Local Crime and Gun Demand: Evidence from Handgun Transactions in California

Shun Yamaya*

Jonathan Rodden†

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Abstract

While Americans cite protection from crime as the primary reason to buy a gun, researchers have lacked the requisite data to assess this claim. We pair geocoded administrative records of all licit handgun purchases in California (2008–2020) with crime data from law enforcement agencies and state mortality records, allowing us to observe the link between crime and guns at a granular level over a long temporal span. Across identification strategies, we find a weakly positive relationship between crime and gun purchases, but the effect is substantively small, unstable, and driven by repeat purchasers. While violent crime clusters near city centers, gun purchases are concentrated in suburban and rural peripheries, and the distribution of gun purchases has shifted in ways that track home ownership and partisanship rather than crime. Our findings reject the claim that crime is a major driver of firearm consumption and highlight the importance of perceptions and politics.

*Department of Political Science, Stanford University

†Department of Political Science, Stanford Institute for Economic Policy Research, and Hoover Institution, Stanford University

1 Introduction

Firearm acquisition has risen rapidly in the United States in recent decades. Debates over gun control and the Second Amendment play a central role in the cultural battles that have permeated contemporary American politics. Beyond politics, firearm ownership is associated with risks of suicide and accidental death [Studdert et al., 2020, Rosenberg et al., 2025], and in 2024 the Surgeon General declared gun violence a national public health crisis [Office of the Surgeon General, 2024]. Understanding where and why Americans buy guns is therefore both a political and public health priority.

This paper addresses a dominant claim in both public debate and academic research that gun purchases are driven by concerns about crime. Personal safety is by far the most frequently cited reason Americans provide for buying guns [Newton and Zimring, 1969, Swift, 2013, Dimock et al., 2013, Ward et al., 2024]. In Gallup surveys, the share of gun owners citing protection against crime rose from 67 percent in 2000 to 88 percent in 2021 [Jones, 2021]. Scholars have argued that local crime fuels a vicious circle: higher crime drives protective firearm purchases[Kleck and Patterson, 1993, Southwick, 1997, Kleck and Kovandzic, 2009, Kovandzic et al., 2012, 2013, Kleck, 2015], which in turn raise the risk of suicide, accidental death, and homicide [Kleck, 1979, McDowall and Loftin, 1983, Studdert et al., 2022]. This perception is reinforced by the common observation that the United States leads other wealthy countries in both violent crime and gun ownership [Grinshteyn and Hemenway, 2019].

Is firsthand experience with local crime an important driver of American gun ownership? The answer to this basic question has been elusive. Previous studies have relied on survey responses or data aggregated to states, counties, or cities, producing mixed findings and limited conclusions. A central obstacle has been the lack of high-quality and high-resolution data on both gun purchasing and crime measured at the same geographic level. This overcomes that obstacle by combining individual firearm sales records in California and geocoded incidents of violent crime and state mortality records. With these data, we test the hypothesis that exposure to local crime leads to gun purchases. Our analysis casts serious doubt on the claim that experience with local crime is a primary, or even significant, driver of gun demand. Instead, we show that gun purchases increasingly correlate with neighborhood characteristics such as home ownership rates and Republican vote share.

To generate these findings, we assemble and combine four novel data sets:

