**Referee 1**

We would like to thank both referees for their insightful comments on the original manuscript. Referee 1 and 2 commented on criteria that were used to select/exclude a cyclone from further analyses resulting in a revised set of criteria. These revised criteria will require re-running the entire analysis and remaking all figures and tables. The figures presented in this reply should, therefore, be considered as examples showing how the revised figures could address referee comments but are not final.

Comment 1. It is true that most studies of cyclone (typhoon) disturbance effects focused on major cyclones such that the effects of typhoons on ecosystems are disproportionally derived from studies of these major typhoons. However, I do not think that researchers assume that their studies represent the overall effect of typhoons on ecosystems. I think the studies tried to show that major typhoons could have large impact on ecosystems. With some exceptions, intense typhoons generally caused greater damage. Naturally weak typhoons (e.g., category 1) are unlikely to cause large canopy damage. Thus, I think one piece of information that needs to be added to the manuscript is the proportions of typhoons of different intensity categories for the typhoons passed the selection in this study.

> *One of the reasons why we consider our findings worth to be published in Biogeosciences Letters is that intensity class ranked low as a possible driver in the random forest analysis (Fig. 2). Leaving intensity class out of the random forest analyses decreased the accuracy of the regression trees with less than 10%. Leaving it out might even result in an increase in accuracy (Fig. 2). Nevertheless, we agree with*

*the referee that the intensity categories should be better integrated in the study and we will, therefore, stress this point by:*

*(a) Adding a sentence reporting the share of the different intensity classes (1 to 5) for both the census of 580 cyclones as well as the share for the subsample of cyclones that were further analyzed in this study. This information will be added around L93 of the original submission.*

*(b) Returning to the importance of wind speed in the discussion around L127 of the original submission.*

*(c) Adding the proportion of the different intensity classes in a revised version of Fig. S2. That way the readers can see the distributions for themselves. Following a comment of referee 2, we will show nine subplots to document the differences between the nine definitions for affected areas applied in this study (Table S1). This change also helps to address comment 2 of referee 1. The revised Fig. S2 could look as follows (note that this figure is based on the selection criteria of the original submission. The selection criteria will be revised which will affect all figures and tables):*

