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Interactive comment on “Storm pulses of particulate and dissolved organic carbon in a forested headwater stream and their environmental implications – importance of extreme rainfall events” by B.-J. Jung et al.

Anonymous Referee #2

Received and published: 8 July 2014

Review of Manuscript “Storm pulses of particulate and dissolved organic carbon in a forested headwater stream and their environmental implications – importance of extreme rainfall events” by Jung et al.

This manuscript covers an interesting and important topic, namely POC fluxes, in catchments. Thus, it is highly relevant to the scientific community and suitable for publication in Biogeosciences. So far, there is only few studies available providing datasets of POC fluxes along with parallel DOC fluxes, to allow for comparison and interpretation of the relevance of both of these fluxes.

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Generally, the manuscript is well written, although the structure in general may need some improvements according to my point of view (see below).

My major concerns about the study that I wish the authors to comment on are the following:

Regarding the title I expected detailed analysis of DOC and POC exports during flow events, along with high frequency data. As the title states that the manuscript will report on the environmental implications, I expected a detailed discussion about the relevance of DOC and POC fluxes in comparison, under high flow and low flow conditions. However, in the manuscript the environmental implications are DOC production from POC and, even further, the production of disinfection by-products by POC and DOC. From my point of view, these last points go well beyond what the title would make the reader expect. It broadens the discussion of the manuscript very much, which limits the space of a thorough discussion and interpretation of the POC and DOC fluxes themselves. This is already reflected in the results section, where the section about the fluxes is about 13.5 lines of text (one paragraph only), while the disinfection by-product formation makes up one page of the results section (although based on the title this is only one side aspect).

As a second point, I want to mention, as already mentioned by another reviewer, that I find the calculation of the POC fluxes a bit problematic. Firstly, as the calculations of DOC and POC are essential for the manuscript, I think it is not enough to just say that the concentrations were calculated according to an already published manuscript, without giving any short summary about how this was done. This implies that in this manuscript there was a very sophisticated method applied that cannot be presented here due to space limitations. However, when looking up in the reference, the calculation is just based on a simple regression equation. Moreover, the regression becomes noisy and inexact at high concentrations of 20-40 mg /L. Nevertheless, the authors use the equation to predict POC concentrations as high as 50 mg/L (event 2) without any comment on the validity of the applied regression. I think this is not a valid approach

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here and, as already commented by another reviewer) this needs to be crosschecked by parallel sampling. Another point: The authors state that they have recorded a high frequency dataset. Where is the time resolution and how do they make use of this high resolution data?

In the discussion of the storm pulses of POC export, I also missed a deeper discussion about POC and DOC fluxes in parallel. The authors state that there is little data available, but there is hardly any comparison based on numbers: how much is mobilized in which form under which conditions? How much do we miss if we do not measure POC? In the conclusions the authors mention to have analyzed 50 storm events. Where is this data? I think a thorough analysis of the POC fluxes during storm events would suffice to write a conclusive and thoughtful paper with important implications about C fluxes. The part about DBP is in current state too large and obscures the important facts about C fluxes.

Fig.2: The choice of the regression equations is not clear to me. Based on which criteria the authors chose a quadratic or even cubic equation? How would you interpret this relationship in terms of a physical basis?

Fig.3: What is the temporal resolution here? Maybe also a Q-C plot for the events would be interesting.

Generally, I find the topic of this manuscript very interesting. However, in its current state it is not well elaborated to really get a conclusive “message” from this manuscript. I would suggest to revise the manuscript and to decide whether to focus on C fluxes (then deepening the discussion here and showing more data), or focusing on DBP (then changing the title accordingly). Therefore, I recommend major revisions.

Interactive comment on Biogeosciences Discuss., 11, 6877, 2014.

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