1. Individual geo-coded data on all lawful handgun purchases in California from 2008 to 2020,

2. Individual-level geocoded data on homicides during the same period,

3. Aggregate violent crime statistics and street-level crime from law enforcement agencies,

4. Geospatial boundary files for law enforcement agency (LEA) jurisdictions.

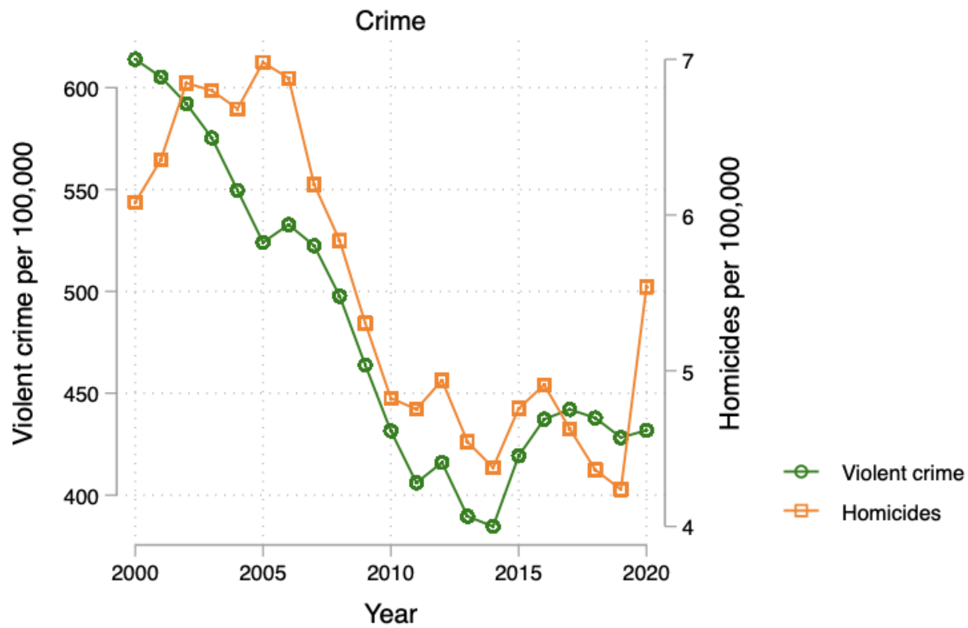
These data are unprecedented in their granularity, coverage, and ability to differentiate between first-time purchases and additional acquisitions. This level of detail is made possible by the unusually assiduous data collection efforts of the California Department of Justice, which records point-of-sale data on all handgun purchases, specifying who bought what, where, and when. California also provides consistent crime data, with LEAs consistently reporting standardized information annually and state mortality records detailing specific circumstances of death. We merge these administrative data using geospatial boundary files both at the level of police jurisdictions, which usually correspond to cities, and the level of small neighborhoods (census tracts) within cities. This approach removes many of the empirical concerns plaguing previous work and brings new evidence to a decades-old question.

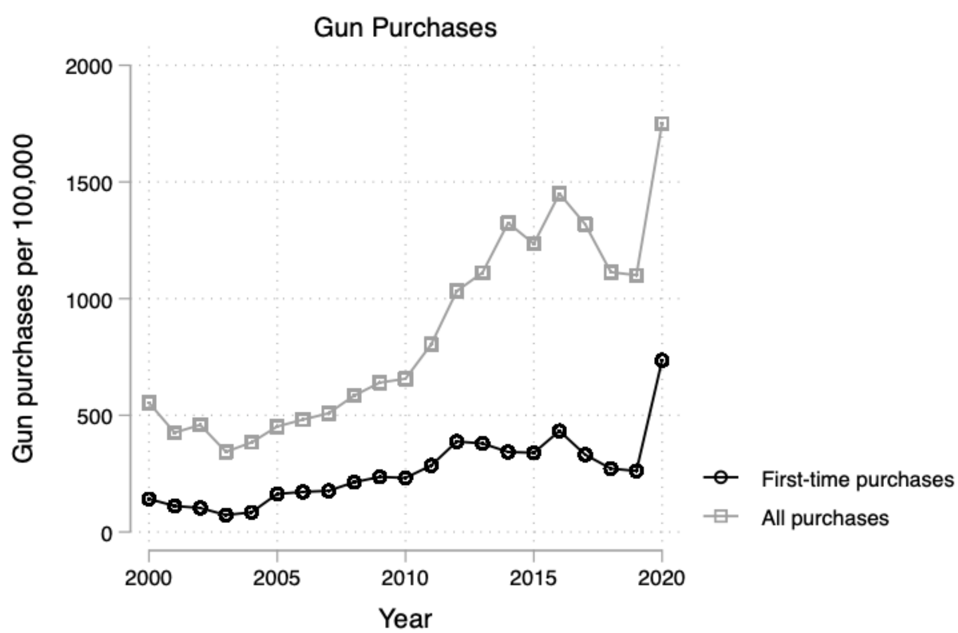
To begin, it is useful to examine aggregate statistics on crime volume and handgun purchases, displayed in Figure 1. Already, these aggregate trends suggest that the link between local crime and gun demand may be weak. First, as is true in the United States more generally, violent crime and homicides have been trending downward in California, while handgun purchases have been steadily increasing.[1] Second, much of the recent upward trend in gun purchases is driven by repeat buyers.[2] If localized fear of victimization is driving increased gun purchases and if the initial gun

