Referee #1 reply:

Remark	Reply
Major remarks	
Major remark concerning the correlation plots (Fig 9, pp. 17), detailed explanation: "If I am right, these correlation plot show both a spatial and temporal correlation (I suppose points represent yearly values within each sub-period for each county). However, spatial and temporal correlations should be considered separately. A possible solution could be to compute the temporal average in each sub-period and thus plot only 8 points. Also, a comparison against the variables in the whole period (2000-2012) is needed. The following paragraphs need to be fully revised and adjusted and final outcomes need to be checked before accepting the manuscript for publication."	• The referee correctly points out that the correlation plots show yearly values within each sub-period for each county. However, these values represent changes relative to the pre-Katrina level of their respective variables. That is, we index the change in night light intensity and the change in, say, population to 2004 (2004=100) for each respective county. As such, the plotted values are all changes relative to the 2004 level of their variable for their respective county. • **If we understand correctly, the referee suggests to compute yearly averages of all 8 counties, to plot only one value per year**. We have not done so, as we report substantial differences in impact of Katrina on counties' respective economic activity (as discussed in detail in sections 3.1 and 3.2). Averaging over these counties within years smooths out much of the variation of interest. **We have, however, done the analysis along the lines of the suggestion of the referee. Averaging across all 8 countries per year, the correlations for 2005-2012 remain rather robust: population (0.747), employment (0.432), income (0.399), and GDP (0.362).** • Related to the bullet above, the **indexation ensures that cross-sectional (spatial) differences in the level of light intensity and the level of the economic variables is controlled for. We have clarified this further in section 2 and by adding a footnote to the first line of section 3.1.** • If instead the suggestion is to compute averages across the entire sub-period (i.e. an average of all years per county per sub-period), we would lose most of the time-variation of the analysis. As we discuss in sections 3.1 and 3.2, the change in the variables of interest is rather heterogeneous over time even within the post-Katrina sub-period. As with the previous bullet, this would imply we lose most of the variation of interest. • Concerning correlation plots for the entire period 2000-2012: we deliberately make a distinction between the pre-Katrina (2000-2004) and post-Katrina (2005-2012) period. As discussed in the paper, the pre-Katrina period is characterized by a higher degree of top-coding. Changes in light intensity pre-Katrina are therefore driven by pixels below this threshold. As this is markedly different from the years post-Katrina, we separate the two periods. **We have, however, added correlations for the whole period (2000-2012) in the appendix, and added a short description of these results at the end of section 3.1**. As this combines the two periods, of which we observe rather weak correlations between light intensity and economic variables in the first period, **overall correlations are lower but not qualitatively different from the reported correlations for 2005-2012.** • **Minor remark on Figure 9: these plots are indeed correct. The correlations are very weak and sometimes negative prior to Katrina**, exactly because of the reasons mentioned in the bullet above. This is also discussed in-text.
"… the paper is written in a very long and, somewhere, written in a convoluted way with several repetitions (especially in the introduction). I would encourage the authors to focus on key and relevant sentences and synthesize the whole manuscript in a more condensed version."	• We thank the referee for pointing this out and will revise the manuscript appropriately. • **We agree that the discussion and conclusion can be merged and have done so,** and we have paid special attention to **condensing the introduction section**. • Minor textual comments have been acknowledged and addressed.
