Terrorism and Local Economic Development*

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Abstract

We examine the local economic effects of terrorist incidents across a global sample of nearly 46,000 districts between 1992 and 2013. Using nightlight intensity as a proxy for local economic activity, we find that each additional fatality per attack is associated with a 0.27 percent decline in nightlight intensity. Event studies indicate that the decline is concentrated in the year of the attack, with no statistically significant effects in subsequent years. Disaggregating by target type, we find the largest negative association for attacks directed at police or military installations. By attack type, each additional fatality per armed assault is associated with a decline of 0.32 percent in nightlight intensity. Spatial analysis indicates significant reductions within 25 kilometres of attack locations after accounting for spatial correlation. The negative relationship remains robust to additional political and conflict controls, restrictions on attack frequency and alternative measures of nightlight activity.

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1 Introduction

Terrorism remains a persistent threat to societies worldwide, causing widespread destruction, loss of life, and disruptions to economic activity. Between 1992 and 2019, the Global Terrorism Database reported over 150,000 terrorist incidents globally, resulting in more than 365,000 fatalities. The economic toll is equally severe: between 2000 and 2018, terrorism cost the world economy an estimated USD 855 billion (Bardwell and Iqbal, 2020).

Terrorism can have far-reaching consequences for economic development. By instilling fear among individuals and businesses, it can deter investment, disrupt local markets, and reduce productivity (Becker and Rubinstein, 2011). In response to such incidents, governments may increase spending on reconstruction and security, potentially offsetting some of the economic losses. For example, following the 9/11 attacks, U.S. policies prioritized the protection and rapid restoration of key assets, including government buildings and transportation systems (UNT, 2004).[1] Similarly, in post-invasion Iraq, reconstruction funds were reallocated to focus on security and public safety infrastructure (Nichols 2004; GAO 2005). While reconstruction spending may support local employment and demand, heightened insecurity and disruption may continue to constrain private economic activity. Spending on reconstruction and security may also divert resources from education, health and other public investments. The net effect on local economic activity therefore depends on the extent of the disruption and the scale and timing of the response.

Existing research documents the economic costs of terrorism in particular regions and local labour markets, including the Basque Country and US counties (Abadie and Gardeazabal 2003; Brodeur 2018).[2] Yet an important question remains: do all terrorist attacks impose similar economic costs, or do these costs depend on the intensity and nature of the violence? Terrorist incidents vary considerably in the number of fatalities they cause, the targets they strike and the methods employed, with potentially different implications for local economic activity. More deadly attacks may generate greater fear and uncertainty, while the economic disruption may also depend on who or what is targeted. An attack on security forces, for instance, may heighten perceptions of insecurity, while an attack on infrastructure may directly disrupt production and transportation. Whether these different characteristics of terrorist violence translate into different economic losses remains unclear. Equally important is the geographic reach of these losses. While physical destruction may be concentrated near the attack location, fear, uncertainty and disruptions to economic activity may extend to surrounding communities. Consequently, examining terrorism at the level of entire administrative regions may obscure both the intensity and spatial distribution of its economic consequences. Understanding which attacks are most

[1] A notable case is the redevelopment of Lower Manhattan, which received over $20 billion in federal aid and saw a dramatic transformation from a primarily commercial district into a vibrant, mixed-use neighborhood with a growing residential population and improved transit infrastructure (GAO, 2003).

[2] E.g. see, among others, Enders et al. (1992); Eckstein and Tsiddon (2004); Gordon et al. (2007); Abadie and Gardeazabal (2008); Straetmans et al. (2008).

economically disruptive, and how far their effects extend, is therefore essential for assessing the local economic costs of terrorism. We address these questions by combining geocoded terrorist incidents with annual nightlight data for 45,962 districts worldwide, examining how economic activity responds to attack intensity, measured by fatalities per attack, across different targets and methods, and at increasing distances from attack locations.

Our analysis combines geocoded terrorist incidents from the Global Terrorism Database (GTD) with annual satellite nightlight data over 1992–2013. We use nightlight intensity (NL) as a proxy for local economic activity (Henderson et al., 2012), allowing us to examine economic changes at both the district level and within distance bands around attack locations. This measure is particularly useful where reliable subnational economic statistics are unavailable and avoids relying exclusively on official GDP figures, which may be subject to measurement error or systematic misreporting (Deaton and Heston, 2010; Johnson et al., 2013; Martinez, 2022).

We estimate the relationship between terrorism and local economic activity using a difference-in-differences framework at the district-year level. Our measure of terrorism is attack intensity (AI), defined as the average number of fatalities per attack in a district-year. Drawing on Grossman et al. (2019), our identification strategy assumes that variation in attack intensity is quasi-random conditional on fixed effects and observed characteristics. Although terrorists may deliberately select attack locations, the number of fatalities can also depend on unpredictable circumstances surrounding an incident. We assess the plausibility of this assumption by examining whether observable district characteristics predict subsequent attack intensity and whether affected districts exhibit differential trends in nightlight activity before large attacks.

Baseline estimates show that one additional fatality per attack is associated with a 0.27 percent decline in nightlight activity. To assess whether this relationship is driven by differences between locations experiencing terrorism and those without attacks, we conduct two additional exercises. First, in a similar vein to Brodeur (2018) and Amarasinghe (2023), we restrict the sample to district-years with terrorist incidents, comparing variation in lethality among places experiencing attacks. Second, we restrict the sample further to district-years with successful attacks, as defined by the GTD. The corresponding estimates indicate declines of around 0.33 to 0.34 percent, suggesting that the baseline relationship is not solely driven by terrorists systematically choosing economically distinct regions. Additional analyses provide no evidence of pre-existing trends in either the distributed-lag or event-study specifications, consistent with similar trends before exposure. Terrorism's effects on local economic activity are transitory, with a statistically significant decline only in the year of the attack.

Terrorism may affect regions differently depending on the target or mode of attack. Attacks on military or police installations can heighten perceptions of insecurity, while bombings can generate widespread fear (Rubin et al., 2007; Bryant et al., 2023). More lethal attacks may produce stronger economic responses through greater fear and uncertainty.[3] To examine these

[3]Government responses also matter. Prompt reconstruction or compensation may mitigate losses, while weak

differences, we construct measures of fatalities per attack separately by target category and by attack method. We find that the negative association is driven mainly by attacks on military or police targets, where each additional fatality per attack is associated with a decline of about 0.29 percent in nightlight activity, and by armed assaults, with a decline of about 0.32 percent. The estimates for other target types and attack methods, including bombings, are smaller and less precisely estimated.

We also examine the spatial reach of terrorism's economic effects by measuring nightlight activity within concentric rings around attack locations. The clearest evidence of economic losses is concentrated within 25 kilometers of the incident.[4] This geographic concentration highlights how aggregate measures can obscure local disruption, as losses near attack locations are diluted when combined with economic activity in more distant areas.

A substantial body of research has investigated the economic consequences of terrorism. Cross-country studies using national economic aggregates document adverse effects of terrorism on economic growth, with the magnitude varying across economic and institutional settings (Blomberg et al., 2004; Meierrieks and Gries, 2013; Gaibulloev and Sandler, 2009). Other studies examine particular countries or regions: Abadie and Gardeazabal (2003) estimate the economic losses associated with terrorism in the Basque Country; Benmelech et al. (2010) show that suicide attacks in the Palestinian territories increase unemployment and reduce wages; and Bilgel and Karahasan (2017) document lower real GDP per capita following PKK-related terrorism in Turkish provinces.[5] While these studies provide important insights into the economic costs of terrorism, the literature provides limited systematic evidence on which attack methods and targets are associated with the greatest local economic losses and how far economic disruption extends beyond attack locations. We address these questions within a common empirical framework, distinguishing attack methods and targets across districts in developed and developing countries. Using geocoded incidents, we also measure economic activity at increasing distances from attack locations, allowing us to trace the geographic reach of disruption independently of administrative boundaries. These analyses clarify both which forms of terrorism are most economically disruptive and where the associated losses are concentrated.

The remainder of the paper is organized as follows. Section 2 describes the data. Section 3 outlines the estimation strategy. Section 4 presents the results, and Section 5 concludes.

2 Data

Reliable economic activity data at the sub-national level are often unavailable, particularly in developing countries (Henderson et al., 2012), where even national GDP figures tend to be

responses may intensify them.

[4]These findings account for spatial correlation using the methods of Conley (1999) and Colella et al. (2019).

