Decarceration and Gun Violence During COVID-19 in the United States

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I use nationwide data on gun violence and jail population to characterize the firearm homicide surge during COVID-19 in the United States and its association with the mass decarceration shock in the early pandemic. I employ linear and Poisson panel event studies to provide the first nationwide evidence that that U.S. counties exposed to a large jail population reduction experienced a weak, positive, and heterogeneous impact on firearm homicides from 2020 to 2022. The results are consistent across multiple research designs and robustness checks.

I. Introduction

The number of firearm homicides—defined as homicide incidents in which a firearm was the primary weapon—increased by 30% in the United States from 2019 to 2020[1] and remained elevated through 2022 (Figure 1). The surge, which began in May and June 2020 in many regions, was preceded by a 25% decline in the U.S. jail population—measured as the number of inmates held in local jail facilities—between February and April 2020. Neither series exhibited discernible pre-trends prior to 2020, and both returned to their pre-pandemic levels by the end of 2024.

Several additional facts can help clarify the relevance of the observed patterns.

First, firearm homicides account for roughly 80% of all homicides in the United States. Moreover, while non-firearm homicide incidents had not increased during the pandemic (Figure A1), non-lethal gun violence rose dramatically during the same period. Note that these patterns motivate treating firearm homicides as a subset of overall gun violence. For example, if the fatality rate per firearm incident remains stable over time and can be always predicted by the size of non-fatal gun violence, then separate analysis of firearm homicides can be redundant. However, the ratio of firearm homicides to total firearm incidents increased during the pandemic, indicating that gun violence became more lethal (Figure A6). This shift implies a higher social cost per incident, strengthening the case for analyzing trends in firearm homicides as standalone measure.

Second, recent data shows that major property crimes—robbery, larceny, and

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[1] Centers for Disease Control and Prevention (2021), Federal Bureau of Investigation (2021)

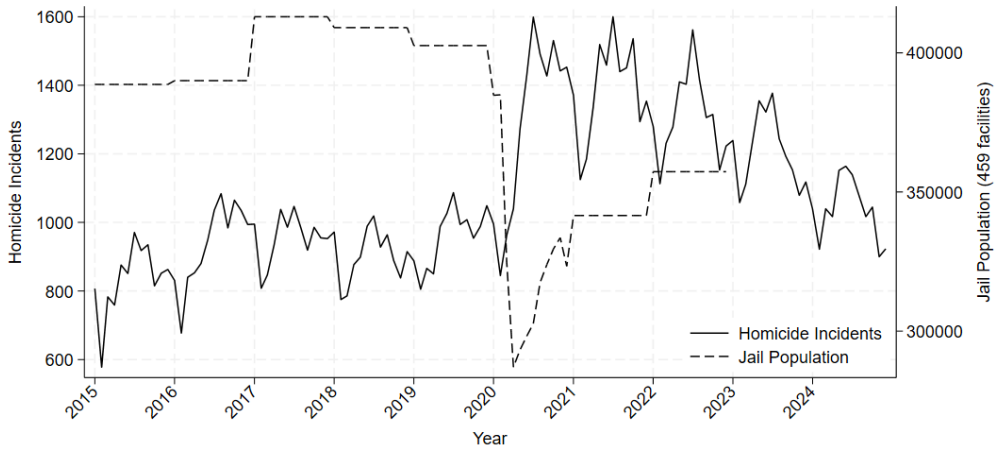


FIGURE 1. AGGREGATE SUMMARY

Note. Data are from the Gun Violence Archive (GVA) and the FBI's Annual Survey of Jails (ASJ). The GVA panel includes only non-suicidal firearm homicides. The ASJ provides monthly jail population estimates only for 2020 via a COVID-19 supplement; data for 2023–2024 are unavailable. In Figure A5, I use supplementary data from the Jail Data Initiative to show that jail populations returned to pre-pandemic levels in another nationwide sample. The GVA and ASJ panels cover 3,166 and 550 counties, respectively; ASJ counties account for roughly 70% of the U.S. population.

burglary—had not increased during COVID-19[2] (Figure A2). This challenges a prominent hypothesis that pandemic-related economic distress increased criminal incentives, thereby driving up homicide rates. Instead, trends in aggravated assault and arson more closely mirrored the rise in firearm homicides during the pandemic. It hints that the pandemic marked a broader escalation in violent behavior rather than a rise in economically motivated crime.

Third, the mass jail depopulation during the pandemic was driven primarily by reduced admissions rather than increased releases. The left panel of Figure A3 shows that the ratio of total releases from jail to total admissions rose by approximately 2.5 percentage points, consistent with reports that many U.S. states—such as California—permitted the early release of thousands of inmates charged with nonviolent offenses as part of pandemic mitigation efforts. In this study, however, I assume that the effect of early release and the potential subsequent recidivism on homicides is negligible because the inmates charged with nonviolent offenses or minor misdemeanors were the only group eligible for special early release. Second, the right panel of Figure A3 shows that total jail admissions declined by over one million in 2020 and 2021. This scale of reduction further supports the view that

[2]In this study, I assume that all regions in the United States had been almost uniformly affected by the COVID-19 pandemic between March 2020 and the end of 2022.

decarceration was achieved primarily through reduced admissions rather than accelerated releases.

Fourth, while jail population is commonly used as a measure of incarceration, I posit that it more broadly proxies for the overall level of law enforcement activity. In the U.S. criminal justice system, pre-trial detention in jail facilities occupies a middle ground between front-end processes (such as policing and arrest) and back-end processes (such as prosecution, adjudication, and corrections). As such, changes in the jail population reflect the net outcome of a variety of mechanisms operating simultaneously within the system. For example, a county's jail population may increase due to higher arrest rates (e.g., new offenses or probation violations), reduced release rates, or prolonged stays caused by delays in court proceedings—which actually happened during the pandemic. Rather than separately modeling each of these dynamics and their interactions, this paper treats the jail population as a reduced-form measure of the intensity of local law enforcement. Partly due to that, the fundamental simultaneity between incarceration and crime is less addressed in this study.

Taken together, the effect of the jail population on firearm homicides in this context can thus be interpreted as the effect of local law enforcement intensity on extreme violence, viewed through the lens of lethal gun violence during the COVID-19 pandemic in the United States. A large and growing literature has examined the pandemic's impact on various categories of crime and generally finds limited or statistically insignificant effects. Studies that specifically examine shifts in law enforcement activity—such as reductions in policing, court operations, or correctional supervision—reach similar conclusions. However, the nationwide impact of pandemic-era decarceration and the associated decline in local enforcement capacity on gun violence remains unclear, despite heightened public concern and ongoing policy debate surrounding gun violence in the United States.

The purpose of this paper is to address this gap and provide empirical evidence on whether the shift in law enforcement during the pandemic meaningfully increased either the magnitude or lethality of gun violence in the United States. I implement the main analysis using nationwide data on gun violence and jail populations. First, pooled zero-inflated regression estimates suggest that the loss in expected jail population in 2020 had no statistically significant effect on the number of homicides in that year, after controlling for population and historical average homicide levels. I then employ a panel event study to estimate the dynamic effects of jail population reduction (the "treatment") across multiple post-pandemic years. Lastly, quantile regression models indicate that, holding population constant, previously high-violence regions experienced greater increases in homicides following jail population reductions. Two additional analyses show that (1) jail population reductions had a notable impact on the lethality of gun violence, and (2) the core GLM results remain robust to interactions with police effort indicators.

The rest of this paper is organized as follows. Section II reviews relevant literature. Section III describes data sources and sample construction. Section IV examines the effects of jail population on homicides under various parametric assumptions. Section V implement additional analysis. Section VI discusses potential mechanisms and limitations of this paper. Section VII concludes.