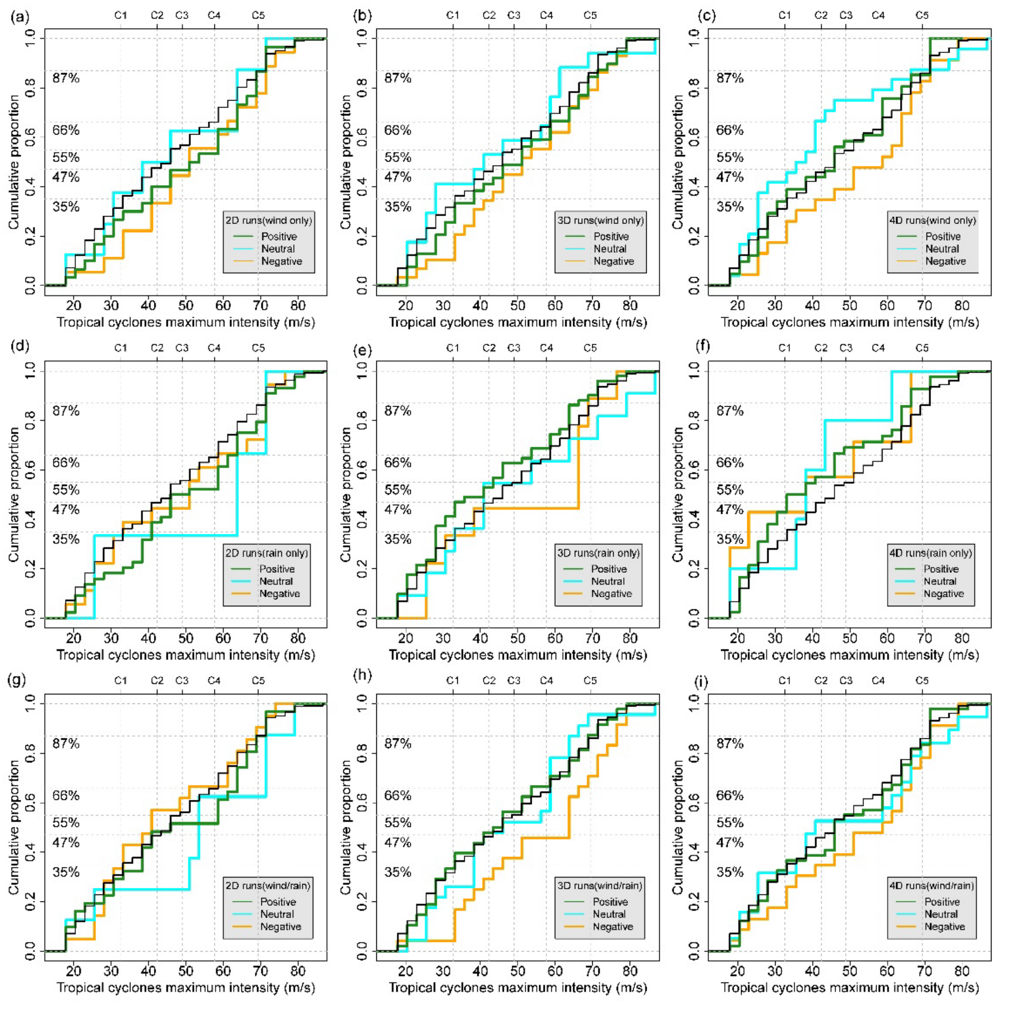


*Figure S2. Cumulative distribution functions of tropical cyclones as a function of their maximum intensity for the nine definitions of affected area used in this study. Panel (a) shows wind only for 2 diameters, (b) wind only for 3 diameters, (c) wind only for 4 diameters, (d) rain only for 2 diameters, (e) rain only for 3 diameters, (f) rain only for 4 diameters, (g) wind or rain for 2 diameters, (h) wind or rain for 3 diameters, and (i) wind or rain for 4 diameters as detailed in Table S1. The intensity distribution for tropical cyclones with a negative effect size is shown in orange, for tropical cyclones with a neutral effect size is shown in blue, and for tropical cyclones with a positive effect size in green. The grey solid line shows [to be added in the revised figure] the distribution for the specific definition (n = 145 ± 42 cyclones depending on the definition). The black solid line shows the distribution of the 580* *events that occurred between 1999 to 2018. Small deviations between the grey and the black line suggest that the sample well represented the 580 cyclones in terms of their intensity class. The thresholds for the intensity classes are based on (Schott et al, 2021). The maximum wind speed of category 1 cyclones is between 32.6 m/s and 42.6 m/s, between 4.7 m/s and 49.3 m/s for category 2, between 49.4 m/s and 57.8 m/s for category 3, between 57.9 m/s and 69.4 m/s for category 4, and exceeding 69.5 m/s for category 5. In East Asia, tropical cyclones of intensity class 3 or higher are called typhoons.*

Comment 2. Related to this, I would also suggest break the analysis by intensity categories of typhoons. If the patterns stay the same (i.e., 30% typhoon did not cause detectable canopy damage) among all categories, that would be a much more interesting finding. If on the other hand, the proportion of no-damage concentrated among weaker typhoons, the results would basically confirmed the findings of previous studies.

*We agree with the referee and will more prominently feature the intensity categories in the revised manuscript. Proposed revisions addressing this issue are listed in the reply to comment 1 of referee 1.*

Also related to this is the definition of the width of the cyclone track area. I wonder if a more conservative definition is used, would the results stay the same. Because wind velocity decreases with increasing distance from the typhoon eye. A liberal definition is likely to include areas with not strong winds and as such it is not surprising to see limited typhoon impact on forest leaf area.

*As we share this concern with the referee, the original submission introduced nine different definitions for affected area. These definitions varied, as suggested by the referee, the width of the potentially affected area as a function of the cyclone diameter in combination with different climatic thresholds (Table S1). Each definition comprises a combination of at least two out of three criteria, e.g., the diameter of the cyclone, the maximum wind speed at each location during the passage of the cyclone and accumulated precipitation at each location during the passage of the cyclone. This approach was justified in the methods of which a summary follows "Being located within the track of a specific cyclone is essential but not sufficient for damage to occur. Within a storm track, only forested pixels that experienced high wind speeds or high precipitation were counted as in the potentially affected area. Forest pixels that were located within the storm track but did not experience high wind speeds or high precipitation were counted as in the reference area. Each forested pixel within each individual storm track was classified as either affected area or reference area based on these nine definitions. To better account for the uncertainties arising from this approach, the threshold values for wind speed and precipitation were also increased as the track diameter increased (Table S1). For a narrow storm track it is reasonable to assume that there would be damage in all pixels except those where wind speed or precipitation did not exceed a relatively low threshold value. For wide storm tracks the opposite applies; it is reasonable to assume that few of the pixels are damaged except those where wind speed or precipitation exceeded relatively high threshold values".*

*More details on the classification of forest pixels into cyclone-specific reference and affected areas methods can be found in the method section from L193 to L222. Differences in the results coming from differences in the definitions were used throughout the analysis to estimate semantic uncertainties. Figures 1, 2, 3, S2, and S3* *show this semantic uncertainty. The captions of Figures 4 and S1 mention they shows the result for only definition 3b. Fig S4 does not depend on the nine definitions for affected area (Table S1). It is, therefore, unclear how this comment could be addressed beyond what is already included in the manuscript.*

It is important to note that in situ wind speed experienced by the forests could be very different from that of the global dataset.

