Anonymous Referee #2

<u>Author comment:</u> We would like to thank referee #2 for constructive comments to improve the overall quality of our manuscript. Our responses to the overall comments are given below.

Major comments

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.

<u>Author comment:</u> To clarify our study and its objectives, the title will be changed in revised version with this new title "Dynamic of particulate and dissolved organic carbon in small mountainous tropical watersheds"

Moreover the introduction and discussion will be completed, with the importance of POC export in these rivers, and the impact of floods on POC export (positive trend between POC concentrations and discharge, linked to slope steepness).

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.

   <u>Author comment:</u> We thank the referee for the references. The section 5.1 will be modified in revised version to account for this comment (and this part has been moved in results section: new section 4.4). References to the available literature and a critical discussion will be added to the text.

   Moreover we will add a new section in discussion to look at the positive trend observed between DOC and POC concentrations and discharge. The slope steepness in Guadeloupean watersheds (slope > 25%) is one of major factors which influences the POC export and the non dilution of POC during high flow. The other factors are the leaching of surface soil layers (enriched in organic carbon) during meteorological events, which exports high amounts of organic carbon in dissolved and particulate form. Here we also give the reply made to the referee 1 that had comments on this section :"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"

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).

<u>Author comment:</u> **We agree and will use the following term: the time available for soil organic carbon aging ($Time_{SOC}$). This is an important to a better understanding of setting ecosystem age and preventing retrogression. This will be the main topic of section 5.4 of the revised version.**

Specific comments

Pg18

#1: replace with 'tropics', remove 'the' before small, replace with 'play'.

<u>Author comment:</u> **"the tropic" will be modified in "the tropical zone".**

#2: sentence isn't clear. Fluxes to the ocean? This paper also deals with inorganic carbon.

<u>Author comment:</u> **Yes, this is the flux to the ocean. This part will be completed in revised version with a sentence about inorganic carbon.**

#5: define acronyms.

<u>Author comment:</u> **the different abbreviations will be spelt out in revised version.**

#12: a repeat of information provided in line #9-10. Condense.

<u>Author comment:</u> **this part will be condensed in revised version. The sentence line 12 compares the dissolved carbon exports which are different according to hydrodynamic of rivers.**

#14: bit of a jump to 'residence time'. Think how to make this flow more logically.

<u>Author comment:</u> **this sentence will be completed in revised version to have a link with the part about fluxes.**

#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'?

<u>Author comment:</u> **lixiviated will be changed in revised version by "leached"**

Pg19

#1: not clear what 'translocated' refers to.

<u>Author comment:</u> **translocated is moved from one part of the landscape to another (Lal, 2003, 2004)**

#5-6: there are additional references to cite in addition to Lal and Hedges here.

<u>Author comment:</u> **References will be completed in revised version with Hansell and Carlson, 2002; Benner, 2004.**

#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.

<u>Author comment:</u> **this sentence will be rephrased in revised version for more clarity (cf conclusion Burdige, 2005).**

#16-17: come back to this point later since this is clearly not the partitioning that you observe.

<u>Author comment:</u> **the global carbon flux will be specified in revised version as being total carbon flux (DOC + DIC + POC + PIC).**

Pg20

#5-9: ok, so what? Not clear rationale (see point 1 above).

<u>Author comment:</u> this part will be completed in revised version. In fact, meteorological events can play a role on form, reactivity and storage of terrestrial organic matter.

#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).

<u>Author comment:</u> The new sentence will be: its monolithologic volcanic composition, their lack of fossil organic carbon, helps to constrain the influence of other factors such as climate, soil composition and age of the bedrock.

#26: if this result is published for Guadeloupe Rivers, why do it again here? Make the link to the previously published work clearer

<u>Author comment:</u> this study is different from the work of Lloret et al. (2011), because it includes the high frequency monitoring of 3 rivers while the previous work addressed the spatial variability of DOC characteristics and fluxes only based on a bi-annual sampling. The calculation of fluxes is more accurate, and the most important of this work is that POC and PN are discussed and new data are reported, since POC is the most important part of organic carbon export. We can now make a better discussion on fluxes and potential impact of each specific hydrological event (low water, flood and extreme flood) that was not possible before.

Pg21

#5-7 this is attempted, but is a bit awkward – relates to point 1 above.

<u>Author comment:</u> this part will be rephrased and completed in revised version to show the differences with Lloret et al. (2011). Please take into account also the comment given above.

#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.

<u>Author comment:</u> the why of this paper will be correctly defined in revised version to present the role of the organic carbon in these rivers and the importance of POC which implicate different reactivity, storage of terrestrial organic matter come from these rivers compare to large rivers.

#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?).

<u>Author comment:</u> We will complete in revised version the introduction, with a sentence about the particulate nitrogen. The DON and DIN concentrations are very low (under detection limit of different material), that's why we do not present these parameters.

#15: replace 'the one' with 'those'

<u>Author comment:</u> "the one" will be replaced by "those" in revised version.

#24: why three watersheds? Make more of this.

<u>Author comment:</u> Three watersheds were studied because they are monitored by the DEAL for the discharge. Just 6 watersheds in Guadeloupe are currently monitored (hydrology mainly), including our 3 studied watersheds. Two of the three other are near to Capesterre and Vieux Habitants rivers and are not very easy to access. And the third is the largest river of Guadeloupe and includes the Bras David River in its watershed.

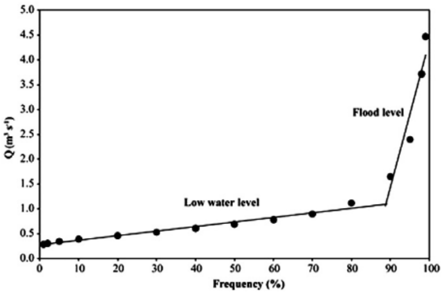
Pg22: these paragraphs would be better switched around. Also, here make clear what the catchments offer your study, in terms of gradients in runoff, slope etc.

