

Interactive comment on “Response of dissolved and particulate organic carbon and nitrogen in runoff to monsoon storm events in two watersheds of different tree species composition” by Mi-Hee Lee et al.

Anonymous Referee #1

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Lee et al. in this manuscript have tried to understand the effect of storm events on dissolved and particulate carbon and nitrogen in runoff from two different watersheds dominated with different species of vegetation. In general, study of this kind can provide an improved understanding of nutrients and material transfer from different terrestrial set up under monsoon condition. It is understandable that it requires a lot of effort to carry out study of this nature; it would have been better if there were more sampling events. Keeping aside the limitation in numbers of sampling events, the manuscript in present form is very poorly written with lots of mistakes in presentation of results and figures. I believe it is not suitable for publication in Biogeosciences. Below are some of

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my specific observations:

* Abstract does not clearly bring out the findings of the study. It merely describes the variability in results. I think they should modify it to include the processes involved for such observation.

* Introduction is poorly written with no focus. It wanders from one topic to other other without gravitating towards the focus of the work. The first line of the introduction itself appears confusing to me.

* One common observation throughout the manuscripts is regarding the references. I think it should be chronologically arranged.

* Framing of the sentences from previous works is such that they appear as if they are from the present study (line 15-20).

* Introduction last sentence: What is measuring campaign?

* How can annual air temperature range from 10 - 15 oC with -6oC in January and 26 oC in August?

* Page 3: Line 20: '(deciduous watershed) (Figure 1)' should be replace with (deciduous watershed; Figure 1).

Page 4: The text suddenly jumps to Oi and Oe+Oa layers without providing any context to it.

Page 4: What do you mean by partly below detection limit. Please provide the number of samples or occasions when it went below detection limit.

* The authors mention the statistical methods followed for analysis but it hardly comes during discussion.

* Page 5: Line 25: Elaborate on the meaning of freeze drying the samples for mass spectrometric analysis or rephrase the sentence.

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* Should not mineral N include nitrite? * Authors state that ‘the soil $\delta^{13}\text{C}$ and soil $\delta^{15}\text{N}$ values significantly increased with soil depth from -29 to -24‰ and from 0 to 8‰ respectively’. However, it would be nice to see the vertical profiles of such data. Was the surface $\delta^{15}\text{N}$ always near 0 ‰

* I could not make the sense out of the sentence ‘In the study period, the highest precipitation coincided with the maximum precipitation intensity, the highest precipitation intensity and the maximum discharge at the 10 mixed watershed and at the deciduous watershed on July 14th, 2013’

* While the difference in DOC concentrations with discharge between deciduous and mixed watershed appears to be convincing (Fig 2a), the POC and PON increase with discharge relies heavily on one data point from high discharge. I do not doubt the increase but I believe that to make unequivocal conclusion more points would have been an asset.

* Please rephrase the sentence “The fluxes of DOC and $\text{NO}_3\text{-N}$ increased with a much steeper slope at the deciduous and at the mixed watershed, respectively”.

* There is problem with the symbols and its representation in Figure 3. I think authors should be careful with these kinds of mistakes before submitting their manuscript for review. It is very tiring to review a manuscript with these kinds of mistakes.

* Result section discussing Chemical properties of DOM and POM in runoff should be modified with proper emphasis on isotopic data. At present the isotopic data has just been mentioned as passing comment.

* In the discussion, authors have admitted that the numbers of events are rather low in the study and observations made by them have already been observed before by Dhillon and Inamdar (2013). I am wondering what novel finding they are discussing which warrants publication in a journal like Biogeosciences.

* Discussion section needs to be re-written with proper emphasis on the major findings

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from this work. The mechanisms and processes behind the differences in observation need to be discussed properly. The effect of altitude, nature of littler and specific nature of the two watersheds needs to be take n in account.

*Fig 2: What are FPOC/FPON?

Fig 5: A succinct Fig 5 will be better.

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