Descending into Chaos: The Great American Homicide Rise*

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Abstract

Homicide rates in America began rising dramatically in the mid-1960s and remained elevated for the next three decades. This transformation has often been characterized as a pervasive phenomenon experienced throughout the nation. Using newly-assembled homicide data across local labor markets and a data-driven clustering approach, I show that the national homicide trend conceals two distinct trajectories. While one group of areas experienced a steep climb in homicides, the other witnessed only a modest increase. Areas that saw a sharp rise were disproportionately concentrated in the Sunbelt and major urban centers. Importantly, the two-cluster structure and geographic divide are primarily driven by firearm-related homicides. I then investigate the factors that contributed to the rise in homicides. Adopting an event-study framework, I demonstrate that the racial riots of the 1960s contributed to the surge in homicide rates that then persisted for decades. In terms of mechanisms, I provide evidence that the riots accelerated urban decline by inducing outmigration and amplifying residential tipping, while also prompting an intensified law enforcement response. These developments may have disrupted established social arrangements and weakened informal social control, creating conditions conducive to further violence. Beyond the riots, I document that the broader countercultural transformations of the 1960s, including the liberalization of sexual norms and the spread of recreational drug use, were associated with rising homicide rates. Together, these findings suggest that the Great American Homicide Rise was shaped by geographically uneven social forces, with firearm violence accounting for much of the divergence across communities.

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1 Introduction

In the mid-1960s, America experienced a dramatic surge in homicides that would shape the course of the nation for the next three decades. Figure 1 traces the national trend in homicide rates over the 20th century. Between 1965-1975, the homicide rate nearly doubled, climbing from roughly 5.5 to 9.9 homicides per 100,000 persons. These elevated rates then persisted until the end of the century. The sharp spike in violence drew widespread public concern. Gallup surveys from this period document a sudden increase in the proportion of Americans who viewed crime as the most important problem facing the country (Figure A.1). What made the Great American Homicide Rise particularly alarming was the perception that it was a pervasive phenomenon, affecting communities throughout the nation. In a special message to Congress on March 8, 1965, US President Lyndon B. Johnson declared that:

> Crime is no longer merely a local problem. Every city, every state is troubled by the same hard statistical—and human—facts.

The perception of a nationwide phenomenon was subsequently echoed in academic research.[1] Examining homicide trends in the 50 largest American cities, Barnett et al. (1975) conclude that differences in homicide growth across cities can largely be attributed to random fluctuations around a common trend. Similarly, Ousey (2000) observes remarkable similarities in homicide trends between US regions.

Was the Great American Homicide Rise of the 1960s truly a nationwide phenomenon? Consider the contrasting experiences of two Commuting Zones (CZs): New York and Pittsburgh.[2] Figure 2 plots their respective homicide trajectories over time. While New York CZ experienced a steep climb in homicide rates from the 1960s that closely mirrors the national trend, Pittsburgh CZ saw little change over the same period. Are these divergent paths isolated cases, or do they reflect a more systematic pattern across the country? In this paper, I document substantial geographic heterogeneity underlying the aggregate trend in homicides. This spatial variation, in turn, provides an opportunity to investigate the factors that led to the dramatic rise in homicide rates during the 1960s and the persistence in elevated rates for several decades thereafter.

I start by systematically classifying the trajectory of homicides across local labor markets in the US. To do this, I compile a new database of homicide rates at the CZ level, spanning the period 1945-1988. A k-means approach is then employed to group CZs based on their homicide time paths after 1960. This procedure identifies two distinct trajectories. One group of CZs experienced a sharp rise in homicide rates around the mid-1960s, with the rates remaining elevated for several decades thereafter; the other group only saw a modest rise in homicides over the same period. About

[1] More recent studies have challenged the earlier emphasis on common homicide trends. Stults (2010) identifies distinct homicide time paths across Chicago neighborhoods between 1965-1995, while McCall et al. (2011) concludes that there are four distinct homicide trajectories among large US cities between 1976-2005. I return to these studies in the literature review below.

[2] CZs are clusters of counties that exhibit strong commuting ties within CZs and weak commuting ties across CZs (Autor and Dorn, 2013b,a; Tolbert and Killian, 1987). These approximate local labor markets.

28 percent of CZs fall into the first group, accounting for roughly half of the population in 1960; 72 percent of CZs are in the second group, making up the remaining half of the population. Notably, CZs that witnessed a surge in homicides are not randomly distributed across the country—they tend to be found in the Sunbelt and among major urban centers. Rather than a pervasive phenomenon experienced similarly across the nation, the Great American Homicide Rise of the 1960s appears to have occurred in specific parts of the country. Importantly, the two-cluster structure and geographic divide are primarily driven by firearm-related homicides. When I examine firearm and non-firearm homicides separately, the former closely reproduces the original two distinct trajectories and the Sunbelt-heavy concentration, while the latter exhibits no discernible spatial pattern. Moreover, the two-cluster result and geographic distribution are not simply artifacts of racial differences in violent crime and location of residence (larger black populations in the South), as both findings are evident even when restricting the analysis to white homicides.

The geographic heterogeneity in local homicide trajectories documented in this paper opens an avenue to explore the underlying causes of the Great American Homicide Rise. Any explanation for the aggregate increase in homicides during the 1960s must be able to account for the stark differences in homicide trajectories across local labor markets. I propose that two major social upheavals of the 1960s may have contributed to these divergent paths: the racial riots and the broader countercultural transformation of American society.

Consider first the racial riots of the 1960s. Between 1964-1971, hundreds of riots broke out across American cities. While the US is no stranger to racial unrest, the riots that engulfed the nation in the 1960s were unprecedented in their frequency, geographic reach, and scale of destruction (Collins and Margo, 2004). Here, I adopt an event study framework to investigate the link between these riots and homicide rates. I show that prior to the 1960s, CZs which would subsequently experience a riot exhibited similar homicide trends to CZs which would remain unaffected. Starting in the mid-1960s, however, their trajectories begin to diverge, with riot-hit CZs experiencing significantly larger increases in homicide rates. By 1970, riot-affected CZs saw approximately 2 additional homicides per 100,000 persons annually, equivalent to roughly one-third of the sample mean. Crucially, the association between racial riots and homicides was not confined to the 1960s—it persisted throughout the 1970s and 1980s, suggesting that the riots contributed to the prolonged elevation of homicide rates in affected areas.

Why did the riots cause homicide rates to surge in the 1960s and remain elevated for several decades thereafter? That is, what are the underlying mechanisms? The number of deaths directly attributable to the riots is too small to account for the rise in homicides. This indicates that the riots generated spillover effects that extended well-beyond the immediate violence. I explore two potential channels through which these spillovers may have operated. First, the riots accelerated urban decline. The uncertainty, fear, and destruction brought about by the riots can reduce the attractiveness of affected areas, prompting residents to relocate. Even if their neighborhoods are not the actual sites of the riots, residents might still worry about the broader consequences of the unrest, including a shrinking tax base, rising insurance premiums, and the possibility of violence

spreading to surrounding areas. Using a panel of cities, I show that places experiencing a riot saw declines in the share of residents living in central city areas. This does not reflect a reallocation between central city areas and suburbs, but an outflow from the broader city as a whole. Such population losses, in turn, can accelerate urban decline. Moreover, I observe non-linearities in this process of outmigration. In particular, during the 1960s when racial unrest was at its peak, I demonstrate that the riots amplified residential tipping (Card et al., 2008), triggering even larger outflows of residents that decade. Second, riots could have affected homicides through the response they elicited from law enforcement. Rather than restoring order, intensified policing can exacerbate tensions and escalate confrontations between law enforcement and local communities (Hinton, 2011; Masera, 2021; Perez et al., 2003). Consistent with an intensified law enforcement response, I find that riot-affected areas experienced greater increases in both homicide clearance rates and police officers per capita. Taken together, the array of findings assembled here suggests that the riots of the 1960s had consequences extending far beyond the immediate destruction—by accelerating urban decline and eliciting a stronger law enforcement response, the riots potentially disrupted existing social arrangements and weakened community stability, creating conditions conducive to persistent interpersonal violence.

Beyond the racial riots of the 1960s, I also examine how the broader countercultural transformation of American society affected homicide rates. Two defining features of this transformation are the liberalization of sexual norms and the spread of recreational drug use, both of which accelerated beginning in the mid-1960s. To investigate their potential contribution to the rise and persistence in violence, I construct local measures of these social changes. First, I use the increase in non-marital childbirths and gonorrhea infections to proxy for shifts in sexual behavior. Second, I calculate the change in drug poisoning mortality to capture the spread of recreational drug use. I find that areas experiencing larger countercultural shifts based on these measures also witnessed a greater increase in homicide rates during the 1960s, followed by a persistence in the elevated rates for several decades. Like the racial riots, I argue that these broader social transformations disrupted established norms and weakened informal mechanisms of social control, creating conditions conducive to interpersonal violence.

My paper contributes to four strands of literature. The first is a growing econometrics literature that models latent heterogeneity across units by grouping them according to common underlying patterns or time profiles (Bonhomme and Manresa, 2015; Su et al., 2016; Su and Ju, 2018; Su et al., 2019). This literature has developed a range of clustering methods, with applications increasingly extending to empirical research. In the context of crime, for example, Stults (2010) and Griffiths and Chavez (2004) employ latent trajectory techniques to examine homicide trends in Chicago neighborhoods, while Parker et al. (2016) explores race-specific homicide trajectories from 1980-2006. Closest to my paper is McCall et al. (2011), who identify distinct homicide trajectories across 157 large American cities between 1976-2005. Compared to these studies, my paper expands the geographic coverage to local labor markets across the entire country, encompassing major metropolitan centers, smaller cities, and rural areas, whilst extending the analysis back to 1945.

This broader spatial and temporal scope allows me to examine not only the emergence of divergent homicide paths in the 1960s, but also their persistence over subsequent decades.

Next, my work relates to a large literature seeking to explain the long-run trends in violent crime in America (Figure 1). Much of this field has focused on the dramatic fall in crime that began in the 1990s. Levitt (2004) identifies four factors that collectively account for this decline: increases in the number of police, increases in the size of the prison population, the waning of the crack epidemic, and the legalization of abortion in the 1970s. In contrast, the surge in violent crime which started in the 1960s has received comparatively less attention. Latzer (2017) attributes this earlier crime wave to several demographic and social developments, including the coming-of-age of the Baby Boom generation, the Great Migration of blacks to northern cities, and the diffusion of violent cultural norms through internal migration. My paper adds to our understanding of this earlier episode by investigating how the racial riots of the 1960s and the broader countercultural transformation of American society contributed to the initial rise in homicides and the subsequent persistence of elevated rates for several decades.

In addition, this paper ties in with studies on the racial riots of the 1960s, which examine both the causes and consequences of the unrest. Earlier work focused primarily on identifying the factors that precipitated the riots, with black population size and geographic location emerging as particularly consistent predictors of riot severity (Carter, 1986; DiPasquale and Glaeser, 1998; Spilerman, 1970, 1971). Subsequent research then shifted attention to the consequences of the racial riots. In particular, a series of papers in the 2000s documented the adverse economic impact of the riots, such as deteriorating labor market opportunities, declining property prices, and growing population losses (Collins and Margo, 2004, 2007; Collins and Smith, 2007). My paper extends this literature by investigating the impact of the racial riots on violent crime, showing that their effects on homicides emerged in the 1960s and persisted for decades to come.

Finally, my paper speaks to the literature on the countercultural transformation of American society over the 20th century. Existing work has focused on the origins of the Sexual Revolution, along with the accompanying changes in family formation and marital institutions. Studies have examined the role of access to contraception (Akerlof et al., 1996; Bailey, 2006, 2010), the introduction of no-fault divorce laws (Stevenson and Wolfers, 2006; Wolfers, 2006), and the Vietnam War (Bailey and Chyn, 2020), among other developments. Rather than investigating the causes of these social transformations, my paper explores their impact on violent crime. I provide suggestive evidence that the liberalization of sexual norms and the spread of recreational drug use contributed to the rise in homicides during the mid-1960s and the persistently high rates of homicide in subsequent decades. These findings are consistent with the hypothesis that rapid social transformations can disrupt established norms and mechanisms of informal social control, expanding the scope for more violent behavior.

2 Classifying Local Homicide Trajectories

I begin by exploring whether the Great American Homicide Rise of the 1960s was a pervasive phenomenon experienced widely across the nation, or if different parts of the country followed distinctive homicide trajectories.

2.1 Data

To trace the time path of violent crime in each locality, I assemble annual homicide statistics at the substate level from various mortality records, covering the period 1945-1988. I focus on 1945-1988 as mortality data outside this range are more limited—information before 1945 is typically restricted to just urban areas or cities, while local counts after 1988 are often suppressed for confidentiality reasons.[3] For the years 1945-1988, I can draw on two sources of data. First, homicide figures for 1945-1958 are digitized from published *Vital Statistics of the United States* reports. These indicate the number of deaths each year, by cause and county. Second, data for 1959-1988 come from the National Center for Health Statistics (NCHS) Multiple Cause of Death files. These contain details on individual deaths annually, from which homicide cases can be identified and aggregated to the county level.[4] Compared with the leading causes of death in America such as heart diseases or cancer, homicides are relatively infrequent events, particularly at the county level and in less populous places.[5] County homicide data may thus display some idiosyncratic variation from year-to-year. I partially address this by mapping the county statistics to the corresponding CZ.[6] Each CZ encompasses about four counties on average (Dorn, 2009), helping to average out some of the idiosyncrasies. My final dataset is then a CZ panel of homicide counts spanning 1945-1988. From this, I can construct my primary outcome of interest: the number of homicides per 100,000 persons in a given CZ and year.[7]

2.2 Approach

With the necessary data, I then examine whether the trajectory of homicides differs across CZs. The contrasting time series for New York and Pittsburgh CZs in Figure 2 hint at potential differences on a wider scale. To move beyond these arbitrary examples, I adopt a data-driven approach that classifies CZs based on their respective homicide time paths. Specifically, I use k-means clustering to

[3] While the NCHS Multiple Cause of Death files are available after 1988, county identifiers are only provided for large counties in these later years. A related dataset is the NCHS Compressed Mortality Files, which can be accessed through the Centers for Disease Control and Prevention (CDC) Wonder database. Unfortunately, this database suppresses cases of low homicide levels for confidentiality reasons, affecting a large number of areas. For example, I calculate that 49.2 percent of all counties in 1990 have suppressed homicide counts in the CDC Wonder database.

[4] I exclude 1972 as the NCHS data that year only contain a 50 percent sample.

[5] For instance, there were 674,747 deaths due to ischemic heart diseases alone in 1968, compared with 14,336 deaths caused by homicide (author's tabulations from the CDC Wonder database). In this year, around 46 percent of counties recorded zero homicide deaths.

[6] The mapping is done using the county-to-CZ correspondence in Chetty and Hendren (2018).

[7] For each CZ and year, the population counts that form the denominator are based on decennial census data from Manson et al. (2023). Population figures for intercensal years are imputed using linear interpolation.

group CZs according to the similarity of their homicide trajectories (Hartigan and Wong, 1979; Liu et al., 2023; MacQueen, 1967). This procedure relates to a broader literature in econometrics that models latent heterogeneity across units by grouping them in terms of some common underlying pattern or time profile (Bonhomme and Manresa, 2015; Su et al., 2016; Su and Ju, 2018; Su et al., 2019). While a wide range of techniques have been developed in this field, I select k-means as my baseline clustering method for its simplicity, transparency, and ease of interpretation. Alternative clustering approaches will also be considered in the robustness checks that follow.

I implement my classification approach in four steps. First, to focus on the *shape* of local homicide trajectories rather than *level* differences across CZs, I normalize the homicide rate in each year relative to a benchmark period. *A priori*, different years could serve as my benchmark. As a baseline, I use the mean homicide rate of each CZ between 1955-1959, when violent crime was relatively stable (Figure 1). Normalized homicide rates for each CZ are then obtained by subtracting the mean rate between 1955-1959 from the homicide rate in a given year. Second, to reduce the influence of idiosyncratic noise or outliers in the annual data, I group the normalized homicide rates for each CZ into 5-year bins and take the mean within each bin. Third, I apply k-means clustering to the normalized homicide trajectories in 5-year bins, focusing on the dynamics between 1960-1988 when homicide rates surged.[8] Because the number of underlying clusters is unknown, I consider a range of candidate values $k = 2$ to 8. For each candidate k, I run the k-means algorithm from 50 random initializations to guard against any sensitivity to starting points, retaining the solution that minimizes the within-cluster sum of squares—that is, the sum of squared distances between each CZ's trajectory and the centroid of its assigned cluster. Fourth, I select among the candidate partitions $k = 2$ to 8 using the Caliński-Harabasz (CH) variance-ratio criterion (Caliński and Harabasz, 1974). The CH statistic compares the variation *between* clusters to the variation *within* clusters, adjusting for the number of clusters. A higher value thus reflects a partition characterized by greater separation across clusters and greater cohesion within clusters. The value of k which maximizes the CH statistic is then taken as the baseline partition.

2.3 Baseline Results

Applying the four-step approach above leads to a key finding of this paper: the Great American Homicide Rise of the 1960s was not uniformly experienced across the country. That is, it was not a pervasive phenomenon. Figure 3 plots the CH statistic obtained for each candidate partition $k = 2$ to 8. This metric declines as the number of partitions increases, thus attaining its maximum at $k = 2$. In words, the k-means procedure identifies two distinct homicide trajectories across CZs. To illustrate the substantive differences between these regimes, Figure 4 traces the average homicide time path for each cluster, unweighted (A) and weighted by the CZ population in 1960 (B). A striking divergence emerges from the 1960s. One cluster displays a sharp rise in homicide rates

[8] The 5-year bins in the 1960-1988 interval are: 1960-1964, 1965-1969, 1970-1974, 1975-1979, 1980-1984, 1985-1988, where the last bin comprises 4 years.

(solid line with circle markers), resembling the pattern observed at the national level (Figure 1).[9] For ease of exposition, I refer to this group of CZs as the "high-exposure" cluster (greater exposure to the Great American Homicide Rise). Some 28 percent of CZs fall into this category, yet they contained about half of the US population in 1960. In contrast, the other cluster exhibits a modest rise over the same period (dashed line with triangle markers). I refer to this set of CZs as the "low-exposure" cluster (less exposure to the Great American Homicide Rise). Around 72 percent of CZs are in this group, accounting for the remaining half of the population. Together, my results reveal substantial geographic heterogeneity in homicide trajectories beneath the national trend. Rather than a pervasive phenomenon experienced similarly across the country, the Great American Homicide Rise was concentrated in less than a third of CZs where half the population lived.

What are the characteristics of CZs in the high- and low-exposure clusters? Table 1 compares the two groups along several dimensions. As a start, I look at homicide rates between 1955-1959, prior to the national rise in violent crime (also used previously to normalize my data). Perhaps unsurprisingly, violent crime is around 40 percent higher among CZs in the high-exposure cluster than the low-exposure cluster. Beyond crime, I also consider a range of socioeconomic features: age profile, racial composition, migration history, educational attainment, labor market conditions, and region of residence, all constructed from the 1960 census (Ruggles et al., 2025). The two clusters of CZs differ in several ways, with some differences being particularly pronounced. CZs in the high-exposure cluster tend to have larger population shares of blacks, Hispanics, and central city dwellers. There are relatively fewer blue-collar occupations in these places, with a correspondingly stronger presence of college graduates. Additionally, a stark geographic divide is observed: CZs in the high-exposure regime are substantially less common in the Northeast and Midwest census divisions, concentrating instead in the South and West (to a lesser extent). Breaking down the regions further, Table A.1 reveals that high-exposure CZs are especially likely to be found in the South Atlantic division, the West South Central division, and the Pacific division.[10] To better visualize these spatial patterns, Figure 5 maps CZs according to their assigned cluster. Consistent with the summary statistics in Table 1, CZs in the high-exposure cluster are primarily located in the Sunbelt and among major urban centers. This visual representation further confirms that the Great American Homicide Rise was not experienced in the same way across the country, but was limited to a distinct subset of places.

2.4 Stability

How reliable is the two-cluster homicide trajectory structure derived in the baseline analysis? In this section, I test for stability: if the k-means procedure is perturbed, can one still recover a similar two-cluster regime? To shed light on this, I look at both algorithmic and sample stability in turn.

[9] For the unweighted series (Figure 4), a sharp spike is observed among high-exposure CZs in 1946. This is driven by a small outlier CZ, thus fading away in the weighted series (Figure 4B).

[10] The South Atlantic division comprises: Delaware, District of Columbia, Florida, Georgia, Maryland, North Carolina, South Carolina, Virginia, and West Virginia. The West South Central division includes: Arkansas, Louisiana, Oklahoma, and Texas. The Pacific division consists of: California, Oregon, and Washington.

I start with algorithmic stability, which asks whether my two-group classification is sensitive to the choice of initialization point in the k-means procedure? Recall that my baseline analysis used 50 random initializations for each candidate partition to avoid "bad" local solutions, selecting the result with the smallest within-cluster sum of squares. To determine if the 50 random initializations are sufficient to recover a solution that is insensitive to starting values, I apply the 50-start algorithm 100 times, each time drawing a different set of 50 initializations randomly. Every one of these 100 iterations yields a partition which I then compare with the baseline two-cluster partition using the Adjusted Rand Index (ARI) (Hubert and Arabie, 1985). The ARI measures the similarity between a pair of classifications, adjusting for agreement that could occur by chance. An ARI of 1 indicates identical partitions; an ARI of 0 corresponds to chance-level agreement; while negative ARI values reflect less agreement than would be expected by chance. Reassuringly, I obtain an ARI of 1 for all 100 iterations. My baseline partition thus displays substantial stability with regard to the particular set of starting values used. Put differently, setting 50 starts in the k-means procedure is enough to eliminate initialization sensitivity.

Next, I look at sample stability, where the issue is whether my baseline two-cluster partition is robust to the set of CZs included in the analysis? To evaluate this, I repeat the full k-means procedure 100 times, each time using a random 80-percent subsample of CZs. Again, these iterations produce partitions of the CZ sample, which I then compare with the original two-cluster partition using the ARI. I find that 83 (99) percent of the 100 iterations have an ARI over 0.9 (0.8). The initial two-cluster structure is therefore remarkably stable to changes in estimation sample, suggesting that the grouping is not driven by just a small number of influential CZs.

2.5 Robustness

When classifying homicide trajectories across CZs, several empirical choices had to be made: the benchmark period for normalization (1955-1959), the bin size for temporal aggregation (5-year intervals), the clustering method (k-means with CH statistic), and the underlying homicide data (mortality records). This section investigates if the baseline two-cluster structure is broadly robust to alternative choices along these margins.