II. Existing Literature

This paper contributes to multiple bodies of literature. First, a large theoretical and empirical literature examines the effect of incarceration on crime. A common framework decomposes the effect into incapacitation (the mechanical prevention of criminal activity through confinement) and deterrence (the increase in expected cost of offending).[3] Several studies have estimated the aggregate impact of incarceration using panel data on crime and prison populations. An early contribution is Marvell and Moody (1994), which documents a negative crime–prison elasticity and supports the finding using Granger causality tests. A subsequent foundational study, Levitt (1996), introduces an instrumental variables (IV) approach to address endogeneity arising from simultaneity—i.e., higher crime rates leading to increased incarceration under constant clearance rates. More recently, Johnson and Raphael (2012) instrument the incarceration rate using the deviation from its steady-state level and find diminishing marginal effects of incarceration on crime as the incarceration rate rises.

More closely related to this paper is the literature leveraging discrete policy events as exogenous shocks to incarceration. Buonanno and Raphael (2013) exploit Italy's 2006 collective pardon, which led to the abrupt release of roughly one-third of inmates, and find evidence consistent with incapacitation mechanism based on a discontinuous increase in crime rates. The U.S. evidence is more mixed: Lofstrom and Raphael (2016) find limited effects of California's 2011 Public Safety Realignment—which substantially reduced the state prison population—on violent crime. This paper contributes to this literature by leveraging a novel source of decarceration: reductions in admissions rather than release-driven shocks.

The literature on the effect of police force on crime is also relevant. Levitt (1997) exploits the timing of mayoral and gubernatorial elections as an instrument for police force size and finds a significant negative elasticity.[4] Devi and Fryer (2020) and Mas (2006) show that reductions in police effort—triggered by federal investigations following high-profile use-of-force incidents and adverse salary arbitrations, respectively—lead to higher crime, including homicides. These findings suggest that police effectiveness depends not only on force size but also on factors such as officer morale and perceived fairness. In this context, the killing of George Floyd in May 2020 and the ensuing de-policing movements introduce

[3] The deterrence effect is further divided into general deterrence among potential offenders and specific deterrence among former inmates (Johnson and Raphael (2012)).

[4] McCrary (2002) challenges this result, identifying a coding error that weakens the instrument's validity.

confounding channels in analyzing law enforcement shocks during the pandemic. For instance, Premkumar (2019) finds that high-profile police killings followed by widespread public scrutiny lead to reduced police effort and increased violent crime. I avoid this concern by using annual data and assuming that any Ferguson effects dissipated after 2020. Hence, the 2020 shift in law enforcement, proxied by changes in jail population, captures both pandemic-driven policy changes and police reactions to the killing of George Floyd.

Most relevant to this work is a growing literature examining whether and how COVID-19 affected crime in the United States. Abrams (2021) documents sharp declines in most crime categories and arrests—except for homicides and certain property crimes—following the onset of the pandemic. Closest to this study, Kubrin and Bartos (2024) employs a fuzzy synthetic control design and finds no consistent evidence that pandemic-related early releases in California's 58 counties affected violent or property crime rates. This paper contributes to the literature by (i) providing, to the best of my knowledge, the first nationwide analysis of this question, (ii) focusing specifically on gun violence as the outcome, and (iii) leveraging longer and more recent time series than earlier studies. My findings broadly corroborate Kubrin and Bartos (2024) and other localized estimates showing limited effects of law enforcement shocks on homicide rates.

III. Data and Sample

I use incident-level data from the Gun Violence Archive (GVA), an independent nonprofit organization established in 2013, as the primary source for firearm homicides in the United States. The GVA compiles daily records of firearm incidents drawing primarily from web-scraped sources and aggregated media reports, including local newspapers. I employ data from the GVA spanning 2015–2024, with each observation reporting the incident's geolocation (latitude and longitude), victim and offender demographics (when available), fatality status, arrest occurrence, and incident type. I exclude suicides, though a small number of homicide-suicide cases (wherein the offender dies by suicide immediately following the homicide) remain. The sample is restricted to incidents occurring within the contiguous United States.

Although the FBI's Uniform Crime Reporting (UCR) program—specifically the annual Supplementary Homicide Reports—is the official source for U.S. homicide statistics, I rely on the GVA for two reasons. First, UCR coverage declined substantially in 2021, the year when the national homicide peaked, capturing only 80 percent of the population and thus requiring imputation for non-reporting jurisdictions. Second, GVA firearm homicide counts closely align with UCR trends and in 2021 exceed UCR totals, illustrating underreporting problem of UCR (Figure A4). Nonetheless, I draw on UCR data in two auxiliary reasons: to validate the main results and to construct measures of non-firearm homicides, which the GVA does not capture by design.

The primary source for local jail population data is the Bureau of Justice Statis-

tics' Annual Survey of Jails (ASJ). The ASJ provides monthly, facility-level population counts for 2020 via a COVID-19 supplement. However, data for other years are limited to a single-day snapshot on the last weekday in June. To address this limitation, I assume that within-facility jail populations were approximately constant over each calendar year prior to 2020, consistent with modest observed seasonal variation. Following the sharp depopulation and subsequent rebound in 2020, I further assume a linear recovery in jail populations through the end of 2022.

These dynamics are not directly observable in the ASJ data due to the restricted temporal coverage. Hence, to validate these assumptions, I incorporate data from the Jail Data Initiative (JDI) run by New York University's Public Safety Lab. The JDI collects daily and individual-level jail roster records from hundreds of local jails using techniques largely analogous to those employed by the GVA. The JDI data indicate a sharp drop in jail populations between March and April 2020, a rapid rebound through the remainder of that year, continued recovery through mid-2022, and relative stability—with a mild downward drift—thereafter (Figure A5). This post-2020 pattern is consistent with the assumption of within-year stability in jail populations during the pre-pandemic period.

I include several covariates in main analysis. First, I use the poverty level estimates of each U.S. county from the Census Bureau's Small Area Income and Poverty Estimates (SAIPE) program to control for heterogeneous economic conditions. Second, I use data from the Bureau of Justice Statistics' Law Enforcement Management and Administrative Statistics (LEMAS) series to account for potential shifts in law enforcement capacity during the COVID-19 pandemic. For each county, LEMAS includes binary indicators for whether agencies reported reducing their police force in 2020 through foot patrol, car patrol, or arrest. For it relies on voluntary responses and lacks data on reduction size, however, the measure is a limited proxy for police effort at most.

Taken together, I construct a county-year panel of 791 U.S. counties by linking data from the Gun Violence Archive and the Annual Survey of Jails. Each geocoded GVA incident is assigned to its corresponding county. For tractability, I do not implement fuzzy geographic matching near county borders. In the ASJ, most local jails map uniquely to counties; where multiple facilities exist, I aggregate by summing the confined population across facilities. The resulting panel covers 25.2 percent of U.S. counties (3,144 in total) and approximately 67 percent of the national population as of 2020. [5]

Figure 2 visualizes the main panel that I use. The geographic coverage is concentrated in more populous regions, particularly along the coasts, the Midwest, and the Southeast. Although it omits many sparsely populated rural counties, it captures nearly all major urban centers and dense metropolitan corridors where

[5]Relative to the balanced time-series panel used in Figure 1, this sample exhibits lower ASJ attrition, as the analysis here requires only the percent change in jail population between February and April 2020 as a predictor. Jail population data from other years are not used.

firearm violence is most prevalent. This overlap between jail data availability and homicide incidence enables the construction of a high-coverage panel in terms of population despite the partial geographic sampling of ASJ.