[5]Further research examines investment (Abadie and Gardeazabal, 2008), firm survival (Rozo, 2018) and consumer sentiment (Clark et al., 2020).

imprecise.[6] Given these limitations, we follow a growing body of literature that uses satellite-recorded nighttime light intensity as a proxy for local economic activity.[7] Our NL data are sourced from the National Oceanic and Atmospheric Administration (NOAA), available in raster format for the period 1992–2013 (NOAA, 2022).[8] The United States Air Force Defense Meteorological Satellite Program (DMSP) has captured Earth-based luminosity using its Operational Linescan System (OLS) sensors since the 1970s, with digital archives available from 1992 onward (Henderson et al., 2012). Using sub-national administrative boundaries from the Global Administrative Areas Database (GADM, 2021), we compute, for each district and year, the average luminosity of the DMSP-OLS stable-lights pixels whose centres lie within the district, averaging across satellites in years observed by two satellites. This yields annual nightlight values for 45,962 districts between 1992 and 2013.

Data on terrorism are sourced from the Global Terrorism Database (GTD, 2022), maintained by the National Consortium for the Study of Terrorism and Responses to Terrorism (START) for the U.S. Department of Homeland Security. The database provides detailed information on terrorist incidents worldwide from 1970 onward, including the location, date, number of casualties, and the nature of each attack. Using the geographic coordinates of each incident, we assign all terrorist events occurring between 1992 and 2013 to the district in which they occurred; incidents located just outside district boundaries (for example, on coastlines) are assigned to the nearest district in the same country within 10 kilometers. According to the summary statistics in Table 1, 1.95 percent of district-years in our sample experienced terrorist activity. The mean attack intensity, defined as the ratio of fatalities to the number of attacks, is 0.053, and the average number of fatalities across all district-years is 0.144. For district-years with at least one terrorist incident, these values rise to 2.706 and 7.396, respectively.[9]

Environmental and demographic factors such as rainfall, temperature, population, and the incidence of natural disasters are known to influence both economic development and terrorist activity (Aman-Rana 2014; Nemeth et al. 2014; Montalvo and Reynal-Querol 2019; Curtis et al. 2021).[10] To account for these potential confounders, we include them as controls at the district level in our analysis. Rainfall and temperature data are sourced from the ERA5 monthly reanalysis (Hersbach et al., 2020), which provides global gridded estimates at a resolution of 0.25 degrees. For each district, we average the monthly values of the grid cells that overlap the

[6] See Martinez (2022), who show that official GDP statistics are more prone to political manipulation in authoritarian regimes.

[7] See, for example, Henderson et al. (2012), Hodler and Raschky (2014), Alesina et al. (2016), and Khalil et al. (2021). For a detailed discussion of nightlight (NL) data, refer to Henderson et al. (2012) and Khalil et al. (2021).

[8] NL data not only offer broader geographic coverage but are also considered more reliable in many contexts, due to known issues in income estimates used for international comparisons (Deaton and Heston, 2010) and the measurement error present in widely used datasets such as the Penn World Tables (Johnson et al., 2013). In some cases, NL data may even outperform conventional sources of income data in accuracy.

[9] A potential limitation of the GTD dataset is that, for most incidents, it does not indicate whether the attack was carried out by domestic or transnational actors. However, Enders et al. (2011) note that the vast majority of attacks are domestic in origin.

[10] See Burke et al. (2015) for a detailed review of the literature on the relationship between climate change and conflict.

district, weighted by the share of the district's area in each cell, and compute annual averages of monthly precipitation (in millimeters) and temperature (in degrees Celsius) for 1992–2013.

District-level population estimates are derived from the Global Human Settlement Layer population grid (Schiavina et al., 2023), which provides gridded population data at five-year intervals. We aggregate the grid cells within each district and interpolate linearly between the five-year values to obtain annual population estimates for each district. Data on natural disasters are compiled from Rosvold and Buhaug (2021), which provides geo-referenced records of disaster events that are spatially matched to the districts in our sample.

3 Estimation Method

We follow a difference-in-differences (DID) approach to estimate the impact of terrorism on local economic development. Specifically, we estimate the following model:

$$Y_{isct} = \beta_0 + \beta_1 Attack_{isct} + \lambda_i + \lambda_t + X'_{isct}\delta + \varepsilon_{isct} \tag{1}$$

where Y_{isct} refers to the average nightlight intensity in district i (second-level administrative unit), within state s (first-level administrative unit), in country c, and in year t, measured in log form.[11] Our treatment variable of interest is $Attack_{isct}$, referred to as "attack intensity," and the coefficient of interest is β_1. Following Grossman et al. (2019), we define $Attack_{isct}$ as the ratio of terrorism-related fatalities to the number of terror attacks in a district-year. The variable takes a value of zero when there are no fatalities in a given year, and a positive value when at least one fatality occurs. Thus, $Attack_{isct}$ is positive for treated units and zero for control units. To ensure that our results are not driven by outliers, we winsorize the number of fatalities (with at least one fatality) at the 99th percentile when constructing the treatment variable.[12] A potential concern with this measure is that it does not distinguish between, for example, ten deaths from ten attacks and one death from a single attack. In practice, most affected district-years experience few attacks: 90 percent of district-years with an attack have six or fewer incidents. In Section 4.4.1, we show that the results are robust to restricting the sample by the number of incidents.

We include district fixed effects, λ_i, to control for unobserved, time-invariant differences in nightlights across districts, such as those arising from geographical characteristics. Year fixed effects, λ_t, account for common shocks that affect all districts in a given year. In addition, we control for a set of time-varying covariates, denoted by X'_{isct}, which include annual rainfall, temperature, population, and the occurrence of natural disasters.[13] The error term, ε_{isct},

[11] A potential concern is that the NL intensity data from DMSP are top-coded at 63. In Section 4.4.7, we show that the results are unchanged when we exclude the top one percentile of NL values.

[12] In Section 4.4.7, we show that the results are robust to using the raw (non-winsorized) variable.

[13] Missing values for the variable "Population" are replaced with a value of 99, and an indicator variable for missing observations is included in the specifications.

captures unobserved, time-varying shocks to the outcome variable.

Our primary coefficient of interest is β_1. Following Grossman et al. (2019), we interpret the estimate as causal for the following reasons. Our measure of terrorism is attack intensity, which is directly related to the number of fatalities caused by a terrorist attack. Perpetrators might target a location based on some unobservable characteristics, which can induce a selection issue at the extensive margin. However, their chances of succeeding and being able to cause fatalities depend upon a multiplicity of factors that are hard to predict ex ante, such as whether they are able to bypass security to carry out the attack, the day of the attack, and its exact location. Hence, we assume that, conditional on fixed effects and observed characteristics, variation in attack intensity is quasi-random. We assess the plausibility of this assumption using balance and pre-trend tests. Observable district characteristics have little predictive power for subsequent attack intensity after accounting for district and year fixed effects (see Section 3.1 below). Neither the distributed-lag nor the event-study specifications reveal significant pre-existing trends in nightlight activity. We also apply the sensitivity analysis of Rambachan and Roth (2023) to assess the robustness of the event-year finding to departures from parallel trends.

To assess whether our results are driven by selection into attack locations and years, we conduct two additional exercises. First, we restrict the sample to district-years with at least one terrorist incident, using non-fatal attack years as the comparison group. Second, we restrict the sample to district-years with successful attacks, as defined by the GTD, again comparing variation in lethality. These exercises draw on Brodeur (2018) and Amarasinghe (2023), who compare successful and failed attacks. Our approach instead exploits differences in fatalities among attack-exposed district-years, reducing reliance on comparisons with places or years without attacks, while retaining the assumption that lethality is conditionally exogenous.

3.1 Covariate Balance

We assess the plausibility of our identifying assumption by examining whether observable district characteristics predict subsequent attack intensity. We examine whether demographic, environmental and political characteristics in year t predict attack intensity in year $t+1$, including population, rainfall, temperature, disaster exposure, election-related conditions, political and media freedom, and recent nightlight activity. Figure 1 reports the association for a one-standard-deviation change in each characteristic, expressed as a percentage of the standard deviation of subsequent attack intensity.