2.5.1 Setup

Like the stability analysis above, I compare the partition under each robustness check with the baseline classification. Unlike the stability exercise, every robustness check here produces a single alternative partition rather than a distribution of partitions across repeated initializations or samples. This makes it practical to use several complementary measures of how similar the alternative and baseline groupings are.[11] First, I look at the raw agreement between partitions: the share of CZs that are assigned to the same regime under the two classifications. Because my baseline partition is unevenly sized (28 percent in the high-exposure cluster; 72 percent in the

[11] When comparing partitions under the alternative and baseline regimes, I ensure that cluster labels are aligned such that high- and low-exposure partitions correspond across the classifications being compared.

low-exposure cluster), the raw agreement alone can be mechanically high even if few observations in the smaller cluster retain their original classification. Alongside the raw agreement, I thus report cluster-specific retention rates—the share of high-exposure CZs in my baseline that are still classified as high-exposure under the alternative specification, and the corresponding share of low-exposure CZs in the baseline that remain low-exposure. These reveal whether any reclassification of CZs is disproportionately concentrated in one of the two regimes. Second, I calculate the correlation in regime assignment between the alternative and baseline partitions. This summarizes the strength of association between a pair of classifications. Third, I use the ARI measure described above, which captures similarity in the overall partition whilst adjusting for agreement expected by chance. Importantly, the three metrics reflect different notions of similarity and are therefore best viewed together to build a more complete picture. Raw agreement maps most directly to the substantive question of whether the same CZs are classified as high- or low-exposure. The correlation further accounts for the pattern of assignment across alternative regimes. Finally, the ARI provides a more stringent assessment of similarity in overall partition by adjusting for chance agreement. Consequently, a relatively low ARI can coexist with substantial raw agreement and a strong correlation, particularly when cluster sizes are unequal (as is the case here). Such a result need not imply that the substantive classification has changed dramatically. Rather, it indicates that agreement beyond what is expected tends to be is weaker.

Four sets of robustness checks are conducted in this section, each focusing on one of the four margins of choice described above. First, I vary the benchmark period for normalization, switching from the mean homicide rate between 1955-1959 to the corresponding mean over 1950-1954 or 1959-1963. The latter also serves as a reference for my subsequent race-specific analysis, for which homicide data are only available from 1959 onward. Second, I adjust the degree of temporal aggregation, narrowing the baseline 5-year bins to 4- or 3-year intervals. Third, I consider a range of alternative clustering methods. As a start, I implement the enhanced optimization procedure of Bonhomme and Manresa (2015), which augments the standard k-means search process with a variable-neighborhood algorithm designed to reduce sensitivity to local minima. I then use their BIC-type criterion to select the number of groups.[12] Additionally, I also apply Ward's hierarchical clustering as well as Gaussian mixture (latent profile) clustering. Fourth, I use homicide data from a different source, replacing the NCHS mortality records with the UCR Offenses Known and Clearances by Arrest (OKCA) dataset (Kaplan, 2025d), which is available from 1960. Unlike mortality records which are compiled from death certificates, the UCR OKCA data are based on the number of crimes reported (or otherwise known) to police agencies.

[12]The grouped fixed-effects objective of Bonhomme and Manresa (2015) reduces to the standard k-means minimum-sum-of-squares problem when there are no controls, as is the case in my setup. The main difference with my k-means approach thus lies in Bonhomme and Manresa's (2015) optimization procedures and selection criterion.

2.5.2 Results

Table 2 summarizes the results from my suite of robustness checks. Each row corresponds to a particular check, reporting both the number of clusters selected under that specification (column 1) as well as various measures of its similarity with the baseline partition (columns 2-6).

Reassuringly, a two-cluster solution emerges in almost every scenario. The one exception is when UCR data are used in place of the mortality records, for which three clusters are selected. Nonetheless, even the structure under this specification is dominated by two large groups, with the third group containing fewer than 5 percent of CZs. On the whole, the two-cluster regime thus appears to be a relatively robust feature of US homicide trajectories.

Diving deeper into the makeup of the two-cluster structure under each robustness check, I ask how closely group membership in the alternative partition corresponds to the baseline classification. First, changing the normalization period produces moderately similar groupings (Panel A). I find high raw agreements exceeding 80 percent, correlations of 0.4-0.5, and ARIs between 0.36-0.48. The reclassification of CZs under the alternative partitions is disproportionately concentrated among high-exposure CZs in the baseline: approximately 64 (71) percent remain high-exposure under the 1950–1954 (1959–1963) benchmark compared with a retention rate of 88 (91) percent among low-exposure CZs. This sensitivity to the normalization period is perhaps unsurprising—unlike 1955–1959, the alternative benchmarks considered here encompass years when national homicide rates were changing, potentially altering the measured shape of homicide trajectories post-normalization. Second, my results are considerably stronger when varying the degree of temporal aggregation (Panel B). All similarity measures score close to 1, indicating that the baseline partition is not an artifact of the particular bin width used to smooth annual homicide rates. Third, changing the clustering method produces more heterogeneous outcomes (Panel C). Ward's hierarchical clustering generates a partition close to the baseline. Gaussian mixture clustering generates high overall agreement, although this masks the asymmetric reclassification across regimes where the low-exposure group is highly stable while the higher-exposure CZs are more susceptible to reclassification. Applying Bonhomme and Manresa's (2015) optimization and group-selection procedure generates the largest departure among the different clustering methods, with approximately 77 percent agreement, a correlation of 0.50, and an ARI of 0.29. Nonetheless, the retention rate within each cluster (high- and low-exposure) is reasonably high. Fourth, replacing the mortality records with UCR data leads to a classification that is somewhat less similar to the baseline (Panel D). This discrepancy could partly reflect the additional cluster under this setup (column 1) as well as several constraints inherent in the UCR data. Because the UCR series only begins in 1960, I use 1960–1963 rather than 1955–1959 as the normalization period. Local UCR numbers are also subject to well-known measurement issues arising from incomplete reporting and the need to allocate agency-level crime counts geographically.[13]

[13] Kaplan (2025a) and Maltz and Targonski (2002) discuss the two measurement issues associated with the UCR data. First, the data are incomplete, either because law enforcement agencies do not report information to the FBI, report for only some months, or fail to give the full details when they do report. These missing records need to be imputed but current methods for doing so can be inadequate and inconsistent. Second, the UCR data are collected

Taken together, the series of robustness checks suggests a distinction between (i) the existence of heterogeneous homicide regimes and (ii) the precise assignment of individual CZs to those regimes. The key finding of two sharply differing homicide trajectories is broadly robust across alternative normalization periods, temporal aggregation bins, clustering methods, and data sources. However, there can be some variation in the specific CZs under each cluster. In particular, the exact composition of the high-exposure cluster tends to be more sensitive, especially when changing the clustering technique or the underlying homicide data. On the other hand, the makeup of the low-exposure cluster is considerably more stable. Put differently, there is a clearly identified set of CZs which did *not* experience the Great American Homicide Rise, while there is a less sharply bounded continuum among places that did. This circles back to my key result: the Great American Homicide Rise was no pervasive phenomenon.

2.6 Nature of Homicides

Thus far, my analysis has looked at homicides as a whole, without distinguishing between different types of homicides—this section delves further into the two-cluster structure derived in the previous sections, exploring homicide trajectories by method of homicide and across racial groups.

I begin with the method of homicide. Specifically, I differentiate between firearm- and non-firearm-related homicides. I contend that the surge in homicides from the mid-1960s and the associated two-cluster trajectory across CZs are largely driven by cases involving firearms. This can be illustrated in three ways. First, Figure A.2 plots the national homicide rate by method, revealing a steep climb in firearm-related homicides during the 1960s but a much smaller gradual increase in non-firearm-related homicides. Second, as a result of these divergent paths, Figure A.3 shows that the share of all homicides involving firearms exhibits an inflexion in the mid-1960s, rising sharply thereafter. Third, applying the k-means approach to homicides that did and did not involve firearms separately, I obtain a contrasting set of results. Figures A.4A and B reveal that the trajectory of firearm-related homicides can be grouped into two clusters, one experiencing a rapid increase in homicides (high-exposure cluster) and the other displaying a modest rise (low-exposure cluster). Again, high-exposure CZs are concentrated in the Sunbelt, as depicted in Figure A.5A. For non-firearm-related homicides, the pattern is markedly different. Here, a three-cluster solution is selected. Although this is dominated by two large clusters, Figures A.4C and D suggest that differences in homicide trajectories are smaller here than the corresponding divergence observed with firearm-related homicides (Figures A.4A and B). In addition, Figure A.5B shows that CZs in the high-exposure group based on non-firearm homicides are widely dispersed across the country rather than centered in the Sunbelt.[14] Putting the pieces together: the overall surge in homicides

at the agency rather than the county level. Because an agency's jurisdiction can straddle multiple counties, one needs to allocate the crimes under a given agency to the respective counties, potentially introducing more error.

[14]The clustering of non-firearm-related homicides is more sensitive to the sample of CZs used in the analysis, compared with firearm-related homicides. Applying the sample stability test, I find that 33 percent of iterations have an ARI of 0.8 or higher for non-firearm-related homicides; this is 100 percent when looking at firearm-related homicides.

from the 1960s, the two-cluster structure derived, and the greater rise in the Sunbelt, are all driven primarily by firearm-related homicides.

Next, I turn to race. A possible concern with the baseline two-cluster result is whether it simply reflects the differences between American's two largest racial groups—whites and blacks having different homicide levels and living in different parts of the country. This seems unlikely. First, while homicide rates are indeed lower among whites than blacks, both experienced a dramatic surge in violent crime during the 1960s (Figure A.6). Second, CZs in the high-exposure cluster are found across large swathes of the Sunbelt (Figure 5), which includes both places where relatively few blacks live as well as areas with large black populations. Third, redoing the k-means procedure by race reinforces a two-cluster regime. Beginning 1959, the NCHS mortality records allow one to distinguish homicides by race.[15] I thus apply the k-means procedure to each racial group separately, using 1959-1963 as the benchmark years for normalization. For whites, this exercise yields a two-cluster solution that broadly mirrors the baseline structure. Figures A.7A and B show the mean trajectory of white homicides for CZs under the two regimes, again differentiating a group that experienced a surge in homicides (high-exposure cluster) from another that saw a modest increase (low-exposure cluster). Geographically, Figure A.8A reveals that high-rise CZs are once again concentrated in the Sunbelt. These findings indicate that the original two-cluster structure was not simply driven by differences in where whites and blacks lived. When looking at black homicides instead, I narrow the focus to a subset of 370 CZs that had at least 1,000 black residents each year. Even under this setup, a two-cluster solution is again selected. Almost 80 percent of CZs in the narrow subsample experienced a rapid surge in black homicides (Figures A.7C and D), covering the vast majority of the 370 CZs (Figure A.8B).[16]

2.7 Summary

To summarize, this paper set out to determine whether the Great American Homicide Rise of the 1960s was a pervasive event that affected people across the country to a similar degree. Applying a data-driven clustering approach, I find that the US is better-described as having two distinct regimes, where some places experienced a dramatic surge in violent crime while other areas saw modest increases. This geographic heterogeneity is primarily driven by firearm-related homicides and is not a mechanical artifact of whites and blacks living in different parts of the country.

[15] A caveat here is that race as reported in the mortality records refers to the victim rather than the perpetrator. Nonetheless, because the vast majority of homicides are intraracial, the victim's race is a good proxy for the perpetrator's race. Drawing on the UCR Supplementary Homicide Records (SHR) from 1976 (the earliest available year) through 1988 (the end of my study period) (Kaplan, 2025c), I calculate that 87.3 (92.4) percent of white (black) homicide victims were killed by a white (black) person.

[16] Compared with white homicides, the results for black homicides may be somewhat more sensitive to the analysis sample. Applying the sample stability test, I find that 96 percent of iterations in the white homicide analysis have ARIs of at least 0.8 but this falls to 48 percent when studying black homicides. This appears to be a feature of black homicides rather than the narrow subset of 370 CZs. Running the analysis with all homicides while keeping the 370 CZ subset yields a more stable partition, where 92 percent of iterations in the sample stability test have ARIs of 0.8 or higher.

3 Explaining Local Homicide Trajectories

By demonstrating that the Great American Homicide Rise of the 1960s occurred in some parts of the country but not others, this paper opens an avenue to re-evaluate why violent crime rates surged at the time and remained elevated thereafter. In particular, any explanation for the national rise in homicides must also be able to explain the pronounced geographic heterogeneity in its trajectory. This section explores several potential contributors.

I argue that major social upheavals in the 1960s may have disrupted existing forms of social organization and informal social control, creating conditions that are more conducive to interpersonal violence. This interpretation ties in with the broader literature on social disorganization theory, which emphasizes how weakened community institutions, social ties, and shared norms can reduce a community's collective capacity to regulate behavior, resulting in more crime (Sampson and Groves, 1989; Sampson et al., 1997; Shaw and McKay, 1942). The question then is: what happened in the 1960s that weakened such collective efficacy, expanding the scope for violent behavior? Guided by the timing of the homicide surge, which began around the mid-1960s (Figure 1), I focus on two contemporaneous social upheavals: the racial riots and the broader countercultural transformation of American society.

3.1 Racial Riots

I begin with the racial riots of the 1960s. Between 1964-1971, hundreds of riots erupted across American cities, resulting in a large number of injuries, deaths, and arrests, as well as considerable property damage. While the US has a long history of race-related unrest tracing back to the 19th century, the riots that emerged in the 1960s were collectively unique in their high frequency (tightly clustered in time), wide geographic distribution, and unprecedented scale of destruction (Collins and Margo, 2004).[17] In addition, unlike earlier clashes which were often initiated by whites attacking blacks, the riots of the 1960s were characterized by "black aggression" (although whites were rarely targeted) (Collins and Margo, 2004).[18] Such disturbances could potentially disrupt the existing social equilibrium in an area, causing a place to descend into chaos.

Much has been written about the causes and consequences of the 1960s racial riots. In the wake of severe unrest in Newark, Detroit, and other cities, US President Lyndon B. Johnson established the National Advisory Commission on Civil Disorders (Kerner Commission) in 1967. Its task was to investigate the root causes of the urban unrest sweeping across America. The Commission released its findings in 1968, attributing the disturbances to a combination of racial discrimination, concentrated poverty, limited employment opportunities, and hostile police-community relations, among other things. This report was followed by a series of empirical papers examining the causes of the riots (Carter, 1986; DiPasquale and Glaeser, 1998; Spilerman, 1970, 1971). These studies

[17] The destruction caused by the riots of the 1960s has since been surpassed by more recent disturbances like the Los Angeles riots in 1992 and the civil unrest that followed the murder of George Floyd in 2020.

[18] To be clear, the 1960s were not the first instance of black-initiated riots. The Harlem riot of 1943, for example, was chiefly directed by black residents against white-owned properties.

tend to find black population size and geographic region to be the most consistent and important predictors of the riots. Recent work has also explored the factors which amplified the severity of riots, such as access to legal redress and black-oriented radio (Bernini, 2023; Cunningham and Gillezeau, 2018; Cunningham, 2021). Beyond the causes, several papers have examined the impact of the riots, focusing primarily on economic dimensions such as labor market prospects and property prices (Anti et al., 2025; Collins and Margo, 2004, 2007; Collins and Smith, 2007). Could the riots have gone even further, driving the sharp escalation in homicide rates from the 1960s and the persistently elevated rates over the ensuing decades?

3.1.1 Empirical Setup

To shed light on the link between the racial riots of the 1960s and the surge in homicides, I draw on the riots data assembled by Carter (1986). These data span the years 1964-1971, extending the list of riots originally compiled by Spilerman (1970, 1971).[19] They were popularized in the economics literature by Collins and Margo (2004, 2007). Following Spilerman (1970, 1971), a riot here is defined as a spontaneous event involving at least 10 participants, some of whom are black, that results in property damage, looting, or other aggressive behavior. By this definition, 752 disturbances enter into Carter's (1986) dataset. The riots data report the date(s) and location(s) of each riot as well as the associated number of arrests, injuries, deaths, arsons, and days of rioting. Figure A.9 traces the frequency and characteristics of the riots across 1964-1971, revealing a peak in riot activity and intensity around 1967-1968.

From the riots data, I can construct my key variables of interest. I start by mapping the information in the riots dataset to the CZ level. I then measure each CZ's exposure to the 1960s riots in two ways. The first is an indicator for whether a CZ experienced any riot between 1964-1971. This focuses on the *extensive* margin of riot exposure. The second looks at the severity of the riots, capturing the *intensive* margin of riot exposure. As alluded to previously, riots can be characterized in terms of the associated arrests, injuries, deaths, arsons, and days of rioting. Following Collins and Margo (2004, 2007), I construct an index of riot severity for each CZ, based on these characteristics. Specifically, I assign each riot r a value $S_r = \sum_i (X_{ir}/X_{iT})$, where X_{ir} is a severity component i (deaths, injuries, arrests, arsons, and days of rioting) and X_{iT} is the sum of component X_{ir} across all riots.[20] These S_r values are added across all riots in a given CZ to create a local index of riot severity.[21] To facilitate the analysis below, I define a dummy for CZs with severity scores at the 95th percentile or higher, identifying areas with the most severe riots. Figures 6A and B map the location of CZs where at least one riot (extensive margin) or a severe riot (intensive margin) occurred, revealing some overlap with the distribution of CZs in the

[19] The underlying sources of riot information include the Congressional Quarterly's *Civil Disorder Chronology* in 1967, the Kerner Commission Report in 1968, an index prepared by the *New York Times*, and the "Riot Data Review" compiled by the Lemberg Center for the Study of Violence at Brandeis University.

[20] S_r is then the proportion of all arrests that occurred during riot r, plus the proportion of all riot injuries that occurred during riot r, plus the proportion of all riot deaths, and so on.

[21] The limitations of this index have been discussed in Collins and Margo (2004, 2007). For example, counts of destructive events need not correspond to economic damage.

high-exposure cluster (Figure 5), particularly among the major urban centers.

Combining the riots data with my panel of homicide statistics, I then run the following event study regression to study the relation between riots and homicides:

$$y_{ct} = \sum_{j=1945}^{1988} \beta_t \; Riots_c \text{ x } 1[t = j] + \theta_c + \theta_{st} + \varepsilon_{ct} \tag{1}$$

where the subscripts c, s, and t denote the CZ, state, and year, respectively. The outcome, y, is the number of homicides per 100,000 persons in a given CZ and year. My explanatory variable is the interaction between the term *Riots* and year indicators. *Riots* captures the degree of exposure to the racial riots between 1964-1971.[22] As described above, this is either (i) an indicator for whether a CZ experienced any riot (extensive margin) or (ii) a dummy for CZs that saw severe riots (intensive margin). With regard to the year interactions, it is *a priori* unclear which year should serve as the omitted period. Racial riots were happening in the US even before 1964, making it difficult to pinpoint a precise treatment start date. Throughout this paper, I select 1960 as the reference year for both conceptual and practical reasons. Conceptually, 1960 precedes the peak in urban unrest (Figure A.9) as well as the acceleration in countercultural transformation (discussed below), the social upheavals of interest. Practically, the controls that will later be included in my regression as a robustness check can be measured in 1960, based on the corresponding census. Nonetheless, because 1960 is not necessarily the exact treatment start date, any impact of the riots on homicides need not emerge immediately after 1960. Finally, completing the regression is a vector of CZ and state-by-year fixed effects, the θs. Underlying the event study is the assumption that homicide trends would have been similar across CZs if not for the riots. This allows one to interpret the post-1960 β estimates as the causal effect of the riots. I return to the identifying assumption below.

3.1.2 Results

Figure 7 presents my event study results from regression (1), with 95 percent confidence bands based on standard errors clustered by state. Figures 7A and B focus on the extensive measure of riots while C and D use the intensive measure; A and C include year fixed effects whereas B and D incorporate state-by-year fixed effects.

I find that the racial riots of the 1960s contributed to the surge in homicides at the time and the subsequent persistence in elevated homicide rates. This is evident under either measure of riot exposure, without or with state-by-year fixed effects. To facilitate the analysis, I divide my discussion into three phases: before 1960, from the mid-1960s, and after the 1960s.

First, before 1960, there is little evidence that CZs which would later experience a riot were on a different trajectory from CZs that would not be hit by the riots. The point estimates in

[22] Using the riots between 1964-1971 as the treatment does not assume the absence of large-scale racial riots after 1971. Later outbreaks did occur, for example, in Miami (1980) and Los Angeles (1992). Rather, my setup takes the 1960s riots as a unique shock that was historically unusual in its frequency, geography, and damage, and then quantifies the impact of this intense period of violence.

this phase are usually statistically insignificant, with no systematic or consistent pretrends.[23] This aligns with existing work which suggests that conditional on black population size and region, riot severity is essentially idiosyncratic (Spilerman, 1970, 1971). Indeed, many riots were precipitated by idiosyncratic sparks that turned minor altercations into full-blown riots, be it an encounter between a black motorist and the police (e.g. Marquette Frye versus a California Highway Patrol officer), an incendiary speech (e.g. H. Rap Brown), or the assassination of a prominent figure (e.g. Martin Luther King Jr.). A similar observation was made by the Kerner Commission Report, which noted how seemingly small triggers could explode into major episodes of unrest. Importantly, the lack of pretrends provides support for the identifying assumption of my event study. This, in turn, allows one to attribute any subsequent changes in homicide trends to the racial riots.

Second, from the mid-1960s, the event study estimates become positive and statistically significant. That is, CZs hit by a riot(s) see relatively larger increases in homicide rates, beginning around the time of the riots. Quantitatively, the riot effect is sizable. In 1970, for instance, CZs that had at least one riot saw an additional 1.94-2.15 homicides per 100,000 persons (Figures 7A and B), about a third the sample average of 5.81 homicides per 100,000 persons. The impact is even more pronounced when looking at severe riots (Figures 7C and D). Again using 1970 as an example, CZs with a severe riot witnessed 2.93-3.15 more homicides per 100,000 persons.

Third, after the 1960s, the riot effects persisted even though the most intense period of unrest had already passed. While there is some fluctuation in the event study estimates, the riot effects tend to last till the end of my study period. This suggests that the riots of the 1960s caused more than just a temporary contemporaneous change—there was also a long-term shift in the social equilibrium of an area. Interestingly, the profile of the riot effects (Figure 7) mirrors the national trend in homicides (Figure 1), further pointing to a role for the riots.

Could the positive relation between riots and homicides after 1960 simply reflect some omitted confounder? Table 1 revealed several differences between CZs in the high- and low-exposure clusters, as measured in 1960. Some of the more pronounced differences include the share of blacks, hispanics, and central city dwellers. For each of these characteristics, I construct an indicator for CZs where that particular group made up at least 10 percent of the local population in 1960. These are interacted with a full set of year dummies and added simultaneously to regression (1). My extended specification is demanding both because of the number of interactions involved as well as the sizable correlation between the additional variables of interest and where the riots occurred—for example, race-related riots primarily arose in urban city centers where relatively large black populations lived.[24] The resulting estimates could thus have more noise. Figure A.10 shows the event study coefficients after including the interaction controls. While the point estimates are indeed noisier, the impact of the riots on homicide rates can still be detected from the mid-1960s. My findings

[23] While Figure 7A hints at a downward trend before 1960, this is unlikely to pose a serious problem. The apparent pretrend (i) goes in the opposite direction as the impact of the riots on homicides after 1960, (ii) stabilizes in the years leading up to the riot effects, and (iii) flattens out with state-by-year fixed effects (Figure 7B).

