Biogeosciences Discuss., 9, C673–C677, 2012
www.biogeosciences-discuss.net/9/C673/2012/

Biogeosciences Discussions

Interactive comment on "Organic matter sources, fluxes and greenhouse gas exchange in the Oubangui River (Congo River basin)" *by* S. Bouillon et al.

Anonymous Referee #2

Received and published: 16 April 2012

This is an excellent study of the Oubangui tributary of the Congo river. It meets a gap in knowledge of rivers in that region, and more generally of tropical rivers that are more widely representative of conditions across the globe. The authors make a good case for the under-representation of African rivers relative to other tropical regions in assessments of carbon fluxes. The study was very well executed, includes a great range of complementary analyses, and represents a robust time series of a poorly studied system. The multiple assessments of lateral carbon flux estimates is a particularly strong component. I strongly suggest the paper be published, after relatively small changes.

One broad comment I'd like to make is about the presentation of goals and of the struc-

ture of the discussion. These areas need some improvement. For example, the last sentence in the abstract feels like a hanging, highly specific comment, not a comprehensive conclusion. At the end of the Introduction (last 2 paragraphs), motivations for the study are listed, but they are not entirely clear:

- Why is having the majority of studies in the Congo from the 1980s and 1990s a problem per se?

- Given all the recent studies cited regarding more detailed geochemical investigations, it looks like the gap on such investigations is much less true today than the statement implies.

- "A greater range of parameters and analyses have been examined": relative to what studies??

- The point about the value of long-term flux datasets is valid, but this manuscript doesn't address that; maybe the intent was to say that the study that's been initiated will meet that need, once it's been in place for several years? In the Discussion section, it would be best if it was preceded by a paragraph describing the goals and organization of the discussion. More importantly, a broad conclusion paragraph would greatly improve the thrust of the paper; as it stands, the manuscript just ends without any broad statement

ADDITIONAL, MORE SPECIFIC COMMENTS

I'd like to make a number of relatively small comments about specific discussions:

- In "Sampling and Analytical Techniques" (p. 69), the robustness of sampling at a single, near-surface depth (0.5m) is defended by referencing an assessment by Coynel et al (2005). What is not clear is if the comparison in that study also corresponded to a near-surface depth, rather than a depth closer to the middle of the water column.

- TSS concentration is low overall compared to many world rivers, even at high discharge. This is a fairly well known characteristic of the Congo as a whole, but it may be worth highlighting in this study of the Oubangui.

- In discussions on seasonal variability of POC sources (p. 79, lines 26-29), it's not clear if the authors are implying that POC at high discharge originates from top soils vs deeper soil layers

- "Flushing" effect and DOC: the studies cited (eg, Boyer et al, 1996; Lambert et al, 2011) involve relatively small streams, so the analogy to this large river seems inappropriate

- The discussion on DOC sources and their seasonality (pp. 80-81) is so focused on hysteresis dynamics that it neglects to fully address the actual, overall seasonal patterns of 13C-DOC ranges, and likely sources, as discussed with POC (eg, C3 vs C4 vs phytoplankton). The highly depleted excursion in 13C-DOC at the flood peak is not addressed well, particularly given that its values are even more depleted than the low-water 13C-POC values attributed to phytoplankton.

- (p. 83) The statement that low-flow DOC conditions "can be proposed to represent baseflow signatures from a savannah-dominated region" seems at odds with the fairly depleted and C3-like 13C values (-27 to -31o/oo)

- (p. 84, section 4.3) The issue of possible differences in African rivers vs other tropical rivers is important, but this discussion doesn't propose factors (geological, climate, ecosystems, etc) that may explain such differences (if they indeed exist). Without such discussion, these statements read as a simplistic suggestion of an unlikely African exceptionalism.

- At the end of p.84, the focus on importance of sampling over different hydrological conditions, while fully shared, seems a bit overwrought in the context of estimates of tropical CO2 evasion fluxes. In particular, the approach used by Richey et al (2002) in the Amazon river system included considerations of seasonal variability.

- Paragraph at end of p 85 and start of p.86: Is the 13C-DIC seasonal variability truly

"large"? It'd be useful if a comparison to other systems were provided, so that this statement is more grounded. Likewise, the following statement also needs a more explicit discussion: "On the other hand, even the lowest d13CDIC signatures are 5–6o/oo less negative than what would be expected based on the estimate of Probst et al. (1994) that 75% of the bicarbonate flux in the Oubangui is derived from silicate weathering." There are many systems dominated by silicate weathering with 13C-DIC in the range of -12 to -16 o/oo. The low-water enriched 13C-DIC values of about -9o/oo also seem plausibly consistent with an increased carbonate-weathering contribution (as indicated also by pH and alkalinity) as observed in many other rivers, without resorting to "higher d13CDIC values during low discharge conditions is consistent with the idea of significant in situ phytoplankton production as outlined above".

- Fig 4: DOC13 has the opposite seasonal trend as POC13; it has a highly depleted excursion at high water, specially relative to C4 and even C3 contributions; it's even more depleted than the low-flow POC that's proposed to have phytoplankton influence!

- Wind estimates from NCEP reanalysis can be biased and have high errors; try to compare to some measured winds from the region, to assess uncertainty. Also, Alin et al (2011) may have more relevant parameterizations for CO2 evasion flux calculations. The conclusion of relatively low CO2 evasion fluxes could be more robust by looking at these

- In a very recent publication, Ellis et al (2012) provide a useful analysis on the potential role of phytoplankton production on CO2 evasion in low-flow seasons in Amazon rivers. That study could serve as a useful comparison to the work presented in this manuscript.

- A useful, illustrative comparison to other systems could be done via carbon export yields. Coynel et al (2005) alraedy provided a very nice comparison from the Congo to other systems. A brief discussion of yields estimated from these study vs the estimates from a range of systems in Coynel et al would be provide a useful, broader context.

** Cited references missing from the bibliography: - Coynel et al 1995 (p. 68) - Frankingnoulle and Borges, 2001 (p. 70)

** Small comments on figures: - Figs 3 & 5: Add a legend similar to Figs 2 & 4 - Fig 7A: Show 13C-DOC together with 13C-POC

- LIST OF REFERENCES (NOT CITED IN THE MANUSCRIPT)

Alin, S.R., M.F.F.L. Rasera, C.I. Salimon, J.E. Richey, G.W. Holtgrieve, A.V. Krusche and A. Snidvongs. 2011. Physical controls on carbon dioxide transfer velocity and flux in low-gradient river systems and implications for regional carbon budgets. J. Geophys. Res.-Biogeosci. 116: GO1009, doi:10.1029/2010JG001398

Ellis, E.E., J.E. Richey, A.K. Aufdenkampe, A.V. Krusche, P.D. Quay, C.I. Salimon and H.B. da Cunha. 2012. Factors controlling water-column respiration in rivers of the central and southwestern Amazon Basin. Limnol. Oceanogr. 57(2): 527–540, doi:10.4319/lo.2012.57.2.0527

Interactive comment on Biogeosciences Discuss., 9, 63, 2012.