Panel (a), which excludes fixed effects, shows that several characteristics predict attack intensity. Population has the largest association, at approximately 3.9 percent of a standard deviation, while political indicators and past nightlights also exhibit associations. These relationships may reflect persistent differences across districts and common changes over time rather than changes in local conditions that predict attack intensity. Panel (b) accounts for these sources of variation through district and year fixed effects, as in our baseline specifica-

tion. The associations become substantially smaller: all fall below 0.5 percent of a standard deviation in absolute value, and most confidence intervals include zero. This attenuation highlights the importance of fixed effects in separating differences across locations and years from variation within districts. In particular, prior nightlight activity and indicators of economic conditions have little predictive power for subsequent attack intensity once these differences are accounted for, easing concerns that the baseline relationship reflects pre-existing economic conditions. We complement this exercise with pre-trend tests to examine whether economic changes precede attacks.

4 Results

4.1 Baseline Results

We begin by examining the effects of terrorism on local economic activity at the sub-national level. Table 2 presents the baseline results, starting with a specification that excludes fixed effects and control variables. In column (1), one additional fatality per attack is associated with a 0.0258 log-point decline in nightlight intensity. In column (2), we include district and year fixed effects; the estimated coefficient declines substantially in magnitude but remains statistically significant at the 1% level. Column (3) additionally includes the control variables and is our preferred specification. The estimated coefficient of -0.0027 implies that one additional fatality per attack is associated with a 0.27 percent decline in nightlight activity.[14] Henceforth, we use column (3) of Table 2 as the baseline specification.

4.1.1 Dynamic Treatment Effects

The Difference-in-Differences framework relies on the parallel trends assumption, which requires that, absent treatment, treated and control units would have evolved similarly over time. We assess the plausibility of this assumption by estimating the effects of past, contemporaneous, and future terrorism intensity on current economic activity. In this setting, the inclusion of leads serves as a falsification test. Since future terrorism intensity cannot affect current nightlight activity, insignificant coefficients on leads would support the validity of the specification.[15] Panels (a) and (b) of Figure 2 report results from an unconditional and a conditional specification, respectively, while panels (c) and (d) repeat them after excluding district-years at or above the 99th percentile of attack intensity. The lead coefficients capture whether future terrorism intensity is correlated with current nightlight activity. Across all panels, the lead coefficients are statistically insignificant, indicating that future terrorism intensity is not

[14] The percentage change in nightlight activity for a one-unit increase in attack intensity is calculated as $100[e^{\beta} - 1]$. Using $\beta = -0.0027$ from column (3) of Table 2 gives $100[e^{-0.0027} - 1] = -0.2696\%$, or approximately -0.27%.

[15] As treatment is measured as a continuous variable rather than a discrete event, no single period is omitted as a reference category.

systematically correlated with current nightlight activity. This absence of pre-trends provides support for the parallel trends assumption and bolsters the credibility of our DID estimates.

The contemporaneous coefficient is negative and statistically significant, while the lagged estimates provide limited evidence of persistence. This pattern is consistent with the event-study estimates below, which show the clearest decline in the year of the attack. One possible explanation for the near-zero first lag is that emergency assistance or heightened security temporarily offsets economic disruption, although our data do not allow us to test this mechanism directly.[16]

As a further robustness check, we implement an event study analysis based on a discrete treatment indicator that equals one for district-years with more than six fatalities per attack, which corresponds to the 90th percentile of the distribution of fatalities per attack among district-years with at least one attack.[17] The results, reported in Figure A1 in the online appendix, lead to the same conclusion. Relative to the omitted year (-1), future terrorism exposure has no statistically significant effect on current nightlight activity, while the effect is concentrated in the year of the event, and the coefficients in the following years are negative but imprecisely estimated. Taken together, these results provide consistent and complementary evidence supporting the parallel trends assumption in our empirical setting.

We also assess how sensitive the event-year estimate is to violations of parallel trends using the approach of Rambachan and Roth (2023). This approach allows the post-event trend to deviate from the pre-event trend by up to $\bar{M}$ times the largest deviation between consecutive pre-event periods, and reports 95% confidence sets for the effect in the year of the event. Panels (c) and (d) of Figure A1 report these sets for the unconditional and conditional specifications. The confidence set excludes zero for all values of $\bar{M}$ up to one, so the event-year effect remains negative and statistically significant even if the post-event deviation from parallel trends is as large as the largest pre-event deviation. It includes zero only when $\bar{M}$ exceeds one. The pattern is similar when outliers are excluded (Figure A2, panels (c) and (d)).

4.2 Heterogeneity Analysis

We examine whether the economic consequences of terrorism differ by target and attack method. For each target category and attack method, we construct a separate measure of fatalities per attack, which allows us to assess which types of lethal attacks are associated with the greatest economic disruption.

[16] Similar dynamics are documented in the context of natural disasters by Skidmore and Toya (2002) and Ponnusamy (2022), who find limited short-run effects followed by significant negative impacts once emergency support dissipates.

[17] Since our main treatment exploits quasi-random variation in fatalities per attack, defining treatment based on any fatality would remove this intensive-margin variation. We therefore use a high-lethality threshold (the 90th percentile, equal to more than six fatalities per attack) to construct a discrete event for the event study.

4.2.1 Heterogeneity by Target

The economic consequences of an attack may depend on the activities it disrupts and the response it prompts. Attacks on businesses can interrupt production and commerce, while attacks on police or military targets may undermine perceptions of security beyond the immediate site. Fear and uncertainty can discourage investment and encourage outmigration (Becker et al., 2004). Reconstruction and public spending may offset some of these losses, but the extent of this response is likely to vary across targets. Distinguishing target types therefore helps assess which incidents are associated with the greatest economic disruption.

The GTD identifies the intended target of each incident, which we classify into four categories: business targets, government targets, military targets, and other targets.[18] For each category, we construct a target-specific measure of fatalities per attack, equal to zero if there was no attack on that target type in the district-year.

Table 3 presents the results. Columns (1)–(4) examine target categories separately, while column (5) includes them jointly. Attacks on police or military targets exhibit the largest negative association with nightlight activity. Each additional fatality per attack is associated with a decline of approximately 0.29 percent in column (3) and 0.28 percent in column (5), both significant at the 5% level. Attacks on other targets have smaller negative coefficients of about 0.22 percent, significant at the 10% level. The coefficient for business targets is small, significant at the 10% level only when entered separately in column (1), and the estimate for government targets is not statistically significant. The prominence of police and military targets is consistent with disruption extending beyond physical damage to concerns about local security, although these estimates do not directly establish that mechanism.

4.2.2 Heterogeneity by Attack Method

We also examine whether the economic response varies with the method of attack. Following GTD definitions, we classify incidents into armed assaults, bombings, infrastructure-related attacks, and a residual other category.[19] As in the target analysis, we construct method-specific measures of fatalities per attack. For example, Attack Intensity (Bombing) measures fatalities per bombing incident in the district-year.

Table 4 presents the results. Columns (1)–(4) examine attack methods separately, while column (5) includes them jointly. Armed assaults exhibit the strongest negative association

[18] Business targets include businesses, private citizens and property, transportation and utilities. Government targets include general and diplomatic government targets, such as government buildings, political movements, state-sponsored institutions and embassies. Military targets include police and military personnel and installations such as barracks, checkpoints and recruitment centers. The remaining target types, such as non-military aircraft and educational institutions, are grouped as other. See GTD (2022).

[19] Armed assaults involve firearms, incendiary devices, or sharp instruments. Bombings include explosion-type incidents. Infrastructure-related attacks involve damage to non-human assets such as buildings, transport systems, or pipelines. Assassinations form a separate GTD category and are not included; all remaining attack types are grouped as other.

with nightlight activity: each additional fatality per attack is associated with a decline of approximately 0.32 percent in column (1) and 0.31 percent in column (5), both significant at the 1% level. The bombing coefficients are negative but not statistically significant, while infrastructure-related attacks and the other category also show no significant relationship. Although bombings can generate psychological distress (Rubin et al., 2007; Bryant et al., 2023), these estimates do not establish a corresponding economic decline. Together with the target analysis, the results indicate that the economic losses associated with terrorism are concentrated in lethal armed assaults and in attacks on security forces.

4.3 Spatial Analysis

District-level estimates may obscure the geographic concentration of terrorism's economic consequences. Losses near an attack location can be diluted by activity elsewhere in the district, while disruption may also extend across administrative boundaries. We therefore examine nightlight activity at increasing distances from geo-coded attack locations. Following an approach similar to Feyrer et al. (2017), we construct concentric distance bands that allow the geographic reach of disruption to be examined independently of administrative boundaries.

We measure average nightlight activity within 5 km of each attack location and in non-overlapping rings of 5–15 km, 15–25 km, 25–50 km, 50–100 km and 100–150 km. For each band, the dependent variable is the log of average nightlight intensity, and the explanatory variable is fatalities per attack at the central location. The unit of observation is the attack location by year. All specifications include attack-location and year fixed effects, together with rainfall, temperature, population and disaster exposure measured for the district containing the location. Locations that cannot be assigned to a district are excluded.

