Referee 2

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. I do have many issues with how certain studies are cited (see line comments), so I think the attribution of findings needs to be done far more carefully. This is my main criticism of the study, so I hope if there is a revised manuscript, that the attribution of citations will not have so many large mistakes.

We will carefully check our citations and come back to this issue when discussion the line comments of referee 2.

Also, it might be a bit contrived to state that it is surprising that cyclones could benefit LAI by increased rainfall. Cyclones bring rainfall over a larger area than the area where they deliver high wind speeds. But this is not a big issue, as it is good to quantify these things. I would argue that the title is a bit too broad and assertive of the occasional positive precipitation effect on LAI. We should keep in mind that this is an analysis focused on the (satellite estimated) LAI of East Asian forests, but that there are several other important other aspects relating to forest response to cyclones that this study does not address (e.g. tree mortality and damage, branchfall, landslides, floods, etc). In my opinion, and even in light of these results, the current title overstates the importance of precipitation on forest responses to cyclones.

We see the point made by the referee and propose to change the title in "Tropical cyclones facilitate recovery of forest leaf area from summer droughts in East Asia". Some of the comments by referee 1 and referee 2 suggested that we partly failed in highlighting the novelty of this study. We hope that this new title that will be supported by a thoroughly revised discussion better stresses the take home message of the study.

Note that tree mortality due to turnover or stem breakage, branchfall, and landslides could be expected to result in direct changes in LAI and are, therefore, largely accounted for in the

analysis. Given the time window used in this study, legacy effects that emerged 60-days after the passage of the cyclone are not accounted for.

Comment 2. Line comments. Mha (mega hectares?) is not easy to interpret as a unit. I suggest the authors convert this to km2.

Mha is indeed mega hectares. We will change this units in the text as well as in Figs 1 and S1.

A lot of unnecessary acronyms are used, which make the MS more difficult to read. Given the looser length requirements of Biogeosciences, I suggest using as few acronyms as possible.

Agree with the suggestion. The following acronyms will be removed from a revised manuscript: SPEI (standardized precipitation and evapotranspiration index), LAI (leaf area index), ES (effect size), JTWC (Joint Typhoon Warning Center).

Some of the sentences are overly long. Reducing these run-on sentences would help.

Sentence length will be checked while revising the manuscript.

A figure, or alteration to one of the existing figures, would be useful for the reader to understand where forests currently exist in the region.

This information can already be found in the Fig.1 in which the shaded and color pixels show the forest cover. We will better describe this in the caption of Fig. 1.

It is unclear how much of a buffer was applied to the central track of each storm for selecting which pixel locations were affected by cyclones.

The search area applied to each cyclone was 2, 3 or 4 times the reported diameter. The diameter information for each cyclone was taken from the JWTC database. We will clarify issue in the main text around L46 of the original submission. The approach has been described in detail in the method section of the original submission (L193-222).

Minor methodological question: How were pixel locations dealt with that received multiple cyclones within the same year?

Locations that received multiple cyclones are dealt with in the same way as location that received only one cyclone. Given the approach used in this study, the time frame should not be one year but should be shortened to 60-days. In the revised manuscript we will try to quantify how many pixels (expressed as the percentage to of the total forest area affected by tropical cyclones are hit multiple times within two months. The two months period is justified by the 60-day window used in this study. Although the effect size should not be strongly affected by the occurrence of more than one cyclone within 60 days (due to the use of a cyclone-specific reference area), the change in LAI might be attributed to the wrong cyclone. The current analysis would benefit from a low share of pixels that received multiple cyclones within two months.

Kudos to the authors for adhering to policies regarding open data and reproducible code. One comment is that the git repository for the code linked on Zenodo is exceptionally large at nearly one GB. Perhaps posting another git repo of the final code (with no commit history to reduce size) would be useful. I could be wrong.

We will look into this and will try to reduce the size of the files that accompany a revised manuscript.

Comment 3. Figure 1: This is a nice figure but I have some suggestions that I think will increase its interpretability for the reader. I strongly suggest not to use decimal degrees in the denominator, given the actual area will vary with latitude. I suggest presenting the Affected Area as a fraction of the total area per year.

This comment made us realize that the unit can indeed be misinterpreted. We aggregated the affected area for 1 degree latitudinal bands. deg^{-1} can and will be dropped from the units.

I suggest selecting a color-blind friendly color palette for panel a, and a legend to indicate areas where forest is not the dominant land cover.