[1]Note that data reported to the FBI by law enforcement agencies do not include unreported crimes. However, the downward trend in violent crime is also evident, both nationally and in California, in the National Crime Victimization Survey conducted by the Census Bureau. For aggregate national statistics, see Lauritsen and Lopez [2025]. For California, see Tinney and Thompson [2025]. The same is true of the separate California homicide data we received from mortality records.

[2]While Figure 1 shows a rise in the number of first-time purchasers, the proportion of existing owners has consistently accounted for around 70% of all sales, with the notable exception of 2020 during the COVID-19 pandemic, when new owners comprised 42%.

Figure 1: Trends in crime and guns





Note: The top panel plots violent crime per 100,000 people, including murder and nonnegligent manslaughter, rape, robbery, and aggravated assault, assembled from FBI reports, with the scale indicated on the left-hand vertical axis, and homicides per 100,000 people, from the same FBI data source, with scale indicated on the right-hand axis. The bottom panel plots first-time gun purchases and repeat purchases per 1000,000 people, as reported by the California DOJ.

is more important for protection than a second or third gun, we would expect the surge in gun purchases to be driven by first-time buyers.

Moving beyond these statewide aggregates, this paper leverages fine-grained data on the timing and location of gun purchases and crime. We report three key takeaways from our work. First, we conduct a number of panel-based statistical analyses to test the crime-gun hypothesis. Overall, the results reveal a consistent pattern: there is little evidence of a strong relationship between local crime rates and gun purchases, whether at the city or neighborhood level. In the majority of cases, the effect is not distinguishable from zero, but in some specifications we do find statistically significant increases in gun purchasing following crime events. Thus, while we do find some support for the crime-gun hypothesis, we conclude that it is at best a weak connection and far from the primary factor driving the surge in gun purchases observed in recent years.

Next, in order to better understand these small and inconsistent effects of crime shocks on gun purchases, we conduct two additional descriptive exercises. First, we explore patterns in the geography of guns and crime, and discover that crime tends to be relatively consistently concentrated near city centers, whereas gun purchases are concentrated on the urban periphery and in rural areas. It is difficult to escape the conclusion that the distinctive geography of gun purchases corresponds to some other cultural or social factors that are largely orthogonal to acute crime risk.

Third, to incorporate these factors and place the role of crime into context, we run a series of repeated cross-section regressions and compare the estimated effects of crime with the effects of other demographic and social characteristics of neighborhoods and LEAs. Consistent with an emerging literature in political science [Lacombe, 2019, 2021], we demonstrate that by far the best predictor of the geography of gun purchases is political partisanship, and the effect is much larger than that of violent crime.

Taken together, these findings suggest that the crime–gun connection, if it exists at all, is best understood as a response to diffuse perceptions of danger, filtered through social and political context. Prior research in criminology and psychology shows that fear of crime often reflects broader anxieties about social disorder or decline rather than concrete fears of victimization [Ferraro, 1995, Warner and Thrash, 2020]. Survey evidence likewise finds that gun owners and prospective buyers report elevated concern about crime [Kleck et al., 2011, Hauser and Kleck, 2013].