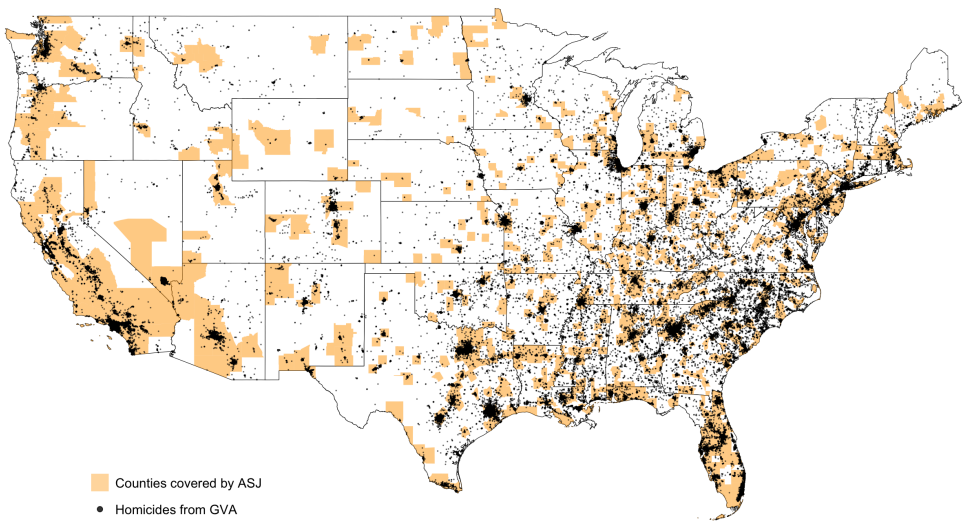


FIGURE 2. MAIN PANEL, VISUALIZED

PRELIMINARY CUT. — In Figure 3, I plot 'excess' homicide rates for each individual U.S. county i:

$$\frac{\text{Homicide}_i^{2020} - \text{Homicide}_i^{2019}}{\text{Population}_i^{2020}} (\times 100,000) \tag{1}$$

against the relative magnitude of jail population shock in the beginning of COVID-19:

$$\frac{\text{JailPop}_i^{Feb2020} - \text{JailPop}_i^{Apr2020}}{\text{JailPop}_i^{Feb2020}} (\times 100). \tag{2}$$

The vast majority of counties experienced a decline in jail population between February and April 2020, with both the mean and median reduction around 28%. In contrast, excess homicide rates—defined as the difference in homicides between 2020 and 2019 per 100,000 population—are centered around zero across the full sample, yielding an OLS estimate near zero (gray dotted line). Among

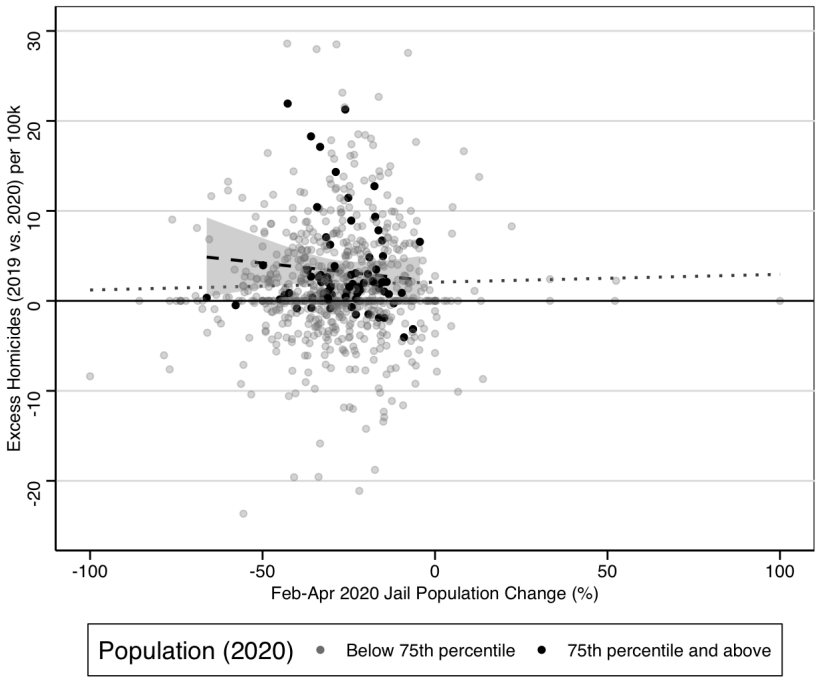


FIGURE 3. OBSERVATIONAL CUT: HOMICIDE VS. JAIL POPULATION

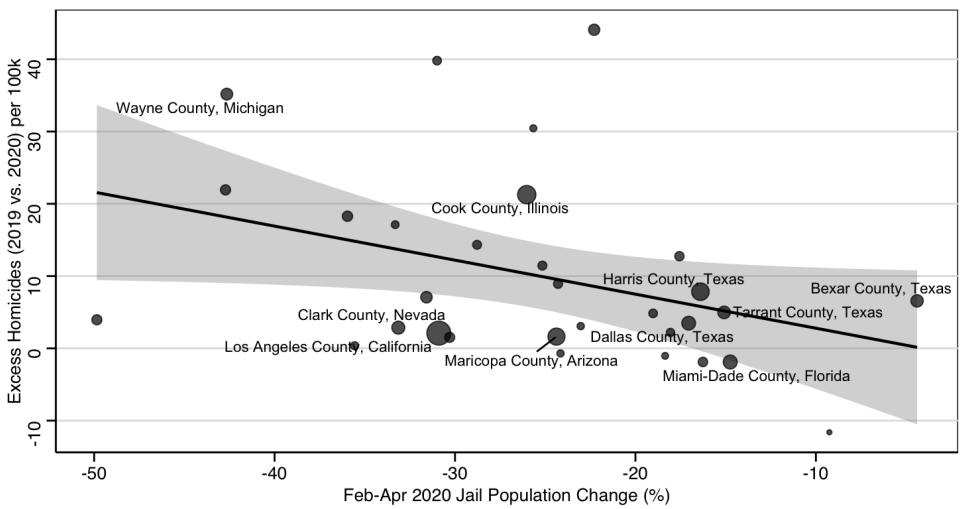


FIGURE 4. LARGEST COUNTIES (SIZE: POPULATION (2020))

counties in the top quartile of total population, however, the estimated effect of jail population change on excess homicides is negative, though statistically insignificant—suggesting that larger jail reductions may be associated with greater increases in homicides in more populous regions. Notably, most populous counties experienced positive excess homicides in 2020, diverging from the nationwide average (Figure 4)[6]. This pattern indicates population-dependent heterogeneity in the relationship between decarceration and lethal violence, even when using per capita measures. This motivates the heterogeneity tests appearing later in this paper. Moreover, the substantial variance in outcomes among less populous counties and high frequency of zeros in the homicide data supports the use of generalized linear models instead of simple linear regression.

IV. Effects of Jail Depopulation on Homicides

This section presents regression and event study estimates of the effect of jail population reductions in 2020 on homicide counts during and after 2020. I highlight two findings. First, from a set of zero-inflated models I find no consistent evidence that decarceration in 2020 increased homicides in that year. Second, the panel event study models that estimate the time-varying treatment effects of 2020 decarceration on homicides in subsequent years largely support the pooled regression results.