We noted that Biogeosciences provides a link to a simulator that shows to those who are not color blind how the figure will look like for color-blind readers. A revised version of Fig 1, 3, 4, S2 and S4 will make use of a color-blind friendly color palette. We will improve the caption of Fig 1 to clarify that blank land pixels have a low forest cover.

A legend for the different lines would aid interpretation, in addition to a slightly more detailed or paraphrased explanation in the legend. Maybe rename the groups to something more informative (wind, precipitation, wind and precipitation) than groups a, b, c.

We will add a legend to make it easier to link the different lines to the different definitions. An example of a "paraphrased explanation" can be found in the revised caption for Fig S2 shown in the reply to comment 1 of referee 1.

Comment 3. Figure 2: This figure is useful, but I have some suggestions: I suggest adding a legend for the surface and cyclone characteristics.

We will revise this figure by adding a legend. We will replace the ENSO index by the Japan-Taiwan Pacific index in random forest analysis. This substitution will help linking the results (Figs 2 and 4) to a revised discussion (in line with the new title). Figure 4 is a newly added figure and can be seen in the reply to comment 6 of referee 1.

Comment 4. Figure 3: Any reason that SPEI is not used in the random forest analysis, but is used in Figure 3?

SPEI is used both in the random forest and the regression tree. Following internal revisions, we forgot to update the label "prior drought state" in Figure 2. A more consistent label should have read "prior SPEI". We will revise Figure 2 accordingly.

suggest: "Affect area" -> "Affected area"

Thanks for spotting. This will be corrected.

I would have thought the boxplot of the decrease in accuracy always be a positive number?

A negative importance means that removing a given feature from the model actually improves the performance which is possible because of the use of permutations. If a variable was hardly predictive of the outcome, but still selected for some of the splits, randomly permuting the values of that variable may send some observations down a path in the tree which happens to yield a more accurate predicted value, than the path and predicted value that would have been

obtained with the original ordering of the variable. We will clarify this issue around L127 of the original submission.

It is a bit odd that wind speed (or some other wind metric) is not included here.

Note that following comments by referee 1 and 2 on the selection criteria all analysis will have be re-run. Given revised selection criteria, wind speed may become more (or less) prominent in the revised manuscript.

In the original submission we selected the top 6 drivers (based on the random forest; Fig 2) to build the regression tree (Fig 3). Wind speed ranks 7th in the random forest and was therefore not included in regression tree. Likewise we decided, for clarity reasons, to show only four levels in the regression tree. These are arbitrary choices but no matter how we changed these choices, precipitation comes in first and dominates the regression tree. Which we think is an interesting finding as it seemed that both referees were expecting wind speed (this comment referee 1) or intensity category (referee1 several comments) to be among the most important drivers to explain changes in forest leaf area. Although the effect on leaf area of the precipitation brought by cyclones is easy to grasp (comment 1, referee 2), the frequency of this process is surprising and can be explained by the pressure field (New Fig 4, see reply to comment 6 of referee 1). This pressure field is responsible for summer droughts being ended by tropical cyclones.

I suggest also briefly describing how this decision tree was derived and selected in the figure caption text.

The revised caption will read "Figure 3. Environmental drivers contributing to an increase of leaf area index following the passage of a tropical cyclone. The fractions of a negative, neutral and positive effect size are listed for each box in respectively orange, blue, and green. The number of events is listed as the percentage of the total number of events in the random tree (n=1309). To reduce the collinearity of the input variables, only the six variables with the highest accuracy (Fig. 2) were used to create a four-layer decision tree."

The numbers in yellow are not going to be very visible if/when this is formatted.

We will change the color scheme of Fig 3 (see also comment 3 of referee 2) and ensure that it remains consistent with the colors used in Fig. S2.

I know this is sort of the single best decision tree from the ensemble, but perhaps it would be good to report something like an R2 value?

According to our understanding of the R-package used, R2 cannot be easily calculated. As an alternative, the performance of the decision tree could be evaluated by splitting the data in a training and an evaluation set. *We will look into this request when preparing a revised manuscript.*

Comment 5. Table A1: I suggest spelling out Effect Size, instead of the ES acronym.

We will do so in the revised manuscript.

Comment 6. Figure A1: Copying my comment from Figure 1 -> I strongly suggest not to use decimal degrees in the denominator, given the actual area will vary with latitude. I suggest presenting Forest Area as a fraction of the total area, and presenting Affected area in km^2 yr^-1 km^2 (or just a fraction per year).

This comment made us realize that the unit can indeed be misinterpreted. We aggregated the affected area for 1 degree latitudinal bands. deg-1 can and will be removed from the units.