Our findings relate to political science research demonstrating that beliefs about crime and the costs and benefits of protective gun ownership have become folded into partisan conflict and broader struggles over cultural identity in the United States [Melzer, 2012, Lacombe, 2019]. Fears about crime are increasingly amplified through partisan messaging and media ecosystems, shaping how individuals interpret local conditions and national trends [Gilliam Jr and Iyengar, 2000, Dyck and Pearson-Merkowitz, 2023]. Our results suggest that protective gun purchases may best be understood not as a direct response to local crime, but as an expression of perceived diffuse social threat mediated by political worldviews, media consumption, social networks, or messaging strategies of political parties and aligned interest groups [Lacombe, 2019, Lacombe et al., 2019, Lacombe, 2021].

1.1 Toward an understanding of the crime-gun connection

A long line of research attempts to explore the empirical relationship between fear of crime, local experience with crime, and gun purchases. For decades, scholars have used surveys and administrative data to examine the notion that gun purchases are driven by fears of crime rooted in local experience. More recently, scholars have introduced the possibility that fears about crime, and associated gun purchases, have more to do with diffuse fears about social disorder that are rooted in ideology and politics rather than in experiences with crime in one's neighborhood or city.

Protective gun purchase as response to acute local threat: Perhaps the clearest aggregate evidence in support of the notion that violence facilitates firearm purchases comes from the observation that purchases tend to spike in the wake of violent civil unrest [Clotfelter, 1981] or mass shootings [Levine and McKnight, 2017, Laqueur et al., 2019, Studdert et al., 2017]. But violent riots and mass shootings are rare events, and the estimated impact of such incidents can only explain a limited portion of overall gun demand in the United States. Moreover, the impacts of these events on gun purchases spill over well beyond the locations where unrest or mass shootings take place.

When it comes to experience with daily crime, the evidence in the literature is mixed. Survey research has extensively addressed the possibility that victimization or experience with local crime drives gun purchases. In a study of Cincinnati residents, Cao et al. [1997] find that concern over neighborhood-level crime is associated with increased reported gun purchases. Kleck and Kovandzic [2009] found that respondents living in cities with higher crime rates report higher gun ownership rates. Kleck et al. [2011] find that perceived risk of crime and prior victimization were significant predictors of individual gun ownership and plans for future ownership. Hauser and Kleck [2013] find fragile evidence in a panel survey that people who are afraid of crime in an initial interview are more likely to have acquired a gun by the second interview, and that those who report being crime victims are more likely to acquire guns.

On the other hand, a number of similar studies have also cast doubt on the notion that local crime influences purchases. Williams and McGrath [1976] find no impact of reported victimization or fear about neighborhood crime on gun ownership. Hauser and Kleck [2013] find no evidence that local crime rates have an impact on reported purchasing. Moreover, researchers have documented several observable inconsistencies with the fear of crime hypothesis. Demographic groups that are more likely to be victims of crime, such as racial minorities, low-income people, and residents of dense urban neighborhoods, report lower rates of firearm ownership [Miller et al., 2022, Wintemute et al., 2022]. In one recent study, gun owners were more likely than non-owners to report living in safe neighborhoods where gunshots and shootings in the neighborhood are "not a problem," and were much less likely than non-owners to report seeing any sidewalk memorials where people died by violence [Wintemute et al., 2022].

Survey-based studies are important, but have clear limitations in identifying the role of local crime. Self reports of neighborhood crime and gun ownership may be unreliable, and sample sizes within specific cities or counties in surveys are too small to adequately assess the effects of the neighborhood crime context on gun purchases, while panel studies that might capture changes over time are rare and often under-powered.

Studies using administrative data have been hampered by the lack of appropriate data [Council, 2004, Kleck, 2015]. Existing studies rely on very indirect proxies for gun ownership, including magazine subscriptions [Duggan, 2001a], gun suicides [Kleck and Patterson, 1993, Kovandzic et al., 2012, 2013], or shares of crimes committed with guns or data on gun theft [Kleck and Patterson, 1993].