Zero-Inflated Model. — In this section, I present estimation strategies and results from Zero-Inflated Poisson (ZIP) and Zero-Inflated Negative Binomial (ZINB) models.[7] Several considerations motivate this choice. First, the outcome—number of homicides—is a count variable, encouraging the usage of generalized linear models with non-identity links. Second, the variance of homicides substantially exceeds the mean, violating the equidispersion assumption of the Poisson model. Third, the data contains many zero. Moreover, I assume that counties with zero versus positive homicides may be governed by distinct data-generating processes. Mixture models are needed to accommodate this. Specifically, following Hilbe (2014), I assume that the observed zeros are a mixture of "good zeros" (counties with truly no homicides) and "bad zeros" (counties with unobserved or misclassified homicides due to measurement error). Models such as the hurdle model, which attribute all zeros to a single censoring mechanism, are thus less appropriate under this assumption. ZIP and ZINB models allow for such heterogeneity in the zero-generating process and are standard tools for modeling excess zeros.

I define a new main predictor. The average jail population loss in 2020 is defined

[6]The variance is smaller in Figure A7 in which the relative change of homicides from 2019 to 2020, instead of excess homicides per capita as in 4, was used in the y-axis

[7]I use the `zip` and `zinb` commands in Stata.

as

$$\text{AJPL}_i := \frac{1}{10 \cdot \bar{P}_i} \sum_{m=3}^{12} \left(\bar{P}_i - P_{im} \right) \tag{3}$$

where $\bar{P}_i$ is the average jail population in county i during January and February 2020, and P_{im} is the population in month m. I treat $\bar{P}_i$ as the baseline jail population level at county i in 2020, based on the historical observations that jail populations are typically stable in the absence of external shocks. This new measure summarizes the total population shortfall over the ten months following the pandemic's onset, normalized by the pre-shock level. Unlike the percent drop from February to April, it captures both the magnitude and duration of decarceration, allowing for comparisons across counties that experienced different recovery patterns.

Let Y_i be the observed count of homicides in 2020 for county i. With probability π_i, Y_i is deterministically zero. With probability $1 - \pi_i$, Y_i is drawn from a count distribution conditional on covariates and exposure. π_i, the zero-inflation probability, is modeled using a logit link:

$$\pi_i = \frac{\exp\left(\mathbf{X}_i^\top \gamma\right)}{1 + \exp\left(\mathbf{X}_i^\top \gamma\right)}, \quad \mathbf{X}_i^\top = \left(1, \text{AJPL}_i, \mathbf{Z}_i^\top\right) \tag{4}$$

where $\mathbf{Z}_i^\top$ includes covariates such as (1) local poverty rate, (2) local average unemployment rate (between March and December 2020), and (3) average number of yearly homicides in the last 5 years (2015-2019). In the count component of the ZIP specifications, Y_i follows the Poisson distribution with the mean:

$$\lambda_i = \exp\left(\mathbf{x}_i^\top \beta + \log(\text{ pop }_i)\right) \tag{5}$$

where $\mathbf{x}_i^\top$ may include the same covariates as $X_i^\top$. Population in each county is considered as an exposure to the outcome. From this, the likelihood of Y_i can be written as:

$$P\left(Y_i \mid \lambda_i, \pi_i\right) = \begin{cases} \pi_i + (1 - \pi_i)\, e^{-\lambda_i}, & \text{if } y_i = 0 \\ (1 - \pi_i)\, \frac{\lambda_i^i}{y_i} e^{-\lambda_i}, & \text{if } y_i > 0. \end{cases} \tag{6}$$

In the ZINB model, the count component is instead governed by a negative binomial distribution with overdispersion parameter α. Proceeding similarly, the

likelihood for the ZINB model is given by:

$$P\left(Y_i \mid \lambda_i, \pi_i, \alpha\right) = \begin{cases} \pi_i + (1-\pi_i)\left(\frac{1}{1+\alpha\lambda_i}\right)^{1/\alpha}, & \text{if } y_i = 0 \\ (1-\pi_i)\cdot f\left(y_i \mid \lambda_i, \alpha\right), & \text{if } y_i > 0 \end{cases} \tag{7}$$

Table 1 summarizes the estimation results. ZINB models (column (3) and (4)) yield lower information criteria, and the significant overdispersion parameter confirms better fit in the count process than ZIP models. The models with a full set of covariaties (column (2) and (4)) suggest that a larger jail population reduction is positively associated with more homicides in 2020 after adjusting for population exposure but with low statistical power.[8]. Poverty rates and historical homicide averages are positive and significant predictors in all models. The unemployment shocks, however, show no significant effect. Estimates from the inflation model support heterogeneous zero-generation. The coefficient on historical homicides is large and negative across all specifications, implying that counties with higher past homicides are less likely to produce structural zeros. Other inflation covariates—average loss, poverty, and unemployment shocks—are not consistently significant. For robustness, I estimate alternative specifications across other GLMs (e.g. hurdle model), different dataset (UCR), and alternative measures (e.g. Feb-Apr reduction rate). In all variations, the coefficient on AJPL remains consistently small and statistically insignificant (Table TBD).

Event Study Analysis. — I estimate the effect of jail population reduction on homicide outcomes using linear and Poisson two-way fixed effects (TWFE) models. The linear TWFE specification is:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{\substack{k=2016 \\ k\neq 2019}}^{2024} \beta_k \left[\mathbf{1}\{t=k\}\cdot D_i\right] + X_{it}'\delta + \varepsilon_{it} \tag{8}$$

where Y_{it} denotes homicides per capita in county i and year t, α_i and γ_t are county and year fixed effects, and X_{it} includes time-varying covariates (e.g., poverty rate). The coefficients β_k capture differential changes in outcomes for counties with higher values of D_i, relative to 2019. All results presented henceforth exclude time-varying covariates from the specification.

I consider three definitions of D_i. First, $D_i = \mathrm{LR}_i$, the Average Loss in jail population between March and December 2020, where treatment is continuous. Second, I define a binary treatment indicator based on the median of LR, classifying counties above the median as treated. Third, I restrict the sample to counties in the top and bottom quartiles of LR_i, defining treatment accordingly

[8]See Table TBD for coefficients scaled as incidence rate ratios (IRRs).

Table 1—Zero-Inflated Model Results

	Dependent variable: Firearm Homicides in 2020			
	(1)	(2)	(3)	(4)
Count model	ZIP	ZIP+Ctrls	ZINB	ZINB+Ctrls
Average Loss	−0.060	0.015	−0.017	0.005
	(0.039)	(0.055)	(0.060)	(0.014)
Poverty Rate (%)		12.798***		11.022***
		(1.801)		(1.001)
Unemployment Shock		−0.145		−0.026
		(0.123)		(0.105)
Avg. Homicide (2015–2019)		0.002***		0.010***
		(0.000)		(0.002)
Constant	−9.762***	−11.484***	−9.939***	−11.715***
	(0.098)	(0.280)	(0.088)	(0.182)
Inflation Model (Logit)				
Avg. Homicide (2015–2019)	−1.556***	−1.402***	−4.339**	−2.448***
	(0.226)	(0.219)	(1.769)	(0.788)
Average Loss		0.145		0.152
		(0.121)		(0.140)
Poverty Rate (%)		−0.934		−1.788
		(2.448)		(4.310)
Unemployment Shock		−0.735**		−0.900
		(0.365)		(0.570)
Constant	0.341	0.804	0.412	0.791
	(0.210)	(0.583)	(0.380)	(1.073)
Dispersion (ZINB only)				
$\ln \alpha$			−0.161*	−1.063***
			(0.096)	(0.097)
α			0.851	0.345***
			(0.082)	(0.034)
Observations	791	791	791	791
Population Exposure	Yes	Yes	Yes	Yes
AIC	13124.1	7878.7	4239.8	3829.5
BIC	13142.7	7925.4	4263.2	3880.9

*p<0.1; **p<0.05; ***p<0.01

and excluding the middle 50 percent.

In the continuous specification, β_k represents the change in homicides per unit increase in jail population reduction in year k; in the binary specifications, β_k captures average differences in outcomes between treated and control counties. The identifying assumption is that, in the absence of jail population reductions, counties with different values of D_i would have followed similar trends.

