

Interactive comment on “The dynamics and export of dissolved organic carbon from subtropical small mountainous rivers during typhoon and non-typhoon periods” by Tsung-Yu Lee et al.

Anonymous Referee #1

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General comments:

This paper investigated the relationship between the dissolved organic carbon (DOC) concentration and river discharge or air temperature and addressed to estimate annual DOC yields from three mountainous watersheds in the subtropical region, Taiwan, in consideration of dynamics of river DOC during typhoon and non-typhoon periods. The data presented are precious because few studies have examined runoff characteristics of DOC in subtropical mountainous rivers during typhoon events. However, I can neither find scientifically something new in this paper nor understand what the significance of the paper is. It has been often described in several studies that temporal variations in DOC concentration appear to be related to river discharge, but the clear relationship

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between those variables cannot be found because DOC concentration varies depending on flow path, DOC sources, microbial activity (temperature), the magnitude and timing of storm events, land covers and topography as well as river discharge. This paper just follows existing knowledge by presenting the results of insufficient analyses on the relationship between the DOC concentration and the river discharge or air temperature.

Analyses on the relationship between the DOC concentration and the river discharge or air temperature in the former part of the paper are not reflected in the estimation of DOC yields by rating curves in the latter part. This is a problem because a scientific paper should be logical but the structure of this paper is not consistent. If the authors just tried to say that typhoon events export a large amount of DOC from the watersheds whereas they occur during a short period of the year, it would be enough to just calculate the annual and the typhoon DOC yields by using rating curves derived from relationships between the DOC flux and the river discharge during the typhoon and non-typhoon periods.

The authors describe that the DOC flux is controlled largely by river discharge, but in Figure 6, it appears that the DOC flux varies up to an order of magnitude or more at a given discharge level. This means that a simple rating curve of $F = aQ^b$ could not precisely estimate the annual DOC yield. I think these variations should be related to other factors, such as, air temperature and flow path.

I am wondering whether the authors understand that the DOC flux under high flow conditions can be underestimated by using a rating curve ($F = aQ^b$) derived from the least square method (Ferguson, 1986). The authors should correct the bias of the estimation toward low flow conditions originating from the log transformation by using the moment-generating function. Or they should determine the parameters, a and b , by an iterative method, such as, Newton's method. Otherwise, they cannot accurately evaluate the proportion of the DOC yield during the typhoon periods to the annual DOC yield. In the first place, I do not understand why the authors selected a rating curve

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method to estimate the annual DOC yield even though there are several methods for estimating element yields (e.g., Cooper and Watts, 2002; Johnes, 2007; Webb et al., 2000). Additionally, the rating curves during typhoon periods ignore the fact that the DOC concentration varies partly depending on river discharge because the b values are approximately equal to 1 ($C = F/Q = aQ^{1/Q} = a$).

Specific comments:

Abstract and Introduction: The objectives of the study are not specific; the first objective described in L72 is not an objective of the study, but a methodology, and the second objective described in L73 is very ambiguous. Additionally, I do not understand why the annual DOC yields were evaluated in the three watersheds.

The authors should describe what research gaps on the river DOC study are in the Introduction section, while they emphasize lacks of the river DOC study in subtropical regions.

Results: As a whole, analyses are not enough in this paper. The authors do not identify or detect the factors causing variations in DOC concentration that could not be explained by river discharge alone, but just take a look at time series of DOC concentration and river discharge and the relationship between the DOC concentration and the river discharge (C–Q relation). The authors describe that DOC concentrations were higher during the rising limb of hydrograph than during the falling limb, resulting in a clockwise hysteresis loop. If this is the case, why didn't they separately analyze the C–Q relation between the rising and falling limbs of hydrograph at each typhoon event? Additionally, the authors try to ascribe variations in DOC concentration to flow paths and available DOC sources that are linked to the degree of saturation in soils in the discussion section, but why didn't they explore associations between the DOC concentration and those factors using the data obtained? I think it is possible to analyze the associations in more detail using several hydrological indices, such as, rainfall intensity, rainfall duration, magnitude of typhoon, magnitude of direct runoff, runoff co-

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efficient, recession constant, antecedent rainfall, and antecedent moisture conditions.

The authors plotted all of the DOC concentration data obtained during 2002–2014 against river discharge in Figure 3, but weren't there inter-annual variations in DOC concentration? If any, they should examine those variations to characterize dynamics of river DOC.

Why is there a blank period in data during 2006–2011?

I do not understand why the authors examined the relationship between the monthly-average DOC concentration and the monthly-average air temperature in Figure 4, whereas they examined the relationship between the DOC concentration/flux and the river discharge at instantaneous values in Figures 3, 5 and 6.

A hysteresis loop in the C–Q relation should strongly affect the relation of DOC flux to water discharge (F–Q relation) because the DOC flux varies up to an order of magnitude or more at a given discharge level as I described earlier. The high values of R^2 of the F–Q relation in Figure 6 make no sense because F and Q variables are not independent.

Discussion: The authors just describe dynamics of DOC within forest watersheds, but they should consider effects of agricultural lands on DOC concentration and yield, in addition to those of forests, because agricultural lands are distributed alongside rivers as shown in Figure 6. In general, water quality in rivers should be affected by nearby land covers and agricultural soils contain a large pool of organic carbon.

I want to see a figure showing the frequency distribution (histogram) of DOC concentration and yield in the world rivers including this study sites because the authors describe that DOC concentration in this study is ranked in the lowest 1% whereas DOC yield is ranked in the top 30%.

Cited literature

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