[24] For example, the correlation between an indicator for CZs with at least one riot and an indicator for CZs where at least 10 percent of the population lived in the central city area in 1960 is 0.509.

are also robust to adjusting the definition of severe riots. Figure A.11, for instance, traces similar results when CZs with a riot severity index at the 90th percentile or higher are considered to have experienced a severe riot.

3.1.3 Mechanism I: Urban Decline

Why did the racial riots cause homicide rates to surge in the 1960s and remain elevated for several decades after (Figure 7)? It is important to clarify from the outset that this relation is *unlikely* to be direct. That is, the estimated riot effects on homicides are not driven by homicides resulting directly from the riots. Given my focus on the 1964-1971 riots as the treatment, these cannot, by definition, add to the homicide counts in later decades (the long-term effect). What about the initial rise in homicides from the mid-1960s (Figure 1), which overlaps with the 1964-1971 interval? Carter's (1986) riots data tally 228 deaths between 1964-1971. Among CZs with at least one riot, there were around 81,980 homicides between 1964-1971 and about 46,416 homicides in the preceding 8-year window from 1956-1963. This implies that even if all the deaths associated with the 1964-1971 riots were homicides (an over-count), they would only account for a small fraction of the overall increase in homicides across the two windows. Furthermore, while the riots primarily occurred in black neighborhoods, Figure A.12 shows that white homicides also surged during the 1960s in CZs that were exposed to the riots, based on a variant of regression (1) where white homicides rates are the outcome of interest. There must thus be more than a direct mechanism—the riots had broader spillover effects that cut across race, neighborhoods, and time.

I propose that one way in which the racial riots generated spillover effects on homicides was by accelerating the decline of urban areas, disrupting the existing social equilibrium in these places. Intuitively, riots can reduce the amenity value of a neighborhood. This could come in various forms, such as worsening local employment opportunities, the physical destruction of facilities, or a perceived decline in safety. The decrease in amenity value may then incentivize residents (and potentially firms) to leave. These departures, in turn, contribute to the process of urban deterioration. Importantly, those who leave need not come from neighborhoods where the riots occurred. Outmigration might also emerge from other neighborhoods and even among white residents who have no contact with blacks, whether due to the visceral effect of seeing the riot-inflicted chaos in the local press and on TV or as a result of shrinking tax bases and rising insurance premiums, among other possibilities. Empirically, the riots did worsen employment opportunities and weaken property prices (Collins and Margo, 2004, 2007). In addition, Collins and Smith (2007) find that neighborhoods in Cleveland which were exposed to the riots saw population losses while Boustan (2006) observes that the riots exacerbated the pace of white flight from urban city centers, consistent with my hypothesis of urban decline.

To provide a more comprehensive picture of how the riots hastened urban decline by accelerating the departure of residents, I draw on data from Curci and Masera (2025). Curci and Masera (2025) assemble an annual panel of 307 cities from 1960-2014, with information on the population in each city and year. Their data are aggregated from the population under each police agency's

jurisdiction, as reported in the UCR OKCA dataset. Further population disaggregations by central city area and suburb are also included in their data. For consistency with the preceding analysis, I restrict Curci and Masera's (2025) city panel to 1960-1988. Combining these population data with Carter's (1986) riots database, I then run the following regression:

$$ShareCC_{ct} = \sum_{j=1960}^{1988} \gamma_t \; Riots_c \text{ x } 1[t = j] + \theta_c + \theta_{dt} + u_{ct} \tag{2}$$

where the subscripts c, d, and t denote the city, census division, and year, respectively. The outcome, $ShareCC$, is the share of each city's residents that are residing in the central city area. My explanatory variable follows regression (1), but is now measured at the city rather than the CZ level. As before, two metrics of riot exposure are used for $Riots$: either an indicator for places with at least one riot or one severe riot between 1964-1971. Because the sample for analysis is much smaller here compared with the baseline, I use city and division-by-year fixed effects instead of state-by-year fixed effects. The γ coefficients then capture the impact of riots on the share of residents in central city areas. Negative values, in particular, would reflect a relative outflow of people. One limitation of this setup is that the data only begin in 1960, leaving little scope to assess pretrends.

Figure 8 presents my event study results from regression (2). Beginning in the mid-/late-1960s, negative and statistically significant point estimates can be observed. This suggests that riot-hit places experienced a decline in central city population shares. To unpack this effect, I ask whether the decline reflects (i) a reallocation of people within cities, from the central city area to the suburbs, or (ii) an outflow of people from the city as a whole. I investigate this with regression (2), replacing the outcome with the log population in the central city area, suburb, or city as a whole. Figures A.13 and A.14 show the corresponding results for riots and severe riots. In line with the preceding findings, population losses from central city areas are evident for places that were hit by the riots (A and B). In the short-run, there is some hint that people may have been moving from central city areas to the suburbs in response to the riots, though nothing is statistically significant here (C and D). The cleaner set of results is the outflow from the city as a whole (E and F). That is, the riots accelerated urban decline by driving people away from the central city area and ultimately from the city itself. This differs from Curci and Masera (2025), who find that violent crime (which includes more than just homicides) leads to a central-urban reallocation of residents within a city. The difference might reflect the more potent effect of the racial riots compared with violent crime more generally, given the scale of destruction associated with the riots. Another issue of interest is the characteristics of those who left in response to the riots. Again, this can be studied with regression (2), switching the outcome to the decadal share of blacks in central city areas. Figure A.15 plots my estimates by decade, revealing a growing share of black residents in central city areas over time. This suggests that the riots-induced departures were, on average, a form of white flight. To the extent that whites have higher mean wages than blacks, this will shrink the tax base of affected neighborhoods, exacerbating urban decline.

Could there be more to the outflow from urban areas? I argue that the riot-induced departures incentivized *other* residents to also leave. This knock-on effect connects with the literature on tipping points (Card et al., 2008; Gladwell, 2002), where small changes can have major consequences. I begin with a brief overview of residential tipping. Suppose that white residents have a given preference (taste) over the racial composition of their neighborhoods, such that beyond some share of blacks (the tipping point), they would move elsewhere en mass (the tipping phenomenon). Empirically, Card et al. (2008) show that once the share of blacks in a neighborhood exceeds the tipping point, there is a sharp decline in white population growth. These nonlinearities are highly suggestive of social interaction effects driven by white preferences over neighborhood composition. A limitation of Card et al.'s (2008) analysis, however, is the focus on 1970-2000, which misses the crucial 1960s when the racial riots peaked. Here, I extend Card et al.'s (2008) work in two ways. First, I ask if residential tipping occurred between 1960-1970, the decade of the racial riots. Second, I examine whether the riots further *amplified* the tipping phenomenon.

I proceed in four steps. First, I assemble population counts at the census tract level for 1960 and 1970, drawing on Manson et al. (2023). The raw data are standardized to 1970 boundaries by adapting the boundary adjustment procedure in Hornbeck (2010). A caveat here is that tract-level data are limited to urban areas and cities as the US was not fully tracted in the 1960s. Second, assuming that a discontinuity in white population growth exists at some share of black residents, I follow Card et al. (2008) in searching for a structural break in white population growth as the share of blacks increases. This yields a candidate tipping point for each city. Third, I test whether residential tipping actually occurred at the candidate thresholds, adapting the regression discontinuity specification in Card et al. (2008):

$$D_{zc,6070} = p(\delta_{zc}) + d1[\delta_{zc} > 0] + \theta_c + v_{zc} \tag{3}$$

where the subscripts z and c denote a census tract and city, respectively. The outcome, D, is the change in white population from 1960-1970 scaled by the total population in 1960. The running variable, δ, is the deviation in the initial black share (measured in 1960) from the candidate tipping point. This is modeled as a fourth-order polynomial $p()$. The parameter of interest here is d, which captures any discontinuity in white population growth around the tipping point. Figure A.16 provides a visual representation of regression (3), plotting the relative change in white population for each tract D against the deviation in the initial share of blacks from the tipping point (δ). The graph hints at a discrete change around the tipping point, where tracts just below the tipping point experience relative increases in the white population, whereas tracts just above the tipping point see relative declines. Column 1 of Table 3 estimates this discontinuity d more formally based on regression (3). I find a discontinuity of -19.3 percentage points at the tipping point. Residential tipping was thus happening even back in the 1960s. Fourth, to elucidate the role of the racial riots, I add to regression (3) an interaction between the discontinuity indicator and a dummy for severe riots.[25] Column 2 of Table 3 show the results from this extended regression. The interaction

[25]I consider a city with a riot severity index at the 90th percentile or higher to have experienced a severe riot. Due

term is negative and statistically significant, about 15 percent of the baseline effect. In words, the racial riots of the 1960s amplified the tipping phenomenon in that decade, magnifying the overall outflow of residents. Not surprisingly, since the racial riots were concentrated in the 1960s, the amplification effect observed at the time is not seen in later years (columns 3-8 of Table 3).

Putting the pieces together, I find suggestive evidence that the riots of the 1960s accelerated urban decline by causing people to leave cities. These departures may have disrupted the existing social equilibrium, weakening a community's capacity to regulate behavior and thus leading to more interpersonal violence.

3.1.4 Mechanism II: Law Enforcement Response

I propose that another way in which the racial riots led to more homicides is through the response of law enforcement. The idea here is reflected in the old-adage: violence begets violence. More precisely, I posit that the riots attracted a greater intensification of law enforcement, which rather than calming the storm, further escalated and reinforced the cycle of violence.

In 1965, against a backdrop of rising crime and urban unrest, US President Lyndon B. Johnson declared a national "War on Crime" with the signing of the Law Enforcement Assistance Act (LEAA). The LEAA supplied local police with Federal funding, typically in the form of small grants. This was dramatically expanded by the Omnibus Crime Control and Safe Streets Act in 1968. Together, these initiatives formed part of the broader expansion in policing at the time.

Was the law enforcement response particularly pronounced in places that were hit by the racial riots? To shed light on this, I return to regression (1), replacing the outcome with two measures of enforcement: homicide clearance rates from the UCR OKCA dataset and the number of police officers per 100,000 residents tabulated from the UCR Law Enforcement Officers Killed and Assaulted (LEOKA) database (Kaplan, 2025b). As with the previous analysis with UCR data, the two outcomes here are only available from 1960 onward, limiting the scope for evaluating pretrends.

Figures 9 and 10 trace the law enforcement response in riot-hit CZs versus other CZs over time. The point estimates are somewhat noisy, but they paint a similar story for both measures of law enforcement. In particular, a hump-shaped profile is observed in each case—riot-affected CZs experience larger increases in homicide clearance rates and police officers per capita, beginning in the late 1960s and extending into the early-to-mid 1970s before fading. These dynamics are not driven by the year-to-year changes in the coverage of the UCR OKCA and LEOKA datasets. Similar results can be obtained when restricting the underlying UCR data to only agencies that report crime statistics for every year between 1960-1988 (Figures A.17 and A.18). That the two independently-constructed measures of law enforcement converge in findings only reinforces the case that riot-affected places saw a temporary intensification of law enforcement.

To close the link between the riots, law enforcement, and homicides, I now discuss how the response of law enforcement could itself have contributed to an escalation of violence. Historical

to the relatively small number of cities, using the 95th percentile would leave little variation for regression estimation.

scholarship emphasizes how the expansion of policing from the 1960s was accompanied by heightened surveillance, coercive encounters, and recurring conflicts between residents and law enforcement officers. Hinton (2011), for example, documents how intensified policing and police violence beginning in the 1960s contributed to further black rebellion. Perez et al. (2003) argue that excessive force during urban uprisings often fails to suppress disorder, worsening the conflict instead. In addition, Masera (2021) finds that aggressive policing can raise violence on both sides of the civilian-police interaction, with more killings of police and more killings by police. More generally, aggressive policing may weaken cooperation between residents and law enforcement. Desmond et al. (2016) observe a sharp decline in 911 calls after the highly-publicized police beating of Frank Jude, while Ang et al. (2025) conclude that civilian reporting of gunshots declined following the murder of George Floyd. These findings suggest a potential escalation mechanism: the riots prompted an intensified law-enforcement response, but the resulting increase in coercive community-police interactions might also have weakened trust and informal cooperation, destabilizing the existing social equilibrium governing interpersonal violence. If this hypothesis is true, one should expect other forms of violent crime besides homicides to also witness a surge around the 1960s in riot-hit places. Figures A.19 and A.20 explore the impact of the riots on aggravated assault and reported rape, based on the UCR OKCA data. Interestingly, both display a rise from the mid-1960s that is particularly pronounced in areas experiencing the riots. In contrast, burglary and theft were already on differential upward trajectories even before the major waves of riots (Figures A.21 and A.22). Together, the evidence presented here is consistent with a story of riots and the response they elicited contributing not simply to a general increase in criminal activity, but to an escalation in interpersonal violence.

3.2 Countercultural Transformation

Beyond the racial riots of the 1960s, could other social upheavals at the time have also contributed to the Great American Homicide Rise? This section turns to the broader countercultural transformation of American society, which was sweeping across the nation in the 1960s. Encyclopedia Britannica describes the counterculture of the 1960s as:

> ... a broad-ranging social movement... that rejected conventional mores and traditional authorities... an alternative approach to life that manifested itself in a variety of activities, lifestyles, and artistic expressions, including recreational drug use, communal living, political protests, causal sex, and folk and rock music.

3.2.1 Sexual Revolution

A key feature of the 1960s counterculture was the liberalization of sexual norms, also referred to as the Sexual Revolution. While the seeds of this movement may have been sown decades earlier, the 1960s marked a turning point. Among other events of that decade, the Food and Drug Administration (FDA) approved *Enovid* for use as an oral contraceptive in 1960, Betty Friedan published

The Feminine Mystique in 1963, contraception laws were liberalized after *Griswold v. Connecticut* in 1965, and the National Organization for Women (NOW) was formed in 1966. Against this backdrop, dramatic changes in people's sexual behavior began to take shape. Figure A.23 documents a surge in out-of-wedlock births and a spike in gonorrhea infections, both emerging in the mid-1960s. Like the racial riots, such drastic changes in societal norms could disrupt the existing social equilibrium, weakening a community's capacity to regulate behavior.

To explore the link between the Sexual Revolution and the Great American Homicide Rise, if any, I need to measure the shift in sexual norms at the local level. I do this in two ways. First, for each CZ, I compute the change in the share of 18-to-29-year-old women who have never been married but who have children, drawing on the 1960 and 1980 censuses. This gives the growth rate in unwed motherhood from 1960-1980, capturing the local prevalence of sex outside or before marriage, a lifestyle that gained increasing acceptance over time (Figure A.24). Second, as a complementary measure, I also digitize gonorrhea incidence rates between 1961-1976 from various issues of the *VD Statistical Letters*.[26] These were published by the US Department of Health, Education, and Welfare, and report gonorrhea statistics at the city level. I map this city information to the corresponding county, which yields a final set of 181 counties.[27] For each of these counties, I measure the change in mean gonorrhea incidence rates between the intervals 1961-1965 and 1972-1976. This serves as a proxy for the rate at which sexual norms were becoming more liberal in a given area.

Figures 6C and D map where the transformation in sexual norms was greatest (and least), based on changes in either the share of unwed mothers or the incidence of gonorrhea infections respectively. The vast majority of localities in each sample saw a liberalization in sexual activities, whether in terms of an increase in unwed motherhood or a rise in gonorrhea infections. In the case of unwed motherhood where the sample size is much larger, a distinct geographic pattern can also be observed—larger increases in unwed motherhood are found in large swathes of the Sunbelt and in major urban areas, mirroring the distribution of CZs from the high-exposure cluster (Figure 5). This hints at a potential link between the Sexual Revolution and the Great American Homicide Rise.

To examine the relation between the Sexual Revolution and homicides more formally, I return to regression (1). When using unwed motherhood to capture the Sexual Revolution, I simply replace the variable *Riots* with an indicator for CZs experiencing above-median increases in the share of unwed mothers. With gonorrhea infections as a proxy for the Sexual Revolution, I (i) implement regression (1) at the county level, (ii) replace *Riots* with a dummy for counties that had above-median increases in gonorrhea infections, and (iii) switch to census-division-by-year fixed effects in light of the small set of counties. Figure 11 displays my event study results. Reassuringly,

[26] The *VD Statistical Letters* were published from the 1950s to 1970s, with several editions reporting gonorrhea infection rates at the (large) city level.

[27] As the sample of observations is much smaller here, I focus on counties instead of CZs to allow for greater variation. I choose the county rather than the city to match the geographic unit available in the NCHS data, from which my homicide statistics are derived.

no systematic trends are observed before 1960. Thereafter, from the mid-1960s, I find a strong positive association between the liberalization of sexual norms (by either metric) and homicide rates. This effect persists for several decades thereafter. For the unwed motherhood metric, there is ample sample size to further include year interactions with the share of blacks, hispanics, and central city dwellers in 1960—the impact of the Sexual Revolution remains (Figures A.25A and B). Similar conclusions can be reached when measuring the change in unwed motherhood from 1960-1970 (instead of 1960-1980), albeit with more noise since the Sexual Revolution had not reached its peak in this earlier interval (Figures A.25C and D).

Why might large and rapid changes in sexual norms affect the prevalence of homicides in an area? Reviewing the literature on social structures and homicides, Pridemore (2002) concludes that family disruption is one of the consistent correlates of homicide. Similarly, Schwartz (2006) finds a strong association between family disruption—including female-headed households and non-marital or teen childbearing—and both male and female homicide. At the macro level, Errol et al. (2021) argue that weakened family and community structures help explain the cross-national variation in crime over two centuries. One possibility, therefore, is that the dramatic liberalization of sexual norms in the 1960s altered family formation patterns and diminished existing forms of informal social control. In this sense, changing sexual behavior may have formed part of the broader disruption to the prevailing social equilibrium, creating an environment more conducive to interpersonal violence.

3.2.2 Recreational Drug Use

Besides the Sexual Revolution, another defining feature of the countercultural transformation was the spread of recreational drug use. These included marijuana, cannabis, LSD, and heroin, among others. Just like the Sexual Revolution, Figure A.26 shows a dramatic acceleration in drug-related arrests around the mid-1960s, based on UCR data. Measuring the increase in recreational drug use at the local level, however, poses a challenge as no such metrics are readily available. To make progress, I return to the NCHS mortality data, this time focusing on deaths due to substance poisoning as a proxy for recreational drug use. In particular, I tabulate the change in such deaths per 100,000 persons for each CZ between the periods 1959-1963 and 1984-1988. Figure 6E maps the rise in substance poisoning deaths across the country. As with the transformation in sexual norms, a majority of CZs experienced an increase in such deaths, but the shift is more pronounced in large parts of the Sunbelt, overlapping with CZs in the high-exposure cluster (Figure 5).

To investigate whether the liberalization of recreational drug use is linked to the Great American Homicide Rise, I again turn to regression (1), replacing the variable *Riots* with a dummy for CZs that saw above-median increases in substance poisoning deaths. Figure 12 presents the corresponding event study results. The point estimates are somewhat noisier, possibly reflecting my imperfect proxy for recreational drug use. Nonetheless, I find suggestive evidence of an increasingly positive relation from the 1960s. That is, places experiencing a greater liberalization of drug use also see a greater rise in homicides. This association remains even when adding year interactions

with the share of blacks, hispanics, and city dwellers (Figure A.27). Importantly, the observed impact of recreational drug use is not simply a contemporaneous correlate of the Sexual Revolution. Put differently, the liberalization of recreational drugs and sexual norms are not capturing the exact same underlying phenomenon. Figure A.28 shows the event study coefficients when the impact of recreational drug use and the Sexual Revolution are estimated simultaneously in a single regression—both still lead to a surge in homicides from the 1960s, independently of each other.

The association between recreational drug use and homicides deserves greater scrutiny. I argue that the primary mechanism through which recreational drug use affected violent crime was not by directly contributing to the homicide count. Drawing on the UCR SHR data, I find that homicides involving narcotics—whether pertaining to narcotics drug laws or brawls due to the influence of narcotics—comprised just 3 percent of all homicides with known circumstances between 1976-1988. This indicates that homicides directly arising from drug use are unlikely to be the main mechanism. Rather, I posit that the rapid diffusion of recreational drug use may have weakened informal mechanisms of social control by normalizing behavior that stood outside established legal and community norms.[28] In doing so, it contributed to a broader erosion of conventional authority. Viewed this way, the proliferation of recreational drugs can be seen as part of the broader social transformation of which the Sexual Revolution was also a part of. If these changes weaken the institutions and norms that had previously constrained interpersonal conflict, places experiencing larger increases in drug use may then be more susceptible to violent crimes as well.

3.3 Alternative Mechanisms

This paper proposed that large rapid social upheavals in the 1960s—the racial riots and countercultural transformation—disrupted the existing social equilibrium, causing a surge in violent crime that persisted for several decades. What about other prevailing theories on the Great American Homicide Rise? Just as it allowed me to identify a role for riots and counterculture, the geographic heterogeneity in homicide trajectories also offers a way to evaluate alternative theories by checking if they can explain the spatial differences in homicide time paths. For each hypothesis, I run regression (1) but replace *Riots* with the relevant variable of interest.

The first explanation is the coming-of-age of the Baby Boom generation. By the 1960s and 1970s, the first cohorts of the Baby Boomers would have entered their teens and early adulthood. This mechanically increases the share of persons who belong to age groups that are more susceptible to violent crime (Figure A.29). Sagi and Wellford (1968) and Wellford (1973) find that the growth in young persons durings the 1950s and 1960s can explain some of the crime rise at the time. Examining violent crime, Cohen and Land (1987) observe that homicide movements closely track changes in the proportion of 15-24-year-olds. Beyond a demographic argument, O'Brien (1989)

[28] Goldstein (1985) propose a tripartite framework linking drug use to crime. The first is psychopharmacological violence, where drug intoxication itself leads to aggression. The second is economic-compulsive violence, where drug users commit crimes to finance their habits. The third is systemic violence arising from illicit drug markets. This framework may be more applicable to the crack cocaine epidemic of the 1980s. The primary recreational drugs of the 1960s, marijuana and LSD, are unlikely to fit this framework.

contend that a larger cohort could also worsen labor market opportunities, thus raising the propensity for crime. To measure the extent to which a CZ is exposed to the Baby Boom shock, I tabulate the ratio of persons born between 1946-1948 (early Baby Boom cohorts) to those born in 1938-1945 (pre-Boom cohorts) at the CZ level. As an alternative, I calculate the share of 10-19-year-olds for each CZ in 1960 (who would be 15-24 by 1965). Incoporating these measures into my event study, Figure A.30 shows no relation between the size of the Baby Boom shock and homicide rates in the 1960s.

