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Discussions

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

Anonymous Referee #2

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The manuscript of Lloret et al., presents new data which should contribute importantly to our understanding of how weathering and erosion impact the global carbon cycle, providing new constraint on carbon transfers (both organic and inorganic) from a volcanic tropical island. Mountain islands where land-ocean coupling is strong are thought to be globally important for the riverine dissolved and particulate organic carbon (DOC and POC respectively) flux to the ocean, with very high rates of POC transfer. However, most available data comes from regions draining sedimentary bedrocks (e.g. Taiwan, New Zealand, western US) which contain fossil POC, and Lloret's study is the first of its kind (to my knowledge) from small rivers on a volcanic mountain island. The sampling strategy accounts for the role of extreme floods and the study quantifies their importance, providing new insight to the role of extreme events on soil erosion and carbon transfers from land to ocean. I also like the attempt to examine the impact of ero-

sional OC loss on the ecosystem through its role as a time-limiting step for biochemical reactions. The findings should be of interest to the readership at Biogeosciences.

However, in my opinion there are some important issues that need to be tackled prior to publication. They relate to clearly defining the purpose of the study and questions to be addressed, and require a firmer link to the process-based literature in discussion. I have listed 3 major comments herein, and followed this with specific comments as they appear in the manuscript.

1. Justification for the study: The title poses an important question. However, as it is written we already know the answer – yes, as shown by Lyons et al., (2002) Geology, and Kao and Liu (1996), L&O, for tropical islands of the western Pacific. Do the authors mean to ask whether a specific subset of tropical islands are important – i.e. volcanic islands? Dessert and Gaillardet have previously shown that these islands are very important in dissolved fluxes and weathering-induced CO2 drawdown. A clearer rationale may be to pose the question as to the role of organic carbon fluxes and their relative importance. A key finding seems that POC yields are almost always higher than DIC in the Capesterre where data is available (Table 1), and this answers this motivation quite nicely. Also, the discussion regarding the role of floods also needed to be tightened up. Why are floods important? Hilton et al., 2008a proposed in Taiwan that floods may efficiently sequester POC due to the high clastic sediment loads. Here, you could question whether floods are important in all settings, and compare the sediment loads and the likely fate of the POC eroded from Guadeloupe. So, in general, the introductory paragraphs need to be re-thought to make the rationale for the study much clearer.

2. Link to published methods and process literature: The discussion of flux calculations needs to be grounded better in the available literature on this subject. Some of the work by Des Walling or Rob Fergusons' is a good starter for the relative merits of averaging versus rating curves. Also, the manuscript deals with some new data regarding DOC and POC mobilisation and transfer. It would be nice to provide a firmer link to the hydrological and geomorphologic process literature and integrate this data

in more detail. For instance, why is POC not diluted at high flow? Increase transport capacity of the flow? Increased supply from hillslopes? By what processes? linking the concentration data to the discharge and discussing why these trends exist (done a bit for DOC) would be a good way to go about this.

3. ‘Residence time’ calculation: As I mentioned above, I like this in principle. However, I would suggest that you change the term ‘residence time’ as it may confuse the community who use it to mean something different. In reality, respiration is the major flux controlling residence time of organic matter (see fig. 8). Instead, what you’re really calculating is the time available to age organic matter set by the export rate of material from the site through POC and DOC export. It is a time available for OC aging imposed by the export functions. I suggest the authors explain this section in this way. They also need to make it clear why this is important in the introduction and discussion (it is for setting ecosystem age, preventing retrogression, and even promoting primary productivity in young forest sections – plenty of refs out there).

Specific comments (referring to page 7128, line 10 as ‘p28 #10’): Throughout the manuscript some attention is needed to tighten up the text. These mainly relate to grammatical slips and translation errors which should be easy to reconcile given the calibre of the authors on this paper. I’ve mentioned some below but not all, instead focusing on scientific comments.