Minor remarks	
Concerning the claim that few studies examine how night lights and economic activity relate to each other in shock times, and that there is relatively poor understanding of what changes in night light intensity reflect exactly especially when downturns are considered (pp. 2): "I (partially) respectfully disagree with this sentence. Please check here: https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1002/2014GL	• **We thank the referee for pointing us to this paper**. The authors show that NTLs along river networks can be used as a reasonable proxy for flood exposure. Using country-level flood loss data (from EM-DAT), the authors show a positive correlation between light intensity along river networks and normalized flood damages (per km², based on country-level aggregates). The link between flood exposure and night lights is therefore evident. However, this paper, like many others, provides no guidance as to how local variability of night light

061859. This was, to my personal knowledge, the first application of NTLs in hydrological studies (where hydrological extremes, such as floods, are considered). Therein, NTLs were linked to economic and human losses associated to flood events at the global scale, yet not focusing on the short-term scale. Therefore, I would suggest slightly revising/updating this first introductory paragraph."	intensity may reflect the occurrence of such flood events at their respective locations. This is especially relevant w.r.t. downturns, which – as the authors also indicate in their paper – is less common than growth or stagnation. • **We have updated the literature list to include a reference to the mentioned study as it is relevant to our literature discussion.**
"I really appreciate your final goal, but how would you operate in less-developed areas, where NTL values are smaller compared to developed regions? How would you manage different study areas, with completely different socio-economic characteristics? Please elaborate more on this, maybe in the discussion."	• We appreciate this comment, as in part it points to the value of our work in areas with less reliable socio-economic data. However, we also acknowledge that further research is required to evaluate the relation we explore in our paper in a less-developed context. Promising avenues for this could be countries with lower income per capita, but high flood risk, that do provide these data. • However, while we do not claim external validity of our results in other parts of the world, for our results not to hold the relation between changes in night light and changes in economic activity would have to be of a different nature, at least in times of natural disasters. While this could be true, this hypothesis really does need empirical testing. • We also acknowledge that night light data may be less useful in areas that are sparsely populated and/or very dimly lit. This has been pointed to for example by Chen and Nordhaus (Journal of Economic Geography, 2015). • **We included a brief discussion on these issues in the discussion/conclusion.**
"Figure 1 (pp. 5) is not necessary in my opinion, since it is also shown as panel a in Figure 2".	• **We agree; this information is now condensed in what was previously Figure 2.**
Question about employment growth in different sectors (pp. 5): "This part is not clear to me. Would you please elaborate more on this?"	• The confusion may have been caused simply by the phrasing. Our point is that services-oriented sectors experienced a severe decline as a consequence of the shock from Katrina. While the construction sector experienced some employment growth (roughly 7 percent), all other sectors experienced losses between 10 to 20% of employment. The net employment loss was large. • **We have clarified this in-text to resolve any ambiguity here**.
Comment on the need for a map of the 8 counties which form the focus of the analysis (pp. 7): "A map showing the geographical locations of these counties is needed (maybe revise Figure 1, which is simply a copy and paste from DFO – also check the copyright."	• **In what is now Figure 2 (in the revised manuscript), the states of Louisiana and Mississippi are displayed, with the counties of interest labeled by name. In Figures 5 through 7 (in the revised manuscript), these counties are depicted again, and now in great detail. We think adding an additional map to show the geographic locations of our study area is not of added value.** • **We have requested and received permission to reprint the DFO flood map for academic publishing purposes. Permission was granted by the author of the flood map, Robert Brakenridge (Founder and Associate Director of the Dartmouth Flood Observatory), in May 2022. We now depict only an excerpt from the flood map as the top left panel of what is now Figure 1 (in the revised manuscript).**
Comment on Figure 2 (pp. 8): "I would recommend showing corrected NTLs."	• We have deliberately displayed uncorrected NTLs here to show that regardless of correction methods, the clear drop in NTL intensity is visible in the affected area. **We believe this to be a powerful message, and it clarifies the starting point of our analysis before getting into discussions on how the temporal corrections on the night lights should be performed**. • In Figures 6 and 7, which also focus on changes in light intensity, we apply the corrected NTL data and show that the pattern holds.