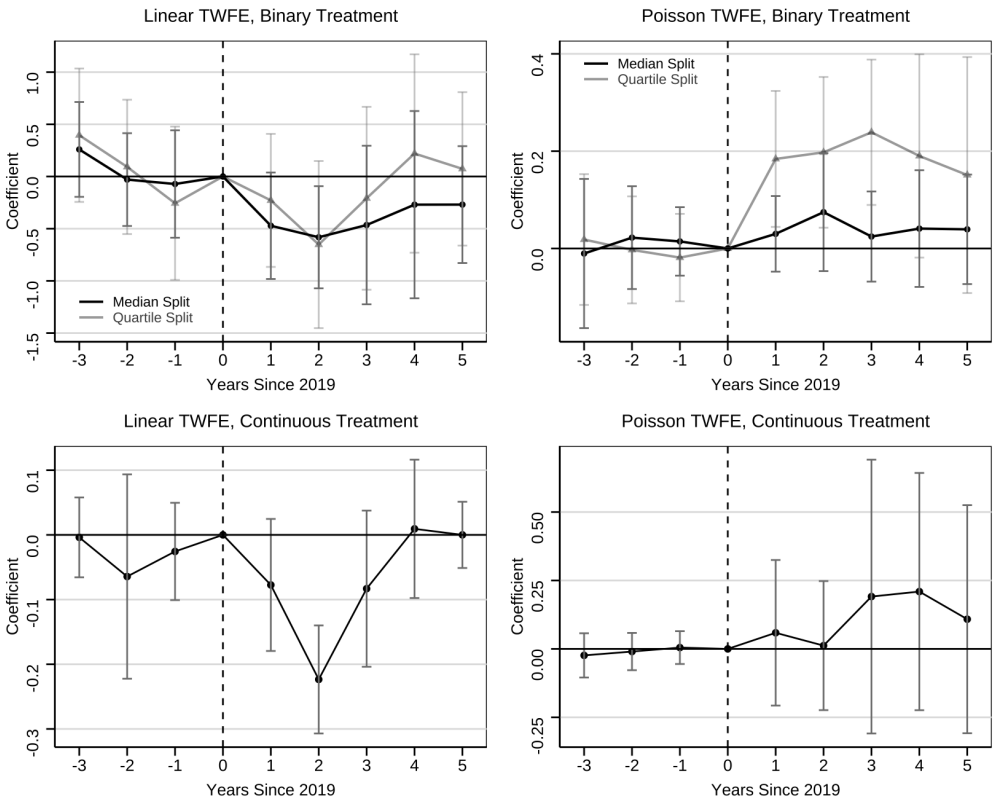


Figure 5. Event Study Results

The Poisson TWFE model is given by:

$$\log\left(\mathbb{E}\left[Y_{it}\right]\right) = \alpha_i + \gamma_t + \sum_{\substack{k=2016 \\ k\neq 2019}}^{2024} \beta_k \left[\mathbf{1}\{t = k\} \cdot D_i\right] + X_{it}'\delta + \log\left(P_{it}\right) \tag{9}$$

where Y_{it} is homicide count and P_{it} is county population, included as an exposure. I choose the Poisson link function as it is known to uniquely avoid the inci-

dental parameters problem present in other GLM models with high-dimensional fixed effects.

Figure 5 presents the estimated β_k coefficients over time for each model, with standard errors clustered at the state level. The left panel indicates possible violations of the parallel trends assumption in the linear models, as the pre-treatment coefficients are unstable and post-treatment effects show no clear pattern. In contrast, the Poisson models exhibit more consistent pre-trends, especially under the continuous treatment definition. While post-2020 coefficients are generally positive across the Poisson specifications, they are estimated imprecisely, with wide confidence intervals. Among the three binary approaches, the 25/75 split—using only the top and bottom quartiles of the treatment distribution—produces larger point estimates and marginally significant effects in 2021 and beyond. This may reflect treatment effect heterogeneity, suggesting that the impact of jail population reductions was concentrated in counties with more extreme changes. However, by dropping the middle 50 percent of observations, this approach risks introducing selection bias. Overall, the Poisson event study results are suggestive of a positive dynamic effect of decarceration on homicides, but the evidence remains inconclusive in the absence of stronger identification or more precise estimates.

Note that the generally positive coefficients from the Poisson event study specification, in contrast to the negative coefficients from the linear specification, are largely attributable to differences in baseline outcome levels. This mechanical divergence is straightforward to observe in simple illustrations, particularly given that the main specifications exclude covariates.

	Outcome: N of homicides per 100K	
Group	Pre(2019)	Post(2020)
Control	3.74	5.15
Treatment	2.40	3.34

Table 2—Linear vs. Poisson

Table 2 reports the 2×2 sample means from the median-split treatment regime. The estimated linear DiD coefficient,[9]

$$(\Delta Y_{\text{Treatment}}) - (\Delta Y_{\text{Control}}) = (3.34 - 2.40) - (5.15 - 3.74) = -0.47 < 0,$$

is negative. In contrast, the estimated Poisson (log-linear) DiD coefficient,[10]

$$\log(3.34/2.40) - \log(5.15/3.74) \approx 0.01 > 0,$$

[9] Visually apparent in the first panel of Figure 5.

[10] While slightly different from the primary specification, this formulation is used here for illustrative purposes.

is weakly positive. This discrepancy arises from the larger baseline means in the control group, which experienced smaller proportional changes than the treated group.

Figure A8 restricts the sample to 44 counties with populations exceeding one million as of 2020. In this subsample, baseline outcomes between treated and control groups are nearly identical in the pre-period. The left panel shows that linear event-study coefficients are no longer significantly negative. Thus, abstracting from the argument that Poisson models are more suitable for count outcomes, these findings suggest that the divergence in coefficient signs across specifications may reflect sample composition rather than inherent model inconsistency.

Quantile Regression. — The exploratory observations in Figure 3 and 4 suggested potentially heterogeneous treatment effects of jail population reduction driven by the baseline outcome levels. In this section, I use a simple quantile regression model to examine whether the jail population reduction had greater relative impacts on counties with already large number of homicides. However, since number of homicides is count data, the standard quantile regression is not well specified. I use the jittering method suggested by Machado and Santos Silva (2005) to address this[11]. Specifically, the model implements standard quantile regression with the following tranformed specification:

$$\log(\max((Y_i \underbrace{+\text{Uniform}(0,1)}_{\text{jittering}}) - \tau, \delta)) = X_i'\beta + \epsilon_i, \quad Q_{\epsilon_i|X_i}(\tau) = 0 \tag{10}$$

where Y_i is the number of homicides in 2020, τ is the quantile of interest, and X_i' is a vector of covariates that includes LR_i and population control. The left panel of Figure 6 shows that the effect of jail population reduction is increasing in quantiles of the number of homicides. For counties with less than or equal to three homicides in 2020, the effects of the depopulation on homicides were close to zero or even negative. By contrast, the counties that had greater number of homicides in 2020 experienced greater response from jail population reduction. In the right panel of Figure 6, I keep the counties with 10 or more homicides in 2020 (approximately the fourth quartile of the orginal dataset) and run the same model, showing positive coefficients again in most of the granularized set of quantiles but with greater variability. These results may explain the conflicting visual trends in Figure 3 and Figure 4. For all these point estimates, however, the clustered standard errors are too big to be valid.