*We agree and this potential mismatch is one of the reasons why we did not base the study on a single definition but used nine definitions instead (Table S1). Likewise, the use of a storm-specific reference and affected area, as well as the many repetitions (145 ± 42) are expected to contribute to robust results.*

<u>Comment 3.</u> The use of images two months following typhoon disturbance bothers me. In tropical and subtropical region, plant growth could be very quick so that leaf area could increase substantially in two months, with and without typhoon disturbance. Even for late typhoons the phenological change could be substantial because most of the affected areas are in the subtropics with long growing season. Thus, I am concerned that the seemly positive effect of typhoon on forest leaf area could be an artifact of the long duration between typhoon passage and image acquisition.

*Exactly this concern convinced us to report an effect size metric rather than an absolute change in LAI. The approach compares, for each individual cyclone, the change in LAI in the affected area against the change in LAI in a cyclone-specific nearby reference area (see equation 1 of the original submission). LAI changes due to leaf phenology are thus accounted for in the effect size. The use of effect sizes makes it* *very unlikely that positive effect sizes are systematically caused by leaf senescence. The justification of using effect sizes is mentioned in the original submission between L79 and L83 and more details on this approach are given in the method section between L265 and L291.*

*A long time window is more likely to result in neutral effect sizes than in positive or negative effect sizes. Long time windows increase the probability that the LAI recovered from its disturbance which would be reflected in a neutral effect size. Likewise, a long time window increases the changes that the LAI observation following the disturbance occurs after senescence which would also result in a neutral effect size.*

*We, nevertheless, agree with the referee that a two-month period is long and if the LAI product would have allowed to study shorter time windows we would have analyzed the change in LAI 10, 20, 30, 40, 50 and 60 days following the passage of a tropical cyclone. The 60-day period is dictated by the limitation of remote sensed LAI product. In this study, we choose to use the LAI product derived both from high resolution SPOT and PROBV satellite sensor with a very high spatial resolution (1km) and 10 days temporal resolution. As any remote sensing product, this products contains observational gaps which are filled with a local spatial and temporal mean LAI. As this study aims to analyze local spatial and temporal changes in LAI gap filled data could not be used. Using a 60 day window resulted in a reasonable amount of LAI observations for pixels in the reference and the affected area both before and after the passage of a tropical cyclone. The 60-day window is thus more determined by data-availability than disturbance related issues. The justification of the 60-day window will be included in the revised manuscript around L286 of the original submission.*

Comment 4. The most interesting finding of this study is the positive effect of typhoon on leaf area which was attributed to increased water availability. I have several concerns on this finding.

First, as described above most of the increase in leaf area could be from weak typhoons. In this case, it is not surprising because weak typhoons are not expect to have major impact on forests. This has been reported before.

> *The random forest analysis suggests that intensity category contributes little to explaining the sign in LAI change. The revisions suggested to address comment 1 by referee 1 will also address this concern. Fig S2 shows that irrespective of which of the nine definitions of the affected and reference area are used, class 3, 4 and 5 typhoons may also result in an increase in LAI 60-day following the passage of the typhoon. The weak relationship between intensity category and the change in LAI is indeed one of the more interesting findings of this study. This will be stressed in the revised manuscript around L126 of the original manuscript.*

Second, also as described above the use of a liberal definition of typhoon track width could also lead to this positive effect because the wind velocity is naturally low in parts of the affected area.

> *This could indeed have been a concern if only diameter was used as a criterion but all of our nine definitions for reference and affected areas make use of at least two criteria to overcome this concern. Agreement across the nine definitions suggests that although the data are far from perfect and each individual definition comes with arbitrary thresholds, the results themselves are not very sensitive to these thresholds.*

Third, the two months interval between typhoon passage and image acquisition described above could also lead to the positive effect. A combination of weak intensity, liberal definition of track width and long duration between typhoon passage and image acquisition makes the claim of positive effect of typhoon on forest leaf area problematic. I am not saying that the finding is not true but the above possibilities must be excluded before such a conclusion can be made with confidence. I would also like to see the changes in leaf area in the reference areas during the same period. If leaf area also increased at the similar magnitude, then attributing the effect to typhoons needs more explanation.