Pg18 #1: replace with ‘tropics’, remove ‘the’ before small, replace with ‘play’. #2: sentence isn’t clear. Fluxes to the ocean? This paper also deals with inorganic carbon. #5: define acronyms. #12: a repeat of information provided in line #9-10. Condense. #14: bit of a jump to ‘residence time’. Think how to make this flow more logically. #24-26: ‘thus’ and ‘the’ don’t work here. Why not ‘Soil erosion represents a major…’. Change ‘leak’ to ‘export’. ‘lixiviated’ is not a common term (as used throughout), do you mean ‘mobilized’? Pg19 #1: not clear what ‘translocated’ refers to. #5-6: there are additional references to cite in addition to Lal and Hedges here. #10-11: I would turn this around. Evidence for much higher burial efficiencies of terrestrial versus marine organic matter

when compared to the input – see Burdige, 2005, citation in the ms. #16-17: come back to this point later since this is clearly not the partitioning that you observe. Pg20 #5-9: ok, so what? Not clear rationale (see point 1 above). #13: somewhere here fossil organic carbon from sedimentary bedrock needs to be mentioned because its absence in these rivers provides a stark contrast to the existing set of studies focusing on small mountain catchments. These pretty much all drain (meta)sedimentary bedrock (e.g. Eel and Santa Clara rivers in California, North and South Island New Zealand rivers, Taiwan). #26: if this result is published for Guadeloupe Rivers, why do it again here? Make the link to the previously published work clearer. On the following page line 5-7 this is attempted, but is a bit awkward – relates to point 1 above. Pg21 #8-15: ok, the aims of the paper are clear, but the reader needs a clearer rationale of why these things will be done – again see point 1 above. #9: nitrogen comes out of nowhere here. The introduction needs to make clearer why particulate N has been included in the study (clearly it can be as it represents an export of a macronutrient, but why not also DON, DIN?). #15: replace ‘the one’ with ‘those’ #24: why three watersheds? Make more of this. Pg22: these paragraphs would be better switched around. Also, here make clear what the catchments offer your study, in terms of gradients in runoff, slope etc. Pg23: how did you decide the water discharges for the flood thresholds – not clear… Pg24 #4: how turbulent are these rivers? i.e. how representative is a surface sample of the suspended load. #9: It needs to be clearer how these samples were distributed amongst the catchments. It seems most came from one, the Capesterre. #22: units should be microns. Pg25 #9: I believe the standard method is HCl – see Galy et al., 2007, Geostandards and Geoanalytical Research. Is there a ref for the H3PO4 method or is this a typo? #20: Taiwan has a much larger range since this river is known to not be representative of most of the catchments – see Dadson et al., 2005, Journal of Geophysical Research. Pg26 #1: again, POC (non-fossil) has been measured >100 mgC L-1 in Taiwan (see the citation Hilton et al., 2008a from the ms). #4: discussion, move to later in the ms. #9: do you mean ‘similar range of’, not ‘the same’? Pg28: perhaps use a normalised discharge (to annual mean?) to help the reader understand the relative magnitude of these flood events Pg29-30: much of 5.1 reads like methods or results or can be part of an appendix (needs references to link to the published literature). I would prefer to see discussion of the observed links between carbon components and water discharge linking to mechanistic literature Pg32: 'pull up parts of soils' rephrase and link this whole discussion much better to the existing geomorphic literature on erosion in mountain catchments, and specifically erosion of organic carbon in particulate form. Pg34 #7: actually, the mean C/N you quote for riverine POC earlier in the ms gets much higher than the measured soils. This suggests some input of less degraded organic matter from live vegetation, which makes sense, and is consistent with findings in small mountain rivers in Taiwan and the US. Can you provide a bit more on this? #20: do you mean is different from? #24: again, try and link these observations to process-based explanation. Pg37 #13: insert 'close to' in place of 'at'. Pg38 #6-7: rather confusing. The calculation you are doing is relevant only on longer timescales (see comment 1 above), so I'm not sure of the relevance of this. Pg39: If the global extrapolation remains, you need to be clearer about why these catchments are representative. #7: what is this n=4, I don't think its meaningful. #25: Galy et al., 2007, Nature and Burdige, 2005, GBC, are useful additional references here. Table 1: I think the individual annual fluxes are only useful if used to examine, for example, the link between runoff and DOC fluxes in the catchments. . . otherwise it may be better to provide a multiannual average. Table 2: not convinced you need this, given the final column is basically soil carbon stock divided by the sum of POC and DOC flux. Can be explained in the text. Fig. 9: not a fan of these because they certainly imply more precision than we know on these estimates. PN is hardly mentioned, and we hear nothing of its consequence, so I recommend removing the figure. Table A3: why is the exponent on suspended sediment and POC fixed at 1? See point 2 above.

Interactive comment on Biogeosciences Discuss., 9, 7117, 2012.