A second prominent explanation emphasizes the deterioration of economic opportunities in American cities. In the aftermath of World War II (WWII), manufacturing employment increasingly disappeared from central cities (Alder et al., 2023), reducing access to stable blue-collar employment for less-skilled workers. Sampson and Wilson (1995) and Wilson (1987, 1996) argue that this process of deindustrialization increased joblessness and concentrated disadvantage, weakening family and neighborhood social organization, thereby contributing to urban violence. Consistent with this account, subsequent city-level studies document an association between the disappearance of manufacturing or low-skill employment and the increase in homicides or other violent crime (Shihadeh and Ousey, 1998; White, 1999; Matthews et al., 2001; Parker, 2004). To approximate the degree to which each CZ is exposed to such deteriorating circumstances, I use the share of workers in a CZ who were in manufacturing as of 1960, before the surge in homicides. These areas subsequently experienced greater declines in manufacturing employment (Figure A.31). Figure A.32 reveals no relation between local manufacturing shares in 1960 (greater loss of economic opportunities subsequently) and homicide rates.

The third explanation involves the Vietnam War. In terms of timing, this event has an intuitive appeal: 1965 was when the Vietnam War escalated dramatically for the US, shifting from an advisory effort to a full-scale land and air war; it was also when homicides began to surge. Although this war was fought abroad, it could still affect homicides back home in different ways. One channel is the legitimization of violence, where state-sanctioned violence can weaken social norms against interpersonal violence, contributing to higher rates of violent crime (Archer and Gartner, 1984; Gartner and Kennedy, 2018). Empirically, several studies observe that participation in some foreign wars is indeed associated with increases in domestic homicides (Bunte et al., 2019; Stamatel and Romans, 2018). Another channel through which the Vietnam War might affect homicides at home is through returning veterans, many of whom would have been directly exposed to the horrors of war. Evidence for this mechanism, however, is mixed (Boehmer et al., 2004; Bouffard, 2003; Boyle et al., 1987). To measure an area's exposure to the Vietnam War, I consider the share of prime-age men in each CZ who were veterans of this conflict. A limitation here is that Vietnam War participation is only recorded in the 1970 census or later, after the spike in violent crime had began. I thus compute my measure from the 1970 census, the closest enumeration to 1960 that has the relevant information. Figure A.33 indicates that exposure to the Vietnam War cannot account for the geographic heterogeneity in homicide trajectories.

4 Conclusion

Homicide rates in America surged in the 1960s and remained elevated for several decades thereafter. In this paper, I showed that the national trend in homicides masks substantial heterogeneity across local labor markets. Employing a data-driven clustering approach, I found two distinct trajectory clusters, one of which experienced a large rise in homicides during the 1960s while the other only saw a modest increase. Exploiting this geographic heterogeneity, I then examined the causes of the Great American Homicide Rise. I argued that the social upheavals of the 1960s, particularly the racial riots and countercultural transformation of society, contributed to the growth and persistence in homicides by disrupting the existing social equilibrium, weakening a community's capacity to regulate behavior and creating more fertile conditions for violent crime.

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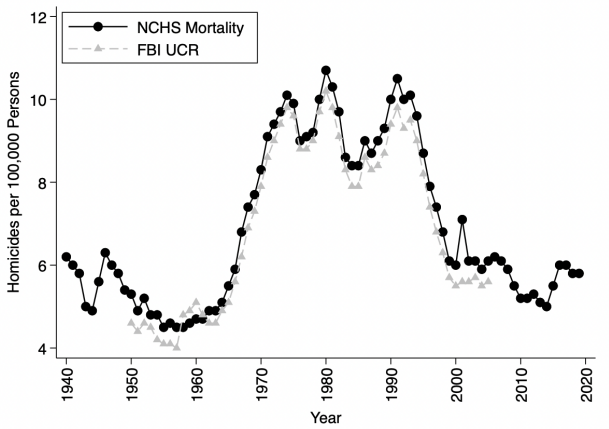
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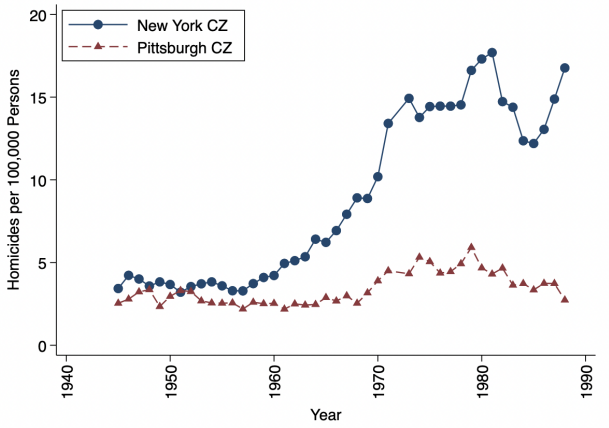
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Figure 1: National Homicide Trend



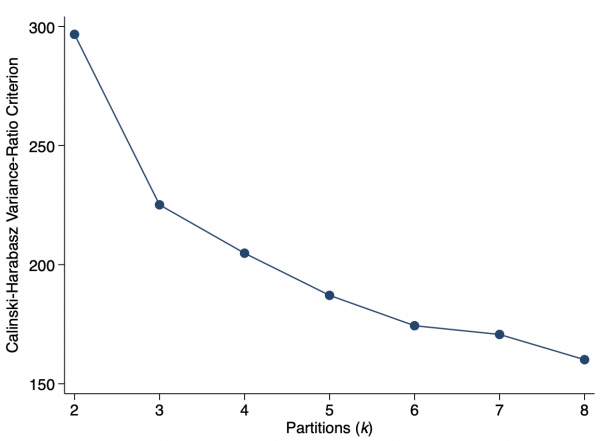
Notes - Data are from the NCHS Birth and Death Tables and the FBI UCR. The figure shows the number of homicides per 100,000 persons each year, based on the NCHS data (solid line with circle markers) or the UCR data (dashed line with triangle markers).

Figure 2: Motivating Examples: New York vs. Pittsburgh



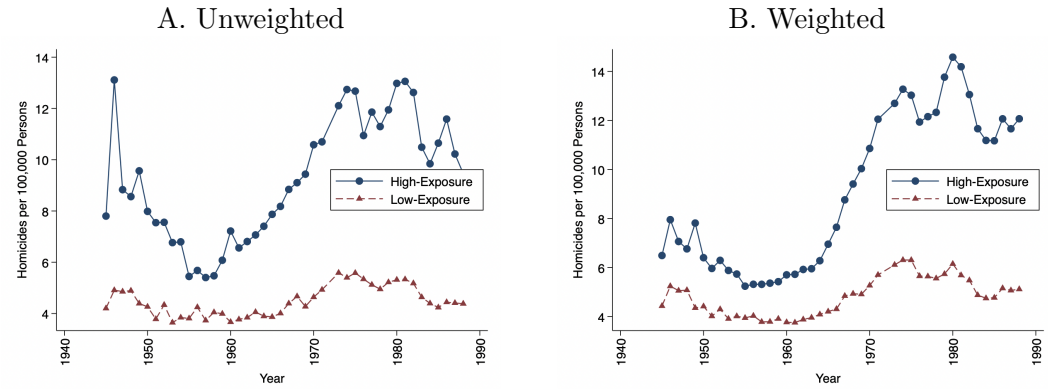
Notes - Data are from the published *Vital Statistics of the United States* reports (for 1945-1958), the NCHS Multiple Cause of Death files (for 1959-1988), and Manson et al. (2023). The figure shows the homicide trajectories for New York CZ (solid line with circle markers) and Pittsburgh CZ (dashed line with triangle markers).

Figure 3: Caliński-Harabasz Statistic



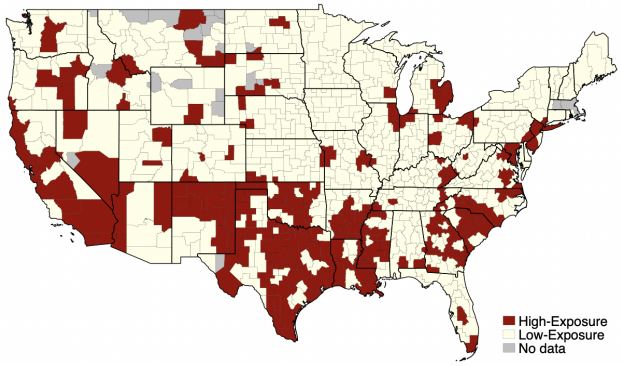
Notes - The figure shows the Caliński-Harabasz (CH) variance-ratio criterion (Caliński and Harabasz, 1974) derived for various partitions of a sample of 700 CZs, based on the associated homicide trajectory between 1960-1988. The number of partitions range from $k = 2$ to 8 (x-axis). A higher value reflects a partition characterized by greater separation between clusters and greater cohesion within clusters. The CH statistic thus attains its maximum at $k = 2$ here.

Figure 4: Homicide Trajectory Under a Two-Cluster Regime



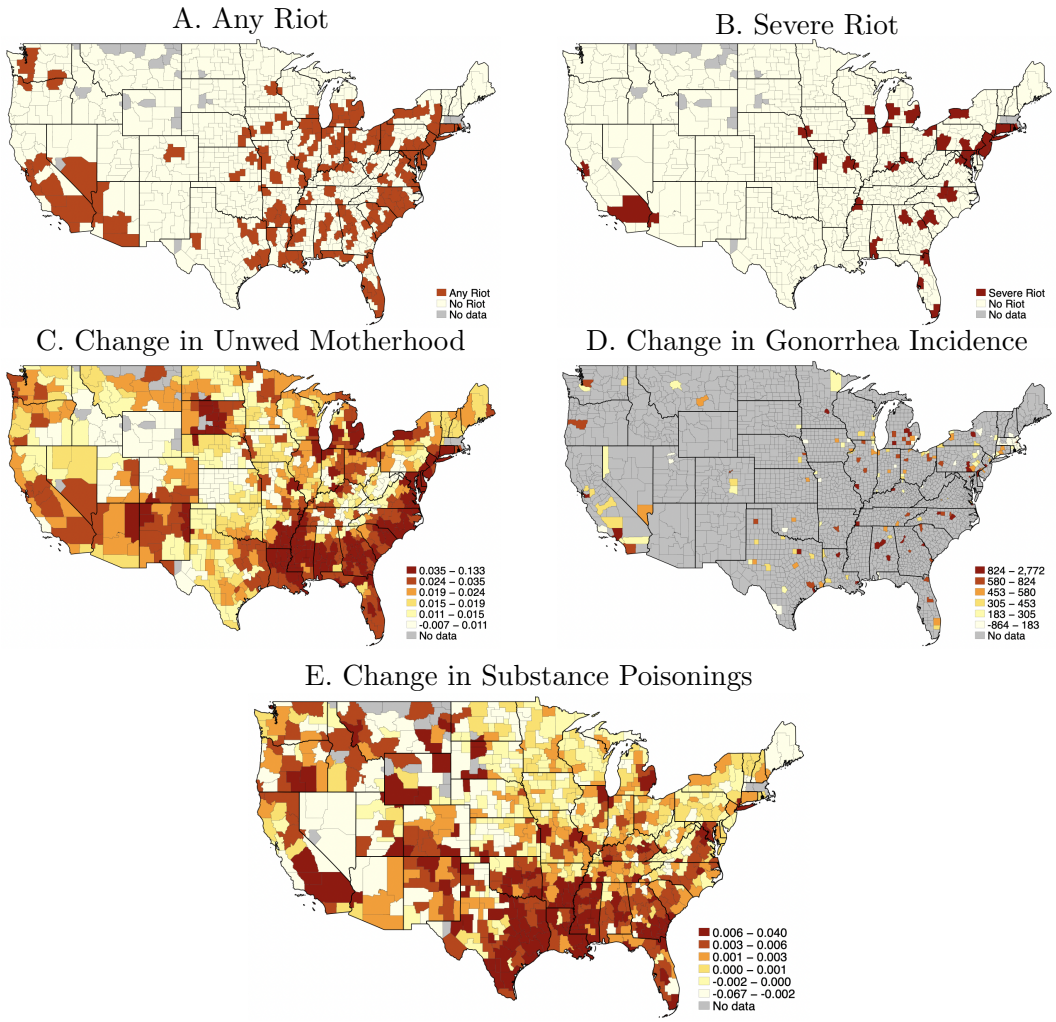
Notes - Data are from the published *Vital Statistics of the United States* reports (for 1945-1958), the NCHS Multiple Cause of Death files (for 1959-1988), and Manson et al. (2023). Each figure shows the mean homicide trajectory for CZs in one of two groups determined using a k-means clustering approach. The cluster experiencing a surge in homicides from the 1960s is represented by a solid line with circle markers (high-exposure). The cluster experiencing a modest increase in homicides over the same period is represented by a dashed line with triangle markers (low-exposure). The trajectories are unweighted in A and weighted by the 1960 CZ population in B.

Figure 5: Geography of High- and Low-Exposure CZ Clusters



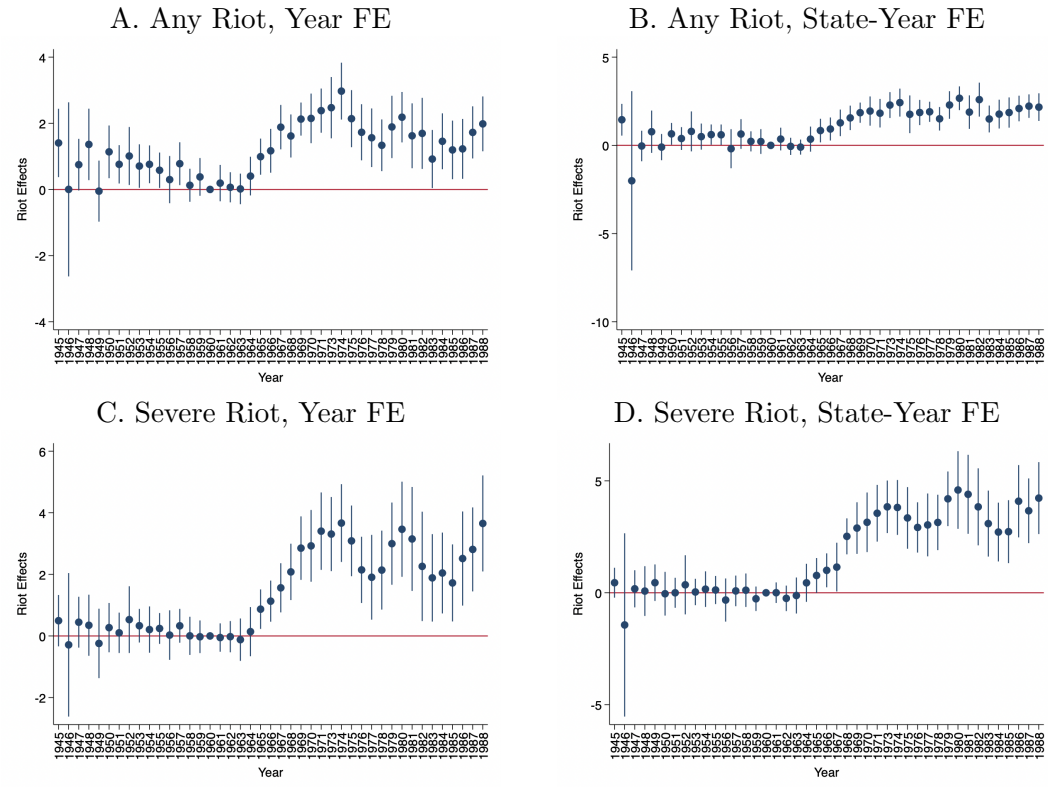
Notes - The figure shows the location of high- (red) and low-exposure (yellow) CZ clusters. High-exposure CZs experienced a surge in homicides from the 1960s, while low-exposure CZs saw a modest increase in homicides over the same period. The sample size is 700 CZs.

Figure 6: Geography of Social Upheavals in the 1960s



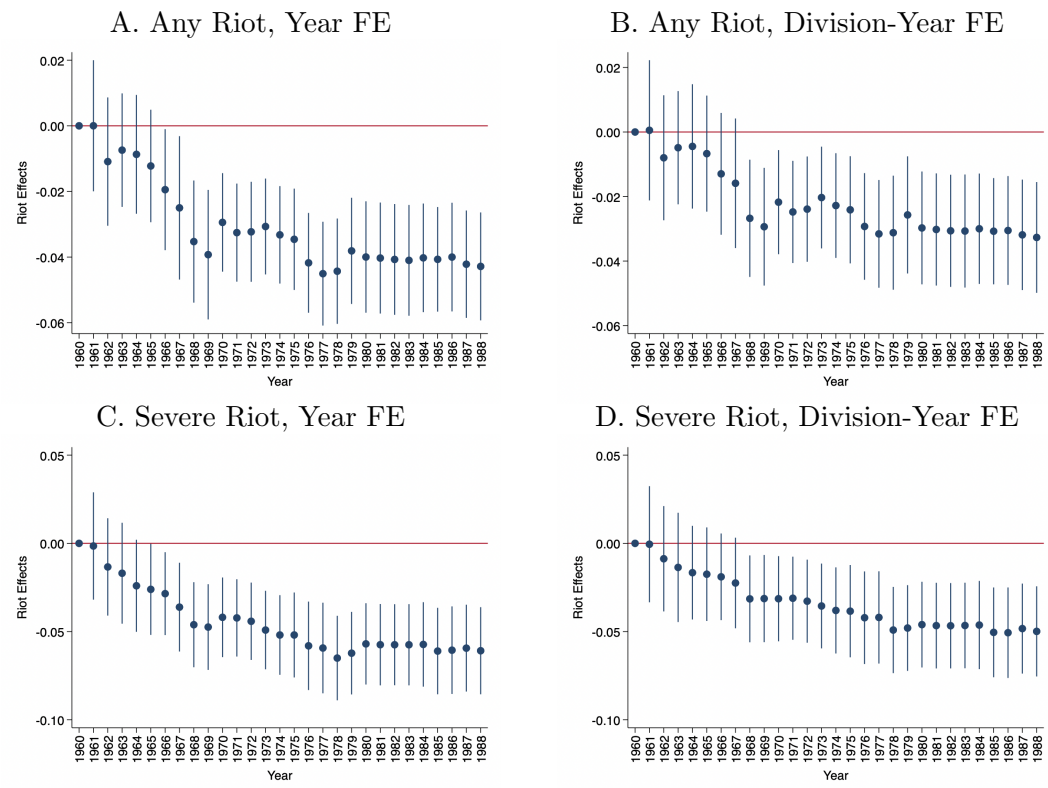
Notes - Data are from Carter (1986), Ruggles et al. (2025), the NCHS Multiple Cause of Death files, and the *VD Statistical Letters*. Each figure maps the intensity of a given social event or change during the 1960s: whether there was at least one riot between 1964-1971 (A), whether there was at least one severe (95th percentile) riot between 1964-1971 (B), the change in the share of 18-29-year-old women who were unwed mothers from 1960 to 1980 (C), the change in gonorrhea incidence rates from 1961-1865 to 1972-1976 (D), and the change in the number of deaths due to substance poisoning per 100,000 persons from 1959-1963 to 1984-1988. Darker shades indicate larger changes. The sample sizes are 700 CZs (A, C, D, and E) and 177 counties (B).

Figure 7: The Impact of Racial Riots on Homicides



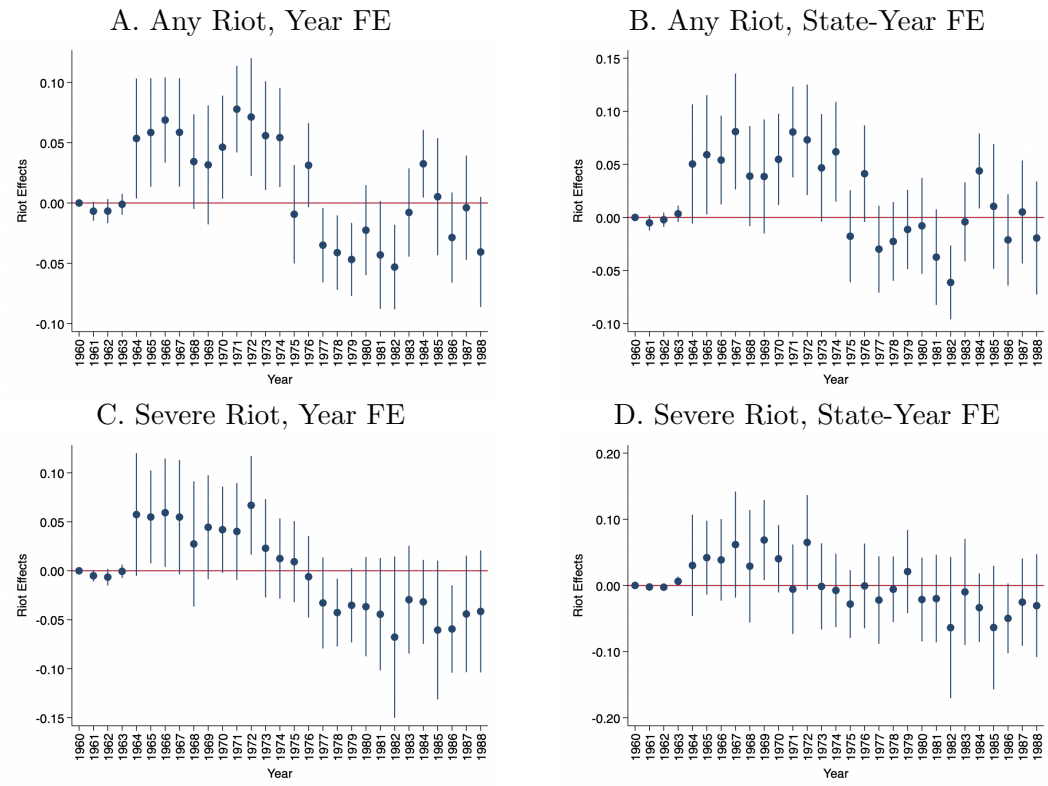
Notes - Data are from Carter (1986), the published *Vital Statistics of the United States* reports (for 1945-1958), the NCHS Multiple Cause of Death files (for 1959-1988), and Manson et al. (2023). The unit of observation is the CZ-year. Each figure shows the interaction coefficients from a regression of the number of homicides per 100,000 persons on an indicator for CZs that experienced a riot (A and B) or severe riot (C and D) between 1964-1971 interacted with year dummies, controlling for CZ and year (A and C) or state-by-year (B and D) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 30,100 CZ-year observations.

