

## ***Interactive comment on “Impacts of tropical cyclones on hydrochemistry of a subtropical forest” by C. T. Chang et al.***

### **Anonymous Referee #2**

Received and published: 2 June 2013

The authors collected 6-yr data and investigated the typhoon impact on water quantities. Meanwhile, they revealed the specification of the strong resilience in subtropical forestry ecosystem and demonstrated the typhoon impacts. The finding is evident and of use to improve our understanding for forestry ecosystem responses and functions. It's innovative for publication. Here, I have three major comments and several minor suggestions.

Major comments: 1. In conclusion. The point #2 in conclusion is very not strong and evident. Also it's far away to the scope of this study. Because the rainfall and wind measurements are incompatible. The wind velocity driven by pressure difference is measured along the typhoon, but the rainfall measurement the authors showed here is quite local. Unless, the authors can show the rainfall measurement along the typhoon.

C2172

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Meanwhile, the samples number (only 14 typhoons) is not sufficient for this argument. Secondary, if the authors want to make this statement strong; they should give more cases to demonstrate the wind effect, rainfall effect, and the coupling effect, separately for convincing readers. Therefore, I suggested removing the statement in point #2 and relevant sentences in abstract.

2. The strong resilience is the key point in this study. I suggested the authors put more efforts and discuss more on this point. Can the authors interpret and infer why the resilience is so strong in subtropical forestry ecosystem? Can the author provide some comparisons with other documents to demonstrate the fluctuation is really high?

3. P.4548 L.2. How did the authors get this value, 31kg-N/ha/month? Such high value in the text indicated the annual export is 365kg-N/ha/yr, this number is inconsistent with the value showed in Table 2 (36 kg-N/ha/yr). Meanwhile, If the annual mean nitrate concentration was  $\sim 20 \mu\text{M}$  and annual runoff was 1570mm (Table 2), I could roughly calculate  $\sim 8 \text{ kg-N/ha/yr}$  export off the watershed. Please check values. Is it the export for  $\text{NO}_3$  or  $\text{NO}_3\text{-N}$ ?

Minor comments: 1. P.4540, L10: Please provide some references of global warming effect on hydrological cycle.

2. P.4545 L.19. As mentioned above. Is it rational to correlate maximum wind velocity at the typhoon center with local rainfall amount/intensity? Such inconsistency in spatial might scatter the mentioned relation. Besides, did the authors monitor daily rainfall or obtain from some installed weather gauges? Only weekly precipitation samples were mentioned in the text.

3. P.4545, in the section 3 – result. I suggested moving the first paragraph to the section of method. This paragraph described how to fill the missing streamflow records which is a little bit out of scope for this study. If the authors insist this paragraph is very important, I suggested plotting this figure to log-log scale and discussing the potential limitation of this method on estimating weekly streamflow.

4. P.4548 L.16. As mentioned in major comment #2.
5. P.4549 L.23. The argument 'The forest was a NO<sub>3</sub> balanced system during non-typhoon period but lost a large amount of NO<sub>3</sub> during typhoon period. . . ' highlights a very interesting question. It means this system is always losing nitrogen. How this forestry ecosystem can still growth? It implies that the nitrogen storage is very large or there are some unknown process can provide considerable nitrogen to this system?
6. P.4549 L.26. How were the value, 10kg-N/ha/yr and 1/4 derived? According to table 2, the values should be 36 kg-N/ha/yr and 42%.
7. P.4551 L.8-13. As mentioned in major comment #2. I suggested the authors try to discuss this point more.
8. P.4551 L.5-13. I could partly agree with the concept of ecosystem resilience and resistance. I could agree with the resilience of the streamwater chemistry to typhoon disturbance. I don't agree that the ecosystem was resilient right after the typhoon disturbances because the fallen leaves are not replaced with new leaves until the next spring (at least). I don't agree the streamwater chemistry is resistant to typhoon disturbance. It seems to me that it is the rainfall amount controlling the fluctuation of ion concentration. More rain water, either directly from the atmosphere or the soil, could simply dilute the streamwater chemistry. Maybe the authors can highlight the characteristics of hydrological control on streamwater chemistry.
9. P.4551 L.25-28. What did the authors mean by ecosystem degradation? Please refer to minor comment # 5.
10. P.4552 L.2. The sentence 'there is not such thing as a typical tropical cyclone event that can be used to characterize tropical cyclone-ecosystem interactions.' is not clear to me. Any episodic event or change which caused the ecosystem response can give some hints for characterizing the interactions, right?
11. P.4552 L.6. Did the authors imply that nitrogen limitation is occurring in this site?

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 4537, 2013.

**HESD**

10, C2172–C2175, 2013

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C2175