<u>Author comment:</u> Slopes are mentioned in Table A1 and the runoff is estimated around 60 % but we do not have more precise data for our watersheds.

Pg23: how did you decide the water discharges for the flood thresholds – not clear…

<u>Author comment:</u> In Lloret et al. (2011), there is explanation for the estimation of the percent of each hydrologic condition (low water levels and floods). If we plot the frequency vs. discharge (example: figure below for Bras-David River, DEAL data; Lloret et al. (2011)), we can observe two distinct hydrological periods. The first part (before the break), corresponding to 90 % of annual flux is characteristic of low water levels, and the second part (after the break), corresponding to 10

% of the annual flux is characteristic of floods. The proportion of extreme floods has been estimated to represent a small part of floods (0.1 % of floods).



Pg24

#4: how turbulent are these rivers? i.e. how representative is a surface sample of the suspended load.

<u>Author comment:</u> **The hydrological regime for Guadeloupean Rivers is torrential. Samplings have been done in the middle of the river when it was possible.**

#9: It needs to be clearer how these samples were distributed amongst the catchments. It seems most came from one, the Capesterre.

<u>Author comment:</u> **All of these events have been collected on the Capesterre River.**

#22: units should be microns.

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

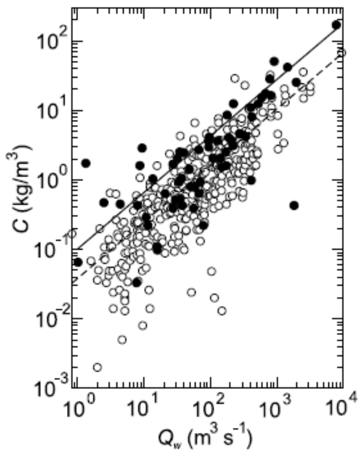
Pg25

#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?

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

#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.

<u>Author comment:</u> **According to Dadson et al., 2005, Taiwan Rivers can have very high sediment concentrations (> 40 000 mg L^{-1}) during specific Typhoons, but the recurrence interval of these concentrations is more than 10 years, so I think they are not representative of sediment concentrations in Taiwan Rivers in standard hydrologic conditions. If we look at the figures 2 and 6 of this paper (cf below figure 6), we can see the most values of concentrations is under 10 000 mg L^{-1}.**



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).

<u>Author comment:</u> **This part will be completed in revised version with citation about Hilton et al 2008a, which presents non fossil POC concentrations during a Mindulle and Aere typhoons in a Taiwan river.**

#4: discussion, move to later in the ms.

Author comment: This part will be moved in revised version in discussion section.

#9: do you mean 'similar range of', not 'the same'?

Author comment: This will be modified in revised version, because we mean "similar range of"

Pg28: perhaps use a normalised discharge (to annual mean?) to help the reader understand the relative magnitude of these flood events

Author comment: Because of we have two distinct trends for discharge vs. frequency (cf. author comment P23), one for low water levels and another one for floods, it is not possible to normalised discharge with an annual mean discharge, but we have represented in graphic the two limits: low water level vs. floods and floods vs. extreme floods.

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

Author comment: The section 5.1 will be moved in revised version in new part 4.4 "Calculations of dissolved and particulate yields" of results part. And a new section 5.1 will be added to discuss the positive trend between DOC and POC concentrations and discharge.

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.

Author comment: It was rephrased in the following way: This is likely due to the strong erosive power of rainfall and the steep slopes of watersheds allowing erosion of organic matter enriched soil layers and leading to important export of DOC and POC (slopes higher than 49 %) (Lloret et al., 2011). The discussion on geomorphic and erosion was already made in lloret et al 2011 where a synthetic diagram was proposed to explain the changes in concentration and nature of DOC. These processes are also valid for POC.

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?

Author comment: This part will be completed in revised version. In fact, C/N in rivers (14.7) are higher than C/N in soils (11.8), this is probably due to some inputs from litter (C/N = 38-43 in litterfall under tropical climate, Schwartz, 1993).

#20: do you mean is different from?

Author comment: Yes we mean different from. We will rephrase this sentence in revised version for more clarity.

#24: again, try and link these observations to process-based explanation.

Author comment: The processes which explain these observations (DOC/POC < 1) for Asian Rivers is probably due to extensive area, present in these rivers, where organic matter could be generated by primary production and exported in particulate form, or flood plains allowing "older geogenic" POC remobilization that will modify POC/DOC ratio.

Pg37

#13: insert 'close to' in place of 'at'.

Author comment: "close to" will be replaced by "at" in revised version

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.

Author comment: As suggested by the referee, this part will be modified in revised version to discuss about the time available for soil organic matter aging imposed by the export functions.

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.

Author comment: It is a mistake which will be removed in revised version.

#25: Galy et al., 2007, Nature and Burdige, 2005, GBC, are useful additional references here.

Author comment: The references suggested by the referee will be added in revised version.

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.

Author comment: We have put separately each year, because in the discussion we compare the different years and the impact of number and strength of meteorological events on organic carbon export.

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.

Author comment: The legend of the Table will be completed in revised version with the reference of text for calculation.

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.

Author comment: As suggested by the referee, this figure will be removed in revised version, and comparison with continent fluxes has been explained in the text.

Table A3: why is the exponent on suspended sediment and POC fixed at 1? See point 2 above.

Author comment: Because the best fit for particulate fractions (suspended sediment, POC and PN) versus discharge is a linear curve so that's why the exponent has been fixed at 1.