

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

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My co-authors and appreciate the comments made by the reviewer. They really help to improve the quality of the manuscript. The following is our point-by-point response to the Anonymous Referee #2.

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. Meanwhile, the samples number (only 14 typhoons) is not sufficient for this argument.

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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.

We agree with the reviewer that it seems illogical to correlate maximum wind velocity at the typhoon center with local rainfall. It can be argued that if local wind velocity is used the correlation would be better. Yet, such local information of wind velocity is, in most cases, unavailable both in Taiwan and other tropical cyclone-affected regions. Because the forecast systems use maximum wind velocity at the typhoon center to classify tropical cyclone to differentiate among intensity categories it is the premise of the system that a category-3 tropical cyclone would have more impact on both human and natural systems than a category-2 cyclone. Here we tested the validity of using wind velocity to predict local rainfall, which has a major impact on both human and natural systems. The lack of good correlation between the two, resulting from the incompatible spatial scales, supports our inference that maximum wind velocity at center of the cyclone is not a good predictor of typhoon impact.

We understand that academically the maximum wind velocity at cyclone-center should not be used to predict local impact of cyclone disturbance. However, again, it is the information that is used to classify cyclones into different intensity categories with the explicit understanding/assumption that cyclones in higher intensity categories have greater impact than those in lower intensity category. Thus, we believe it is important to test the validity of the wind-based classification systems in predicting cyclone impact. We believe that the inclusion of potential rainfall information into the cyclone classification or warning systems would improve the predictive power relative to total damage and thus provide more useful information prospectively for evaluating cyclone impact.

We also agree that 14 typhoons is not a large sample. However, it is the largest number of tropical cyclones that have been examined in one study that we are aware of.

Therefore, the typhoon impact on an ecosystem described in this study should be far less biased than those reported from studies of only a single disturbance event. If the reviewer still feels that our explanation is not convincing we would agree to remove it from the conclusion and the 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?

We also thought that the high resilience of streamwater chemistry is a key point of our study. We added the following paragraph to describe the possible cause of high resilience of streamwater chemistry at Lienhuachi Experimental Forest. A study of typhoon-induced tree mortality at Lienhuachi Experimental Forest and Fushan Experimental Forest indicates that annual tree mortality is very low in both forests (<1%) despite frequent typhoons (Forsyth 2006). Very low tree mortality and minimal damaged to forest understory plants means that the Lienhuachi Experimental Forest is capable of continuously taking up nutrients from soil solution. Therefore, once the heavy rains stop, leaching of nitrate and other nutrients from foliage and soils does as well, returning to stream water concentrations similar to those observed prior to the typhoon disturbance. (lines 358-364)

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 20 μM and annual runoff was 1570mm (Table 2), I could roughly calculate 8 kg-N/ha/yr export off the watershed. Please check values. Is it the export for NO₃ or NO₃-N?

First, we thank the reviewer for checking the numbers closely. The unit should be NO₃- not NO₃-N and we corrected it. Second, to clarify how we got the numbers we edited

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the description and the new sentence is now: “Notably the mean typhoon-induced annual NO₃ loss (output – input) of 10 kg ha⁻¹ (Table 2) occurred during the average 9.5 days during typhoons influenced the study site (Table 1). Over the six-year study period a total of 62 kg ha⁻¹ NO₃ was lost during 57 typhoon days, resulting in a loss of approximately 31 kg NO₃ ha⁻¹ mo⁻¹ or 1 kg ha⁻¹ d⁻¹ during the typhoon period.” (lines 268-271) We hope this clarifies any confusion.

Minor comments:

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

We added four references (one each for the effect of global warming on drought, flooding, heat waves and tropical cyclone intensity).

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.

Please see our response to major comment #2.

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.

We agree with the reviewer so moved it to the methods and removed the figure.

4. P.4548 L.16. As mentioned in major comment #2.

Based on the page and line numbers we believe that the reviewer meant major com-

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ment #1 not #2, please see our response to major comment #1.

5. P.4549 L.23. The argument 'The forest was a NO₃ balanced system during non-typhoon period but lost a large amount of NO₃ 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?

The loss of large amounts of nutrients is common following a typhoon/hurricane disturbance and does not seem to affect forest productivity (e.g in Puerto Rico during typhoon Hugo in 1995). Our results show that the forest often losses large amounts of nitrate during the typhoon period, but the typhoons only impacted the forest an average of 9.5 days/year so the forest is losing nitrogen for a very brief period. The near balance of inputs and outputs of N during non-typhoon period indicates that the forest is not likely to be N-limited. Moreover, nitrogen deposition is very high in many parts of Taiwan including Lienhuachi. Therefore, although the typhoon-induced nitrate loss at Lienhuachi Experimental Forest is high, it currently is unlikely to negatively affect ecosystem productivity.