Another challenge is measuring the threat of crime at the right geographic level. Studies have used aggregate data drawn from a single U.S. time series [Kleck, 1979, Southwick, 1997, Bice and Hemley, 2002b]; a single Detroit time series [Loftin et al., 1983]; cross-section and time-series data from U.S. states [Miller et al., 2007, Siegel et al., 2013]; cross-sectional samples of cities [Kleck and Patterson, 1993] and counties [Kovandzic et al., 2012, 2013]; and panel data from a sample U.S. counties [Duggan, 2001b]. Studies that use U.S. counties offer more granularity than state-level data, but counties may still be too broad to accurately capture the local effects of violent crime on gun purchasing behavior. Crime data is collected by law enforcement agencies, and a single metro-area county can be served by a large number of reporting agencies with very heterogeneous crime rates. For example, Los Angeles County has 88 reporting agencies which include both urban core and inner-ring suburban areas like Compton and Inglewood with high rates of violent crime as well as very safe outer-ring suburbs like Palos Verdes Estates. Even the city of Los Angeles has an extremely uneven spatial distribution of crime, with some residents living in very safe neighborhoods and only rarely venturing into neighborhoods with concentrated crime and poverty. Thus far, researchers have not even come close to acquiring the requisite neighborhood-level data for examining the claim that gun purchases are driven by acute local crime threats.

It should also be noted that many existing studies using aggregate data on crime and guns are focused primarily on the desire to contribute to debates about gun control, and as such, focus on the hypothesis that additional guns lead to more crime [Kleck, 1979, Southwick, 1997, Bice and Hemley, 2002b, Loftin et al., 1983, Miller et al., 2007, Siegel et al., 2013, Kleck and Patterson, 1993, Kovandzic et al., 2012, 2013, Duggan, 2001b]. In these studies, the possible impact of local crime on gun purchases is often viewed as a nagging threat to causal inference rather than an important question in its own right.

Protective gun purchase as response to diffuse perception of danger: In addition to acute local experience with crime, a second class of explanations relate to psychological theories that differentiate the types of threat consumers may perceive prior to purchasing a firearm. In this literature, perceptions or fears about crime might be untethered from actual crime rates [Ferraro, 1995, Warner and Thrash, 2020]. Rather than being driven by an acute threat from local crime, fears of victimization might be driven by a diffuse sense that the world is an increasingly dangerous place [Stroebe et al., 2017]. Salient but non-local events like mass shootings or violent civil unrest might prime a more generalized sense of fear. Survey items that ask about perceptions of crime or fear of victimization may partially reflect these diffuse threats. In a large panel study, Warner and Thrash [2020] find that diffuse fears and anxieties about a dangerous world and mistrust in others are far better predictors of protective gun ownership than specific fears of crime and perceived risks of victimization. This notion of generalized fear is consistent with other research indicating that gun ownership correlates with factors such as economic decline [Carlson, 2015], growing social decay or disorder [Jackson, 2006, Britto, 2013, Hirtenlehner and Farrall, 2013], declining faith in the capacity of the government or police to provide for public safety [Jiobu and Curry, 2001], or the COVID pandemic [Miller et al., 2022, Lacombe et al., 2024, Rosenberg et al., 2025].

Party affiliation is sometimes included as a control variable in survey-based psychological studies of gun acquisition, but many of the constructs, including specific fears of crime and more diffuse anxieties about social disorder, are highly correlated with political partisanship. For instance, Warner and Thrash [2020] find that the impact of their battery of variables capturing "diffuse fears and anxieties" on protective gun ownership fall precipitously when self-described partisanship is included in the model, since partisanship is highly correlated with the their "fear" constructs as well as protective gun ownership. This is related to a large literature in psychology arguing that political conservatives believe the world to be a more dangerous place than liberals (for a review, see Clifton and Kerry [2023]). In the United States, Republicans consistently report being far more worried about crime than Democrats or independents [Brenan, 2022] and view it as a more important political issue [Gramlich, 2024] despite living in safer neighborhoods. Republicans are also vastly more likely than Democrats to believe that crime is increasing [Brenan, 2024]. These partisan gaps in perceptions and concerns about crime have been documented in California-specific studies as well [Bonner, 2022].