Table 5 presents the results. The first three distance bands exhibit similar negative coefficients: -0.0026 within 5 km, -0.0027 between 5 and 15 km, and -0.0024 between 15 and 25 km. These estimates imply declines in nightlight activity of approximately 0.24–0.27 percent per additional fatality per attack. The coefficient is smaller in the 25–50 km ring, at -0.0010. At greater distances, the coefficients are small and positive (0.0007 between 50 and 100 km and 0.0009 between 100 and 150 km). The clearest pattern is therefore a concentration of economic losses close to attack locations, rather than a uniform decline across the surrounding area.

4.3.1 Spatial Correlation in Errors

Nearby locations may experience common economic, environmental or security shocks, making attack-location clustering insufficient to capture dependence across observations. We therefore also compute spatially robust standard errors following Conley (1999) and Colella et al. (2019), using a uniform kernel with a 300 km distance cutoff and treating observations in different years as independent. The resulting p-values, reported in square brackets in Table 5, are

0.025, 0.021 and 0.023 for the first three distance bands. Beyond 25 km, the estimates are no longer statistically significant under spatially adjusted inference.[20]

Accounting for spatial correlation thus provides the strongest evidence of economic disruption within 25 km of an attack. This should not be interpreted as a precise boundary beyond which terrorism has no economic consequences. Rather, the results identify a nearby zone in which the negative relationship is consistently supported, illustrating how broader geographic averages can conceal disruption concentrated around attack locations.

4.4 Robustness Exercises

In this section, we conduct several additional exercises to assess the sensitivity of our baseline findings.

4.4.1 Restricting by the Number of Incidents

A potential concern is that attack intensity may not fully capture the severity of terrorism. For example, ten deaths from ten attacks may represent greater disruption than one death from a single attack, even if both generate the same intensity value. This issue is limited in our context as 90 percent of district-years with at least one attack experienced six or fewer incidents, as shown in the bottom rows of Table A1.

To formally address this concern, we restrict the sample based on the number of incidents. Column (1) of Table A1 reports the baseline estimate. Columns (2) to (5) sequentially exclude district-years with more than one, two, five, or six incidents, which correspond to the 50th, 70th, 85th and 90th percentiles of the number of incidents among district-years with an attack. The estimates remain stable across all restrictions, ranging from -0.0024 to -0.0028 and significant at the 1% level in every column. Even when the sample is limited to district-years with at most one incident (column (2)), the coefficient is -0.0024, so our findings are not driven by districts experiencing unusually high numbers of attacks. As a further check, we control for the total number of incidents directly. The coefficient on attack intensity remains -0.0027 and significant at the 1% level.

4.4.2 Control Group Contamination and Nature of the Attack

In this section, we conduct several exercises to further strengthen the causal interpretation of our findings. Results are presented in Table 6, with column (1) reporting the baseline estimate of -0.0027. We first address potential contamination within the control group. In the main specification, the control group includes district-years with no attack and district-years with only non-lethal incidents. To test whether the inclusion of non-lethal attacks affects our results,

[20] A 100 km distance cutoff yields smaller p-values of 0.005, 0.006 and 0.008 for the first three distance bands, with the same conclusion about statistical significance beyond 25 km.

we re-estimate the model excluding district-years with a non-fatal attack. As shown in column (2), the estimated effect remains essentially unchanged (-0.0026, significant at the 1% level), indicating that including non-lethal incidents in the control group does not bias the findings.

We next adopt an approach used in recent literature (see Brodeur 2018; Amarasinghe 2023) that exploits variation in the nature of the attack. We restrict the sample to district-years that experienced at least one incident. Conditional on being targeted, the lethality of the attack is plausibly exogenous. Column (3) therefore compares district-years that experienced at least one fatality with district-years that experienced only non-lethal incidents; 63 percent of the 14,028 district-years in this sample experienced a fatal attack. Despite the much smaller sample, the coefficient on attack intensity is somewhat larger than the baseline (-0.0034) and statistically significant at the 1% level. This exercise reduces concerns about non-random selection into treatment, as all units were targeted.

We then follow the GTD classification and restrict the sample to district-years with at least one successful attack.[21] In this specification, treated units are district-years with successful attacks that caused fatalities, and control units are district-years with successful attacks that did not. Column (4) shows that the estimated effect (-0.0033) is again slightly larger in magnitude than the baseline and significant at the 1% level. Restricting the analysis to regions that experienced terrorism introduces the possibility that some earlier-treated units may serve as controls for later-treated units. In the presence of negative weights, this setup can bias the estimated treatment effect and potentially alter its sign. We address this concern in Section 4.4.6, where we show that negative weighting is not a concern in our context.

Finally, in column (5), we limit the sample to district-years with at least one fatality. Here, all units experience fatal attacks, and identification comes only from variation in the number of deaths per attack. The estimated effect (-0.0029) remains similar in magnitude to the baseline and significant at the 1% level. Collectively, these exercises provide additional support for the causal interpretation of our baseline results.

4.4.3 Highly Lethal Attacks

We examine the economic consequences of exceptionally lethal attacks by constructing indicators for district-years with attack intensity at or above the 90th, 95th and 99th percentiles of the distribution among district-years with at least one attack. The corresponding thresholds are 6.2, 11 and 31 fatalities per attack. We include each indicator separately in the baseline specification. The estimated coefficients are -0.085 (standard error 0.017), -0.101 (standard error 0.022) and -0.118 (standard error 0.058), implying declines in nightlight activity of approximately 8.1, 9.6 and 11.1 percent, respectively. The first two estimates are significant at the 1% level and the third at the 5% level. The progressively larger point estimates suggest greater economic disruption following more lethal attacks, although the estimates for the most extreme

[21] According to the GTD, an attack is classified as successful if the intended act is carried out. For example, in a bombing, success is defined by whether the device detonates.

incidents are less precise. Such incidents may disrupt local markets through heightened fear and insecurity, but the analysis does not directly identify these channels.

4.4.4 Additional Controls

Our baseline specification controls for district-level rainfall, temperature, population and disaster exposure. We now introduce additional political, economic and conflict-related variables that may influence both terrorism and economic activity. Table 7 reports the results, with columns (1)–(6) adding each control separately and column (7) including all controls jointly.

Columns (1)–(4) add separate country-year indicators from the NELDA database (Hyde and Marinov, 2012). These identify whether a national election occurred, whether there were concerns about its fairness, and whether poor economic conditions or an economic crisis were reported around the election, respectively. These controls account for electoral circumstances that may coincide with changes in terrorism and economic activity. Across all four specifications, the attack-intensity coefficient remains close to the baseline estimate, at -0.0027 to -0.0028, and is significant at the 1% level.

Column (5) adds an indicator for countries classified as "free" by Freedom House (FHI, 2024), leaving the coefficient essentially unchanged at -0.0028. Column (6) controls for the presence of an armed-conflict event in the district-year using the UCDP Georeferenced Event Dataset (Sundberg and Melander, 2013). The coefficient falls in magnitude to -0.0022, approximately 19 percent below the baseline, but remains significant at the 1% level. This attenuation suggests that broader conflict conditions account for part of the baseline association. However, as terrorist incidents and armed-conflict events may overlap, the control may also absorb variation associated with terrorism itself; the change cannot be attributed entirely to confounding.

Including all additional controls jointly in column (7) yields a coefficient of -0.0025, implying a decline of approximately 0.25 percent in nightlight activity per additional fatality per attack. The negative relationship therefore persists after accounting for these additional conditions, although its smaller magnitude indicates that the estimated relationship is sensitive to broader conflict exposure.

4.4.5 Randomization Inference

Conventional inference can be unreliable in difference-in-differences settings with few treated clusters (Conley and Taber, 2011; Khalil et al., 2021). Although our sample contains many districts, exposure to terrorism is sparse: fewer than two percent of district-years experience an attack, and approximately one percent experience a fatal attack. We therefore complement the clustered standard errors with a permutation exercise that assesses how unusual the baseline estimate is relative to estimates obtained by reassigning attack intensity across districts.