Please spell out 'TC' and add a legend corresponding to the different line types.

We will do so in the revised Fig A1.

Comment 7. Figure A2: This figure is quite complicated and I am struggling to interpret it. I suggest using a facet of different panels for each different definition. A legend would also help. Also please remind the reader what C-1 through C-5 are.

*C1-C5 show the different intensity categories according t*o (Schott et al., 2021). W*e will follow the suggestion of referee 2 to show this result for each definition and to add a legend. An example of how a revised Fig A2 could look like is presented in the reply to comment 1 of referee 1. Note that the color scheme still needs to be adjusted to the needs of the color-blind*

and that the numbers underlying this figure have to be revised as a consequence of changing the selection criteria.

Comment 8. Figure A3: Minor point: doing significance tests on discretized groupings of a continuous variable is generally not advisable from my understanding of best practices in statistics. The authors may wish to consider a regression, or using a nonlinear generalized additive model to show the increase and decline of the effect size with respect to return frequency.

Agreed. We will replace the test on the discretized groupings by a regression on the entire dataset.

Comment 9. Figure A4: Nice figure, although the color palette is not suitable for the colorblind. The 0-80% stretch seems to miss the focal part of the distribution of the data. Perhaps rescale the color map from 0-50% to improve the contrasts. TC acronym unnecessary.

The color palette, the scaling issue of the legend and the acronym will be revised and changed accordingly. The revised figure could look as follows:

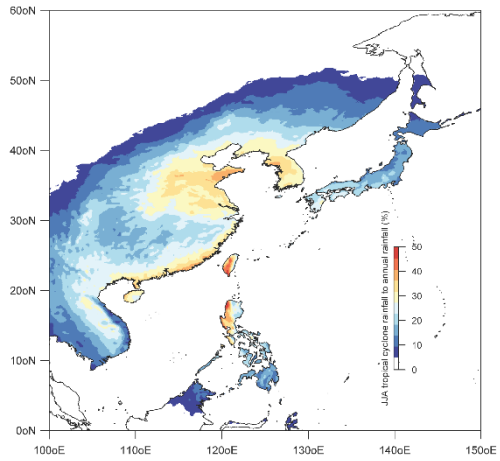

Figure S4. Share (%) of the rainfall contributed by tropical cyclones in June, July and August to the total annual rainfall over Eastern Asia between 1999 to 2018.

L34: I suggest stating the name of the product within each citation.

We will do so in the revised manuscript.

L74-50: This could be rephrased to be clearer. I suggest using commas to separate clauses.

This comment is not clear. Something might have gone wrong with the line numbers.

L133: Would be good to add an average LAI % increase because of the additional rainfall.

This would indeed be a nice result to provide. We will try to extract this number from our analysis and report it in the revised manuscript around L135 of the original submission.

L150-151: I don't think this text, or this paragraph, attempting to connect summer dry spells to cyclone generation is really necessary.

We consider this an essential part of the discussion as it explains the required conditions to have an increase in LAI following the passage of a tropical cyclone. It also provides a meteorological relationship between droughts and tropical cyclones which is essential to accept that summer droughts being ended by tropical cyclones are not just rare events but two events caused by the same atmospheric conditions thus making their occurrence highly correlated.

We interpreted the discrepancy between our position and this comment by the referee as an indication that we need to further develop this part of the discussion. We consider adding Fig 4 (see our answer to comment 6, referee 1) into the revised manuscript and add additional discussion around L159 of the original submission. Changes in the discussion should ultimately support the new title "Tropical cyclones facilitate recovery of forest leaf area from summer droughts in East Asia".

L162: This is a bit confusing to me, or at least the wording is around "forest dwarfing". Is small stature of forests being attributed to confer resistance to cyclone damage?

Small stature of forests is indeed being suggested as a outcome of natural selection in regions with a high return frequency for cyclones. High return frequency should here be regarded in relation to the longevity of an individual tree. This nuance will be added to the revised manuscript.

L164-165: "The observed frequency of positive vegetation responses to cyclones suggests that the present day vision of cyclones as agents of destruction" - this statement has problems. First, the

reference to the Negrón-Juárez and Nelson studies is incorrect. These studies did not focus on cyclones, but on Amazonian downbursts (sometimes coming from squall lines), which is a very different meteorological process.

Thanks for spotting. We will remove the wrongly cited studies from the revised manuscript.