A literature in political science also documents a growing partisan gap in attitudes about and ownership of firearms that is driven in large part by different notions of protection against a variety of diffuse threats– notions that have been shaped in large part by interest groups like the National Rifle Association as well as messaging by party elites and allied media outlets [Melzer, 2012, Lacombe, 2019, 2021]. As a result, according to Dyck and Pearson-Merkowitz [2023], partisans get their views about crime and the benefits of gun ownership from cues delivered by copartisan elites and their media allies, obviating any role for local crime.

Insofar as Americans buy guns to protect themselves, existing literature has not yet discerned whether buyers are responding to threats that are acute and local or perceived threats that are diffuse, generalized, and potentially influenced by partisan politics. Our approach is to rely on fine-grained observational data to examine the extent to which gun purchases respond to local crime events.

2 Data

Our analysis relies on four categories of data: 1) handgun acquisitions, 2) crime events, 3) demographic covariates, and 4) geographic boundary files to process the aforementioned three. In this section, we briefly summarize each data source.

Gun data: Firearm transactions in California must be conducted through a licensed firearm dealer. These transactions are sent to the California Department of Justice (CA DOJ) and recorded in the Dealer Record of Sale (DROS) database. We analyze geocoded handgun transactions from 2008 to 2020 (N $\approx$ 6.7 million), a time frame selected to ensure the greatest overlap with our other data sources. The DROS data includes variables such as the purchaser's home address and the time of acquisition, which we use to geocode and aggregate into per-capita counts to compare against the location of crime.

Crime data: Crime data is available in three forms. First, at the Law Enforcement Agency (LEA) level, CA DOJ collects monthly, standardized records of aggregate counts of crime incidents to report to the Federal Bureau of Investigation's (FBI) Uniform Crime Reporting (UCR) program. We use this data at the year level, and sum the number of criminal incidents across different categories of violent offenses: homicide, rape, robbery and aggravated assault. While useful, this data does not provide specific neighborhood crime locations. To address this, we collect the additional two data sources that allow us to geocode the precise location of the crime incident.

Second, we collect street-level crime incident data from the Los Angeles Sheriff's Department (LASD) and Long Beach Police Department (LBPD). LASD began publicizing this data online in 2009, including information on crime type, location, and estimated date. For access to LBPD's records dating back to 2011, we submitted a Freedom of Information Act request. LASD's jurisdiction covers more than 50% of Los Angeles county, primarily outside of large metro areas, and Long Beach is the second-largest city in Los Angeles county. Although comprehensive sub-LEA violent crime data is available only for these two jurisdictions, we believe that together, they offer a wide window into violent crime patterns within California's most populous county.

And lastly, we extract data from California's mortality records on the number of deaths resulting from homicide. These mortality data contain detailed information on the circumstances surrounding the deaths of state residents, including estimated date of death and home address. We isolate homicide-related deaths by filtering the data using the relevant ICD-10 codes. Although this dataset focuses on just one category of violent crime, it provides point-level data across the entire state, covering all recorded homicides during the study period (2008–2020). In Appendix A, we compare annual aggregated homicide counts at the LEA level with those reported by the California DOJ, finding a correlation of 0.989.

Demographics: We use the United State Census Bureau's American Community Survey to collect estimates of key demographic variables at the census tract level. These variables are: population, median age, median household income, number of men, number of homeowners, number of white and hispanic individuals, and number of people who have attained at least a college degree.

We also collect precinct-level election records from `https://statewidedatabase.org`. For every presidential election, we take the two-party Republican vote share as a measure of an area's political leaning. The precinct-level vote data is aggregated to Census tract-level or LEA data by first, distributing the votes according to census block population distributions, then re-aggregating using GIS software.

Geographic boundary files: Our data on guns and homicides are provided as point data with longitudes and latitudes. To generate the number of counts at either the tract or LEA level, we overlay geographic boundary shape files with the incident data. While tract shapefiles are publicly available from the U.S. Census Bureau, there were no existing statewide LEA boundary shapefiles. Here, we briefly describe how we constructed LEA jurisdiction boundaries over time.