We randomly reassign attack intensity across districts within the same country and, separately, within the same state. We repeat each reassignment procedure 500 times, estimating Equation 1 after each reassignment. Figure 3 shows that the resulting placebo estimates are centered around zero, while the baseline estimate lies in the far left tail of both distributions. None of the 500 placebo estimates is as large in absolute value as the baseline estimate, yielding a permutation p-value of approximately 0.002 using the finite-simulation correction. These results indicate that the observed negative association is unusual relative to the reassigned exposure patterns. They provide complementary evidence on inference, although their interpretation depends on the plausibility of the reassignment schemes and does not establish that attack intensity is randomly assigned.

4.4.6 Negative Weighting Concerns

When previously treated units serve as controls for subsequently treated units, negative weights may arise in two-way fixed effects estimation, potentially biasing estimates when treatment effects are heterogeneous. This issue, referred to as the negative weighting problem (Goodman-Bacon 2018; de Chaisemartin and D'Haultfoeuille 2020, henceforth CH), is evaluated in our setting. Using the `twowayfeweights` command developed by CH, we find that the share of the total weight that is negative is near zero in the full sample, suggesting minimal concern.[22] The share of negative weight remains small in columns (3) and (4) of Table 6, at 2.1 and 2.5 percent, respectively. These results suggest that negative weighting remains a limited concern even when the sample is restricted to district-years with attacks or successful attacks.

4.4.7 Further Robustness Exercises

We conduct several additional exercises to assess the sensitivity of our findings to common shocks, nonlinearities and measurement choices. First, we replace year fixed effects with country-year fixed effects to account for country-specific macroeconomic shocks. The coefficient on attack intensity remains negative at -0.0021 (standard error 0.0007) and significant at the 1% level, although smaller in magnitude than the baseline estimate. Second, we add a quadratic term for attack intensity. The linear coefficient remains negative and significant (-0.0041, standard error 0.0012), while the quadratic term is not statistically significant ($p = 0.171$), providing no clear evidence of a quadratic relationship.

We also examine sensitivity to the nightlight dataset. DMSP-OLS data are subject to blurring, saturation and inconsistencies across satellites and years (Gibson et al., 2021). We therefore repeat the analysis using the harmonized dataset developed by Li et al. (2020), which combines inter-calibrated DMSP-OLS observations with simulated DMSP-like values derived from VIIRS data. The coefficient is -0.0025 (standard error 0.0008), close to the baseline esti-

[22] Following Lundborg et al. (2022), we discretize treatment by defining it on the extensive margin, equal to one if a terror-related fatality occurs in a district in a given year and zero otherwise.

mate and significant at the 1% level. This suggests that the negative relationship is not specific to the original nightlight series. A related concern is that DMSP-OLS nightlights are top-coded at a digital number of 63, so that luminosity in the brightest areas is censored. We therefore re-estimate the baseline specification after excluding district-years at or above the 99th percentile of mean nightlight intensity (62.9, essentially the saturation value), which removes 8,730 district-years. The coefficient on attack intensity is unchanged at -0.0027 (standard error 0.0008) and significant at the 1% level, indicating that the results are not driven by saturated observations.

Next, we construct attack intensity using raw rather than winsorized fatality counts. The coefficient remains negative and significant at the 1% level, at -0.0023 (standard error 0.0007), suggesting that the finding does not depend on limiting extreme fatality values. Alternative outcome specifications yield the same qualitative conclusion (Table A2 in the Online Appendix).[23] Across these specifications, attack intensity remains negatively associated with nightlight activity and significant at the 1% level.

Finally, we distinguish attacks occurring in the first and second halves of the year to examine whether their timing matters for annual nightlight activity. The coefficients are -0.0022 (standard error 0.0010) and -0.0017 (standard error 0.0007), respectively, both statistically significant at conventional levels. The larger point estimate for earlier attacks is consistent with a longer period of disruption within the calendar year. However, the difference is not statistically significant ($p = 0.65$), so the estimates do not establish that earlier attacks have a greater effect.

5 Conclusion

Terrorism, as a form of collective violence, has profound economic and non-economic consequences, affecting both developed and developing countries. While prior research has predominantly relied on cross-country analyses or focused on isolated case studies, this paper addresses a key gap by providing causal evidence on the microeconomic impacts of terrorism using a global sub-national dataset. Leveraging nightlight intensity as a proxy for local economic development at the district level, we examine how terrorism influences economic activity and assess the spatial extent of its effects.

Our findings reveal that terrorism significantly reduces local economic activity. Difference-in-differences estimates indicate that each additional fatality per attack leads to a 0.27 percent decline in nightlight activity, on average. These results are robust across multiple specifications and sensitivity checks, supporting a causal interpretation. We further explore heterogeneity in the effects by the type of target and attack. Lethal attacks on military or police facilities have the

[23] Column (1) reports the baseline estimate. Column (2) uses log nightlights per capita, column (3) uses the log of mean nightlights plus 0.01 to retain observations with zero luminosity, and column (4) uses mean nightlights in levels.

most pronounced negative effects, while the estimates for other target types are smaller and less precisely estimated. In terms of attack method, lethal armed assaults are associated with the most substantial economic losses. Our spatial analysis shows that terrorism's economic effects are geographically concentrated, with measurable impacts extending up to 25 kilometers from the site of the incident.

Over the past two decades, terrorism has posed an increasing threat to both developed and developing countries, resulting in more than 315,000 deaths and over 430,000 injuries. These figures capture only the direct consequences. According to a report by ETR (2022), 58% of the 830 million people facing food insecurity reside in the 20 countries most affected by terrorism. This underscores how terrorism not only disrupts economic activity but can also disproportionately harm vulnerable populations already experiencing severe hardship. Our study reveals the substantial adverse effects of terrorism on local economic development, underscoring the need for targeted government intervention. It is imperative that public resources be directed toward affected districts to offset the economic disruptions caused by terrorism. Moreover, by identifying the spatial extent of terrorism's impacts, this study provides actionable insights for policymakers on the geographic areas most in need of support.

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Tables and Figures

Table 1: Terrorism and Local Economic Development - Summary Statistics

Variable	Mean	Standard Deviation	Source
Log Nightlights$_{i,t}$	0.736	2.052	DMSP-OLS
Incident$_{i,t}$	0.019	0.138	GTD
Attack Intensity$_{i,t}$	0.053	1.127	GTD
Number of Kills$_{i,t}$	0.144	2.985	GTD

The table reports summary statistics for the outcome variable, i.e., nightlight activity, as well as for the terrorism-related variables. *Log Nightlights*$_{i,t}$ refers to the log of nightlight intensity in district *i* during year *t*. *Incident* is an indicator for at least one attack, *Attack Intensity* is the number of fatalities per attack (zero if there is no attack), and *Number of Kills* is the number of fatalities, top-coded at the 99th percentile. The terrorism variables are missing in 1993, for which the GTD has no records.

Table 2: Terrorism and Local Economic Development - Baseline Results

Dependent Variable	Log NL	Log NL	Log NL
	(1)	(2)	(3)
Attack Intensity$_{i,t}$	-0.0258***	-0.0028***	-0.0027***
	(0.0045)	(0.0008)	(0.0008)
District Fixed Effects	No	Yes	Yes
Year Fixed Effects	No	Yes	Yes
Controls	No	No	Yes
Mean Outcomes	0.749	0.751	0.751
Observations	873112	872623	872623

Note: *, **, and *** denote significance at 10%, 5%, and 1% level, respectively. The dependent variable is the log of NL. Columns 2 and 3 control for district-fixed effects and year-fixed effects. Column 3 also includes the following controls: rainfall, temperature, disaster exposure and population (with an indicator for missing population). Standard errors clustered at the district level are reported in parentheses.

Table 3: Terrorism and Local Economic Development - Heterogeneity by the Type of Target

Dependent Variable	Log NL	Log NL	Log NL	Log NL	Log NL
	(1)	(2)	(3)	(4)	(5)
Attack Intensity (Business)$_{i,t}$	-0.0010*				-0.0009
	(0.0006)				(0.0006)
Attack Intensity (Government)$_{i,t}$		0.0011			0.0013
		(0.0010)			(0.0010)
Attack Intensity (Military)$_{i,t}$			-0.0029**		-0.0028**
			(0.0012)		(0.0012)
Attack Intensity (Other)$_{i,t}$				-0.0022*	-0.0021*
				(0.0011)	(0.0011)
Mean Outcomes	0.751	0.751	0.751	0.751	0.751
Observations	872623	872623	872623	872623	872623

Note: *, **, and *** denote significance at 10%, 5%, and 1% level, respectively. Attack intensity for each target type is the number of fatalities per attack on that target type in the district-year (zero if there was no such attack). Business targets include businesses, private citizens and property, transportation and utilities; government targets include general and diplomatic government targets; military targets include the military and the police; the remaining target types are grouped as other. All specifications include district fixed effects, year fixed effects and the following controls: rainfall, temperature, disaster exposure and population (with an indicator for missing population). Standard errors clustered at the district level are reported in parentheses.

