Anonymous Referee #1

<u>Author comment:</u> We would like to thank referee #1 for constructive comments to improve the overall quality of our manuscript. Our responses to the overall comments are given below.

As suggested by the referee 1, the article was reviewed by a native English speaker.

Detailed comments

Abstract: spell out all abbreviations on first use (FWI, DOC, POC, DIC)

<u>Author comment:</u> the different abbreviations will spell out in revised version ("French West Indies", "dissolved organic carbon", "particulate organic carbon", "dissolved inorganic carbon").

Abstract, Line 10-11: this sentence is somewhat redundant, it follows from the statements above

<u>Author comment:</u> Due to the redundancy of this sentence, it will be removed in revised version.

Abstract: remove reference to estimates of soil residence time and global extrapolations based only on this dataset.

<u>Author comment:</u> This part will not be removed in revised version, since we believe that the soil residence time and comparison between carbon and nitrogen export from small volcanic arc islands (under wet tropical climate) and large rivers is valuable results.

P7119 L10: "less than one-third of terrestrial organic carbon": be specific. Do you mean terrestrial carbon transported to the coastal ocean by rivers?

<u>Author comment:</u> yes, it is terrestrial organic carbon transported by rivers from continents to the ocean. The sentence will be completed in revised version for more precision.

Section 2 (Site characteristics) should go under Materials and Methods.

<u>Author comment:</u> This is a minor comment and as the site characteristic part is quite different from the Materials and Methods, we believe it is appropriate to report it in a distinct section like we have done in our numerous previously published paper.

P7122L11, L16 and elsewhere: C/N ratios: are these molar ratios, or expressed as weight:weight ?

<u>Author comment:</u> The C/N ratios are expressed in %wt:%wt. This precision will be done in revised version for all C/N ratios.

P7124 L22: nominal pore size should be 0.7 μm, not 0.7 μmol l^{-1}.

<u>Author comment:</u> This typographical error will be corrected in revised version.

P7125 L2: temperature, pH, conductivity: these data are not presented in the manuscript, only used to calculate DIC. It might be good to at least give a range of values or describe overall trends.

<u>Author comment:</u> Ranges of pH (6.03-8.63) and temperature (20.0-26.5°C) values will be added in revised version, before DIC calculations part.

P7125 L8: a CHNS-Thermofisher analyser: I assume this should be Thermo + specify the model.

<u>Author comment:</u> The model of the CHNS analyser will be added in revised version. It is a Thermo Scientific Flash 2000 elemental organic analyser.

P7125 L9: concentrated H3PO4 acid vapour. Hmm – I guess this should be HCl, not H3PO4 ?

<u>Author comment:</u> This is a typographical error (we use H_3PO_4 for DOC sample acidification) that will be corrected in revised version (HCl is used).

P7125-7126, section 4.1. Two issues here: first, comparison with literature data does not fit in a Results section and should be moved to the Discussion. Secondly, the authors refer to two or three other studies to compare there data to (mountainous rivers in Taiwan, Puerto Rico, New Zealand) but these seem somewhat randomly chosen. There is a huge amount of literature data on riverine fluxes of sediment and POC from a wide range of systems, including many more from mountainous areas than the ones referred to here – I do not see any point in randomly picking out 3 to compare the range of concentrations with. Just a few examples: Coynel et al. (2005) Contribution of small mountainous rivers to particulate organic carbon input in the Bay of Biscay. Biogeochemistry 74:151–171 Alvarez-Cobelas et al. (2010) A worldwide view of or- ganic carbon export from catchments. Biogeochemistry. Kao et al. (2005) Calculating Highly Fluctuated Suspended Sediment Fluxes from Mountainous Rivers in Taiwan. Terrestrial, Atmospheric and Oceanic Sciences, 16: 653-675

<u>Author comment:</u> We feel that these comparisons allow to the reader to have an idea of the order of magnitude of carbon and nitrogen concentrations in Guadeloupean rivers. These comparisons are then discussed in depth in the carbon and nitrogen yields section later in the text.

The comparison with Taiwan, Puerto Rico and New Zealand, is not a random choice. In order to compare watersheds under either the same climate (wet tropical), location (small islands) and geological (volcanic islands) conditions, we selected this appropriate literature.

As suggested by the reviewer, we will complete in the revised version our comparison with Kao et al., 2005 for sediments export by Taiwan rivers and we will add the recently publish work of Bass et al. (2011) that focuses on the same topic under similar conditions (tropical rainforest watershed).