Comment on the indexation of NTL in Figure 4 (pp. 10): "This computational step is not explained in detail in the text. I would strongly suggest you to add this part. Also, in Figure 8, a value of 1 is used rather than 100. Please select a uniform value."	• **We have clarified the indexation step in the main text, at the start of section 2.2 (Visible impacts from space).** • **Furthermore, we hope to have clarified this approach in our reply to the first major comment on the correlation analysis**. • We thank the referee for pointing out the mixed use of 1 and 100 as our base for the index. **We have of course adjusted this to have one value only (100) in all figures and in all text.**
Comment on Figure 6 (pp. 12): "Did you find any increment in NTLs? Also, since in the text you are citing several cities, these names should be added to Figures for a better understanding."	• We hope to understand the first part of this comment correctly: does the referee mean whether we have also found increases in NTL intensity? If so, the answer is yes – albeit only for those locations that were not top-coded prior to Katrina. Increases in

	NTL intensity have been masked in this figure to focus on decline only. • **We acknowledge the request to add city names to the Figures and have added them to what are now Figures 5 and 6 (in the revised manuscript) to guide the reader in the description of regional effect.**
Comment on Figure 7 (pp 12): "It would be interesting to see the difference between 2005 (Katrina's year) and 2006, to identify and locate areas already recovered."	• Strictly speaking this suggestion implies showing a figure with the difference in pixels values between Figure 6 and Figure 7. However, by placing these figures on one page, we feel that this difference can be observed reasonably well by comparing the figures. • Moreover, the difference between 2005 and 2006 crucially depends on the drop in light experienced between 2004 and 2005. First, this implies that changes between 2005 and 2006 should be interpreted conditional on the change in the previous year, which we believe to be hard to communicate in a single figure. It could be achieved by indexing light values to 2004 in the map, but this results in a new (visual) problem: because of the wide range of pixel values, indexed values may represent small absolute changes at low initial light intensity equally with larger absolute changes at higher levels of initial light intensity. This quickly turns into comparing apples with oranges. We therefore chose to depict the changes in the maps in absolute terms, benchmarked to the base year 2004. • After aggregating light intensity to the county level, as we do in the remainder of the analysis, these issues no longer play a role.
Comment on Figure 8 (pp. 15): "While I am in favour of checking the relative difference in NTLs between 2005 and 2004 and in population between 2006 and 2005, the identification of 3 groups looks a bit speculative, especially for Harrison, Jackson and Jefferson, whose variability in time is negligible. Also, NTLs are known to be a proxy for population and GDP, yet they do not represent exactly these variables. For these reasons, it is not possible to observe similar changing rates, but it is more reliable of observing similar directions of change. As a consequence, the whole discussion of results should be smoothed out."	• We thank the referee for pointing out that we may have ventured into too much detail explaining the results variable-by-variable and county-by-county. **We feel that it is imperative to make the point that the patterns in direction of change are similar, but that there is substantial heterogeneity in impacts of Katrina on economic activity from county to county**. We acknowledge that a **focus on similar rates of change is unnecessary and have adjusted the discussion accordingly**. • **We condensed and smoothed the discussion in section 3 considerably, and now focus only on the main observations. We discuss these patterns in relation to the light series and conclude with the correlation analysis that further confirms what we observed and discussed for the county time series.**
Comment on Figure 8 (pp. 15): "Is this an error? Before you stated that results shown here refer to Elvidge calibration method. Please check."	• This is indeed an error and we have rectified this. **The note now correctly reads: "Night lights are calibrated using the Elvidge et al. (2014) method."** We thank the referee for noticing this typo.