Figure 8: The Impact of Racial Riots on Central City Area Populations



Notes - Data are from Carter (1986) and Curci and Masera (2025). The unit of observation is the city-year. Each figure shows the interaction coefficients from a regression of the share of the city population residing in the central city area on an indicator for cities that experienced a riot (A and B) or severe riot (C and D) interacted with year dummies, controlling for city and year (A and C) or division-by-year (B and D) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the division level are displayed. The sample size is 9,344 city-year observations.

Figure 9: The Impact of Racial Riots on Homicide Clearance Rates



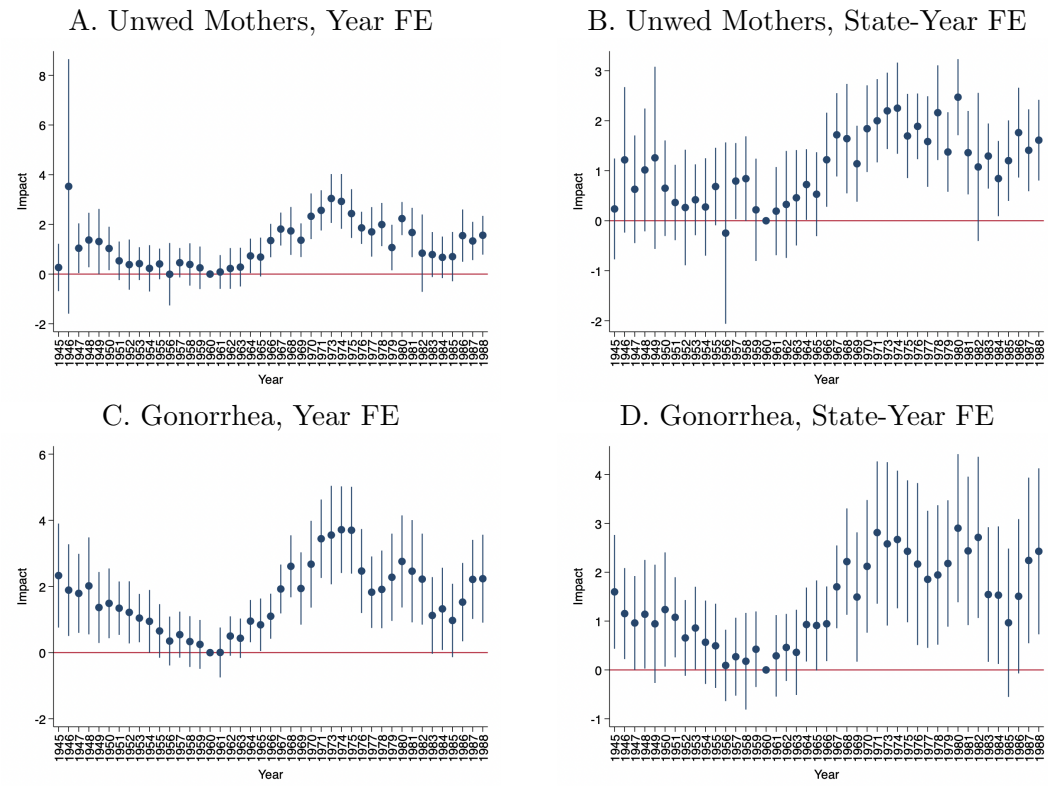
Notes - Data are from Carter (1986), Manson et al. (2023), and the UCR OKCA (Kaplan, 2025d). The unit of observation is the CZ-year. Each figure shows the interaction coefficients from a regression of the share of homicides that have been cleared on an indicator for CZs that experienced a riot (A and B) or severe riot (C and D) between 1964-1971 interacted with year dummies, controlling for CZ and year (A and C) or state-by-year (B and D) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 15,091 CZ-year observations.

Figure 10: The Impact of Racial Riots on Police Strength



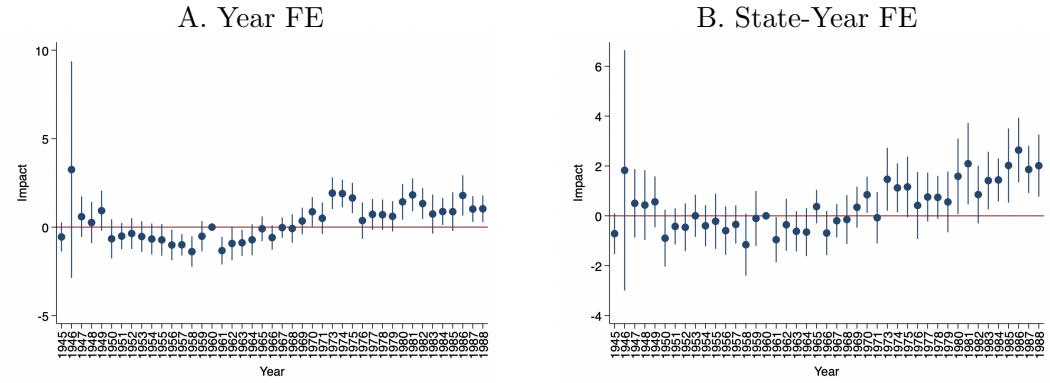
Notes - Data are from Carter (1986), Manson et al. (2023), and the UCR LEOKA (Kaplan, 2025b). The unit of observation is the CZ-year. Each figure shows the interaction coefficients from a regression of the number of police officers per 100,000 persons on an indicator for CZs that experienced a riot (A and B) or severe riot (C and D) between 1964-1971 interacted with year dummies, controlling for CZ and year (A and C) or state-by-year (B and D) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 20,300 CZ-year observations.

Figure 11: The Impact of the Sexual Revolution on Homicides



Notes - Data are from Ruggles et al. (2025), the *VD Statistical Letters*, the published *Vital Statistics of the United States* reports (for 1945-1958), the NCHS Multiple Cause of Death files (for 1959-1988), and Manson et al. (2023). The unit of observation is the CZ-year (A and B) or county-year (C and D). Each figure shows the interaction coefficients from a regression of the number of homicides per 100,000 persons on an indicator for CZs that experienced above-median increases in the share of 18-to-29-year-old unwed mothers from 1960-1980 (A and B) or an indicator for counties that had above-median increases in gonorrhea incidence rates from 1961-1965 to 1972-1976 (C and D) interacted with year dummies, controlling for CZ (A and B) or county (C and D) and year (A and C) or state-by-year (B) or division-by-year (D) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state (A and B) or region (C and C) level are displayed. The sample size is 30,100 CZ-year and 7,461 county-year observations.

Figure 12: The Impact of Recreational Drug Use on Homicides



Notes - Data are from the published *Vital Statistics of the United States* reports (for 1945-1958), the NCHS Multiple Cause of Death files (for 1959-1988), and Manson et al. (2023). The unit of observation is the CZ-year. Each figure shows the interaction coefficients from a regression of the number of homicides per 100,000 persons on an indicator for CZs that experienced above-median increases in the number of deaths due to substance poisoning per 100,000 persons interacted with year dummies, controlling for CZ and year (A) or state-by-year (B) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 30,100 CZ-year observations.

Table 1: Characteristics of High- and Low-Exposure CZ Clusters

	High-Exposure Cluster	Low-Exposure Cluster	Difference
	(1)	(2)	(3)
Homicide rate, 1955-1959	5.62	3.97	1.65***
			(0.325)
Share aged 10-19	0.187	0.180	0.007***
			(0.001)
Share black	0.157	0.060	0.097***
			(0.011)
Share Hispanic	0.061	0.021	0.040***
			(0.008)
Share foreign-born	0.026	0.026	-0.001
			(0.002)
Share born outside state of residence	0.280	0.264	0.016
			(0.012)
Share of home owners	0.605	0.655	-0.050***
			(0.006)
Share in central city	0.228	0.120	0.109***
			(0.024)
Share employed, males	0.785	0.773	0.012***
			(0.005)
Share married, males	0.834	0.827	0.007***
			(0.002)
Share in blue collar jobs, males	0.369	0.409	-0.040***
			(0.008)
Share with children, males	0.567	0.552	0.015***
			(0.004)
Share with college degree, males	0.074	0.069	0.005***
			(0.002)
Share in labor force, females	0.779	0.777	0.001
			(0.006)
Share married, females	0.742	0.751	-0.009***
			(0.003)
Share with children, females	0.582	0.561	0.020***
			(0.004)
Share with college degree, females	0.053	0.048	0.005***
			(0.001)
Log annual income	-8.52	-8.51	-0.007
			(0.015)
Northeast census region	0.017	0.070	-0.053***
			(0.019)
Midwest census region	0.112	0.449	-0.338***
			(0.038)
South census region	0.662	0.315	0.346***
			(0.039)
West census region	0.210	0.165	0.045
			(0.032)
N	196	504	700

Notes - Data are from the published *Vital Statistics of the United States* reports (for 1955-1958), the NCHS Multiple Cause of Death files (for 1959), Manson et al. (2023), and Ruggles et al. (2025). Columns 1 and 2 measure the average characteristics of CZs in the high- and low-exposure clusters respectively, along the dimensions listed in the leftmost column. High-exposure CZs experienced a surge in homicides from the 1960s, while low-exposure CZs saw a modest increase in homicides over the same period. Column 3 shows the difference between CZs in the high- and low-exposure clusters. Standard errors are in parentheses. *** 1 percent, ** 5 percent, and * 10 percent significance levels.

Table 2: Robustness of the Baseline Two-Cluster Structure

		Raw Agreement				
	Clusters	Overall	High-Exposure	Low-Exposure	Correlation	ARI
	(1)	(2)	(3)	(4)	(5)	(6)
Normalization period						
1950-1954	2	0.810	0.638	0.877	0.522	0.357
1959-1963	2	0.856	0.709	0.913	0.635	0.483
Temporal aggregation						
3-year bin	2	0.983	0.974	0.986	0.958	0.930
4-year bin	2	0.976	0.954	0.984	0.940	0.901
Clustering method						
Bonhomme and Manresa's (2015) procedure	2	0.773	0.750	0.782	0.495	0.287
Ward's hierarchical clustering	2	0.934	0.980	0.917	0.853	0.749
Gaussian mixture clustering	2	0.883	0.582	1.00	0.707	0.546
Homicide data						
UCR OKCA	3	0.730	0.587	0.786	0.361	0.230

Notes - This table assesses the robustness of the baseline two-cluster structure for homicide trajectories across CZs. Each row corresponds to a particular robustness check. Column 1 indicates the number of clusters selected under a given specification. The subsequent columns each present a given measure of how similar the alternative partition is to the baseline partition. Column 2 provides the raw agreement. Column 3 gives the retention rate for CZs in the baseline high-exposure cluster, while column 4 does the same for CZs in the baseline low-exposure cluster. Column 5 displays the correlation across regimes. Column 6 shows the ARI.

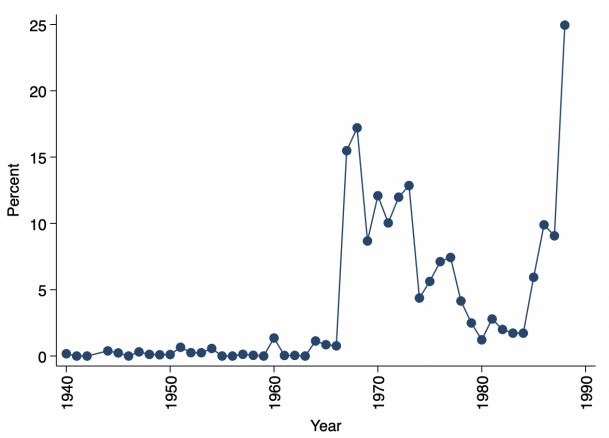
Table 3: Riots and Residential Tipping

	1960-1970		1970-1980		1980-1990		1990-2000	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Beyond tipping point	-0.193***	-0.191***	-0.156***	-0.156***	-0.130***	-0.135***	-0.117***	-0.120***
	(0.023)	(0.024)	(0.042)	(0.043)	(0.044)	(0.045)	(0.021)	(0.021)
Beyond tipping point x Severe riot		-0.031**		-0.001		0.075**		0.108**
		(0.014)		(0.047)		(0.038)		(0.051)
N	17,262	17,262	10,763	10,763	11,722	11,722	11,989	11,989

Notes - Data are from Card et al. (2008) and Manson et al. (2023). The unit of observation is a census tract. Odd columns show the coefficient from a regression of the relative decadal change in white population on an indicator for black population shares above the tipping point, controlling for a quadratic in the deviation in initial black share from the tipping point. Even columns come from a similar regression, but with an additional interaction between the discontinuity indicator (beyond tipping point) and a dummy for cities that experienced a severe racial riot between 1964-1971 (90th percentile or higher). Standard errors are in parentheses. *** 1 percent, ** 5 percent, and * 10 percent significance levels.

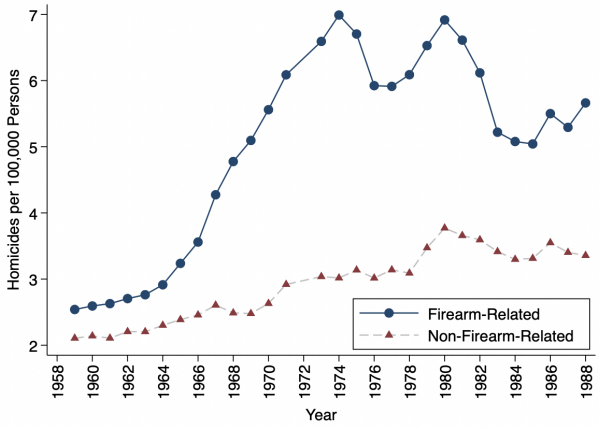
For Online Publication

Figure A.1: Importance of Crime



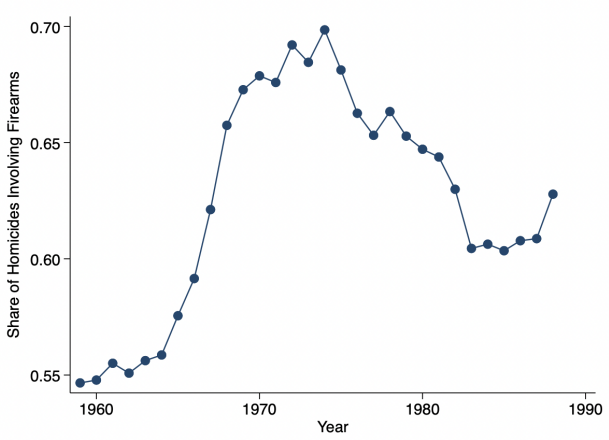
Notes - Data are from the *Most Important Problem Dataset*. The figure shows the percent of respondents who indicate that crime is the most important problem facing the US.

Figure A.2: Homicide Trends by Method of Homicide



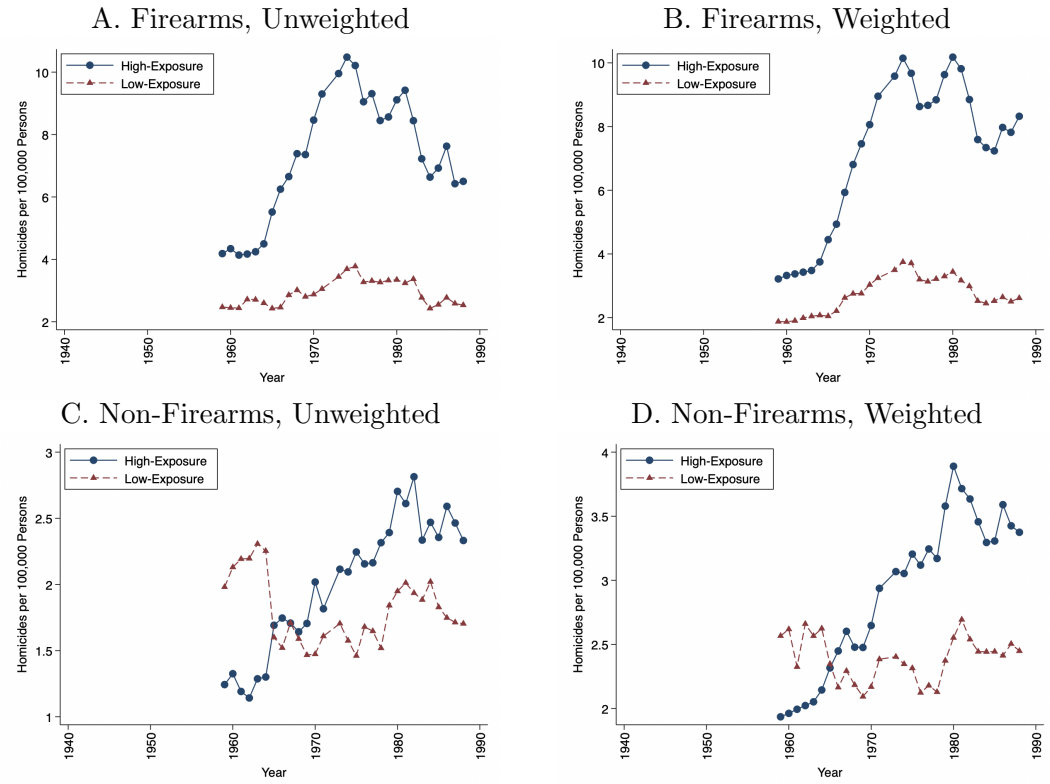
Notes - Data are from the NCHS Multiple Cause of Death files (for 1959-1988) and Manson et al. (2023). The figure shows the number of homicides per 100,000 persons each year, separately for firearm-related homicides (solid line with circle markers) and non-firearm-related homicides (dashed line with triangle markers).

Figure A.3: Share of Firearm-Related Homicides



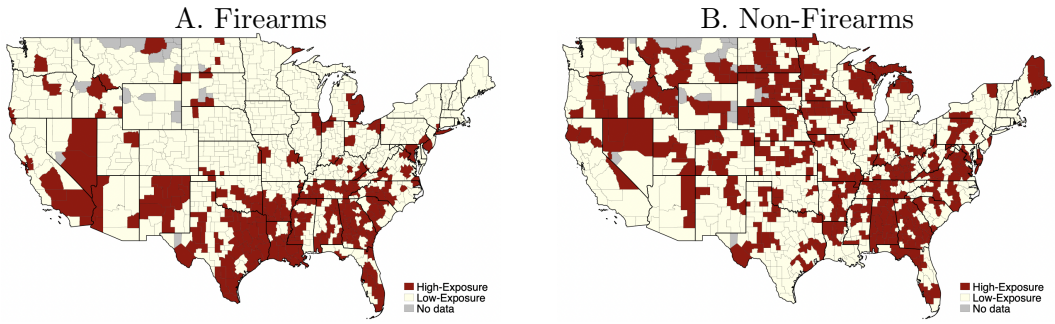
Notes - Data are from the NCHS Multiple Cause of Death files (for 1959-1988). The figure shows the share of all homicides that are related to firearms each year.

Figure A.4: Homicide Trajectory Under a Two-Cluster Regime, by Method of Homicide



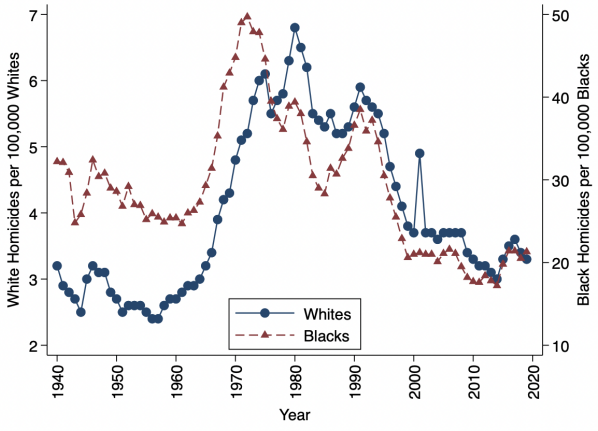
Notes - Data are from the NCHS Multiple Cause of Death files (for 1959-1988) and Manson et al. (2023). Each figure shows the mean homicide trajectory for CZs in one of two groups determined using a k-means clustering approach, separately for firearm-related homicides (A and B) and non-firearm-related homicides (C and D). For each method of homicide, the cluster experiencing a surge in homicides from the 1960s is represented by a solid line with circle markers (high-exposure). The cluster experiencing a modest increase in homicides over the same period is represented by a dashed line with triangle markers (low-exposure). The trajectories are unweighted in A and C and weighted by the 1960 CZ population in B and D.

Figure A.5: Geography of High- and Low-Exposure CZ Clusters, by Method of Homicide



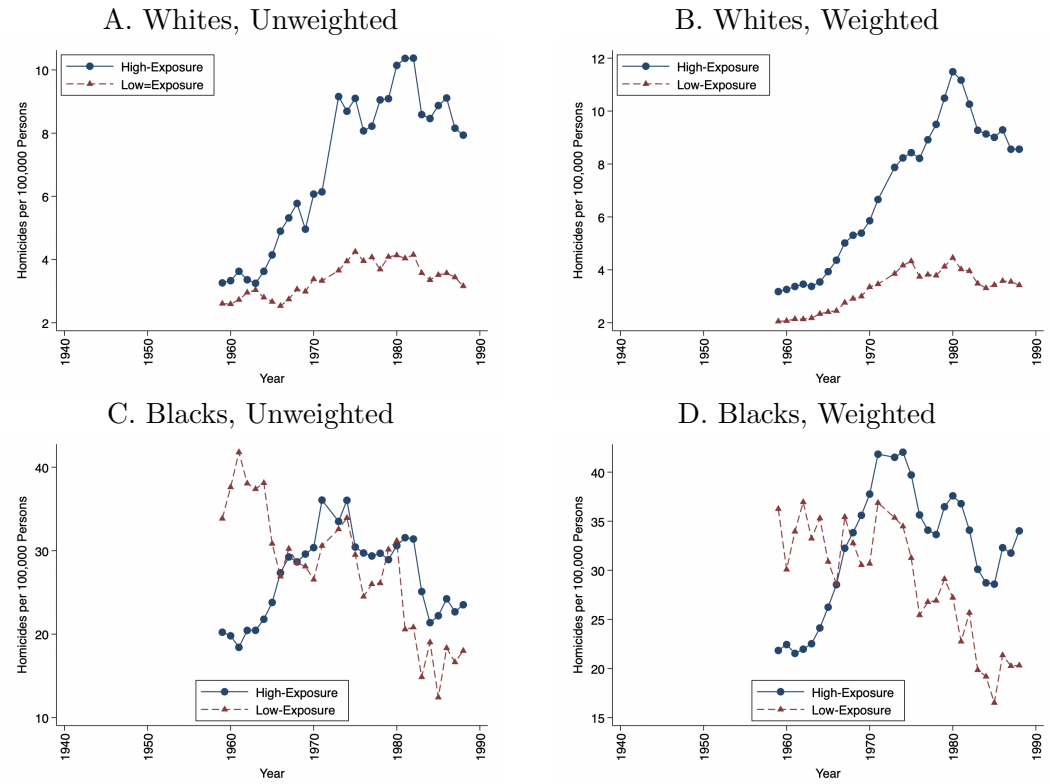
Notes - The figures show the location of high- (red) and low-exposure (yellow) CZ clusters, separately for firearm-related homicides (A) and non-firearm-related homicides (B). For each method of homicide, CZs in the high-exposure cluster experienced a surge in homicides from the 1960s, while CZs in the low-exposure cluster saw a modest increase in homicides over the same period. The sample size is 704 CZs.