V. Additional Results

In this section, I discuss some additional insights that we can learn from this data. The purpose of this analysis is twofold. First, I study whether the jail

[11] I use `qcount` command in STATA for computation

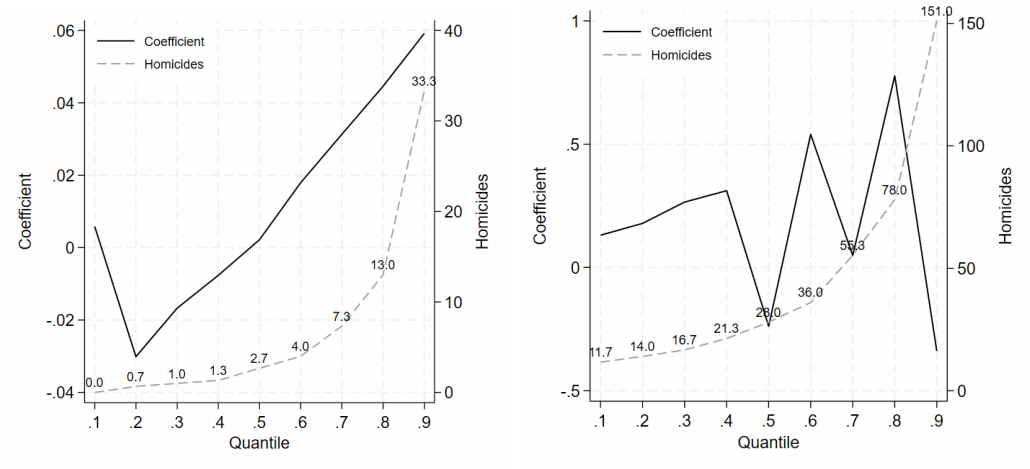


Figure 6. Quantile Regression Results (Left: All Counties, Right: Counties with more than 10 Post-2020 Average Homicides)

population reduction increased the the number of firearm homicides *given* the same number of total firearm incidents. Second, using available data I examine whether there was a local interaction effect between jail population reduction and increase in police effort during the pandemic.

Effects on Lethality. — Throughout this paper, I defined the outcome variable as the number of firearm homicides. However, we saw in Figure A6 that the ratio between fatal non-suicidal gun violence incidents (i.e. firearm homicides) and the total firearm incidents have been very stable throughout multiple years except for the post-pandemic shock. In this section, I examine whether the jail population reduction was indeed associated with the shock. I use a simple linear regression model given by:

$$\begin{aligned} &\frac{Lethality_{i,2020-2024} - Lethality_{i,2015-2019}}{Lethality_{i,2015-2019}} \\ &= \alpha + \beta_1 NL_i + \beta_2 NL_i^2 + \beta_3 \cdot Lethality_{i,2015-2019} + X_i'\gamma + \epsilon_i \end{aligned} \tag{11}$$

where $Lethality_{i,T}$ is defined by the the number of firearm homicides divided by the number of total firearm incidents in county i between time range T. The quadradic term was introduced to reflect the potential nonlinear effects of jail population reduction on lethality shift. Also, because I'm interested in the subset of counties that experienced meaningful increase in lethal firearm violence, I only focus on counties with strictly positive hoimicdes in 2020 and further use four different minimum homicide cutoffs (0, 5, 10, 50) to measure whether the effect

sign and significance is constant across subpopulations. Table 3 suggests that, across all settings, jail population reduction has increased the lethality of gun violence given the same pre-pandemic baseline lethality and other controls. The negative coefficient on the quadratic power term indicates that this effect may decrease as jails miss more inmates.

Table 3—Effects on Gun Violence Lethality

	Dependent variable: Relative Lethality Difference			
	(1)	(2)	(3)	(4)
Average Loss	0.075^{*}	0.133^{*}	0.155	0.173^{**}
	(0.041)	(0.068)	(0.092)	(0.084)
Average Loss^2	0.0034^{**}	-0.214^{*}	-0.298^{*}	-0.524^{*}
	(0.0017)	(0.112)	(0.167)	(0.267)
Avg. Lethality, 2015-2019	0.807^{***}	0.499^{***}	0.470^{***}	0.477^{***}
	(0.058)	(0.038)	(0.057)	(0.095)
Poverty Rate (%)	-0.326^{***}	-0.027	0.030	0.103
	(0.106)	(0.114)	(0.120)	(0.257)
Unemployment Shock	0.048^{***}	0.002	-0.004	0.002
	(0.017)	(0.013)	(0.012)	(0.017)
Population	$-2.72\times10^{-8}{}^{***}$	$-2.05\times10^{-8}{}^{***}$	$-1.74\times10^{-8}{}^{***}$	$-1.21\times10^{-8}{}^{**}$
	(6.22×10^{-9})	(4.33×10^{-9})	(4.86×10^{-9})	(5.87×10^{-9})
Constant	-0.251^{***}	-0.125^{***}	-0.123^{***}	-0.156^{**}
	(0.037)	(0.033)	(0.032)	(0.076)
Homicide Cutoff	0	5	10	50
Observations	705	275	183	61
R^2	0.471	0.412	0.401	0.453

$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$

Interaction with Police Effort. — In the first part of Section IV, the zero-inflated models showed that jail population reduction had largely insignificant effects on firearm homicides. In this section, I investigate whether this null result may stem from the jail population measure capturing only a limited dimension of broader law enforcement dynamics. As an alternative, I incorporate police effort indicators from the FBI's LEMAS data and interact them with the jail population reduction rate. The LEMAS survey reports whether counties reduced police presence through changes in arrest decisions or foot patrols. After merging the previously used panel with the LEMAS data, the final sample includes 538 counties. I include both the main effects and their interactions with the normalized jail population loss. Table 4 shows that the interaction effects between jail population reduction and either police effort indicator are statistically insignificant.

While the interaction between jail population reduction and arrest effort yields a positive coefficient—suggesting that simultaneous reductions in incarceration and policing may have increased homicides—the estimates are imprecise and do not allow for strong conclusions.

VI. Discussion

This section briefly outlines alternative mechanisms that may have contributed to, or predicted, the post-COVID surge in firearm homicides in the United States but are not explicitly examined in this paper. First, firearm acquisitions rose markedly during the early stages of the pandemic. I do not control for this factor, primarily due to the absence of a reliable source of county-level data on gun purchases. Second, this study implicitly treats the Ferguson effect and the associated depolicing movement as transient—assuming that their influence diminished by the end of 2020. However, these forces may have exerted more persistent effects on violent crime and police behavior well beyond that period. Third, the relationship between baseline economic conditions or adverse economic shocks and violent crime is extensively documented. Although I include some economic indicators as control variables, I do not explore this mechanism in depth. Some prior work finds that income transfers have limited efficacy in reducing gun violence (Ludwig and Schnepel (2024)). Whether negative shocks exacerbate homicides or gun violence, however, remains an open and important empirical question. Fourth, a growing application of non-parametric or machine learning-based models on crime study could also provide better empirical insights to this research question, whereas only parametric models were fitted in this paper. Lastly, an alternative to the event-study design employed here would be an instrumental variables approach to address simultaneity between incarceration and crime. For instance, Johnson and Raphael (2012) proposes a stylized model of incarceration with a single crime type, constructs a two-state transition matrix (incarcerated/not incarcerated), and uses the mechanically predicted change in crime as an instrument. The empirical patterns documented in this paper, however, underscore the need to disentangle shifts in criminality from changes in violence in order to explain how rare, high-lethality incidents can surge even absent broad increases in crime rates.