We start with the aforementioned crime records provided by CA DOJ. Broadly speaking, LEAs fall into five main categories based on their jurisdiction: city police departments, county sheriff's offices, school district police, park police, and other specialized agencies (such as those associated with hospitals, ports, train stations, or developmental centers). We collect existing shapefiles for census designated places, schools, and recreational parks, then create a crosswalk between each location to the appropriate LEA. To create county sheriff boundaries, we take a "cookie-cutter" approach, where we assign the remaining land in the county unassigned to any city, school or park, as the sheriff's jurisdiction. For the miscellaneous entities, we manually enter the geographic location of each building. Since these LEAs are entered as points, we do not include them in our analysis.

One complication worth noting is that LEAs can appear or disappear over time. A small number were created during our study period, while others merged, were absorbed into sheriff's jurisdictions, or otherwise reorganized. We manually take note of these changes as they are recorded in the CA DOJ data and reflect these changes in our shapefiles. In the analysis, LEAs that merge, split, or undergo substantial jurisdictional changes are treated as distinct entities over time.

3 Empirical Methods

We examine the relationship between crime and gun purchases using a range of statistical methods for panel data, which fall into three main categories:

Panel methods using continuous treatment: We estimate a panel regression using unit- and year-fixed effects or unit-level first-differences with year-fixed effects. Both the two-way fixed effects and first-differences approaches aim to examine within-unit changes over time. These methods help account for confounding factors that are constant within units (e.g., tracts or LEAs) and those that are specific to individual years—though they do so in slightly different ways. Below, we write down both estimating equations:

$$Guns_{it} = \tau_1^t Crime_{it} + Unit\ FE_i + Year_t + \varepsilon_{it}$$
$$\Delta Guns_{it} = \tau_2^T \Delta Crime_{it} + Year_t + \varepsilon_{it}$$

The treatment variable of interest is the number of or change in violent crime incidents per capita and the outcome variable of interest is the number of or change in handguns purchased per capita. *Unit FE* denotes the unit fixed effect, which is either a LEA or a tract. *Year* denotes a year fixed effect. All models cluster standard errors at the unit level.

We note that the two-way fixed effect estimation may not be suitable, given our empirical setting, but report those results because it is the most popular method in panel studies. Specifically, the outcome time series, gun purchasing, is highly non-stationary as gun purchasing per capita increases in almost all regions over time. In these cases, the standard econometrics approach is to use a first-difference model [Baltagi, 2009]. Moreover, as discussed above, papers like Duggan [2001b] and Bice and Hemley [2002a] provide reason to believe in a positive feedback loop between treatment and outcome; specifically, that an increase in guns may lead to an increase in crime. Thus a standard two-way fixed effect design may overestimate the effect of crime on gun purchasing.

Cross-sectional comparisons using surprising homicides: In our second specification, we leverage the fact that homicides are rare in many smaller LEAs to facilitate cross-sectional comparisons. We restrict the analysis to non-sheriff LEAs covering jurisdictions with populations under 35,000. In these small- to mid-sized locations, homicides occur sporadically and often for seemingly idiosyncratic reasons. We estimate the following regression, controlling for trends in violent crime and prior gun purchases, and comparing LEAs that experience a spike in homicides to other LEAs in the same metropolitan statistical area (MSA) with no homicides:

$$\begin{aligned} Guns_{it} = \tau_3^T Homicide_{it} + \beta_1^T Med\ HH\ Income_{it} + \\ \beta_2^T Violent\ Crime_{it-1} + \beta_3^T Violent\ Crime_{it-2} + \\ \beta_4^T Guns_{it-1} + \beta_5^T Guns_{it-2} + Year_t \times MSA_i + \varepsilon_{it} \end{aligned}$$

This specification assumes that, after accounting for past trends in violent crime and gun purchases, we can isolate a contemporaneous homicide shock. The MSA-by-year fixed effects ensure that all comparisons are made cross-sectionally within the same metropolitan area. In other words, conditional on prior levels of violent crime, the location of a homicide can be treated as as-good-as random across LEAs within an MSA.

