, and would clearly show the dynamics due to metabolism. If you fit a line to this relationship (i.e., Fig 11) you could possibly calculate the respiratory quotient and determine if it truly is unity. This simple comparison is essentially absent in the aquatic literature and could be very useful for analyzing sources and dynamics of CO_2.

Specific Comments: 5183-28: what bias does the calculation of pCO_2 from TA have? The errors are reported, but I thought pH measurements caused a directional bias (see Butman and Raymond, 2011; Nature Geoscience). 5184-10: present the equation used for flux measurements 5188-17: are the rates umol of oxygen? 5193-24: What is the authors position on the hypothesis that pCO_2 is derived from "production of carbon dioxide from soils?" Since none of the sites' CO_2 fluxes can be balanced fully by respiration, is this the missing source? 5194-3: a whole ecosystem metabolism estimate might show a contribution from benthic sources (which in the headwaters might constitute the majority of respiration). 5194-19: Is there data to indicate physical stratification? 5194-25: Is the dam designed to release hypolimnetic waters? Or do the turbines draw from multiple or variable depths? 5196-5: use consistent units throughout 5196-16: the respiratory quotient is still unresolved, but recent work suggests that it may be closer to 1.2 for freshwaters (see Berggren et al. 2011; The ISME Journal). How would this influence your interpretation of metabolic contribution?

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