VII. Conclusion

This paper examines whether the mass, admission-driven decarceration during the early COVID-19 pandemic contributed to the sharp rise in firearm homicides in the United States. Using incident-level gun violence data and jail population records from a high-coverage panel of counties, I find no evidence from zero-inflated Poisson and negative binomial models that reductions in jail population in 2020 increased firearm homicides in the same year. Event study designs show

Table 4—Zero-Inflated Models with Police-Effort Interactions

	Dependent variable: Firearm Homicides in 2020			
	(1)	(2)	(3)	(4)
Count model	ZIP	ZIP+Ctrls	ZINB	ZINB+Ctrls
Average Loss	−1.010*	0.167	−0.543	−0.077
	(0.517)	(0.417)	(0.402)	(0.234)
Norm. Loss × Arrest	0.967*	−0.172	0.552	0.093
	(0.517)	(0.409)	(0.408)	(0.236)
Norm. Loss × Foot Patrol	−0.447	0.843*	−1.010**	−0.374
	(0.630)	(0.488)	(0.486)	(0.307)
Poverty Rate (%)		13.164***		11.993***
		(2.042)		(1.152)
Unemployment Shock		−0.169		−0.013
		(0.132)		(0.117)
Avg. Homicide (2015–2019)		0.002***		0.010***
		(0.000)		(0.003)
Constant	−9.690***	−11.531***	−9.842***	−11.784***
	(0.093)	(0.311)	(0.093)	(0.196)
Inflation model (logit)				
Avg. Homicide (2015–2019)	−1.626***	−1.641***	−0.009*	−3.642*
	(0.316)	(0.337)	(0.005)	(2.155)
Average Loss		0.714		1.124
		(1.050)		(2.188)
Poverty Rate (%)		−2.464		−5.717
		(3.948)		(7.188)
Unemployment Shock		−1.092**		−1.893
		(0.498)		(1.153)
Constant	0.305	1.289	−21.077***	2.402
	(0.286)	(0.888)	(0.332)	(1.889)
Dispersion (ZINB only)				
$\ln \alpha$			−0.103	−1.049***
			(0.101)	(0.106)
α			0.902	0.350***
			(0.091)	(0.037)
Observations	538	538	538	538
Population Exposure	Yes	Yes	Yes	Yes
Interactions	Yes	Yes	Yes	Yes
AIC	11351.3	6871.8	3478.6	3103.4
BIC	11377.0	6923.2	3508.6	3159.1

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

a positive but statistically inconclusive effect of decarceration on post-2020 homicide counts, with larger reductions in jail population weakly associated with higher homicide rates in the years that followed. In terms of heterogeneity, exploratory plots and quantile regressions revealed that counties with higher baseline homicide levels experienced larger estimated increases in homicides following the jail population reduction. While these results do not establish a causal link between decarceration and lethal violence, they challenge the notion that short-term reductions in jail populations broadly undermined public safety. Instead, the evidence suggests that the effects were uneven—concentrated in specific high-risk areas. This underscores the need for more targeted policies tailored to vulnerable communities and highlights the importance of more detailed data on pretrial detention and post-release outcomes.

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Appendix Figures

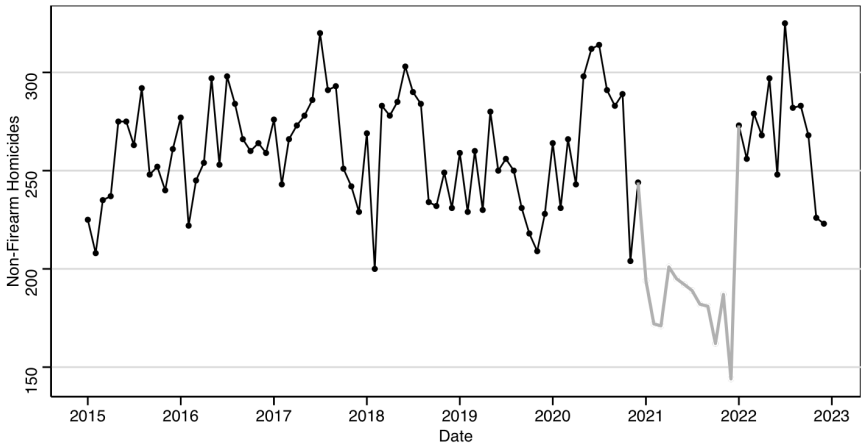


Figure A1. Non-Firearm Homicides Trend

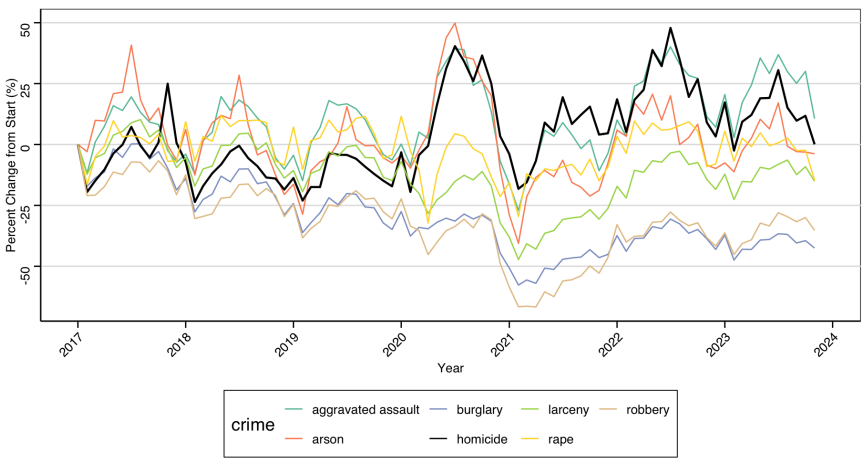


Figure A2. Monthly Trend, FBI Type I Offense (2017–2024)