Figure A.6: Race-Specific Homicide Trends



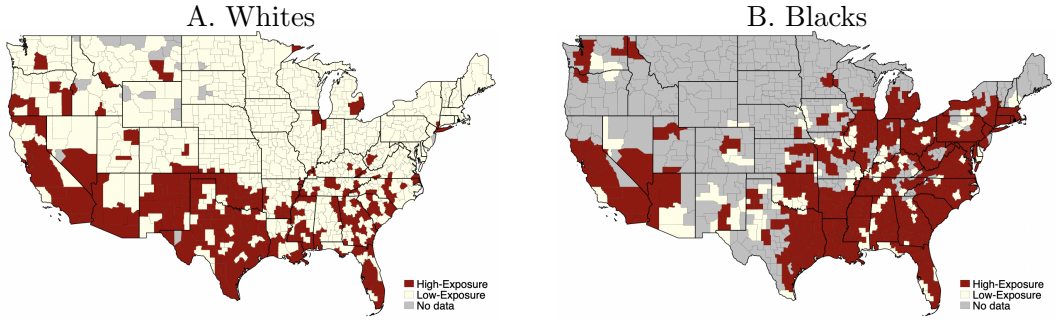
Notes - Data are from the NCHS Death Tables. The figure shows the number of homicides per 100,000 persons each year, separately for whites (solid line with circle markers) and non-whites (dashed line with triangle markers).

Figure A.7: Homicide Trajectory Under a Two-Cluster Regime, by Race



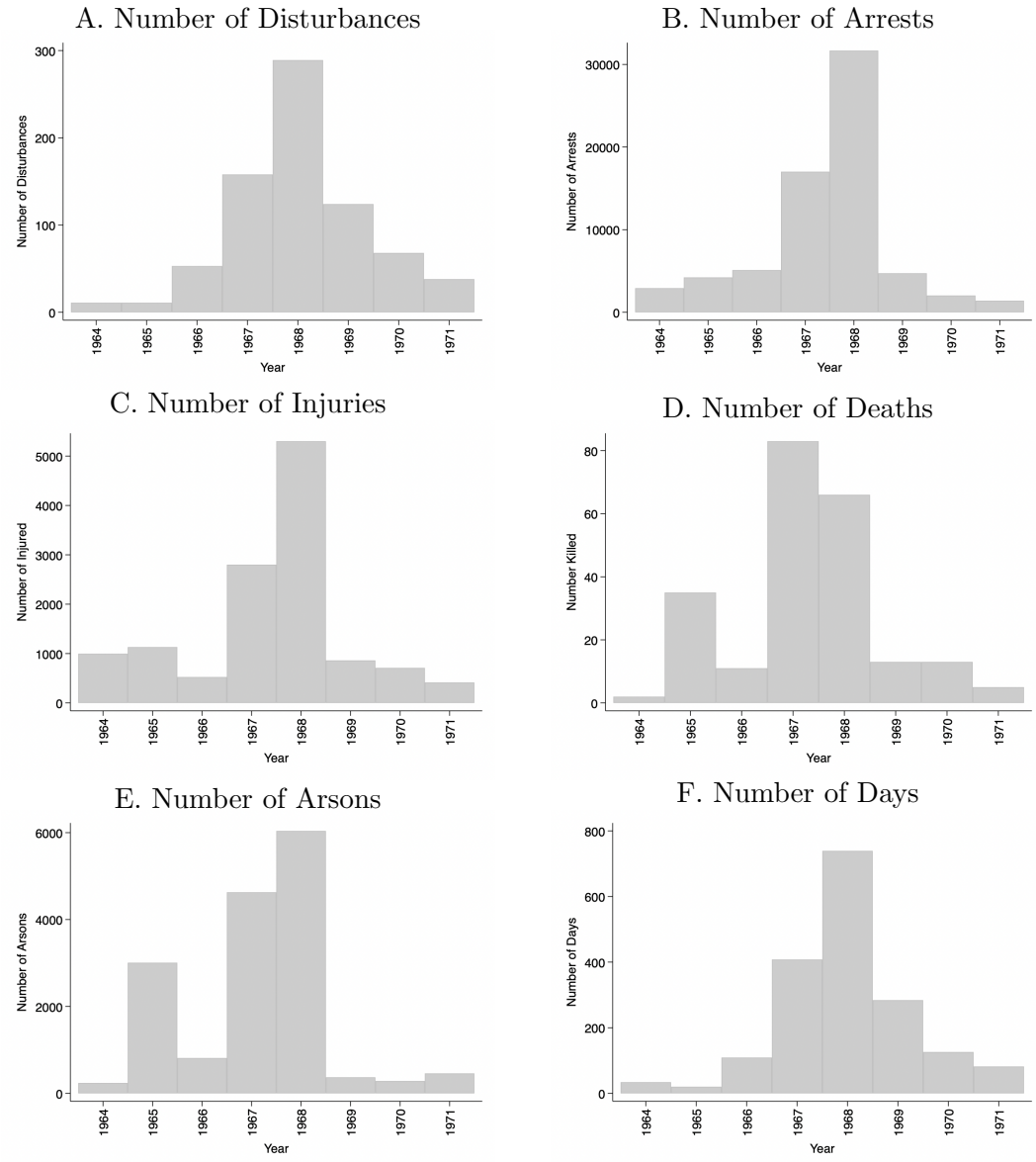
Notes - Data are from the NCHS Multiple Cause of Death files (for 1959-1988) and Manson et al. (2023). Each figure shows the mean homicide trajectory for CZs in one of two groups determined using a k-means clustering approach, separately for white (A and B) and black (C and D) homicides. For each racial group, the cluster experiencing a surge in homicides from the 1960s is represented by a solid line with circle markers (high-exposure). The cluster experiencing a modest increase in homicides over the same period is represented by a dashed line with triangle markers (low-exposure). The trajectories are unweighted in A and C and weighted by the 1960 CZ population (race-specific) in B and D.

Figure A.8: Geography of High- and Low-Exposure CZ Clusters, by Race



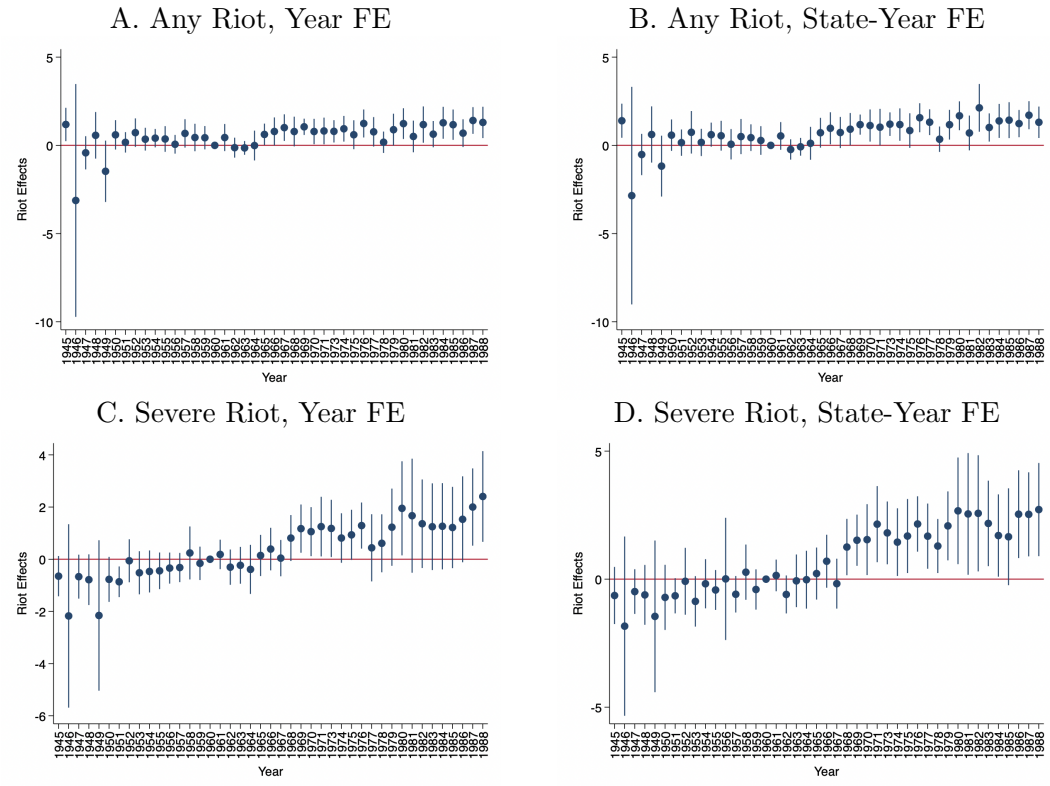
Notes - The figures show the location of high- (red) and low-exposure (yellow) CZ clusters, separately for white (A) and black (B) homicide rates. For each racial group, CZs in the high-exposure cluster experienced a surge in homicides from the 1960s, while CZs in the low-exposure cluster saw a modest increase in homicides over the same period. The sample sizes are 703 (A) and 370 (B) CZs.

Figure A.9: Frequency and Characteristics of Racial Riots



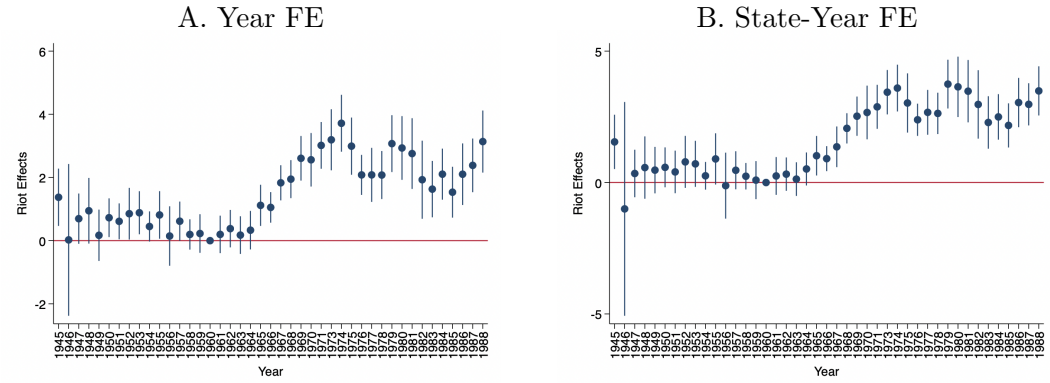
Notes - Data are from Carter (1986). The figure shows the frequency and characteristics of the racial riots that occurred between 1964-1971, by year.

Figure A.10: The Impact of Racial Riots on Homicides, With Additional Controls



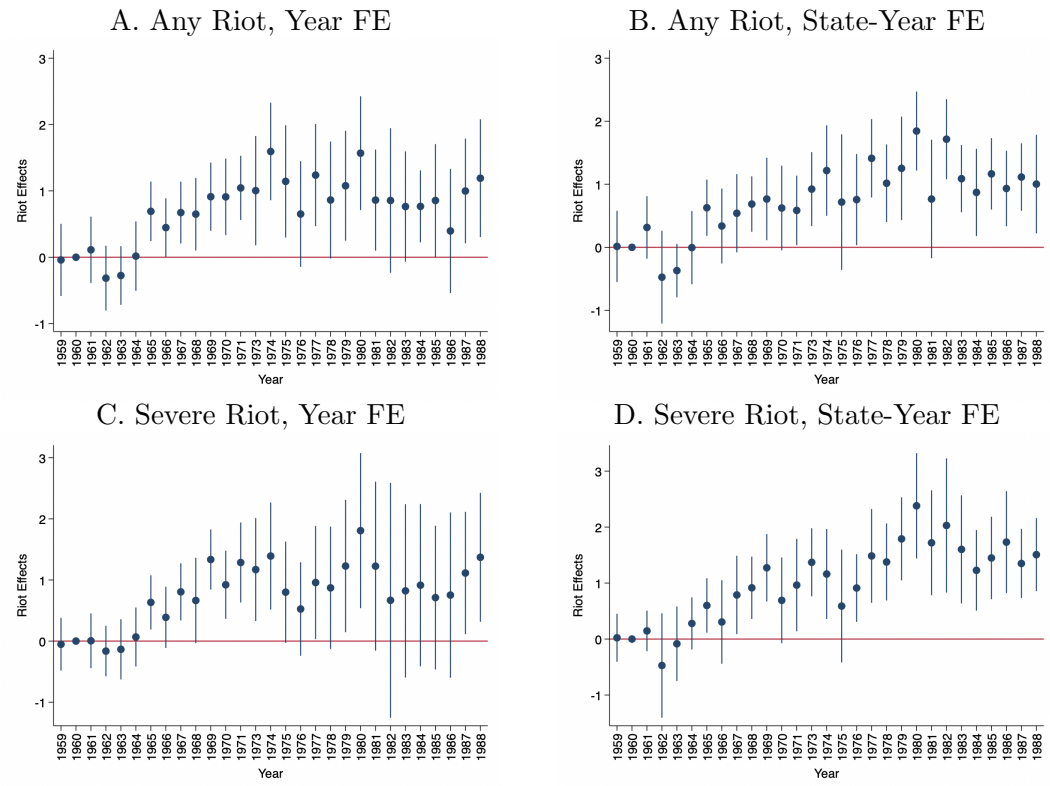
Notes - Data are from Carter (1986), the published *Vital Statistics of the United States* reports (for 1945-1958), the NCHS Multiple Cause of Death files (for 1959-1988), and Manson et al. (2023). The unit of observation is the CZ-year. Each figure shows the interaction coefficients from a regression of the number of homicides per 100,000 persons on an indicator for CZs that experienced a riot (A and B) or severe riot (C and D) between 1964-1971 interacted with year dummies, controlling for CZ and year (A and C) or state-by-year (B and D) fixed effects. Each regression further controls for interactions between indicators for CZs where at least 10 percent of the population were black, hispanic, or living in central cities, separately, interacted with year dummies. The omitted year is 1960. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 30,100 CZ-year observations.

Figure A.11: The Impact of Severe Racial Riots on Homicides, Alternative Definition



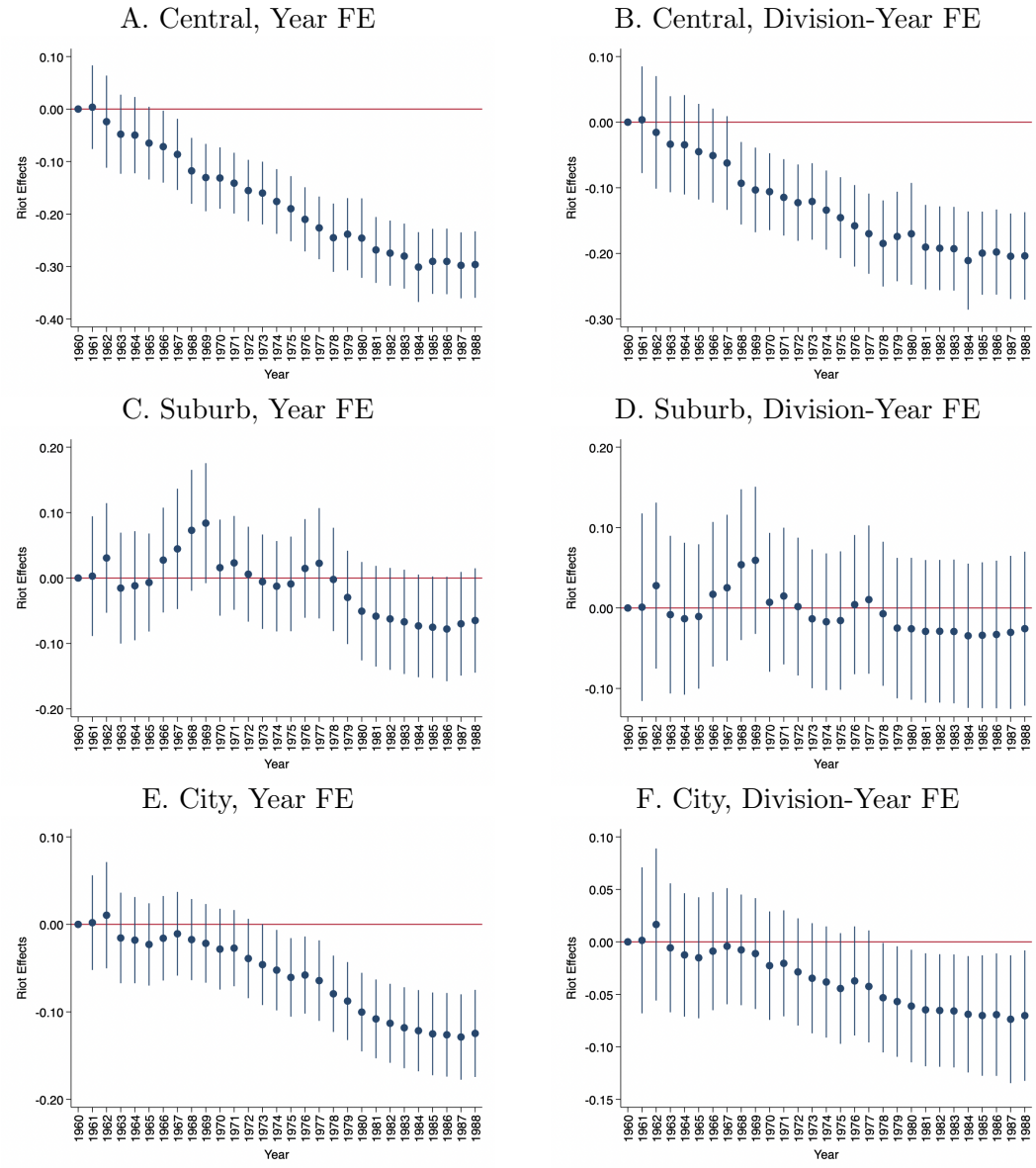
Notes - Data are from Carter (1986), the published *Vital Statistics of the United States* reports (for 1945-1958), the NCHS Multiple Cause of Death files (for 1959-1988), and Manson et al. (2023). The unit of observation is the CZ-year. Each figure shows the interaction coefficients from a regression of the number of homicides per 100,000 persons on an indicator for CZs that experienced a severe riot between 1964-1971 interacted with year dummies, controlling for CZ and year (A) or state-by-year (B) fixed effects. Compared with the baseline riot analysis in Figure 7, CZs with a riot severity index at the 90th percentile or higher are considered to have experienced a severe riot, instead of the original 95th percentile. The omitted year is 1960. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is CZ-year observations.

Figure A.12: The Impact of Racial Riots on White Homicides



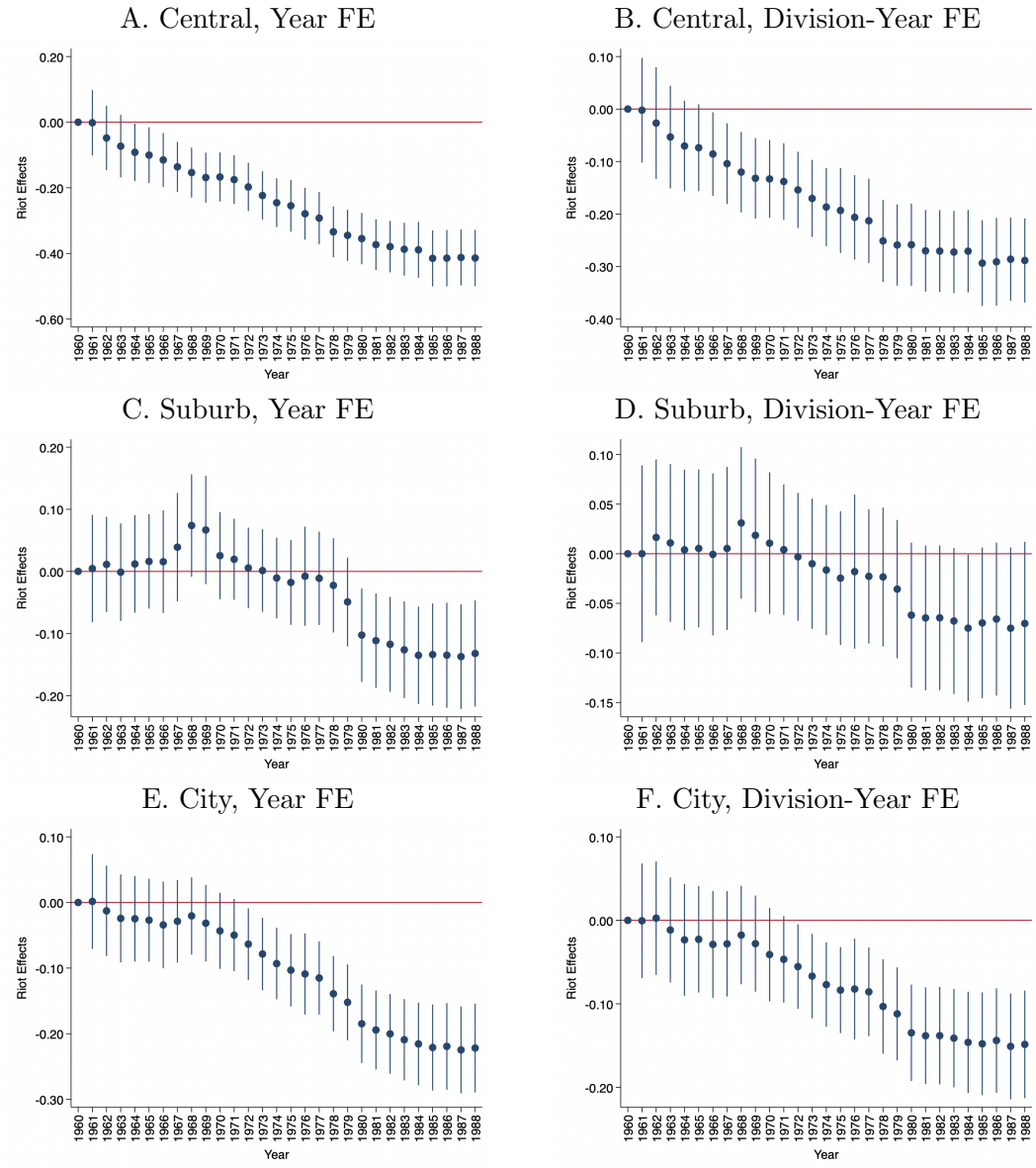
Notes - Data are from Carter (1986), the NCHS Multiple Cause of Death files (for 1959-1988), and Manson et al. (2023). The unit of observation is the CZ-year. Each figure shows the interaction coefficients from a regression of the number of white homicides per 100,000 white persons on an indicator for CZs that experienced a riot (A and B) or severe riot (C and D) between 1964-1971 interacted with year dummies, controlling for CZ and year (A and C) or state-by-year (B and D) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 20,387 CZ-year observations.