Table 4: Terrorism and Local Economic Development - Heterogeneity by the Type of Attack

Dependent Variable	Log NL	Log NL	Log NL	Log NL	Log NL
	(1)	(2)	(3)	(4)	(5)
Attack Intensity (Armed Attack)$_{i,t}$	-0.0032***				-0.0031***
	(0.0010)				(0.0010)
Attack Intensity (Bombing)$_{i,t}$		-0.0011			-0.0010
		(0.0008)			(0.0008)
Attack Intensity (Infrastructure)$_{i,t}$			0.0020		0.0026
			(0.0026)		(0.0028)
Attack Intensity (Other)$_{i,t}$				-0.0011	-0.0009
				(0.0010)	(0.0010)
Mean Outcomes	0.751	0.751	0.751	0.751	0.751
Observations	872623	872623	872623	872623	872623

Note: *, **, and *** denote significance at 10%, 5%, and 1% level, respectively. Attack intensity for each attack type is the number of fatalities per attack of that type in the district-year (zero if there was no such attack). Attack types follow the GTD classification: armed assaults, bombings and explosions, and facility or infrastructure attacks; other attack types, excluding assassinations, are grouped as other. All specifications include district fixed effects, year fixed effects and the following controls: rainfall, temperature, disaster exposure and population (with an indicator for missing population). Standard errors clustered at the district level are reported in parentheses.

Table 5: Terrorism and Local Economic Development - Spatial Spillovers

Dependent Variable	0-5 Kms	5-15 Kms	15-25 Kms	25-50 Kms	50-100 Kms	100-150 Kms
	(1)	(2)	(3)	(4)	(5)	(6)
Attack Intensity$_{i,t}$	-0.0026**	-0.0027**	-0.0024**	-0.0010	0.0007	0.0009
	(0.0006)	(0.0006)	(0.0006)	(0.0005)	(0.0004)	(0.0003)
	[0.025]	[0.021]	[0.023]	[0.258]	[0.362]	[0.165]
Mean Outcomes	1.983	0.887	0.513	0.164	-0.031	-0.097
Observations	313373	346502	362275	393462	412864	417392

Note: *, **, and *** denote significance at 10%, 5%, and 1% level, respectively, based on the p-values from standard errors robust to spatial correlation reported in square brackets. The dependent variable is the log of mean nightlight intensity in a ring (0–5, 5–15, 15–25, 25–50, 50–100 and 100–150 km, columns 1–6 respectively) around each location where an attack occurred; the unit of observation is the attack location by year, and attack intensity is the number of fatalities per attack at that location. All columns include attack-location fixed effects and year fixed effects, together with controls measured for the district containing the location: rainfall, temperature, population and disaster exposure; locations that cannot be assigned to a district have no controls and are excluded. Standard errors clustered at the attack-location level are reported in parentheses. The square brackets report p-values from standard errors robust to spatial correlation in the errors (Conley, 1999), computed with a uniform kernel and a 300 km distance cutoff; observations in different years are treated as independent.

Table 6: Terrorism and Local Economic Development - Restricting to Affected Regions

Dependent Variable	Log NL	Log NL	Log NL	Log NL	Log NL
	(1)	(2)	(3)	(4)	(5)
Attack Intensity$_{i,t}$	-0.0027***	-0.0026***	-0.0034***	-0.0033***	-0.0029***
	(0.0008)	(0.0008)	(0.0010)	(0.0010)	(0.0010)
Excludes Contaminated Regions	No	Yes	No	No	No
Restricts to Incident Years	No	No	Yes	No	No
Restricts to Successful Incident Years	No	No	No	Yes	No
Restricts to Years with Deaths	No	No	No	No	Yes
Percentage of the Sample Treated	0.0118	0.0119	0.6317	0.6647	1.0000
Mean Outcomes	0.751	0.746	1.036	0.990	0.797
Observations	872623	865989	14028	13057	8244

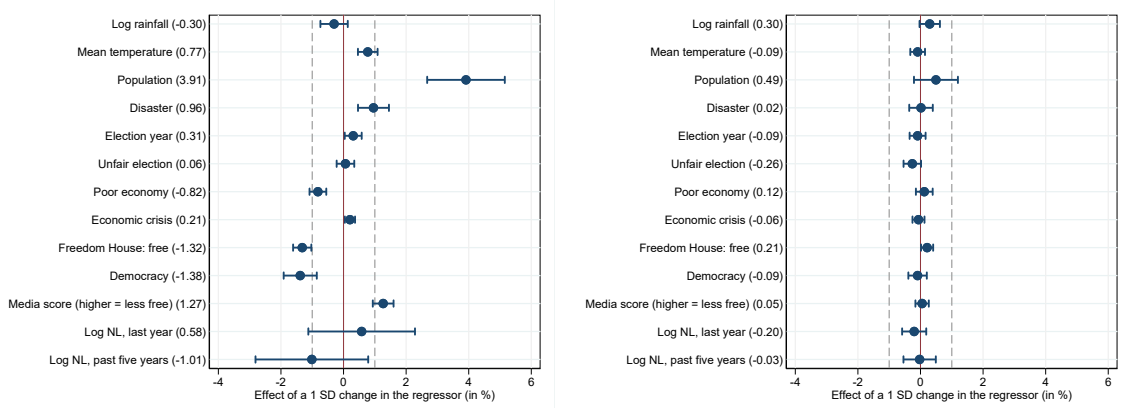
Note: *, **, and *** denote significance at 10%, 5%, and 1% level, respectively. All columns include district fixed effects, year fixed effects and the following controls: rainfall, temperature, disaster exposure and population (with an indicator for missing population). Column 2 excludes district-years with a non-fatal attack, column 3 restricts the sample to district-years with an attack, column 4 to district-years with a successful attack, and column 5 to district-years with at least one death. The share of the sample treated is the share of district-years with a fatal attack. Standard errors clustered at the district level are reported in parentheses.

Table 7: Terrorism and Local Economic Development - Additional Controls

Dependent Variable	Log NL	Log NL	Log NL	Log NL	Log NL	Log NL	Log NL
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Attack Intensity$_{i,t}$	-0.0027***	-0.0028***	-0.0028***	-0.0028***	-0.0028***	-0.0022***	-0.0025***
	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0008)
Additional Controls	Election Year	Unfair Election	Poor Economic Condition	Economic Crisis	Freedom House Index	Armed Conflict	All Additional Controls
Mean Outcomes	0.751	0.751	0.751	0.751	0.748	0.751	0.748
Observations	872623	872623	872623	872623	867363	872623	867363

Note: *, **, and *** denote significance at 10%, 5%, and 1% level, respectively. All columns include district fixed effects and year fixed effects, and control for rainfall, temperature, population (with an indicator for missing population) and disaster exposure, together with the additional control introduced in each column. *Election Year*, *Unfair Election*, *Poor Economic Condition* and *Economic Crisis* are country-year indicators from the NELDA database (an election held in the year, an election flagged as unfair, and poor economic conditions or an economic crisis reported around the election, respectively; zero in years without an election). *Freedom House Index* is an indicator for a country rated "Free" by Freedom House in the year. *Armed Conflict* is an indicator for at least one event in the district-year in the UCDP Georeferenced Event Dataset, covering state-based, non-state and one-sided violence. Column 7 includes all of the additional controls. The Freedom House indicator is missing for some country-years, which reduces the sample in columns 5 and 7. Standard errors clustered at the district level are reported in parentheses.