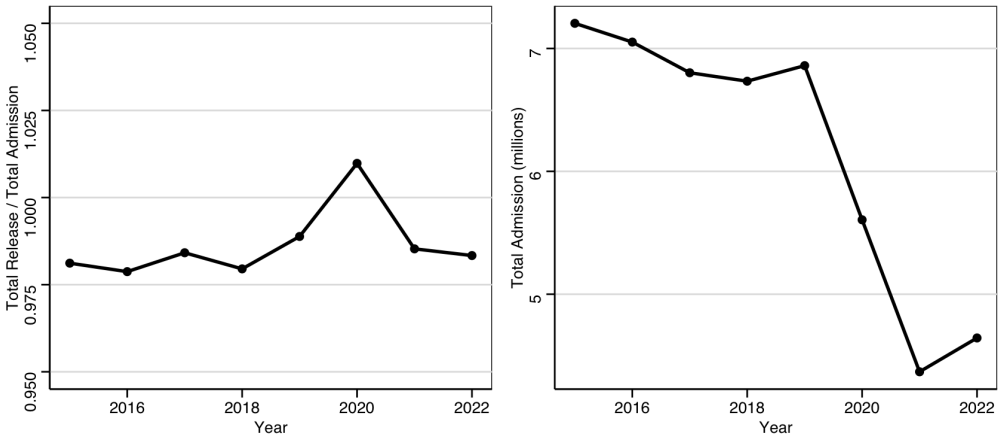


Figure A3. Admission vs. Release

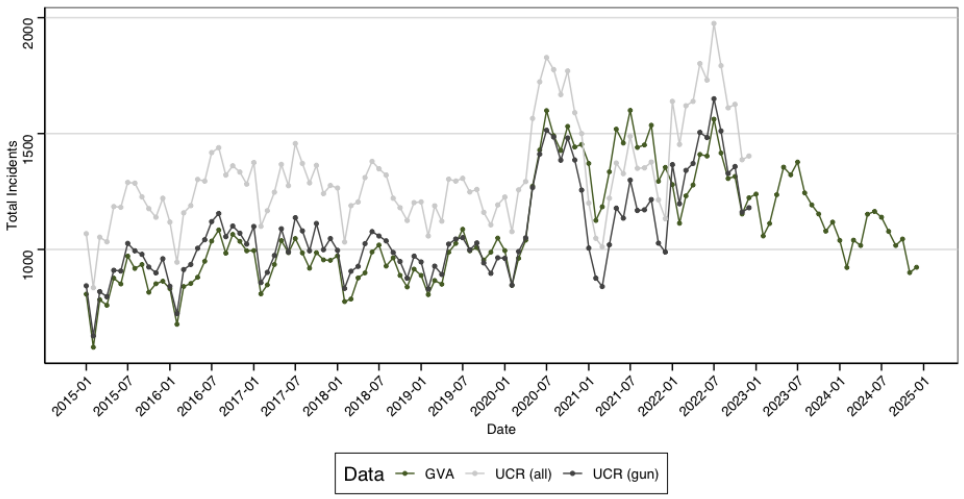


Figure A4. Gun Violence Archive vs. Uniform Crime Reporting

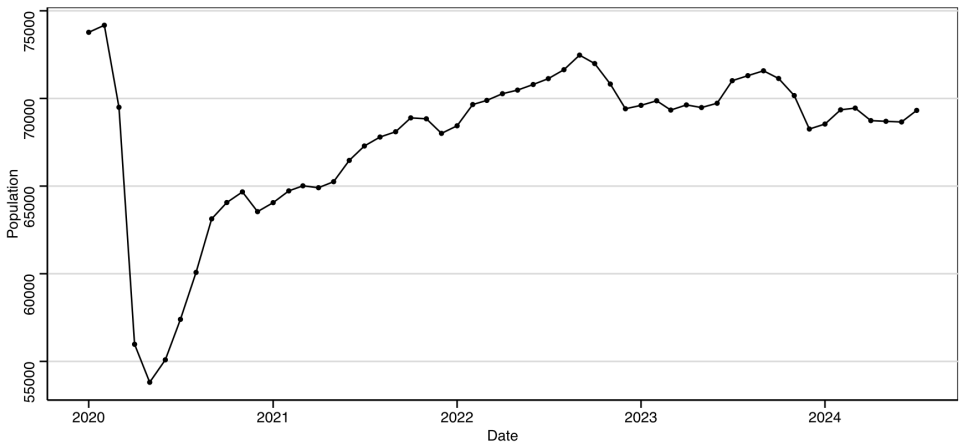


FIGURE A5. JAIL POPULATION TREND FROM JAIL DATA INITIATIVE

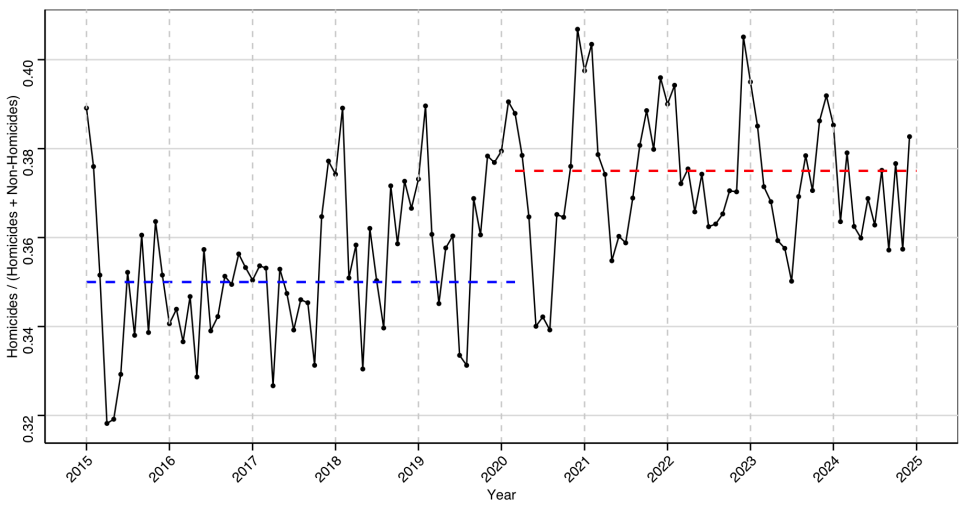


FIGURE A6. GUN VIOLENCE LETHALITY TREND: PRE VS. POST PANDEMIC

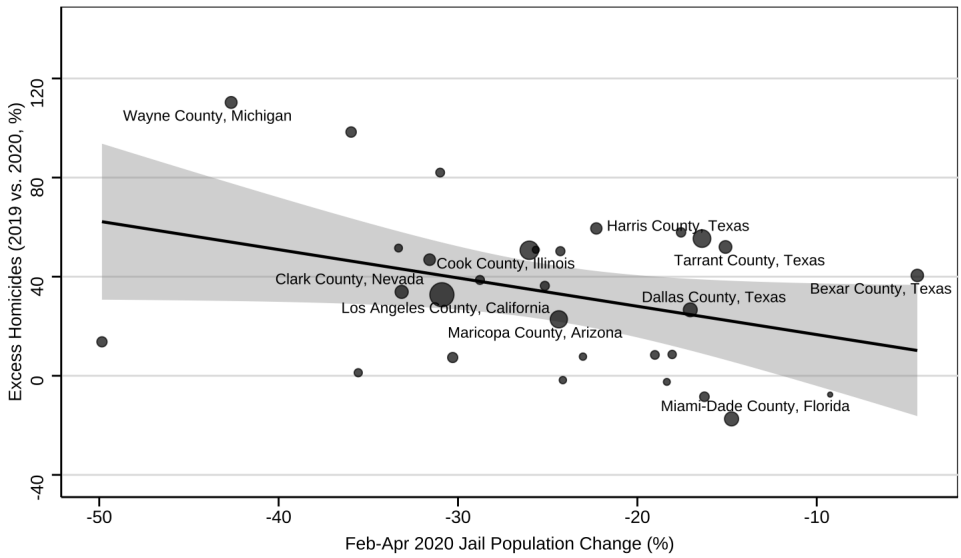


Figure A7. Largest Counties (Size: Population (2020)) - Outcome: Relative Change

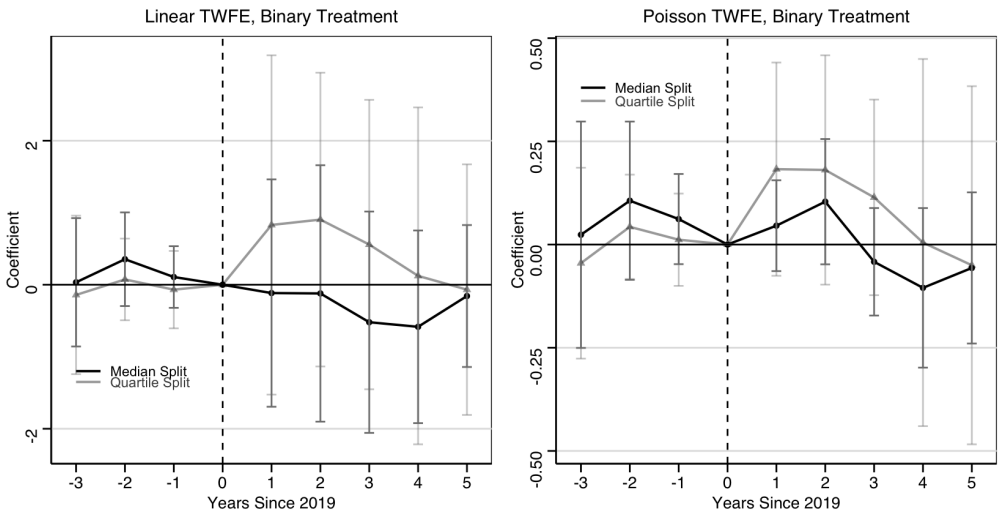


Figure A8. Event Study Results (Population > 1 Million)