P7126 L14 and further: I'm not too keen on the alkalinity "corrections" used here – there is very little good evidence to support the idea that this correction procedure will give you more reliable DIC data.

<u>Author comment:</u> Organic acids represent a non-negligible part in alkalinity measurements (up to 16% in this study), and may lead to an overestimation the DIC, pCO_2.... The alkalinity "corrections" are therefore reliable in order to calculate the DIC. In addition, Hunt et al (2011) have shown recently that the contribution of organic acid is significant in the alkalinity. We agree that the choice made for the model are open to discussion but the concept of the correction is relevant and should be used systematically for organic enriched waters (> 8 mg l^{-1} DOC) where the correction leads to more than 10 % differences since alkalinity measurements are now very accurate (< than a few percent). I, addition Tipping (2002) in his book on humic substances showed also the relevance of taking into account the organic matter charges and acid base properties to achieve a good charge balance when performing water samples analysis.

Section 4.2: Personally I do not see much merit in these monthly averages

<u>Author comment:</u> Monthly averages are further information, useful for studies on yearly trends. It also allows direct comparison with previously published work on other systems that most of the time do not use high temporal resolution sampling but more often monthly discrete sampling. In addition for some parameters only monthly or bi-monthly values are available and therefore the monthly average are valuable because they correspond to the same time span.

Section 5.1. The approach used to calculate fluxes needs some critical discussion, and reference to the available literature on different methodologies. A useful source of relevant references (although the more recent ones are missing) and an overview of limitations of the approach taken by the authors can be found in Letcher et al. (1999) Technical report: review of techniques to estimate catchment exports. Available from http://www.npi.gov.au/publications/pubs/nswreport.pdf I strongly suggest the authors to try alternative approaches and compare different methodologies + include a critical discussion.

<u>Author comment:</u> The section 5.1 will be modified in revised version to account for this comment. References to the available literature and a critical discussion will be added to the text, and will be moved in section 4.4 of result part.

As discussed in section 4.3, chemical data are sparse and acquired with a much larger time step than flow discharges. Following previous authors (Hilton et al., 2008; Liu et al., 2011), we therefore choose to estimate the annual fluxes of DOC, DIC, TSM, POC and PN using the rating curve method which appeared more appropriate than the averaging method (Ferguson, 1987; Letcher et al., 1999). As pointed out by the referee, the rating curve method used in this investigation is likely to underestimate the chemical load. Several methods have been proposed to correct for this bias (Ferguson, 1986; Cohn et al., 1989). However these methods, based on the use of a correction factor,

rely on strong assumptions about the statistical distribution of the data and are still debated (Letcher et al., 1999). Given the sparsely of our chemical data, these assumptions cannot be tested accurately. The values presented in table 1 should therefore be considered as a lower bound to the actual yields

P7130 L11-13: if data gaps are up to 30%, then why would the influence on the annual fluxes be weak?

Author comment: This 30 % is a mistake. If we do not consider Vieux-Habitants River, the highest gap is 17 %, so these gaps do not have a strong influence on calculated annual fluxes.

P7134 : the absense of a clear trend between TSM and %POC is an important finding in this study, as it contrasts strongly with other systems. Unfortunately, no hypothesis is presented to explain this.

Author comment: The absence of a clear trend between TSM and %POC is probably due to the lack of particulate sampling (for example we did not sample particulate samples during extreme flood periods).

Section 5.3. This section is very messy. The authors should update their terminology, the following paper is a good start to get this right: Chapin et al. (2006) Reconciling Carbon-cycle Concepts, Terminology, and Methods. Ecosystems 9: 1041-1050. For example, what is meant with "Carbon mass balance" (=carbon inputs – carbon outputs) ?

The authors then continue to assemble literature data on a range of different components – often only vaguely described and from totally different places whereas the natural variability in each of these components is known to be very high – I honestly do not see any point in this exercise. For example: -an average value for tropical forest biomass, based on a global estimate -average value for NPP for tropical forests -soil organic carbon stock data from soils in Martinique -organic carbon yield from litterfall (what do you mean with this) from the study region -root production and root respiration from a rainforest site in Brazil -river degassing from the authors' work, but with no details provided, only reference to a PhD study which is not accessible for most read- ers. This results in a "summary of the carbon mass balance" (Figure 8) which does not show a closed budget at all. What is the point of this ?