Figure A.13: The Impact of Racial Riots on Population, Varying Levels



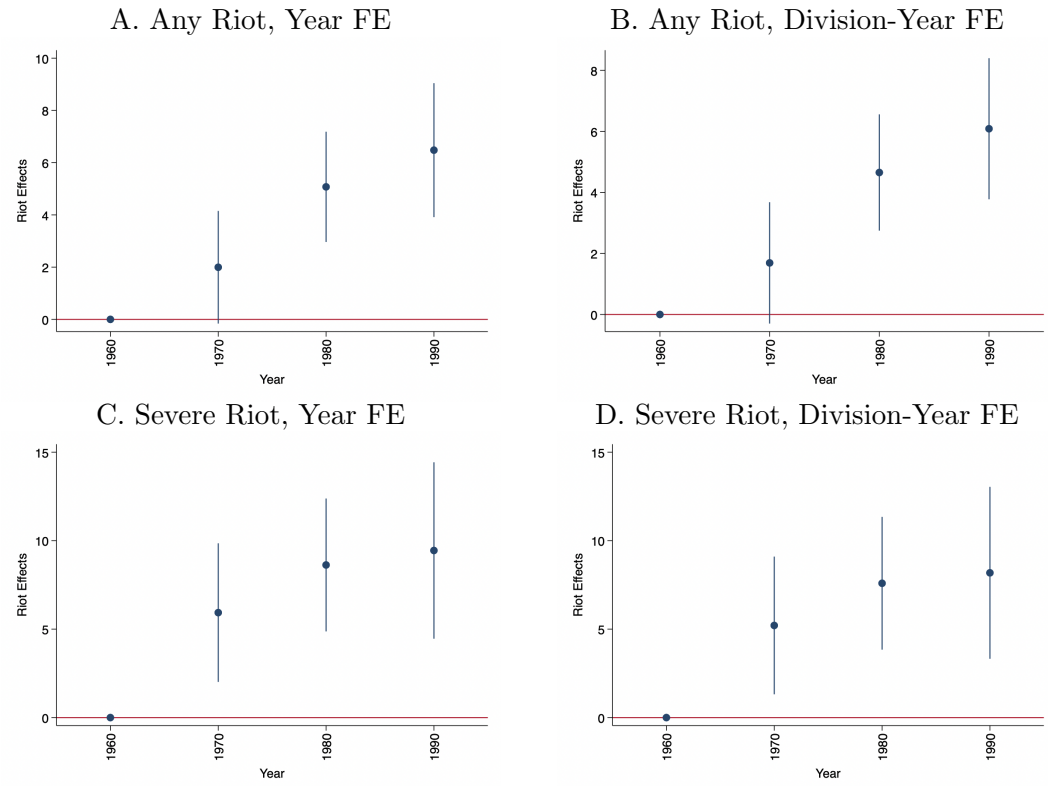
Notes - Data are from Carter (1986) and Curci and Masera (2025). The unit of observation is the city-year. Each figure shows the interaction coefficients from a regression of the log population in the central city area (A and B) or suburb (C and D) or city (E and F) on an indicator for cities that experienced a riot interacted with year dummies, controlling for city and year (A, C, and E) or division-by-year (B, D, and F) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the division level are displayed. The sample size is 9,344 city-year observations.

Figure A.14: The Impact of Severe Racial Riots on Population, Varying Levels



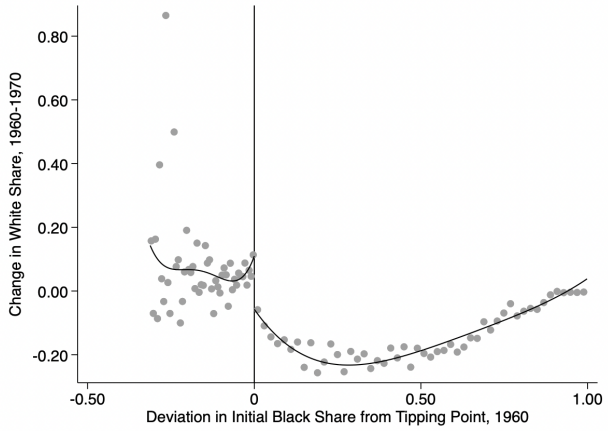
Notes - Data are from Carter (1986) and Curci and Masera (2025). The unit of observation is the city-year. Each figure shows the interaction coefficients from a regression of the log population in the central city area (A and B) or suburb (C and D) or city (E and F) on an indicator for cities that experienced a severe riot interacted with year dummies, controlling for city and year (A, C, and E) or division-by-year (B, D, and F) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the division level are displayed. The sample size is 9,344 city-year observations.

Figure A.15: The Impact of Racial Riots on the Share of Blacks



Notes - Data are from Carter (1986) and Curci and Masera (2025). The unit of observation is the city-year. Each figure shows the interaction coefficients from a regression of the share of central city residents that are black on an indicator for cities that experienced a riot interacted with year dummies, controlling for city and year (A and C) or division-by-year (B and D) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the division level are displayed. The sample size is 938 city-year observations.

Figure A.16: Residential Tipping, 1960-1970



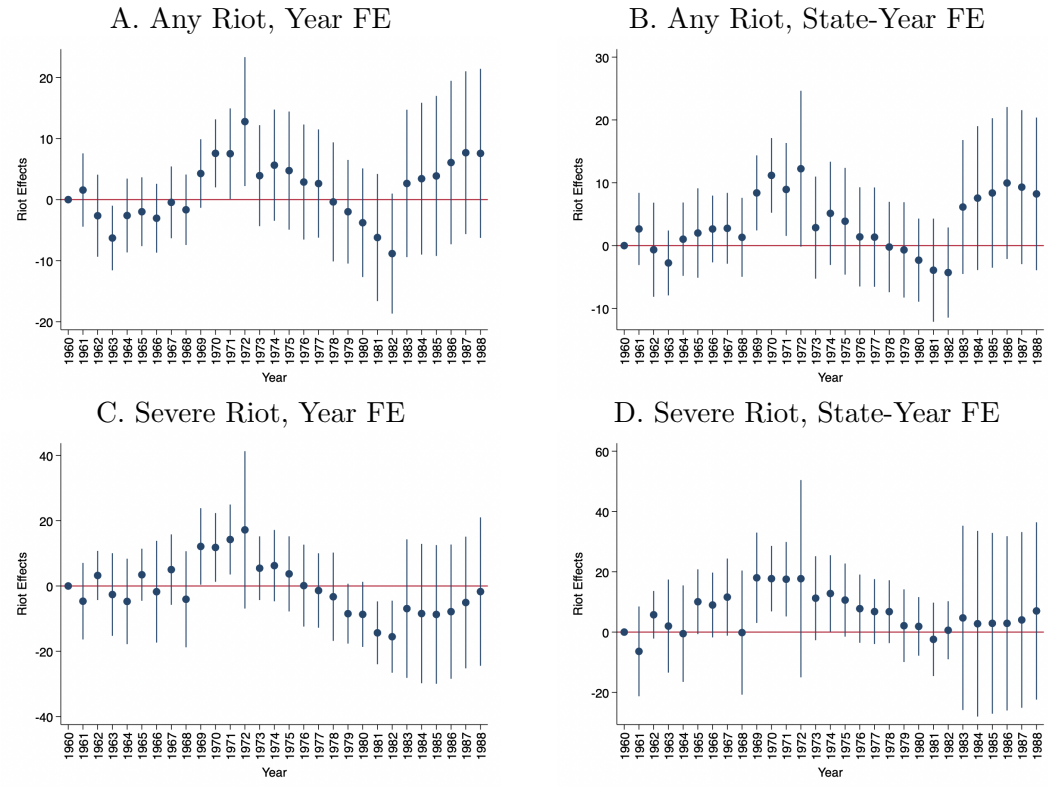
Notes - Data are from Manson et al. (2023). The unit of observation is a census tract. The figure plots the relative change in white population from 1960-1970 (y-axis) against the deviation in the initial share of blacks in 1960 from the tipping point (x-axis). Fourth-order polynomials are fitted on either side of the 0 line. The sample size is 17,262 census tracts.

Figure A.17: The Impact of Racial Riots on Homicide Clearance Rates, Consistent Reporting Agencies



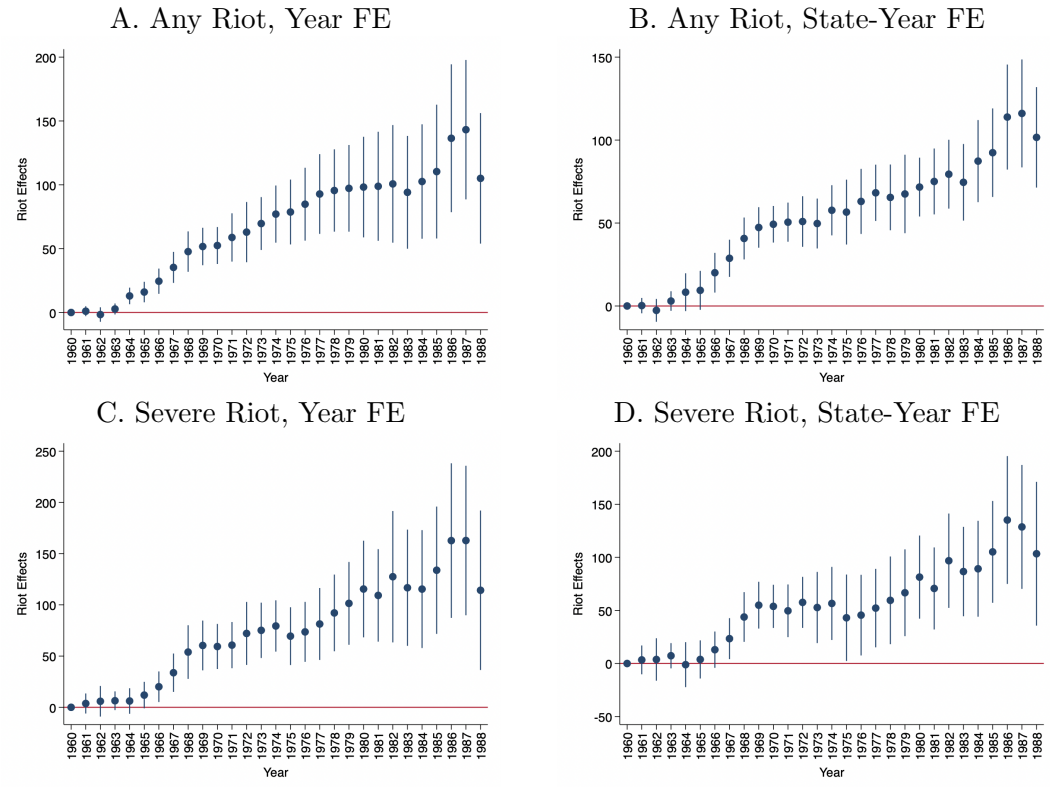
Notes - Data are from Carter (1986), Manson et al. (2023), and the UCR OKCA (Kaplan, 2025d). The unit of observation is the CZ-year. The underlying sample is restricted to police agencies reporting crime statistics every year between 1960-1988. Each figure shows the interaction coefficients from a regression of the share of homicides that have been cleared on an indicator for CZs that experienced a riot (A and B) or severe riot (C and D) between 1964-1971 interacted with year dummies, controlling for CZ and year (A and C) or state-by-year (B and D) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 13,101 CZ-year observations.

Figure A.18: The Impact of Racial Riots on Police Strength, Consistent Reporting Agencies



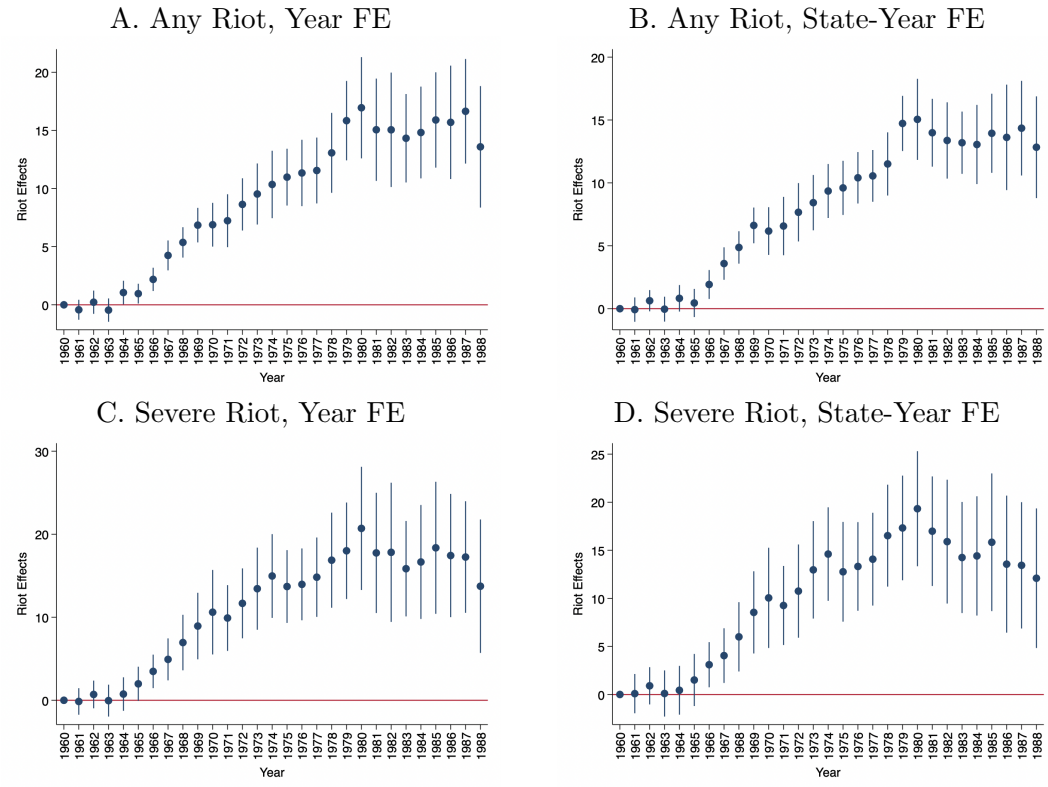
Notes - Data are from Carter (1986), Manson et al. (2023), and the UCR LEOKA (Kaplan, 2025b). The unit of observation is the CZ-year. The underlying sample is restricted to police agencies reporting crime statistics every year between 1960-1988. Each figure shows the interaction coefficients from a regression of the number of police officers per 100,000 persons on an indicator for CZs that experienced a riot (A and B) or severe riot (C and D) between 1964-1971 interacted with year dummies, controlling for CZ and year (A and C) or state-by-year (B and D) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 20,300 CZ-year observations.

Figure A.19: The Impact of Racial Riots on Aggravated Assault



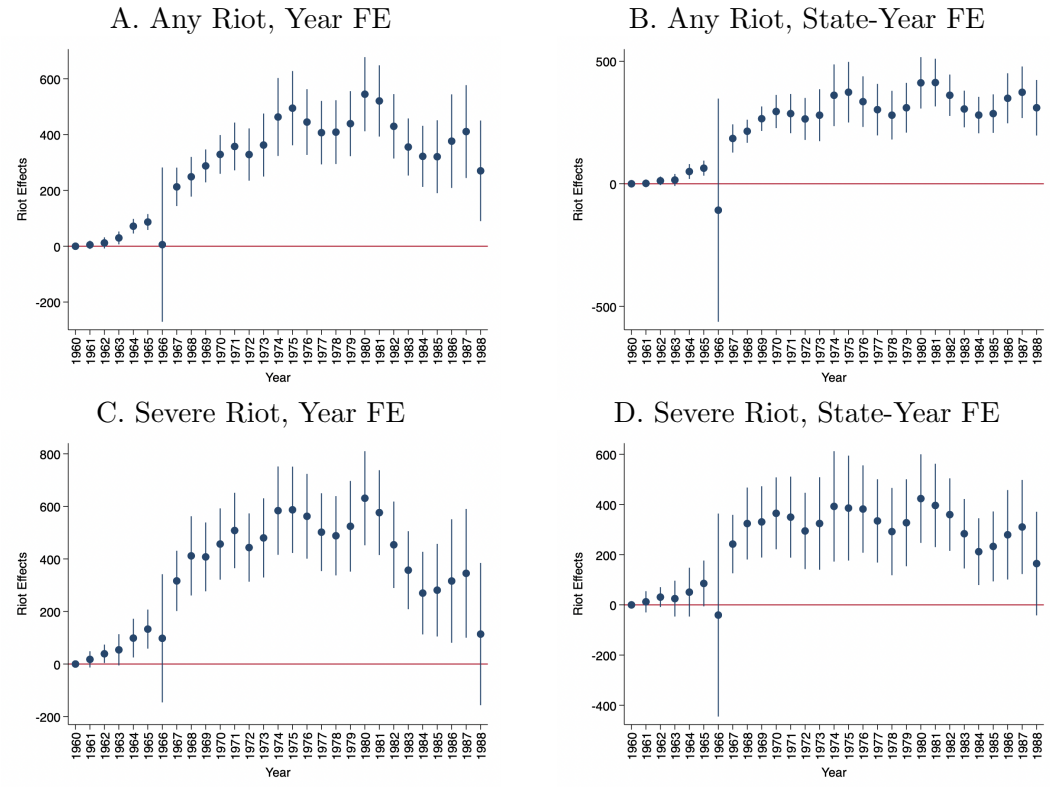
Notes - Data are from the UCR OKCA (Kaplan, 2025d), published *Vital Statistics of the United States* reports (for 1945-1958), the NCHS Multiple Cause of Death files (for 1959-1988), and Manson et al. (2023). The unit of observation is the CZ-year. Each figure shows the interaction coefficients from a regression of the number of aggravated assaults per 100,000 persons on an indicator for CZs that experienced a riot (A and B) or severe riot (C and D) between 1964-1971 interacted with year dummies, controlling for CZ and year (A and C) or state-by-year (B and D) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 20,355 CZ-year observations.

Figure A.20: The Impact of Racial Riots on Reported Rape



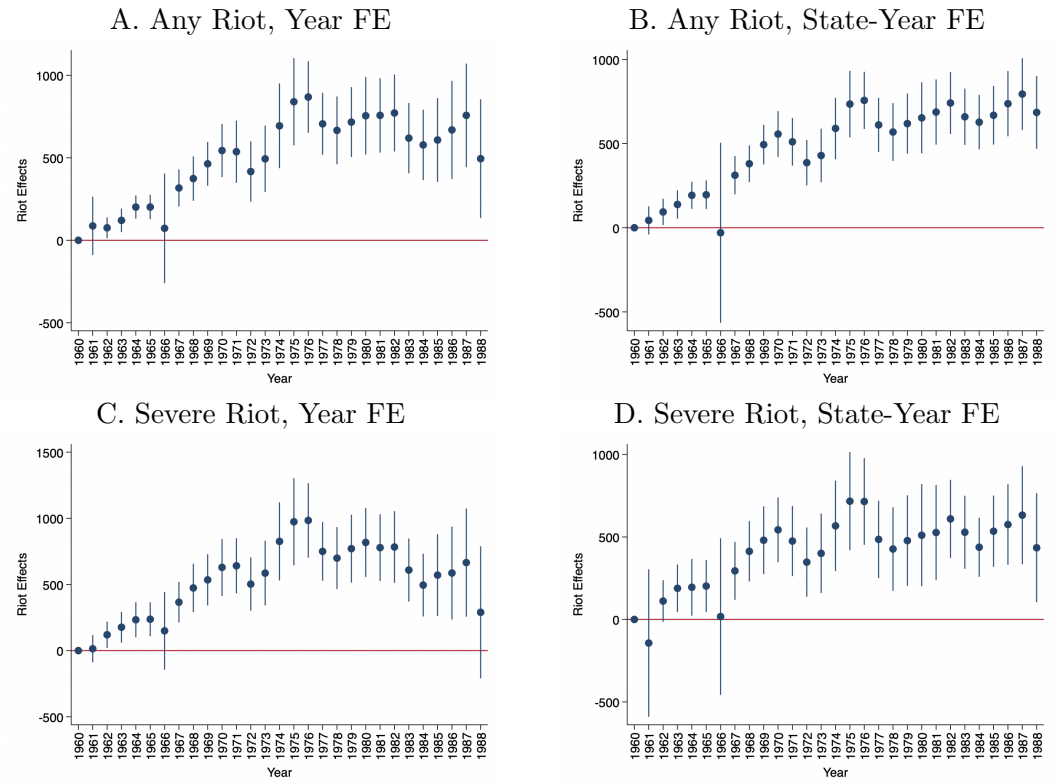
Notes - Data are from the UCR OKCA (Kaplan, 2025d), published *Vital Statistics of the United States* reports (for 1945-1958), the NCHS Multiple Cause of Death files (for 1959-1988), and Manson et al. (2023). The unit of observation is the CZ-year. Each figure shows the interaction coefficients from a regression of the number of reported rape per 100,000 persons on an indicator for CZs that experienced a riot (A and B) or severe riot (C and D) between 1964-1971 interacted with year dummies, controlling for CZ and year (A and C) or state-by-year (B and D) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 20,355 CZ-year observations.

Figure A.21: The Impact of Racial Riots on Burglaries



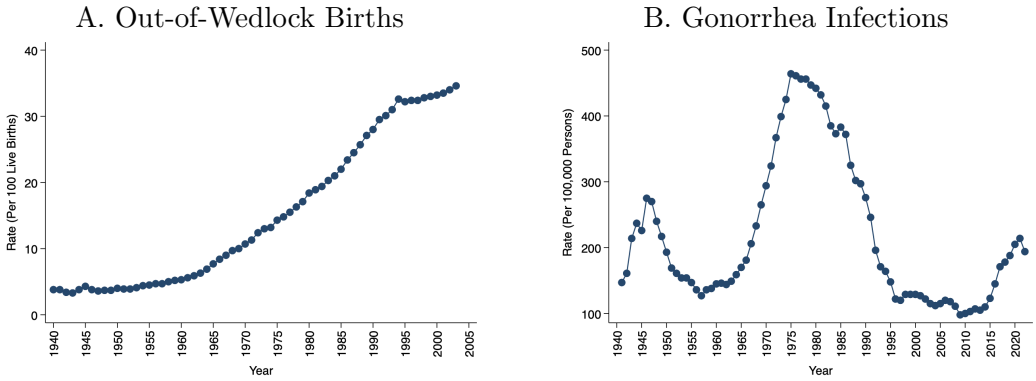
Notes - Data are from the UCR OKCA (Kaplan, 2025d), published *Vital Statistics of the United States* reports (for 1945-1958), the NCHS Multiple Cause of Death files (for 1959-1988), and Manson et al. (2023). The unit of observation is the CZ-year. Each figure shows the interaction coefficients from a regression of the number of burglaries per 100,000 persons on an indicator for CZs that experienced a riot (A and B) or severe riot (C and D) between 1964-1971 interacted with year dummies, controlling for CZ and year (A and C) or state-by-year (B and D) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 20,355 CZ-year observations.

