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Biogeosciences Discussions

Interactive comment on "Are small mountainous tropical watersheds of oceanic islands important for carbon export?" *by* E. Lloret et al.

Anonymous Referee #3

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The basic results reported by this paper are a valuable contribution. Estimates of catchment exports from watersheds with large flow fluctuations require intensive sampling and are rare, especially in remote areas such as Guadaloupe. The paper supports results from other studies that show that carbon exports, particularly POC exports, from small tropical watersheds (but also small non-tropical coastal watersheds) can be large relative to catchment area. The paper also highlights the importance of sampling large runoff events and offers interesting insights regarding the relative responses of DIC, DOC, and POC to flow variations.

The paper could be strengthened by more careful consideration of the factors leading to the high export fluxes. In particular, the high values reported for mountainous catchments in the Western Pacific bear similarities to the results reported here, but seem

to be more strongly related to erosion. The TSM exported from Capesterre is about 12% carbon, which is exceptionally high, and this suggests that it is not so much a high erosion rate, but preferential export of soil organic matter that explains the high export. Another factor that might be given more consideration (it is mentioned) is that small watersheds simply don't provide the residence time, particularly for POC, that allow the labile carbon forms to mineralize within larger river systems (e.g., Battin et al. 2008 Nature GeoSciences, Lighthold et al. 2006 Global Biogeochemical Cycles).

There are two apparent significant errors in the paper. First, the proportion of flow in large storms is misrepresented. The authors distinguish floods as flows exceeded 10% of the year and extreme floods as flows exceeded 0.1% of the year. Figure 7, however represents these distinctions as applying to the percent of cumulative discharge, which is clearly in error: Far more than 10% of the cumulative discharge occurs during flows that are exceeded 10% of the time. The same error appears in the Conclusions (7140:12): "The floods and extreme floods, which represent 10% of the annual discharge. . ." Note that if 57% of the DIC transport occurs under low flow, and DIC is more concentrated at low flow than at high flow, then less than 57% of the water flux must occur at low flow.

The other apparent error involves Figure 9. Comparing to Fig. 9b, I find the POC:PN mass ratios range from 1.04 (North America) to 7.35 (Asia), with one value at 14.8 (Capesterre). A POC:PN ratio of 1.04 is implausibly low. Seitzinger et al. 2005 (Global Biogeochemical Cycles) showed POC:PN ratios for all the continents (plus Oceania) in the range of 6.0 to 7.4. Fig. 9a shows a PN flux for North American large rivers of 3.17 Tg/y, which is about the same as Setiziner et al. reported for the North American continent, including small coastal rivers (3.0 Tg/y). However, the POC flux for North American rivers in Fig. 9b (3.3 Tg/y) is only 17% that reported by Seitzinger et al.

The Globalization section (5.4 pp. 7138-7139) is weak on two counts (in addition to the problem with Fig. 9 noted above). First, it extrapolates to the whole world results that are taken from a single watershed with only four years of data. This is unreasonable. Second it is not clear what the point is. The question that should be addressed is whether tropical islands are unique in such a way that their exports are not properly accounted for in global carbon budgeting. This boils down to the relative contribution they make to the world's land surface, whether the carbon exports are disproportionately large, and whether a correction should be made to the budgets. None of these questions is clearly addressed in this section. While it may be of passing interest to know the contributions of these islands relative to large continental rivers, I think it simply confuses the issues to devote this much text and graphics to that question.

I could not follow the carbon mass balance section. It needs to be presented more clearly. What is the role of the 1100 t of NPP-less-litterfall? Where does it go? What is the meaning of the yield of litterfall? Is this the portion of litterfall that becomes incorporated into the soil? How can root respiration be considered an input rather than an output? What is the role of the 77 t of degassing from the stream? Is this seen as a potential fate for the 40 t total C that is exported? If so, where might the other 37 t come from? Or does it mean that far more than 40 t actually entered the stream?

Given the emphasis in this paper on the importance of sampling high flows, and the emphasis on POC export, it is of some concern that the POC data came from only one of the three streams, and that no POC data were collected at the highest ("extreme") flows (Fig. 5).

I recommend presenting results in past tense. The paper should be edited with an eye towards standard English. I did not find cases in which the English was so weak as to be ambiguous. However, I did find many instances of grammatical error and poor construction. The following are examples from the first two pages:

Abstract 9: Two independent clauses joined by a comma.

Abstract 15-17: "is linked to the intensity of meteorological events than the frequency". This needs the words "more than to" in place of "than".

Abstract 17: "Looking at the " is a dangling prepositional phrase.

7119: "resist to the degradation" should read "resist the degradation". 7120: "enriched surface horizons in organic matter" should read "surface horizons enriched in organic matter"

Detailed comments:

7119:15 clarify what is meant by "the global continental carbon flux"

7122:10 C/N ratio should specify whether mass or molar

7126-7127 The method for correcting alkalinity should be presented in the methods section.

7127:17-20âĂŤThis should be presented in the methods.

To produce an unbiased estimate of flow-weighted average concentration, the sampling probability per unit time must be equal. Noting that the samples were taken at uniform sampling intervals would be sufficient.

7129:10-12 and Fig 5 – this should be in the results section.

7129: 12 Fig. 5 The DOC trend seems to have an asymptote. Is the power law the best fit?

7129:13-7130:10 All or most of this section should be in methods.

7130:8 A sentence is needed clarifying whether and how this measure of uncertainty was used to compute the error intervals shown in Table A3. Otherwise this formula appears interesting but superfluous.

7136:23. What is meant by dissolution of soil?

7138:6. The calculated residence times vary from year to year. Obviously the true residence time (say the average age of carbon in the soil, or the ratio of the long-term average outputs-to-soil stock), cannot vary significantly from year to year. The discrepancy arises from the assumption that the soil is in steady state; it is clear that, for any given year, the soil is not in steady state. The problem can be corrected by specifying that the reported residence times are calculated estimates that vary to the extent that the steady state assumption is violated.

7139:7-8 Clarify that the 2.36 Tg came from the mean of the 4 years in Table 1, and only from Capesterre. (The previous sentences draw from all three streams, so it is difficult to see where the average came from).

7140: 12: The floods are actually much more than 10% of that annual discharge, as noted above.

7153:Table 2. Define Y in the table caption. A line representing the 4-year averages would be helpful.

Supplemental Table A3. Specify what the error ranges represent; i.e., standard errors or 95% confidence limits. The value for beta of 1.00 with an error of zero for each of three variables seems unlikely, especially in light of the scatter apparent in Fig. 5. Verify that these are correct. If so the error interval should be reported to an additional decimal place.

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