Biogeosciences

Discussions

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Interactive comment on "Sources and export of particle-borne organic matter during a monsoon flood in a catchment of northern Laos" *by* E. Gourdin et al.

Anonymous Referee #1

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Small mountain river catchments are thought to play an important role in the erosion of particulate organic carbon (POC) and clastic sediment from the continents, delivering these materials to large river catchments and/or the coastal ocean. However, it is likely much of this POC flux occurs during storm events which are challenging to sample. In order to better understand the processes operating in these catchments (and therefore what sets the rates of carbon export) it is necessary to examine these flashy events at high temporal resolution. Gourdin et al., contribute a detailed dataset, which examines the erosion of POC during an individual storm event in a small, tropical mountain river. In my opinion, the paper provides at least two novel insights which warrant publication and should interest the readership at Biogeosciences. First, the

authors combine hydrological tracers (d18O) to quantify water sources and overland flow contribution. This allows them to interrogate the POC dataset in light of erosion pathways and processes. Second, it consists of three nested gauging stations, sampled at high resolution during the same storm event, providing the opportunity to track POC from hillslope to catchment-scale.

However, the paper was somewhat lacking in its analysis of the tracers of POC source. In my opinion, the work could be much improved by more detailed assessment of the stable isotope measurements (d13C and d15N) and element ratios (C/N) (points 1 and 2). In addition, I felt the two novel aspects I list above could be brought out more, especially the nested stations which weren't really discussed. I've outlined my thoughts on these points below. I've also provided some other comments which I hope the authors find useful when making revisions.

1. d13C and C/N data: This data is very interesting, and not enough is made of it in the current manuscript. The description of the changes through the hydrograph are rather qualitative, and there is much more information to be gained. To illustrate that point, I've plotted d13C versus N/C for the data in the manuscript (see Figure 1) (N/C is chosen because the inverse of concentration versus its isotope ratio will reveal a binary mixture, or fractionating process, as a linear trend). The patterns are fascinating. They show at least two things which need further discussion. First, in S1 and S10, a trend from a C3 'soil-like' signature (d13C∼-26permil, N/C∼0.1) to a 'vegetation-like' signature with a higher N/C. This has a heavier isotopic signature, perhaps suggesting a C4 plant input (or could it be petrogenic? Or carbonate?). Material with this composition has not been collected in the catchment (Table 1). Second, why are the samples from S4, which is downstream of S1 and upstream of S10, not showing that 13C-enriched signal? Is this a sampling issue (different grain sizes?) or a real observation of fluctuating sources downstream? This point links to 3 below. The N/C vs d13C figure should be used to discuss these aspects in the paper.

Related to this point, I think the authors need a more careful discussion of rock-derived

('petrogenic' or 'fossil') POC given the outcrops of sedimentary rocks in the catchment. Without 14C measurements, it is difficult to rule out its contribution with such high TSS (>1g/L is very high) given high petrogenic contributions are seen in other catchments where TSS reach that high (see the Taiwan work, and recent work in the Andes by Clark et al., 2013). The N/C and d13C data do suggest that C4 plant debris with low N/C is important, but they do not rule out a marine rock-source (d13C∼-21permil). Perhaps the high TOC% rule out a significant petrogenic source (which is likely to be more dilute? Unless a black shale?). A more careful discussion, aware of the caveats without 14C data, should be considered.

2. Role of carbonate: This is an important point which needs more open discussion in the methods and results/discussion. The river suspended sediment samples weren't acidified to remove carbonate. This has some benefit, as the inorganic carbon removal protocol is known to attack some of the labile POC (Galy et al., 2007). However, it could severely bias the d13C (carbonate at -5permil to +5permil) and C/N (carbonate very high C/N). I think the N/C vs d13C plot points towards carbonate not being responsible for the 13C-enrichment, because the intercept at low N/C (the carbonate end member) is isotopically light. Still, there needs to be some commentary on this.

3. Nested gauging stations: Very little seemed to be made of the nested gauging station and downstream transmission of sources. Particularly in the light of Figure 1 plotted here. This is a novel aspect which could be expanded upon.

There were some grammatical slips and typos which should be dealt with. Other comments (with Page and Line number):

P9343-L12: Specify what subsurface samples, soil?

L22-25: Really, can you say that. Is it not more likely that the previous dataset don't properly account for high flows? Rather than significant changes in land use?

P9344-L11: why mention South American rivers here? Instead, is it useful to give

some indication of the proportion of the global flux contributed? And which rivers you are referring to?

L15: I find this 'dilution' explantion potentially misleading. It is true that the %TOC can be reduced by adding inorganic (or mineral) sediments with low %TOC. However, at the same time, the total mass of suspended sediment increases (g/L), and therefore, the total mass of particulate organic carbon (g/L) also increases. You show this in your dataset pretty convincingly in Figure 5. I would suggest to rephrase this.

L27-28: This sentence jumps in logic, the thereby is misplaced. Please rephrase.