Second, the following are a couple papers quantifying the negative impacts of cyclones (and hurricanes) on forest biomass or mortality, which are potentially important counterpoints to the assertion that cyclones may be providing a forest benefit.
(Negrón-Juárez et al., 2014 Remote sensing ; https://www.mdpi.com/2072-4292/6/6/5633)
(Negrón-Juárez et al., 2014 Remote Sensing of Environment; https://doi.org/10.1016/j.rse.2013.09.028)
(Negrón-Juárez et al., 2010 JGR Biogeosciences; https://doi.org/10.1029/2009JG001221)

Thanks for suggesting these references we will consider citing them in the revised manuscript.

Otherwise there is a very large literature of forest disturbance impacts from Central to North American hurricanes. However, I take the authors' point that additional rainfall can (occasionally) result in LAI increases.

We do not contest that individual tropical cyclones might be damaging especially towards the eye of the cyclone. With this study we want to point to a circularity in much of the disturbance ecology, i.e., by selecting the most damaging events for further study, the community might overlook many events (including class 3, class 4 and class 5 typhoons, see Fig S2) which are not damaging or might even result in a mean benefit for forest LAI. Note that a mean benefit does not exclude the possibility of serious damage close the track of the eye. Given the conditions which are needed to observe an increase in LAI, the correct conclusion is not necessarily that tropical cyclones increase LAI but is, more likely, that tropical cyclones help forest to recover from summer droughts (an increase in LAI compared to a reference area that experienced the drought but that did not receive the precipitation from the cyclone). We find this to be the case for 31% of the tropical cyclones in the study regions, which we would not label as "occasionally".

We considered this comment as a clear indication that the discussion, conclusion, title and abstract needs to be revised to better stress the nuance of our findings, i.e., the wide-spread

antagonistic effect that might occur when in East Asia a drought is followed by a tropical cyclone.

L170: The Stuivenvolt-Allen et al 2021 paper refers to increased fire weather in northwestern North America. Again, given what the sentence says, I think this citation is used incorrectly.

This citation was chosen deliberately to stress the uncertainty that may come from teleconnection. Sadly, the prefix "tele" was lost during text editing. If this sentence is retained in the revised manuscript we will add it back to restore the integrity of the sentence and the citation. Most likely this thought will be removed from the discussion as it broadens the discussion too much.

L294-296: I think the citations are used incorrectly in this paragraph. "By design, the latter approach is not capable of identifying neutral or positive impacts of cyclones on leaf area." All but one of these studies have nothing to do with cyclones - so why would they be discussed with respect to cyclone precipitation? The Ozdogan et al., 2014 study is not about cyclones, but windthrows caused by downbursts and tornados. Honkavaara et al 2013 is about detecting forest damage from winter ice storms. The Forzieri et al 2020 paper (of which the second author is a co-author of) is about large-scale windstorms over Europe - again, not cyclones, typhoons, or hurricanes. I argue the authors should be far more careful in their review of the literature and attribution of citations.

We reread L294-296 in the light of this comment but disagree with the referee. The sentence reads "…in contrast to studies that attribute decreases in leaf area or related satellite-based indices to different disturbance agents including cyclones (Baumann et al., 2014; Honkavaara et al., 2013; Forzieri et al., 2020) including cyclones (Hayashi et al., 2014)". The use of "different disturbance agents" expands our concern from storm damage to other disturbances such as pests, harvest and fires. To justify broadening our concern we cite studies from different disturbance agents. This sentence continues with "including cyclones" which stresses that the previous part of the sentence did not refer to only cyclones.

Given that the sentence confused the referee, we propose to move the citations closer to the relevant part of the sentence as follows "…in contrast to studies that attribute decreases in leaf area or related satellite-based indices to different disturbance agents (Baumann et al., 2014; Honkavaara et al., 2013; Forzieri et al., 2020) including cyclones (Hayashi et al., 2014)".

L304: This seems odd (or perhaps the phrasing is?), the uncertainty almost certainly scales with the magnitude of the LAI estimate. Is 0.18 the domain mean uncertainty over forests? Also what does 0.18 correspond to - a 95% confidence interval?

Thank you for raising the issue. Referee 1 made a very similar comment (comment 5). Indeed, Fig. 6 displayed in the report discussing the quality of the LAI product (Jorge, 2018*) used in this study 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 (hence, the 0.18 will no longer be used). The section describing the quality control will be adjusted accordingly (around L300-317 in the original submission).*

L306: Minor issue: Should it not be 0.5(sqrt(0.18**2 + 0.18**2)) instead of 0.25(sqrt(0.18**2 + 0.18**2)), because it's within a ±0.25 margin of the affected area?