Figure 1: Balance of Observable Characteristics

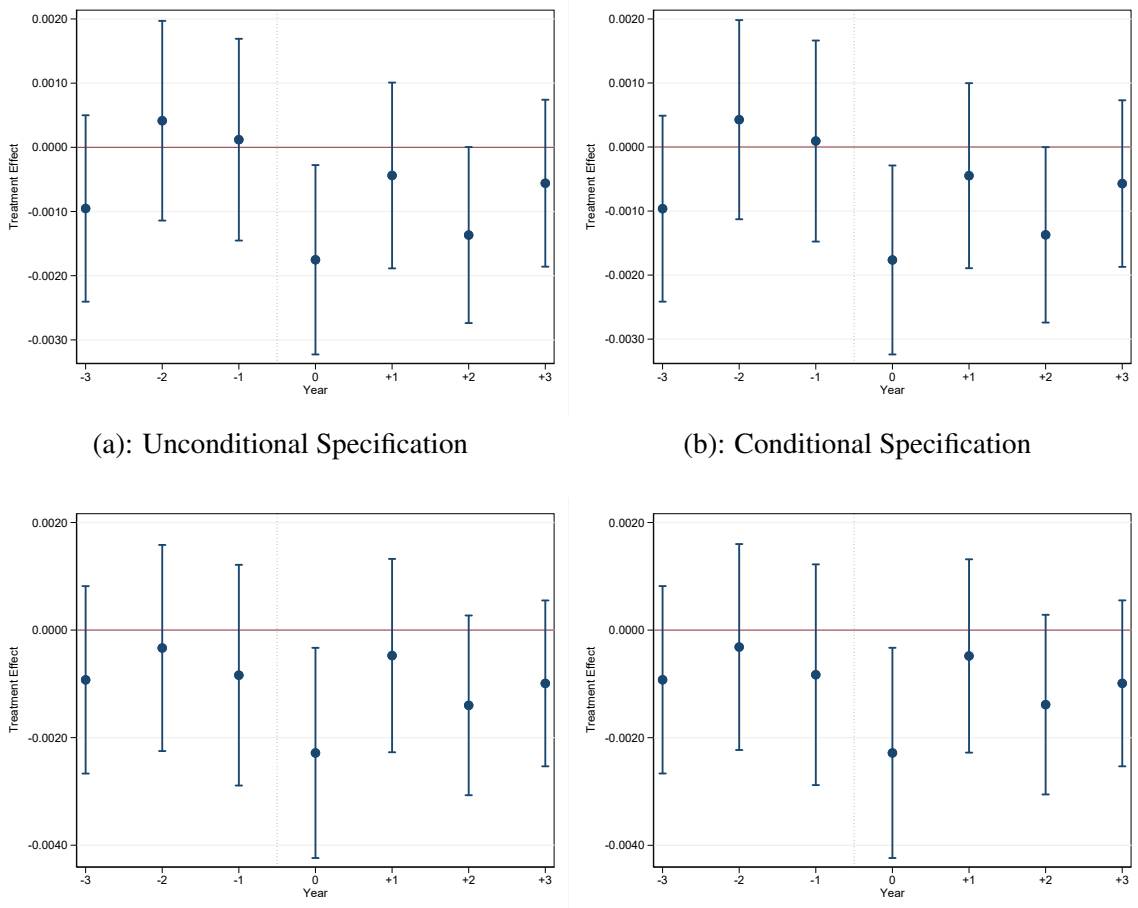


(a): No Fixed Effects

(b): With Fixed Effects

Note: The figure shows the relationship between observable district characteristics in year t and attack intensity (fatalities per attack) in year $t+1$. The characteristics include log rainfall, mean temperature, population and an indicator for missing population, a disaster indicator, an election-year indicator, indicators for unfair elections, a poor economy and an economic crisis (each defined for election years), a Freedom House "free" indicator, a democracy indicator, a media freedom score (higher values indicate less free media), log nightlights in year t, and mean nightlights over years $t-4$ to t. All characteristics enter jointly in a single regression. Each point represents the coefficient associated with a one-standard-deviation increase in the characteristic, expressed as a percentage of the standard deviation of next year's attack intensity, with 95% confidence intervals. Numbers in brackets beside the labels show the plotted point estimates. Dashed vertical lines indicate ± 1 percent, and both panels use the same horizontal axis. Panel (a) includes no fixed effects and uses raw standard deviations. Panel (b) includes district and year fixed effects and uses standard deviations calculated after removing these fixed effects. Standard errors are clustered at the district level. The sample includes district-years with non-zero nightlights and non-missing values of all characteristics; because the democracy indicator is available only until 2010, characteristics are measured over 1993–2010. Armed conflict is not included because conflict events overlap with terrorist attacks.

Figure 2: Terrorism and Local Economic Development: Dynamic Treatment Effects



(a): Unconditional Specification

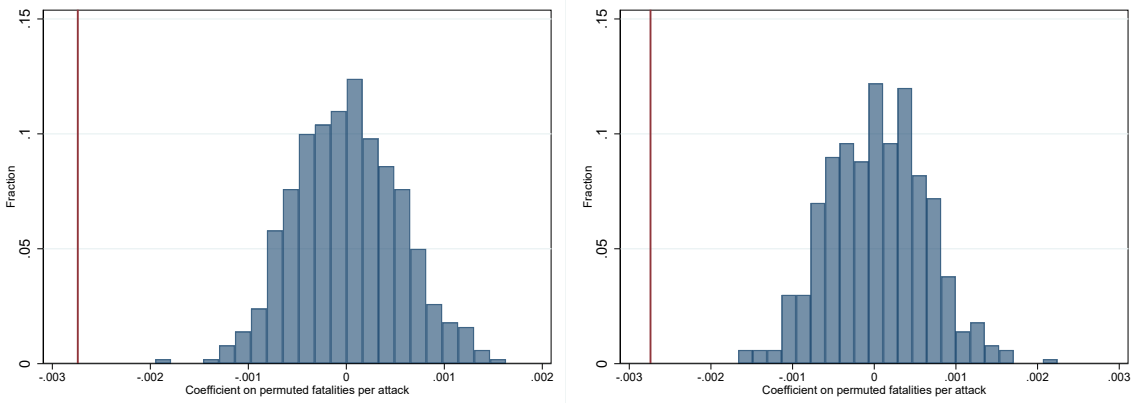
(b): Conditional Specification

(c): Unconditional, Excluding Outliers

(d): Conditional, Excluding Outliers

Note: The figure plots the effect of current, lagged and future terrorism on nightlight activity. Attack intensity (fatalities per attack) in years $t-3$ to $t+3$ is entered jointly in a single regression, and the plotted points are the coefficients on each year, with 95% confidence intervals. In panels (a) and (b), attack intensity is winsorised at the 99th percentile of district-years with at least one death; in panels (c) and (d), district-years at or above the 99th percentile are excluded instead, so that any window containing one drops out. The specifications control for whether any attack occurred in the district in the same year. All specifications include district fixed effects and state-year fixed effects. Panels (b) and (d) additionally control for rainfall, temperature, disaster exposure and population (with an indicator for missing population). Standard errors are clustered at the district level.