Referee #2 reply:

Remark	Reply
"The DMSP data are not cited correctly."	• **Our citation referred to the previous distributor of the data; we have updated the reference according to the referee's suggestion.**
Textual comments: tenses and grammatical errors.	• **We thank the referee for the detailed textual comments and have made the necessary adjustments.** • **We have retained the notion "Based on own calculations" in figure notes, to be fully transparent. The referee has marked these notions in Figures 8, A1, and A2 as sounding strange, but without further suggestions to remove this or to change the phrasing. If indeed desired, we can remove this or change the phrasing.**

Referee #3 reply:

Remark	Reply
Major remarks	
"The empirical approach adopted in the paper, the correlation analysis in Section 3.3, is not suited too well for providing a reliable answer to [the research] question, for at least three reasons: 1. Focus on only 8 severely damaged counties may imply that "the the analysis may not be representative of all counties hit by Katrina, not to mention regions elsewhere hit by other hurricanes. 2. "The second reason is that this correlation analysis lacks a benchmark, i.e., does not control effectively for the	• We appreciate the concerns expressed by the referee. In broad lines, the suggestion is to expand the analysis to have a larger sample: more counties (the entire U.S.) and more hurricanes. We reply to this suggestion in the next bullet. First, we discuss the three issues that are raised: • (1): Our focus on a small set of counties is driven by the goal to compute not only a statistical correlation between changes in socioeconomic variables and changes in light intensity as a result of Katrina, but to place these in the broader context of

regular relationship between night lights and economic indicators in "normal" times. The paper presents correlations for the 8 counties in the five pre-Katrina years, but this analysis produces weird results (negative rather than positive correlations). Taken at face value, these correlations suggest that night lights are poor predictors of economic activity in the 8 counties in "normal" times. Why should they predict economic activity more reliably in times of disaster?" 3. "… the results may be biased by top-coding of the night light data"	the evidence on the wide range of impacts of Katrina. Such a discussion would be impossible to extend to the entire U.S. (and thus a multitude of hurricanes) within the scope of this paper. Moreover, we do not have the necessary data on materialized damages available for the universe of hurricanes in the U.S., again, not within the scope of this paper. This would require the construction of a much larger dataset, that moreover would also have to include other types of disasters. We thus do not wish to claim that our study answers the broader research question in the full sense, but we do wish to provide a case for which we analyze this relation in detail. More cases, or a more extensive statistical analysis for a larger geographical area – as suggested by the referee – are a way forward in this field and are placed on the agenda for future research. • (2) and (3): issues 2 and 3 are related in our view. We indeed present correlations between the change in socioeconomic variables and night light intensity in the five pre-Katrina years. **Given the substantial degree of top-coding in the region prior to Katrina, the relation between changes in socioeconomic variables and light intensity is weak for these years**. Only after Katrina do the top-coded regions show light intensities below the saturation threshold, meaning we can identify meaningful changes only after Katrina hits. While this is evidently an argument for assessing areas where the saturation threshold plays no role prior to a big disaster (and we discuss this as a suggestion for future research), it also implies that we focus on the post-Katrina period. We rely on the broader evidence from the literature on the relation between night lights and economic activity, e.g. Henderson et al. (2012) and the literature that sprouted from this – as also discussed in our paper. • ***Addition revision 05 Nov****: we have extended the analysis on the relation between housing damage and change in NTL intensity to the cases of hurricanes Wilma and Rita and included this in the appendix. We find a similar relation between housing damage and a reduction in NTL intensity Hurricane Rita in the affected counties in southwest Louisiana and eastern Texas. While overall damages caused by hurricane Wilma in Florida were similar in overall magnitude (~19 billion US$), relative housing damage was much more limited in Florida. In line with these relatively lower damages we find no reduction in NTL intensity there. Another reason for the small NTL effect may be that Wilma made landfall in late October, leaving only 2 months of observations for the year 2005 (compared to 4 and 3 months in the case of Katrina and Rita, respectively). Hence, our comparison with these other Hurricanes in Texas and Louisiana shows that our observed relations between NTL and damages are not unique to Katrina when these damages are relatively high.* • ***Addition revision 05 Nov****: to address the issue of generalizability, we have added a paragraph in which we explicitly compare our results to the few studies in the literature that link night light impacts of major natural disasters to economic indicators. We have included a discussion on Elliot et al. (2015) on typhoon impacts in coastal China, and on Gillespie et al. (2014) on the coastal impacts in Aceh of the 2004 tsunami.* • ***Addition revision 05 Nov****: we have also included an explicit reference to Kocornik-Mina et al. (2020) in the main text, who study impacts of flooding on urban economic activity and potential relocation in a worldwide panel, with the majority of cases in developing countries. We confirm in our data, but refer to Kocornik-Mina et al. (2020) to stress that top-coding is of much less concern in low-income countries. As such, our results present a lower-bound of the true impact of hurricanes on economic activity as proxied by the change in night light intensity; Kocornik-Mina et al. (2020) reproduce a similar result for a broader set of floods around the world. However, as we stress in the paper, our paper is among the first to relate these changes in night light intensity to economic indicators, in order to improve our understanding of what these changes in light intensity reflect. (This point was made in a footnote, but*

	admittedly did not get this message across as forcefully as is warranted. We trust that this amendment addresses this issue sufficiently now.)
