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 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"	• 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 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.
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 disaster 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 to gain 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 could be 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.
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 Figure 3, 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 will clarify that we plot indexed light intensity, and thus *changes* in light, rather than total sums of light to avoid any ambiguity. • We propose to do the same for Figure 4, by adjusting the y-axis title to clarify that here too we plot *indexed* light intensity. • 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. Pixel values are then 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 Figure 3. We apologize if this is indeed the cause of this confusion; as indicated in the previous reply, we will adjust the legend title to clarify that we plot *change* in light intensity (indexed values) in Figure 3. • In Figures 6 and 7, 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 6 and 7. In all other figures we use indexed values. • We stress that Figures 6 and 7 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 Figures 8 and 9).
"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 will make the necessary adjustments to the upper left map (the DFO flood map). • In line with the suggestion made by referee #1, we suggest to drop Figure 1 and maintain the flood map as the upper left panel of Figure 2, with the adjustments as suggested by referee #3.