Thanks for spotting. There was a typo in the manuscript the text should have read 0.25 = ($\sqrt{0.18^2 + 0.18^2}$). *This criterion and calculation will no longer be used in the revised manuscript.*

L315: This statement is a bit concerning - "Events for which ES < \delta ES were not further analyzed". Filtering the data on account of small effect sizes will certainly bias any subsequent analysis. I think the way this is written could use some clarification.

The \delta ES is an estimate of the noise present in the LAI data. ES is the signal. If the signal is smaller than the noise, the signal should not be interpreted. Not doing so would mean that we are over interpreting the results. As we would have to decide whether an ES is positive negative or neutral whereas the results tell us that the noise exceeds the signal and that therefore we cannot come to a conclusion.

Nevertheless, our estimate of \delta ES was based on several crude assumptions which resulted, in our opinion, in giving too much weight to a rough estimate. Comment 5 by referee 1 and the previous 3 line comments by referee 2 suggested reviewing the selection criteria. We now propose the following revised and simplified selection criteria (the text below will be added to the method section of the revised manuscript).

The calculation of the effect size assumes having a similar leaf area index between the area that will become the affected area and the area that will become the reference area after the passage of a cyclone. If the absolute difference in leaf area index between the reference and the affected area was less than 15%, the effect size calculated for this event was included in subsequent analyses. This can be formalized as:

$$\left|\frac{\overline{LAI}_{bef\ aff}}{\overline{LAI}_{bef\ ref}} - 1\right| < 0.15$$

Where the 0.15 represents the 15% threshold that was guided by the specifications of the remotely-sensed leaf area product used in this study (Fig 6 in Jorge, 2018). *This quality control criterion reflects the idea that prior to the passage of a tropical cyclone, the LAI needs to be similar in what will become the reference and affected area. If not, changes in leaf area following the passage of the cyclone cannot be assigned to its passage.*

Following the passage of a tropical cyclone, a change in LAI of less than 15% before and after the passage of the cyclone was, in line with the quality control criterion, too small to be considered substantial. Such events were classified as cyclones with a neutral effect size. This classification was formalized as:

$$\left|\left(\overline{LAI}_{bef} - \overline{LAI}_{aft}\right)_{aff} - \left(\overline{LAI}_{bef} - \overline{LAI}_{aft}\right)_{ref}\right| < 0.15 * \left(\overline{LAI}_{bef}\right)_{ref}$$

Due to these changes in the selection criteria all analysis will have to be re-run and all figures and tables will have to be updated when preparing a revised manuscript.

L319-324: Were cyclone characteristics (2 & 3) matched to the corresponding LAI pixel location, or was this an average for the entire trajectory of the cyclone?

We took the average value along the trajectory. We will clarify this around L325 of the original submission.

L327: A cautionary note that the precipitation from ERA5 is known to have strong biases in many locations. I don't suggest reanalyzing this, but perhaps a more recent version of GPCP or GPM IMGERv6 would be better for this.

We considered using the GPCP product but its spatial resolution was considered too coarse (2.5 degree x 2.5 degree) for this study. Following up on this comment we will compare the GPCP to the ERA5-Land data over the study domain and if informative, add the results to the SI of the revised manuscript.

L341: This is the citation for the R package "psych", not "factor analysis". By all means cite the R package, but again the attribution of the citation is written incorrectly.

We agree with the referee and will replace this citation by Kaiser *(1958).*

L351: Please restate what the reference period was in this section.

We will do so in the revised manuscript.

References used in the replies

Forzieri, G., *et al*.: A spatially explicit database of wind disturbances in European forests over the period 2000–2018, *Earth Syst. Sci. Data*, 12, 257–276, https://doi.org/10.5194/essd-12-257-2020, 2020.

Honkavaara, E., *et al.*: Automatic storm damage detection in forests using high-altitude photogrammetric imagery, *Remote Sens.*, 5, 1405–1424, https://doi.org/10.3390/rs5031405, 2013.

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

Kaiser, H. F.: The varimax criterion for analytic rotation in factor analysis. *Psychometrika*, 23, 187-200. doi:10.1007/BF02289233, 1958. Link

Baumann, M., et al.: Landsat remote sensing of forest windfall disturbance, *Remote Sens. Environ*., 143, 171–179, https://doi.org/10.1016/j.rse.2013.12.020, 2014.

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

Hayashi, M., *et al*.: Quantitative assessment of the impact of typhoon disturbance on a Japanese forest using satellite laser altimetry, *Remote Sens. Environ.*, 156, 216–225, https://doi.org/10.1016/j.rse.2014.09.028, 2014.