Author comment: The incorporation of small rivers, streams carbon dynamic into carbon models is a complex multistep long-term goal. A first step to reach this goal is to document carbon stocks and fluxes at the scale of high order stream watersheds and to calculate the time available for soil organic carbon aging ($Time_{SOC}$) which is an important parameter to a better understanding of setting ecosystem age and preventing retrogression. This will be the main topic of this section in the new version.

Dito for the estimates of the residence time of OC in soils – there is little point in this when taking data on soil stocks from a different place. There is also some confusion here as the authors suggest the residence time to be linked to the absence of floodplains (P7138) – but based on the formula they used the presence of floodplains downstream of their sampling sites would have no effect on the estimated residence time of soil organic carbon. Also, presenting the soil OC residence times as being different for 2010 and 2007 is not very meaningful – this residence time is only relevant on longer time frames.

Author comment: The floodplains of course must be before the sampling points to have an effect on the $Time_{SOC}$. We agree with that. This is often the case for large rivers and numerous work for instance on the Amazon basin have shown the importance of such areas on the DOC and POC fluxes. And our point is that the characteristic of those small steep tropical watersheds is that they do not have such type of environment that can lead to POC and DOC modifications before the river reach the ocean. The quality of the carbon reaching the ocean is therefore different from the one arriving from large rivers because the $Time_{SOC}$ is very different and we believe that this will have an impact on the future of organic matter in oceanic carbon cycle.

Figure 9 and 10 are also somewhat problematic. What do the authors mean (legend of Figure 9) with "the mean fluxes of each continent have been calculated to sum literature values of mainly large rivers" ? So they are not estimates for the entire continent, only for some of the major rivers ? Or were these extrapolated ? Either way, this is not appropriate. There is a wealth of data and modeling studies on global export of sediment, POC, PN, and DOC at the global scale.

See e.g. work by Mayorga, Beusen, Seitzinger, Meybeck etc, some examples below. Beusen et al. (2005) Estimation of global river transport of sediments and associated particulate C, N, and P. GLOBAL BIOGEOCHEMICAL CYCLES, VOL. 19, GB4S05, doi:10.1029/2005GB002453, 2005 Mayorga et al. (2010) Global Nutrient Export from WaterSheds 2 (NEWS 2): Model development and implementation. Environmental Modelling & Software 25 (2010) 837–853. Milliman & Farnsworth (2011) River Discharge to the Coastal Ocean: A Global Synthesis. Cambridge University Press.

Author comment: As suggested by the reviewer, this part will be modified in using model data for continent fluxes (Seitzinger and Harrison, 2005; Mayorga et al., 2010; Beusen et al., 2005).

Dito for Figure 10 – where do these data come from ? I doubt these to be resulting from a comprehensive dataset, and they look strongly biased.

Author comment: The figure 10 will be modified in revised version with flux values from model (Seitzinger and Harrison, 2005; Mayorga et al., 2010; Beusen et al., 2005). But the general trend is not modified and we can see that the POC/DOC ratio is very high for small tropical mountainous rivers compare to continents (see table below).

Continent	POC/DOC	
	Calculation with only large rivers	Model data (Seitzinger and Harrisson, 2005)
South America	0.20	0.69
North America	0.36	0.87
Europe	0.25	0.88
Africa	0.34	1.00
Asia	1.98	1.47
Oceania		2.59
Small tropical volcanic islands	4.18 (our study)	4.18 (our study)

Examples of textual corrections needed : P7118 L2: “In the tropic”: in the tropics, or : in the tropical zone P7118 L2: “the small watersheds”: delete “the” P7118 L6: ranged between P7118 L10-11: awkward, rephrase P7118 L118-19: is estimated at 2.0 – 8.9 Tg C y-1 P7118 L26: lixiviated: not frequently used. Leached ? P7118 L26: “transferred in aquatic ecosystem”: rephrase P7121 L8: “The aims of this paper are, on the light of POC, DOC, and DIC . . . concentrations”: ?? P7121 L10: “global carbon export”: do you really mean global (worldwide), or just annual (for your study sites) ? P7122 L21: “a wet rainy season”: I guess most wet seasons are rainy and vice versa.. P7123 L14-15: “flood events. . . were in greater numbers”: occurred more frequently Throughout the ms: “rainfalls”, “precipitations”: do not use plural. Throughout the ms: “meteorological events”: be more specific, this can be anything

Author comment: We will apply in revised version the suggested textural corrections for a better understanding of the manuscript.