Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-394-RC2, 2018

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "Shifts in stream hydrochemistry in responses to typhoon and non-typhoon precipitation" *by* Chung-Te Chang et al.

Anonymous Referee #2

Received and published: 10 January 2018

This manuscript presents relationships between water budget (precipitation and discharge) and weekly hydrochemical data in four watersheds within the Feitsui Reservoir Watershed (FRW) in northern Taiwan. The dataset spans three years and encompasses eleven Typhoon systems. The authors use these data to draw inference about distinct hydrochemical response during typhoon vs. non-typhoon times, both in terms of variability and direction. Additionally, the watersheds differ in size and relative proportion of agricultural land - in this case largely heavily fertilized tea plantations - and they use this difference to examine the effect of land use change on response to typhoon events.

Printer-friendly version

Discussion paper

The authors have an interesting and appropriate dataset to address their questions, and additionally seem to have chosen an ideal location to elucidate dependence of hydrochemical response on storm intensity. Their methods are logical and results indicate striking differences between watershed response during typhoon and non-typhoon times.

I have two suggestions for review I would characterize as “major,” along with multiple minor suggestions and comments, which are delineated below. Given response to these suggestions, this paper seems like a good candidate for publication in Biogeosciences.

1. Discharge estimation and enhanced hydrologic analysis: The author’s use of the “area-ratio” method of estimating discharge is far from ideal, although it seems to be unavoidable in the absence of other gauging stations within the study watersheds. It is surprising they did not at least perform weekly in-situ measurements of discharge (area-velocity or dilution gauging) in conjunction with their chemistry sampling. Additionally, they present no hydrography or additional hydrologic analyses, for example runoff ratio at annual and storm scales, which may help interpret their weekly data and results.

1a. I would like to see a more thorough explanation of both their method for estimating discharge (rather than requiring the reader to reference to one of the author’s previous publications) and a discussion of the method’s limitations. This should not be long or intensive, but should be sufficient to help the reader understand the reasons for doing so and the potential effects on the analysis.

1b. The author’s should strongly consider including standard hydrographs coupled with their hyetographs for context, and potentially also include annual and a storm-by-storm analysis of runoff coefficients. The latter may require some time, but would be very valuable in interpreting their dataset from the standpoint of variable runoff generation processes due to intensity. Additional context such as intensity-duration curves with

differentially colored typhoon and non-typhoon events would also help make points they make primarily in text.

2. Process-based discussion: With the richness of their chemical dataset, I expected the discussion to be an ideal opportunity to discuss the observed non-linearities in hydrochemical response in the context of physical and biogeochemical process. The authors rightly make no claim at their ability to rigorously distinguish the physical or biogeochemical processes responsible for the dynamics the observe, but there is a rich literature within watershed hydrology and biogeochemistry addressing each of these chemical constituents which would appropriately be used to contextualize their findings. What do the nutrient dynamics suggest about stream response to extreme events? Does stream productivity or nutrient saturation factor in to the differences between A and F watersheds? Does the response of weathering products suggest activation of distinct flowpaths in non-typhoon vs typhoon events? Any of these or more would be appropriate discussion points and would help the reader move past observation to interpretation.

See below for minor suggestions and comments delineated by page and line number:

P2 L25-37: More descriptive climatic information would help the portion of the audience not familiar with the location: annual precipitation, climate zone, seasonality, etc. L30-31 could include actual quantification.

P3 L3-6: This watershed description could go more appropriately in the previous section as study area description.

P3 L7: At what frequency? Or was it recorded continuously?

P3 L9-10: Could the authors explain more about how they handled their samples, particularly with regard to the nutrients? As described their methods may be problematic with regard to nutrients. Particularly in what I would assume is a relatively warm, high-productivity system, samples should be field-filtered or chemically stabilized and then

Printer-friendly version

Discussion paper

frozen as soon as possible. Alternatively they may be analyzed immediately upon return from the field. If they did neither, it might be appropriate to include some discussion of the effect of uptake on their samples. For example, in the lower NO3, F watersheds there may have been more relative uptake after sampling than in the presumably eutrophic, potentially saturated, A watersheds. NH4 is also challenging because it is so readily nitrified.

P3 L15-20: More is needed here. As mentioned above, I'd like to see a better description and justification for the discharge scaling method they used.

P3 Section 2.4: Here the authors spend significant time discussing another potential source of error; something similar is needed for discharge.

P4 L20-21: This seems like an interesting finding, rather than differences in regression direction, since many of the regressions for typhoon periods have very low predictability.

P4-5 Section 3.3: Reiterating above, the change in direction of the regression is less compelling to me than the dramatic change in spread of the point cloud between non-typhoon and typhoon periods (i.e., much lower predictability).

P6 L9-10: I'd be interested in a comparison of extreme, non-typhoon events with comparable typhoon events. Do the authors feel that it is merely the high intensity of the typhoons that cause the unique hydrochemical response? Or is there something unique about typhoons? High winds or variable winds that can damage forests and farms that change delivery of ions to streams?

P7 L8-10: To reiterate a point from the methods, I would encourage the authors to think through the potential effect of nutrient uptake after sampling, if indeed there was no filtering or other stabilization and there was a period of multiple days before analyzing the samples. If the F and A watersheds have distinct nutrient regimes, it is also reasonable to expect they may exhibit distinct uptake responses, which could differentially

affect each set of samples.

P7-8 Section 4.3: The authors provide some interesting, process-based context for their findings in this section! This is the type of discussion I would like to see throughout with respect to all of their findings.

Tables 1 and 2: These could go in a supplementary section

Figures 2 and 3: The authors did a good job of making their figures interpretable in grayscale by choosing their colors and using open vs closed circles. They could further improve this by doing something similar for the regression lines shown here, perhaps dotted lines for the black, total dataset regressions? Purple and blue lines should be distinguishable in grayscale.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-394, 2017.

Printer-friendly version

Discussion paper