*Given the 60-day time window, the LAI is expected to change in the reference area especially if this 60-day window includes the start or the end of the growing season. The effect size needs the change in the reference area to evaluate whether the change in LAI in the affected area is faster, similar or slower. The way the effect size is calculated thus accounts for phenological changes in LAI. If the reference area would not be used in the calculation of the effect size, the change in LAI over the affected area would mostly represent leaf phenology and would thus be unsuitable to address the question at hand. This will be stressed in the method section where the calculation of the effect size is detailed (L265-297).*

*The concern about a liberal definition has been addressed by using an ensemble of nine definitions throughout the study. See reply to comment 2, referee 1.*

*As positive effects are not limited to the cyclones from a low intensity class (Fig. S2), and intensity class has little explanatory power (Fig. 2) a systematic bias is highly unlikely. Given the 60-day time window, our method is more likely to be biased towards detecting no changes in LAI than to detecting positive or negative changes in*

*LAI. This bias expectation will also be added into the method section where the calculation of the effect size is detailed (L265-297).*

Comment 5. The justification of the area LAI difference by using 0.5 which is not consisting with the same shared of the area mean LAI value.

*Thank you for raising the issue. Fig. 6 displayed in the report discussing the quality of the LAI product (*Jorge, 2018*) used in this study indeed suggests that making the uncertainty proportional to its absolute value is justified. Given that a proportional uncertainty will be stricter than the previously used fixed uncertainty, all analyses presented in the manuscript will have to be rerun. We will adjust this threshold to be 15% difference of the mean LAI value between reference and affected area and rerun all analyses. The section describing the quality control will be adjusted accordingly (around L300-317 in the original submission).*

Comment 6. The figures are mainly about the statistical results. I do not see any results on the actual leaf area and it changes. Thus, the paper is more statistical than ecological/biological.

*This study makes indeed use of statistical approaches to obtain new insights in disturbance ecology. To address this concern of the referee we propose to add an additional figure that ties atmospheric science to disturbance ecology as already described but not illustrated on L149-159 of the original submission. The new Fig. 4 could look as follows (note that this figure is based on the selection criteria of the original submission. The selection criteria will be revised which will affect all figures and tables).*

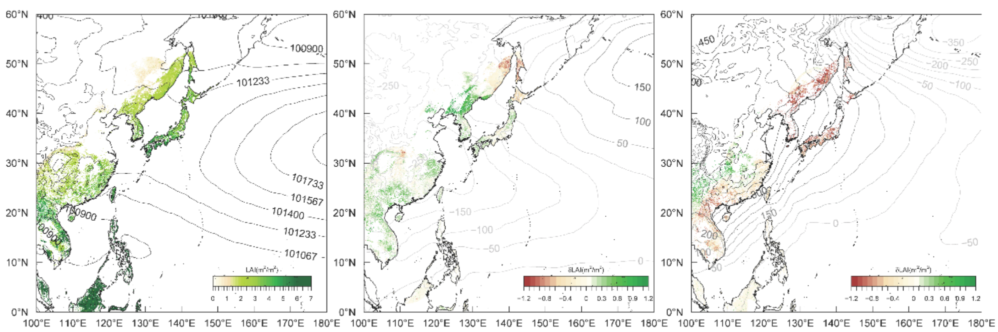


*Figure 4. Pressure fields (Pa) and changes therein 1 day prior to the passage of a tropical cyclone for cyclones that had a neutral, positive, or negative impact on the leaf area (m2 m-2) of forests. (a) Mean atmospheric pressure and leaf area 1 day prior to the passage of a tropical cyclone that had a neutral impact on forest leaf area. (b) Changes in mean atmospheric pressure and leaf area between cyclones with a neutral and positive effect on leaf area. (c) Changes in mean atmospheric pressure and leaf area between cyclones with a neutral and negative effect on leaf area. Results are shown for affected areas defined as locations within an area extending to three times the cyclone width for which the wind or precipitation exceeded a threshold (Table S1)*

Comment 7. I am not all convinced that less than 1/3 of the typhoons passed the quality control check is representative of the overall typhoons in the region.

*It is correct that depending on the definition used, between 15 and 34 % of the of 580 cyclones could be retained for further analysis. As is the case for most observational studies, there was a trade-off between data quality and number of repetitions. We preferred to use smaller samples sizes in favor of more strict data quality criteria. The smallest sample still contained 88 cyclones (for definition 2c) whereas the largest* *sample contained 196 cyclones (for definition 3b; Table S1). These are still very reasonable numbers for ecological studies. More importantly, these samples well represent the entire population of the 580 tropical cyclones in the region with regards maximum wind speed (revised Fig. S2, shown under comment 1 for referee 1). Details on how the representativeness of the samples in terms of intensity class will be better stressed in the revised manuscript are given in the reply to comment 1 of referee 1.*

**References used in the replies**


Jorge, S.-Z. *Copernicus Global Land Operations "Vegetation and Energy"*. PP-51, 2018. Link

Schott, T. *et al.* The Saffir-Simpson Hurricane Wind ScaleSaffir-Simpson. PP-4, 2021. Link