6. P.4549 L.26. How were the value, 10kg-N/ha/yr and 1/4 derived? According to table2, the values should be 36 kg-N/ha/yr and 42%.

Watershed export is 36 kg NO₃- ha⁻¹ annually and 15 kg ha⁻¹ during typhoon period as shown in Table 2. However, on a net input-output basis the watershed lost approximately 10 kg NO₃- ha/yr (15-4.9) during the typhoon period. To make it clear we modified the sentence to "...the net loss (output – input) of, on average, 10 kg NO₃-ha⁻¹ yr⁻¹ during the typhoon period (9.5 d yr⁻¹) could be important as it accounts for more than 1/4 of the annual output (36 kg ha⁻¹) occurring at an average rate of 1 kg NO₃- ha⁻¹ d⁻¹". (lines 311-313) We hope this is now clear.

7. P.4551 L.8-13. As mentioned in major comment #2. I suggested the authors try to discuss this point more.

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Please see our response to major comment #2.

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.

We agree with the reviewer that our data indicates that streamwater chemistry is resilient to typhoon disturbance, but we cannot infer that the ecosystem is as well. In deed this is exactly what we put in this paragraph (i.e. we did not describe that the ecosystem is resilient to typhoon disturbance) "In this regard, streamwater chemistry at the Lienhuachi Experimental Forest is highly resilient to typhoon disturbance". We also modified conclusion #5 so that the resilience of streamwater chemistry not ecosystem resilience is highlighted. "Streamwater chemistry changes during typhoons but returns to pre-typhoon concentrations rapidly, indicating high resilience to typhoon disturbance." (line 405)

Regarding the resistance of streamwater chemistry to typhoon disturbance, we agree that hydrological control should have played an important role on streamwater chemistry. We inferred the resistance of streamwater chemistry to typhoon disturbance from figure 3. Based on figure 3, even in four of the six typhoons in which typhoon rainfall led to the annual peak stream flow fluctuations of streamwater chemistry were not greater during the typhoon period than during the non-typhoon period. In other words the greatest annual hydrological input did not lead to the highest hydrological output. Therefore, we conclude that streamwater chemistry is rather resistant to typhoon disturbance. We think this is not in conflict with the importance of hydrological control on

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streamwater chemistry.

9. P.4551 L.25-28. What did the authors mean by ecosystem degradation? Please refer to minor comment # 5.

Our use of degradation may be ambiguous so we changed it to ecosystem function such as primary productivity. The new sentence is now “. . .would be nitrogen depauperate and in turn negatively affecting a wide range of ecosystem functions, such as net primary productivity” (lines 369-370)

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?

We agree with the reviewer that every event can provide some insights into the disturbance-ecosystem interaction. However, typhoons differ considerably e.g. duration, timing in relation to plant phenology and relationship to other disturbance events, such that the effects on ecosystems could be very different and that difference is reflected among the typhoons we studied. Although we can learn from every disturbance event, special caution should be given to avoiding over-generalization from what can easily be characterized as idiosyncratic events. We point this out because currently much of our knowledge about tropical cyclone-ecosystem interactions comes from a few well studied tropical cyclones. We added a sentence before we conclude that “. . .there is not such thing as a typical tropical cyclone event that can be used to characterize tropical cyclone-ecosystem interactions.” and another sentence following it to clarify our concern. “Currently our understanding of tropical cyclone-ecosystem interactions largely comes from a few well-studied cyclones in the western hemisphere. Little is known about the applicability of the findings gained from studies of these few tropical cyclones to other cyclone-ecosystem interactions.” (lines 371-373) We also modified the sentence following the conclusion of no such thing as a typical cyclone to make our

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point more clear. “Thus, although studies on specific events provides useful insights into cyclone-ecosystem interactions, extrapolating the results to other regions/systems or even other cyclones in the same region requires a great deal of caution.” (lines 376-378)

11. P.4552 L.6. Did the authors imply that nitrogen limitation is occurring in this site?

In this sentence we cited the work of Vitousek et al. (2010) to highlight that tropical cyclone-induced nitrogen loss is high and can lead to limitations over the long run if not limited. However, in the next sentence we also pointed out that at Lienhuachi Experimental Forest it is maintained within what can be perceived as reasonable limits. We also modified the following sentence to make our point clear. “Maintaining fluctuations within a relatively limited range, as observed in the current study, helps to minimize nitrogen loss from the ecosystem and delay the development of nitrogen limitation.” (lines 381-383) Therefore we do imply that cyclone-induce nitrogen may lead to nitrogen limitation.

We also attached a supplement PDF file of this response. The format of the fonts that appear awkward should appear properly in the PDF file.

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/10/C2871/2013/hessd-10-C2871-2013-supplement.pdf>

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