The referee then suggests to consider "… using a more sophisticated empirical approach that can be expected to yield a more reliable answer to the core question of [the] paper."	• We thank the referee for the detailed and extensive suggestion of expanding the work into a systematic analysis that envelops a larger number of counties (the entire United States) and (as that would then also be necessary) a larger number of hurricanes (or disasters more broadly). • However, we would also like to stress that this is an entirely different angle from which to formulate an answer to the central question of the paper. An analysis as suggested by the referee implies expanding the scope of the paper to the entire United States, which also implies not only analyzing the effects of Katrina, but of the universe of disaster (not only hurricanes) that occurred within the U.S. in this time period. That is, for the suggested analysis to provide meaningful results, one cannot assume Katrina to be the only shock that occurred in the US in this time period. The variable D(i) would therefore have to include a much larger set of disasters and their accompanying (threshold) damage, as we cannot assume times to be "normal" when other shocks than Katrina occur (either within the current research area, or elsewhere in the U.S.). While we are fully in favor of a study as suggested by the referee, it is a markedly different approach from our paper and requires the collection of a considerable body of data on disasters and their material consequences (e.g. housing damage by county), or through the use of physical intensity measures such as wind speeds or amount of precipitation as in e.g. Elliot et al. (Journal of Urban Economics, 2015), Kocornik-Mina et al. (American Economic Association: Applied Economics, 2020) and Felbermayr et al. (World Development, 2022). We believe this is (far) beyond the scope of our current paper. • In addition, **we believe that our conscious decision to focus on a smaller area allows gaining a deeper understanding of the meaning of changes in light emissions after a disaster by combining statistical insights with additional research and data.** This quickly becomes harder as the number of shocks grows and the research area expands.
"I find the lengthy verbal descriptions of the association between changes of night light intensities and economic indicators in Sections 3.1 and 3.2 rather uninformative and confusing. I suggest skipping them. In addition to this, Section 4 may be dropped after moving the few points not made elsewhere in the paper to other sections."	• As similarly noted by referee #1, sections 3.1 and 3.2 contain a discussion that may be too detailed**. In line with referee #1 this section has been rewritten to contain less focus on county-by-county and year-by-year changes, and rather describe the broader patterns that we observe in the data. We do think that it is valuable to point out that disaster impacts to local economic activity are not a one-size-fits all pattern, regardless of the extent to which night lights reflect these impacts.** • **As also noted by referee #1, we agree with the suggestion to condense section 4 (discussion) and section 5 (conclusion) into one section and have done so.**
Minor remarks	
"I suggest concentrating [the] discussion of the three methodological issues of the night light data (intertemporal differences, top coding, overglow) in a single subsection. The discussion of overglow on p.9 (FN 10) is misplaced in my view."	• We appreciate the suggestion to group the discussion on methodological issues into a single subsection. However, we also feel that this discussion is already condensed in section 2.2, where we discuss the two main issues and refer to further details in the appendix. **We deliberately placed the discussion on overglow in a footnote – still within this section – as it is of limited importance to our analysis. The point could be made in one sentence in the main text, but this would also imply losing context as to why this issue is of no concern in our study, but why it may play a role in others.**