P9345-L3: It seemed to me, the benefit of studies like this are to better understand the processes which mobilise organic matter (C and N) from soils and export them from river catchments. Surely this is the main contribution? I think this can be better explained.

L20: Linked to the previous comment, why are you doing this? This could be better explained.

P9346-L23: Is 'cliffs' needed here. What do you know about the %TOC content (and ideally C/N, d13C and d15N) of the sedimentary rocks?

Somewhere in this section I feel you need to explain that the soil and gully samples were not collected from the steepest, highest elevation part of the S10 sub-catchment. Also, if rock samples were not collected, you should be upfront about that, and that you don't know their composition.

P9348-L13: Its normally the case that particulate organic matter samples are filtered through 0.2-0.7micron filters. Was this not the case?

P9349-L6: Just a few pages ago you mention carbonate 'cliffs'. How can you be sure there is no carbonate? The TSS values are very high in your rivers (almost reaching Taiwan-like levels). It may be a logical assumption to make in acidic soils, but I'm not convinced that detrital carbonate is not playing a role (see also comments above).

P9350-L17: The d15N values seem very high for tropical vegetation and soil (see compilations from Martinelli et al., 1999 and recent work in Taiwan, Hilton et al., 2013). Is this worth commenting on?

P9351-L4: these TSS values are very high. It is perhaps worth comparing to measurements made on other small tropical rivers.

L10: This observation has been made in small, steep catchments in other tropical settings (Clark et al., 2013) and temperate settings (Smith et al., 2013) and might be worth commenting on at this point.

L24: it might be useful to refer to 2012 when using 23 May in the text (and subheadings) for clarity.

P9356-L1: why is this sentence a separate paragraph?

P9357-L8: This is a very long paragraph, and contains some novel and interesting observations. I'd recommend splitting it. Also, I think the discussion of organic matter sources needs to be more careful. The attached Figure 1 shows covariation of N/C and d13C that needs to be discussed (see main comment above).

L14: I think it would be useful to link these concentration measurements to other places where 'fossil' POC has been observed, e.g. in the Andes (Clark et al., 2013) and Taiwan (e.g. Hilton et al., 2010)

L20: What about the much larger, steeper Houay Xon river catchment (S10)? Where landslides/mass wasting processes occurring there?

L24: Is this a good point to split this long, but important paragraph?

P9359-L4: I wasn't convinced this section was useful, especially as this 137Cs data is not shown here.

P9360-L6: How do these storm specific POC yields relate to other measurements in tropical catchments? There are quite a lot of estimates from work in Taiwan.

P9361-L13: It seems the discussion of the larger S10 catchment has been forgotten at this stage. I think the novelty of this study is the nested approach, which should come out in the conclusions.

References cited here not in the main manuscript:

Clark, K.E., et al., New views on old carbon in the Amazon river: Insight from the source of organic carbon eroded from the Peruvian Andes. Geochemistry, Geophysics, Geosystems. 2013;14:1644-1659.

Galy, V., Bouchez, J. and France-Lanord, C. Determination of total organic carbon content and d13C in carbonate-rich detrital sediments. Geostand. Geoanal. Res. 2007;31:199–207.

Hilton, R. G., Galy, A. West, A. J., Hovius, N. & Roberts, G.G. Geomorphic control on the δ15N of mountain forests. Biogeosciences. 2013;10:1693-1705.

Martinelli, L. A., et al., Nitrogen stable isotopic composition of leaves and soil: Tropical versus temperate forests. Biogeochemistry. 1999;46:45– 65.

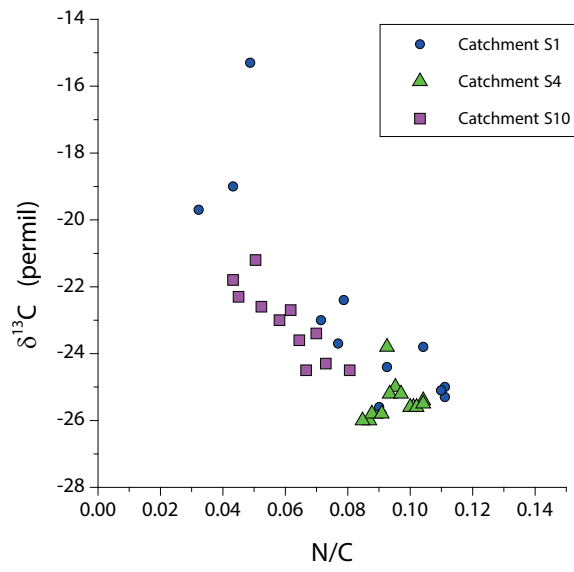Interactive comment on Biogeosciences Discuss., 11, 9341, 2014.

Fig. 1. Stable isotope composition (d13C, permil) and nitrogen to carbon ratio (N/C) of river sediments collected by Gourdin et al., 2014 Biogeosciences Discussions

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