Figure 3: Terrorism and Local Economic Development: Permutation Exercise



(a): Within Country

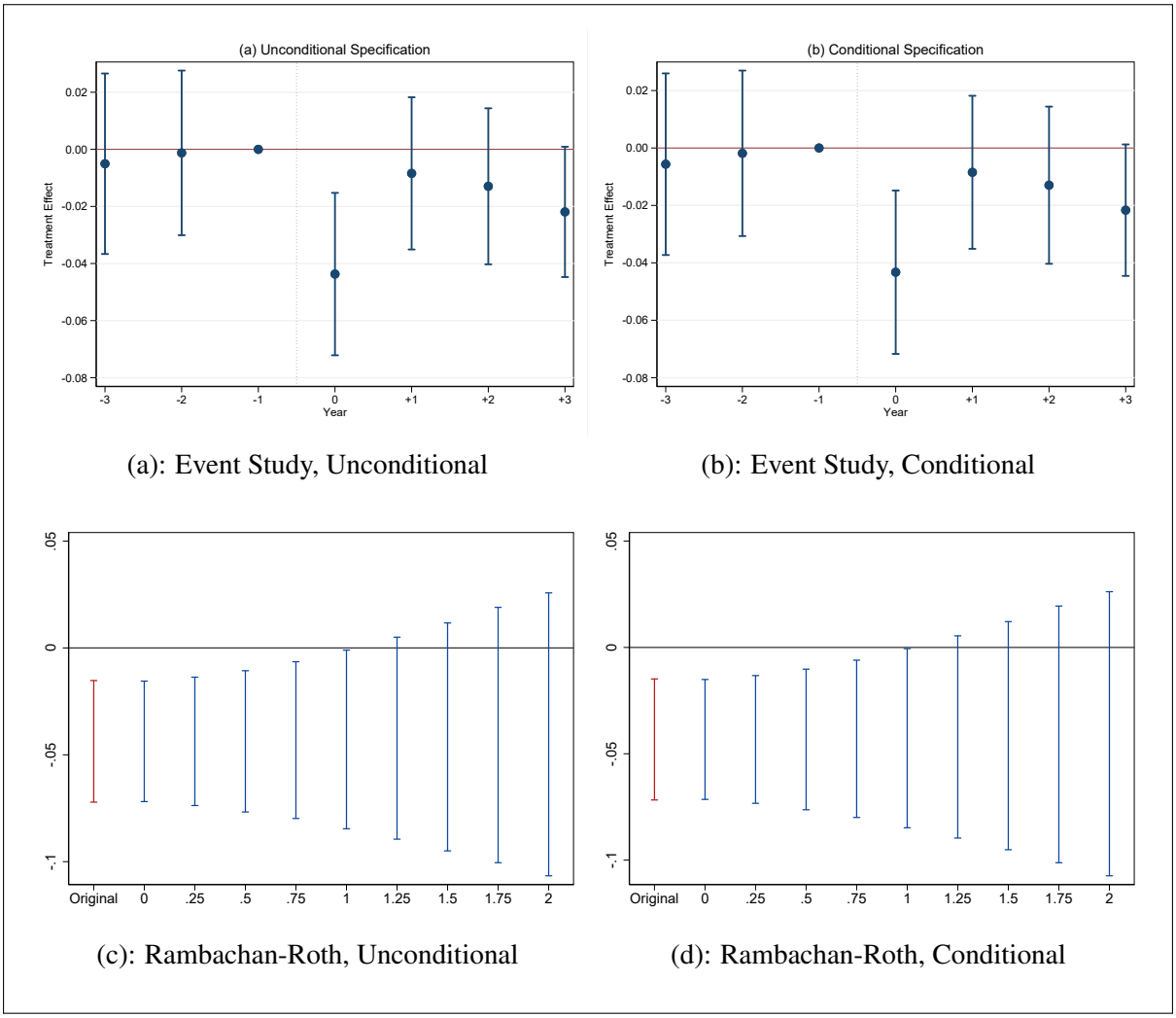
(b): Within State

Note: The figure shows the distribution of coefficients from 500 randomization-inference replications, in which attack intensity is shuffled across districts within the same country (panel (a)) or within the same state (panel (b)). The solid vertical line is the baseline estimate.

Online Appendix (Not for Publication)

Terrorism and Local Economic Development

Figure A1: Terrorism and Local Economic Development: Event Study Results



(a): Event Study, Unconditional

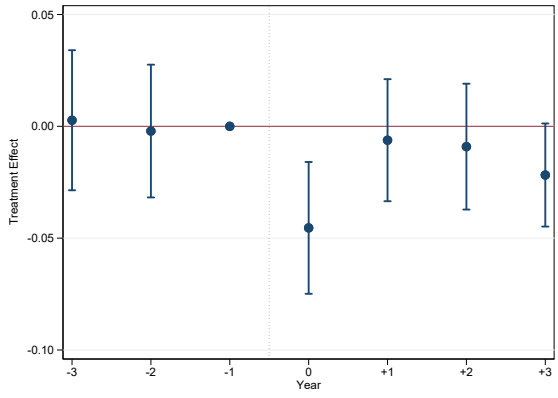
(b): Event Study, Conditional

(c): Rambachan-Roth, Unconditional

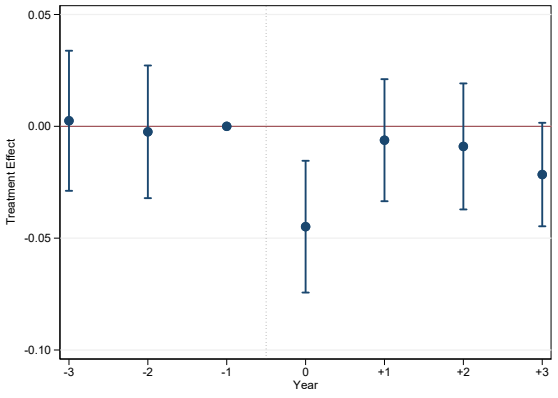
(d): Rambachan-Roth, Conditional

Note: Panels (a) and (b) plot the effect of terrorism on nightlight activity in the years before and after a large attack (a district-year with more than six fatalities per attack; events less than five years after a previous large attack in the same district are not counted). The coefficients are those on indicators for the years from three years before to three years after the nearest event, relative to the year before the event (omitted), with 95% confidence intervals. All specifications control for the fatalities per attack of smaller attacks and for whether any attack occurred in the district in the same year, and include district fixed effects and state-year fixed effects. Panel (b) additionally controls for rainfall, temperature, disaster exposure and population (with an indicator for missing population). Standard errors are clustered at the district level. Panels (c) and (d) show 95% confidence sets for the effect in the year of the event, following Rambachan and Roth (2023): the post-event trend may deviate from the pre-event trend by up to $\bar{M}$ times the largest deviation between consecutive pre-event periods (horizontal axis). The red interval is the original estimate; $\bar{M} = 0$ imposes exact parallel trends.

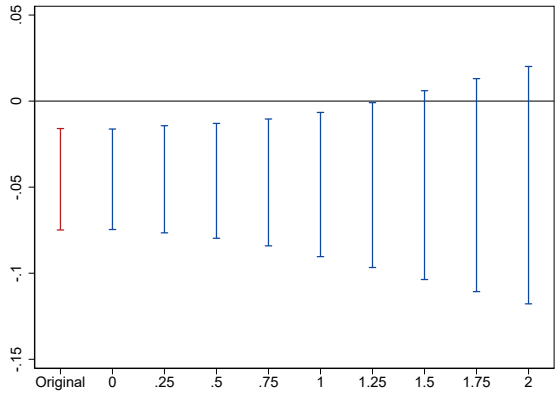
Figure A2: Terrorism and Local Economic Development: Event Study Results, Excluding Outliers



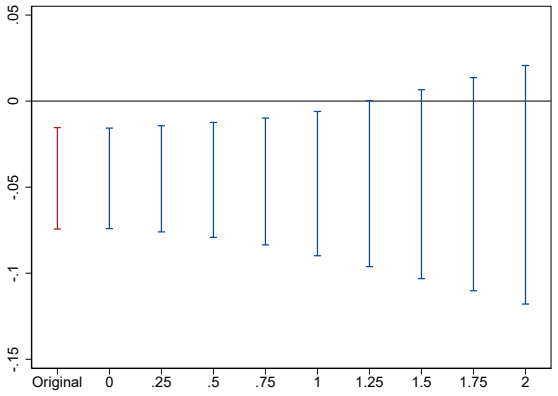
(a): Event Study, Unconditional



(b): Event Study, Conditional



(c): Rambachan-Roth, Unconditional



(d): Rambachan-Roth, Conditional

Note: The specification follows Figure A1, excluding district-years at or above the 99th percentile of fatalities per attack, calculated among district-years with at least one death. Panels (a) and (b) report event-study estimates, while panels (c) and (d) report the corresponding Rambachan and Roth (2023) confidence sets for the effect in the event year.

Table A1: Terrorism and Local Economic Development - Restricting by the Incident Count

Dependent Variable	Baseline	One Incident and Below	Two Incidents and Below	Five Incidents and Below	Six Incidents and Below
	(1)	(2)	(3)	(4)	(5)
Attack Intensity$_{i,t}$	-0.0027***	-0.0024***	-0.0026***	-0.0028***	-0.0028***
	(0.0008)	(0.0009)	(0.0008)	(0.0008)	(0.0008)
Percentiles	All	50	70	85	90
Mean Outcomes	0.751	0.749	0.749	0.750	0.750
Observations	872623	865400	868043	870480	870850

Note: *, **, and *** denote significance at 10%, 5%, and 1% level, respectively. The specifications control for district-fixed effects, year fixed effects and the following controls: rainfall, temperature, disaster exposure and population (with an indicator for missing population). Standard errors clustered at the district level are reported in parentheses.

Table A2: Terrorism and Local Economic Development - Different Functional Forms

	(1)	(2)	(3)	(4)
Attack Intensity$_{i,t}$	-0.0027***	-0.0028***	-0.0019***	-0.0104***
	(0.0008)	(0.0008)	(0.0006)	(0.0027)
Functional Form	Log of Nightlights	Log of NL Per Capita	Log of (NL + 0.01)	Mean Nightlights
Mean Outcomes	0.751	-2.964	0.281	8.495
Observations	872623	871833	965181	872623

Note: *, **, and *** denote significance at 10%, 5%, and 1% level, respectively. The specifications control for district-fixed effects, year fixed effects and the following controls: rainfall, temperature, disaster exposure and population (with an indicator for missing population). Standard errors clustered at the district level are reported in parentheses.