"I strongly suggest using average night light intensities (by square kilometer) rather than sums of night light intensities across pixels throughout the paper. The sums do not control for differences in geographic sizes of the counties. This is particularly relevant for Figure 3."	• All analyses that we present in the paper are based on indexed values of light intensity and socioeconomic variables. That is, we focus on changes in variables relative to the base year (2004 = 100) for each county. As such, using the sum of total light, or average light per square kilometer results in computationally identical changes. This therefore does not make a difference for our results. • This is true also for what is now Figure 2 (revised manuscript), in which we plot the change in light intensity between 2004 and 2005 by county, using the same indexation. **We note, however,**

	that the legend header "Sum of NTL (2004 = 100)" in combination with the description in the figure note may have caused some confusion. We have clarified that we plot indexed light intensity, and thus *changes* in light, rather than total sums of light to avoid any ambiguity. We have also added some guiding text in the figure note to help the reader with interpreting the indexed values (as this is the first encounter of the indexed values in the text). • **We have added a similar clarification in the figure note for what is now Figure 3 (revised manuscript). We have not adjusted the y-axis, since we believe the notion (2004 = 100) and the figure note now clarifies sufficiently that the graph depicts indexed sum of light**. • The only part of the analysis that does not make use of indexed values of light intensity is the intercalibration of the night light images to facilitate cross-time comparison. Here, we follow the methodology by Elvidge et al. (2014) and Zhang et al. (2016) and adjust pixel values with the calibration parameters provided by the two respective studies. Subsequently, pixel values are aggregated to the total sum of light by county. In all analyses that follow, for the sake of completeness, we index light intensity values to 2004 for each individual county.
"I also suggest using a single measure of changes of night [light] intensities over time, percentage changes, consistently throughout the paper. Currently, the paper discusses absolute changes in some Figures (3, 7) and percentage changes (or indexes) in others."	• **In relation to the previous point, we believe this remark may have arisen from the legend title in what is now Figure 2 (revised manuscript). We apologize if this is indeed the cause of this confusion; as indicated in the previous reply, we have adjusted the legend title to clarify that we plot *change* in light intensity (indexed values) in Figure 2.** • **In what are now Figures 5 and 6 (revised manuscript), however, we do plot absolute changes. We do this very deliberately**: first, these figures plot light intensity by pixel, which ranges from DN0 to DN63. An absolute decrease of 1 unit on the DN-scale represents a very large relative decrease at low levels of initial light intensity, whereas it is small in pixels with very high light intensity. Because of the large range of pixel values in the study area, the resulting map highlights changes in dimly lit areas more so than it does it brightly lit areas, while it is the brightly lit areas that experience much of the damage (and much of the absolute decrease in light intensity, especially when expressed in the loss of total light over a larger area). It is really the more brightly lit areas that experience the majority of light loss, and this is depicted much more clearly in absolute changes of light intensity, rather than relative changes. **As discussed in a related comment by referee #1, we therefore believe it is appropriate to use absolute changes in Figures 5 and 6. In all other figures we use indexed values.** • We stress that Figures 5 and 6 are not part of the statistical analysis, but serve as a (detailed) descriptive of changes in light intensity in the affected area, before we compute single yearly values by county (as used in what are now Figures 7 and 8, revised manuscript).
"I suggest either harmonizing the spatial scales of the maps in Figure 2, or putting the upper left map into a separate figure."	• **We thank the referee for this suggestion and have made the necessary adjustments to the upper left map (the DFO flood map).** • **In line with the suggestion made by referee #1, we have dropped Figure 1 and maintain the (excerpt from the) flood map as the upper left panel of Figure 2, with the adjustments as suggested by referee #3.**