Figure A.22: The Impact of Racial Riots on Thefts



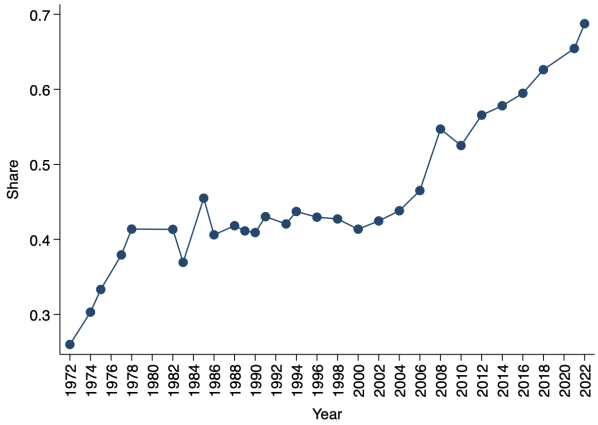
Notes - Data are from the UCR OKCA (Kaplan, 2025d), published *Vital Statistics of the United States* reports (for 1945-1958), the NCHS Multiple Cause of Death files (for 1959-1988), and Manson et al. (2023). The unit of observation is the CZ-year. Each figure shows the interaction coefficients from a regression of the number of thefts per 100,000 persons on an indicator for CZs that experienced a riot (A and B) or severe riot (C and D) between 1964-1971 interacted with year dummies, controlling for CZ and year (A and C) or state-by-year (B and D) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 20,355 CZ-year observations.

Figure A.23: The Sexual Revolution



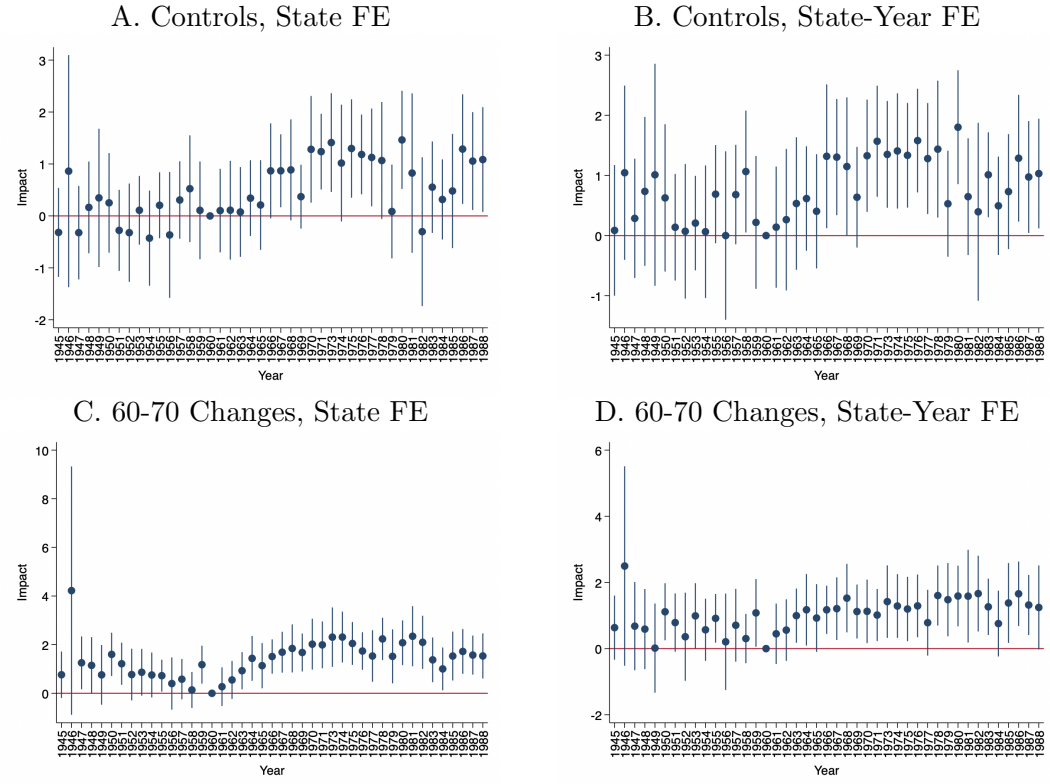
Notes - Data are from Vital Statistics (2003) and the Centers for Disease Control and Prevention (2002). The figures show the national trend in out-of-wedlock births (A) and gonorrhea infections (B).

Figure A.24: Perceptions on Premarital Sex



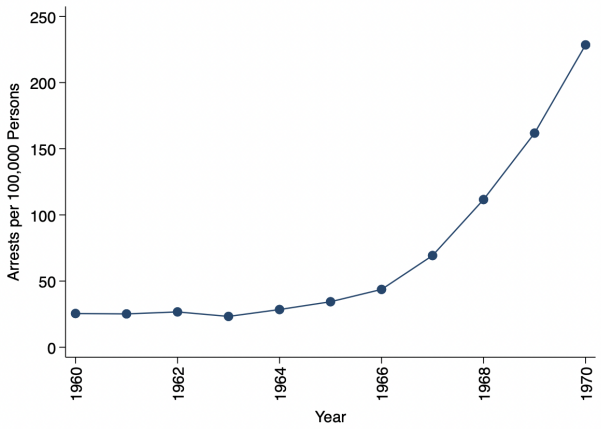
Notes - Data are from the General Social Survey (1972-2022). The figure shows the share of respondents reporting that premarital sex is never wrong.

Figure A.25: The Impact of the Sexual Revolution on Homicides, Robustness Checks



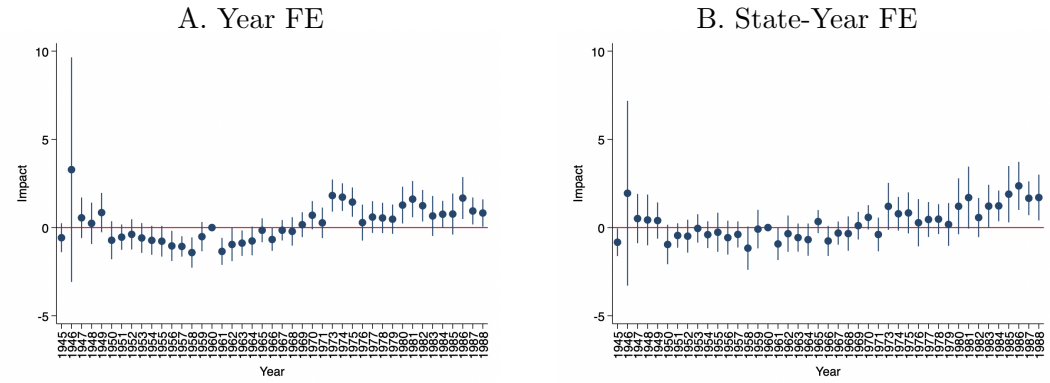
Notes - Data are from Ruggles et al. (2025), the published *Vital Statistics of the United States* reports (for 1945-1958), the NCHS Multiple Cause of Death files (for 1959-1988), and Manson et al. (2023). The unit of observation is the CZ-year. Each figure shows the interaction coefficients from a regression of the number of homicides per 100,000 persons on an indicator for CZs that experienced above-median increases in the share of 18-to-29-year-old unwed mothers from 1960-1980 (A and B) or 1960-1970 (C and D) interacted with year dummies, controlling for CZ and year (A and B) or state-by-year (C and D) fixed effects. In A and B, each regression further controls for interactions between indicators for CZs where at least 10 percent of the population were black, hispanic, or living in central cities, interacted with year dummies. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 30,100 CZ-year observations.

Figure A.26: Trends in Drug-Related Arrests



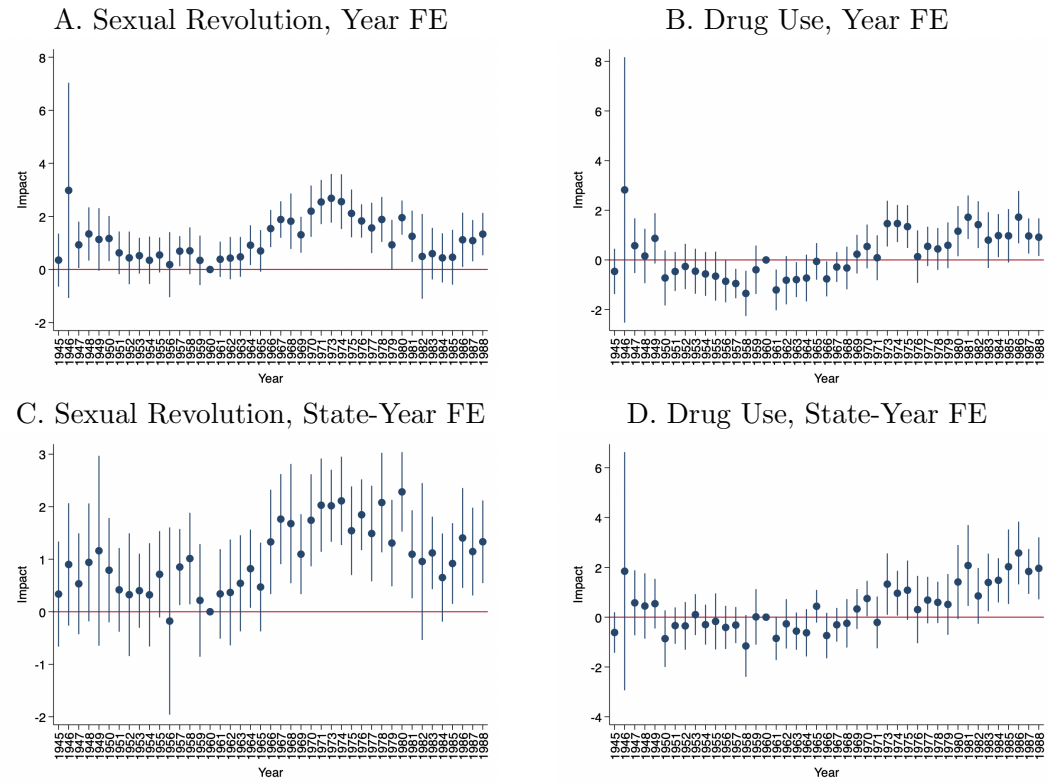
Notes - Data are from the *Annual Report of the Attorney General of the United States* (1971). The figure shows the number of drug-related arrests made each year per 100,000 persons.

Figure A.27: The Impact of Recreational Drug Use on Homicides, with Controls



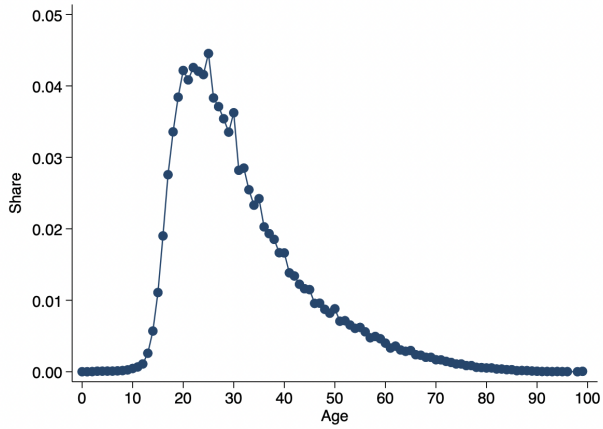
Notes - Data are from the published *Vital Statistics of the United States* reports (for 1945-1958), the NCHS Multiple Cause of Death files (for 1959-1988), and Manson et al. (2023). The unit of observation is the CZ-year. Each figure shows the interaction coefficients from a regression of the number of homicides per 100,000 persons on an indicator for CZs that experienced above-median increases in the number of deaths due to substance poisoning per 100,000 persons interacted with year dummies, controlling for CZ and year (A) or state-by-year (B) fixed effects. Each regression further controls for interactions between indicators for CZs where at least 10 percent of the population were black, hispanic, or living in central cities, interacted with year dummies. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 30,100 CZ-year observations.

Figure A.28: Horserace: The Impact of the Sexual Revolution and Recreational Drug Use



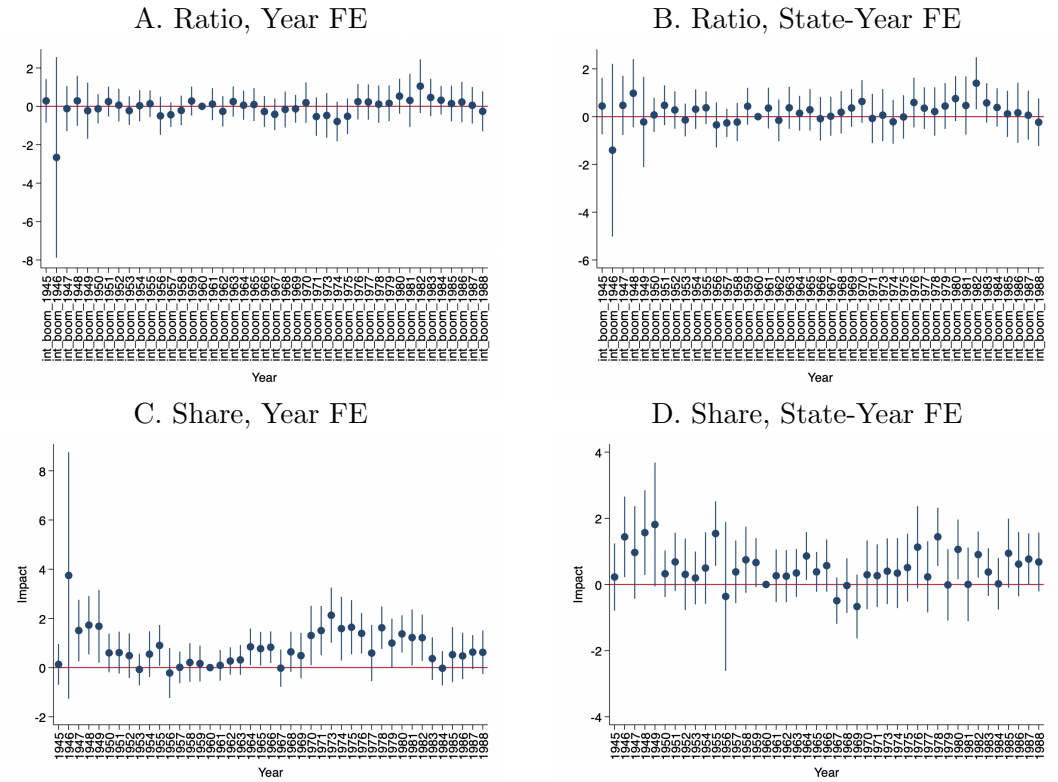
Notes - Data are from Ruggles et al. (2025), the published *Vital Statistics of the United States* reports (for 1945-1958), the NCHS Multiple Cause of Death files (for 1959-1988), and Manson et al. (2023). The unit of observation is the CZ-year. Each figure shows the interaction coefficients from a regression of the number of homicides per 100,000 persons on an indicator for CZs that experienced above-median increases in the share of 18-to-29-year-old unwed mothers from 1960-1980 interacted with year dummies and an indicator for CZs that experienced above-median increases in the number of deaths due to substance poisoning per 100,000 persons interacted with year dummies, controlling for CZ and year (A and B) or state-by-year (C and D) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 30,100 CZ-year observations.

Figure A.29: Age-Homicide Profile



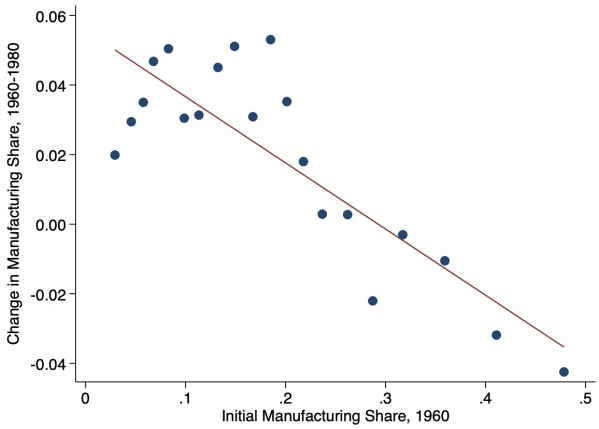
Notes - Data are from the UCR SHR (Kaplan, 2025c). The underlying sample is restricted to 1960-1988. The figure shows the share of homicide offenders who are of a given age.

Figure A.30: Impact of the Baby Boom on Homicides



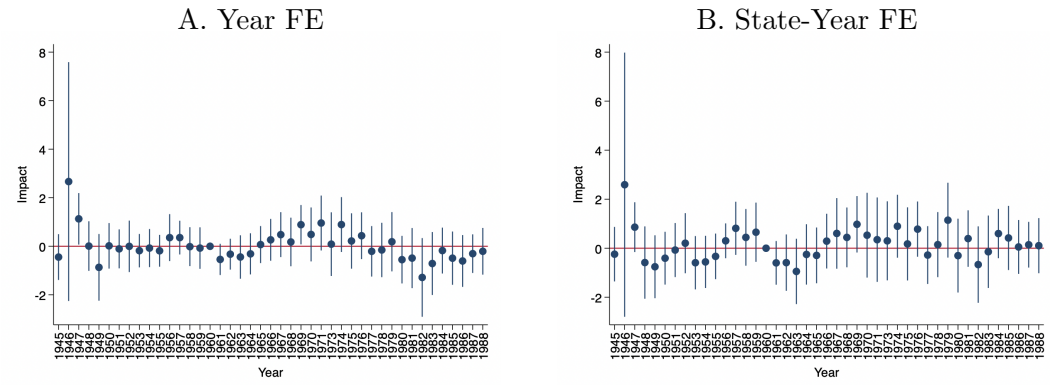
Notes - Data are from Ruggles et al. (2025), the published *Vital Statistics of the United States* reports (for 1945-1958), the NCHS Multiple Cause of Death files (for 1959-1988), and Manson et al. (2023). The unit of observation is the CZ-year. Each figure shows the interaction coefficients from a regression of the number of homicides per 100,000 persons on an indicator for CZs with above-median ratios of the 1946-1948 to 1938-1945 birth cohorts in 1960 (A and B) or an indicator for CZs with above-median shares of 10-19-year-olds in 1960 (C and D) interacted with year dummies, controlling for CZ and year (A and C) or state-by-year (B and D) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 30,100 CZ-year observations.

Figure A.31: Changes in Manufacturing Share



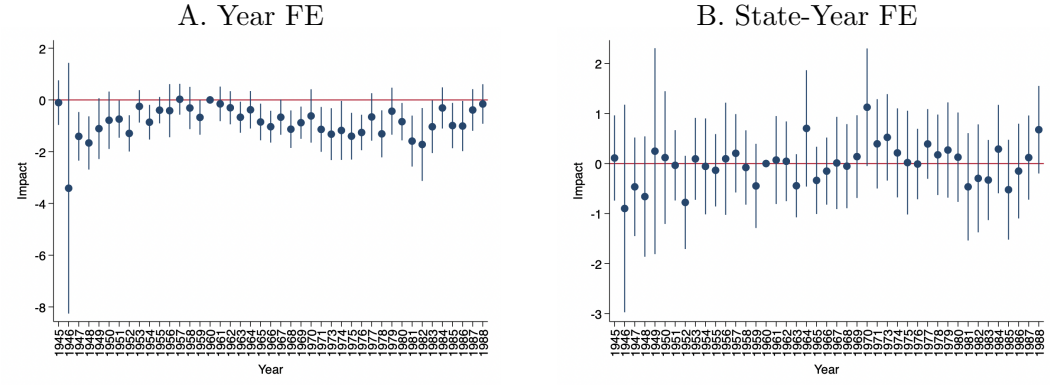
Notes - Data are from Ruggles et al. (2025). The unit of observation is the CZ. The figure plots the change in the share of men aged 25 and over in manufacturing from 1960 to 1980 (y-axis) against the initial share in manufacturing as of 1960 (x-axis).

Figure A.32: Impact of Deindustrialization on Homicides



Notes - Data are from Ruggles et al. (2025), the published *Vital Statistics of the United States* reports (for 1945-1958), the NCHS Multiple Cause of Death files (for 1959-1988), and Manson et al. (2023). The unit of observation is the CZ-year. Each figure shows the interaction coefficients from a regression of the number of homicides per 100,000 persons on an indicator for CZs with above-median shares of men aged 25 and over in manufacturing as of 1960 interacted with year dummies, controlling for CZ and year (A) or state-by-year (B) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 30,100 CZ-year observations.

Figure A.33: Impact of Vietnam War Exposure on Homicides



Notes - Data are from Ruggles et al. (2025), the published *Vital Statistics of the United States* reports (for 1945-1958), the NCHS Multiple Cause of Death files (for 1959-1988), and Manson et al. (2023). The unit of observation is the CZ-year. Each figure shows the interaction coefficients from a regression of the number of homicides per 100,000 persons on an indicator for CZs with above-median shares of men aged 20-40 who were Vietnam War veterans as of 1970 interacted with year dummies, controlling for CZ and year (A) or state-by-year (B) fixed effects. The omitted year is 1960. 95 percent confidence bands based on standard errors clustered at the state level are displayed. The sample size is 30,100 CZ-year observations.

Table A.1: Characteristics of High- and Low-Exposure CZ Clusters, Region Breakdown

	High-Exposure Cluster	Low-Exposure Cluster	Difference
	(1)	(2)	(3)
New England census division	0	0.024	-0.024**
			(0.011)
Middle Atlantic census division	0.017	0.046	-0.030*
			(0.016)
East North Central census division	0.055	0.146	-0.091***
			(0.027)
West North Central census division	0.056	0.303	-0.247***
			(0.034)
South Atlantic census division	0.204	0.131	0.073**
			(0.030)
East South Central census division	0.109	0.102	0.007
			(0.025)
West South Central census division	0.348	0.082	0.266***
			(0.029)
Mountain census division	0.120	0.115	0.005
			(0.027)
Pacific census division	0.090	0.050	0.040**
			(0.020)
N	196	504	700

Notes - Data are from Ruggles et al. (2025). Columns 1 and 2 measure the average characteristics of CZs in the high- and low-exposure clusters respectively, along the geographic dimensions listed in the leftmost column. High-exposure CZs experienced a surge in homicides from the 1960s, while low-exposure CZs saw a modest increase in homicides over the same period. Column 3 shows the difference between CZs in the high- and low-exposure clusters. Standard errors are in parentheses. *** 1 percent, ** 5 percent, and * 10 percent significance levels.