Biogeosciences Discuss., 9, C1121–C1126, 2012
www.biogeosciences-discuss.net/9/C1121/2012/

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.

S. Bouillon et al.

steven.bouillon@ees.kuleuven.be

Received and published: 11 May 2012

REF: 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. REPLY: Thanks for the constructive feedback.

REF: One broad comment I'd like to make is about the presentation of goals and of the structure 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. REPLY: We have modified the last section of the introduction to better formulate the objectives of this study and place it into context.

REF: - "A greater range of parameters and analyses have been examined": relative to what studies?? REPLY: this has been rephrased.

REF: - 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 REPLY: An introductory paragraph to the discussion has been added, as well as a conclusion/outlook section at the end of the Discussion.

REF: - 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. REPLY: Coynel et al. (2005) compared depth-integrated values with those measured at 0.2 m, so near-surface. This is now specified.

REF: - 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. REPLY: We have mentioned this explicitly in the Introduction of the revised version, and come back to this point when comparing sediment yields with that in other systems (see also reply to another suggestion below).

REF: - 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 REPLY: Since the basin is characterized by low slopes and low erosion rates (given the low TSM fluxes), we assume that POC originates from top soils. We have specified this in the revised version.

REF: - "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 REPLY: Correct - we still feel the comparison is relevant, but caution for the difference in scale in the revised version.

REF: - 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. REPLY: We have addressed this point in the revised version. Phytoplankton is indeed an unlikely source given that this would peak during the dry season.

REF: - (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) REPLY: True – but this is a phenomenon we are observing in different C3-C4 mixed systems- C4 contributions are

typically much lower than expected based on their relative cover in the catchment.

REF: - (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. REPLY: We have removed this suggestion for the moment and rewritten this to a more general "Clearly, a data-set from a wider range of tropical rivers with sufficient ancillary data would be useful to examine whether driving factors can be determined which explain the high variability observed in CO2 evasion from these systems."

REF: - 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. REPLY: We have removed "especially in tropical settings where seasonality is often neglected" and refer to Richey et al. (2002) as an example of where it is well addressed.

REF: - 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". REPLY: (i) d13CDIC data span a range of 7.1 ‰ which we find considerable but we did not mean it to be particularly larger than in other systems – we refer to Hellings et al. (2001, reference added) and Brunet et al. (2009) as examples in the revised version. (ii) we have added some extra information (end-members) + weakened the statement reg. phytoplankton production effects on d13C-DIC.

REF: - 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! REPLY: Correct, we have added some text discussion the low d13C-DOC data.

REF: - 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 REPLY: We have been unsuccessful in obtaining winds speed data for the area (only daily maxima). See also replies to Referee #1: we have included a 2nd approach to estimate gas exchange based on water current velocity and depth (in the lines of what was suggested by Alin et al. (2011)), rather than wind speed – and include results from both approaches in the revised version.

REF: - 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. REPLY: Yes – this reference has been added.

REF: - 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. REPLY: Excellent point. We included a short section comparing the sediment and C export yields to those in other river systems. We prefer not to provide yet another overview Table with comparisons of yields, there are ample other papers where such overviews can be found and we refer to some of these (Coynel et al. 2005 as suggested, Laraque et al. 2009 for different tributaries in the Congo basin, Bird et al. 2008 and Stallard 1998 for a broader comparison).

REF: ** Cited references missing from the bibliography: - Coynel et al 1995 (p. 68) – Frankingnoulle and Borges, 2001 (p. 70) REPLY: Coynel et al. (1995) should have been Coynel et al. (2005); Frankignoulle & Borges (2001) has been replaced.

REF: ** Small comments on figures: - Figs 3 & 5: Add a legend similar to Figs 2 & 4 – Fig 7A: Show 13C-DOC together with 13C-POC REPLY: Legend added; new panel added to Figure 7 showing d13C-DOC variations.

REF: - 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 REPLY: These have been cited now and added to the reference list.

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