Events-study analysis using homicides: For our tract-level analysis, we have granular data on homicide occurrences in a given area. For a fixed tract in a given year, this variable is essentially a binary dummy of whether there is a homicide or not (18.0% of tract-year pairs have at least 1 homicide and 3.4% have more than 1 homicide). As a result, we can use a standard difference-in-differences design and estimate an events-study regression.

However, since tracts fall in and out of treatment, we estimate the regression using a counterfactual imputation procedure [Liu et al., 2024, Borusyak et al., 2024]. The estimating equation is the similar to the basic two-way-fixed effect regression laid out before:

$$Guns_{it} = \tau_4^T Crime_{it} + \lambda_i^T f_t + \varepsilon_{it}$$

Except now, f_t is a set of unobserved, low-dimensional common factors that affect the outcome trajectories [Xu, 2017]. The regression is estimated using data from the untreated tract-years to estimate factors and counterfactual outcome of tracts in years that do experience a homicide. The average treatment effect on the treated is estimated by taking the difference in observed gun purchasing against the predicted volume of gun purchasing. Standard errors are calculated using bootstrap.

4 Crime may have some effect on gun purchasing, but the effects are small and imprecise

Table 1 provides a summary of the results and the statistical methods used to explore the relationship between the threat of crime and gun purchasing using the full panel data. Table 1a shows results for the LEA-level analysis and Table 1b shows results for the tract-level analysis. The first three rows of each table provide a summary of the different designs, estimation methods, or samples. The fourth row indicates the operationalized treatment variable of interest. In the case of LEAs, the crime data comes from annual reports compiled by the California DOJ. In the case of tracts, the crime data comes from either the all-cause mortality data or street-level crime data obtained by Freedom of Information requests from specific LEAs. Rows five through eight show the results for different outcomes. Row five shows the effect for total gun purchasing, row six for first-time gun purchases, and row seven for existing gun owners. The credibility of each design and estimation hinges on the assumption that we have isolated a crime shock that is orthogonal to potential confounders. An implication of this is that contemporaneous crime shocks should not affect previous gun purchasing values, so we should expect the coefficient from regressing the pre-treatment outcome on crime to be close to zero and statistically insignificant. Row eight shows the effect for this placebo outcome: gun purchasing in the immediately prior time period. Finally, row nine provides a summary statistic to interpret the effect sizes across designs. We multiply

Table 1: Effects of gun availability on crime outcomes

(a) LEA-level results

Model/Design	*Two-way fixed effect*	*First difference*	*First difference*	*Random homicide location*
Unit & Sample	All LEAs (2008–2020)	All LEAs (2009–2020)	All LEAs (2009–2020)	LEAs <35K pop. (2008–2020)
Sample size	5851	5374	5374	1958
Coefficient of interest	Violent crime per capita	Change in violent crime per capita	Change in homicides per capita	Homicides per capita
Total effect (SE)	0.24^{***} (0.06)	0.17^{**} (0.06)	1.36 (0.81)	1.19 (1.02)
Effect for new guns (SE)	0.062^{*} (0.03)	0.037 (0.04)	0.35 (0.44)	0.90^{*} (0.44)
Effect for repeat guns (SE)	0.17^{***} (0.05)	0.14^{***} (0.03)	1.00 (0.60)	0.29 (0.80)
Placebo test using lagged outcome (SE)	0.28^{**} (0.11)	0.23 (0.21)	0.28 (1.05)	-0.00 (0.00)
Implied % of outcome explained	6.98%	5.13%	0.411%	0.358%

(b) Tract-level results

Model/Design	*First difference*	*First difference*	*Events study (Imputation method)*
Unit & Sample	Tracts in Long Beach (2011–2020)	Tracts in LA Sheriff (2009–2020)	All Tracts in CA (2008–2020)
Sample size	1041	2824	105942
Coefficient of interest	Change in violent crime per capita	Change in violent crime per capita	Homicide occurrence (binary)
Total effect (SE)	0.20 (0.18)	-0.00 (0.01)	5.03 (5.93)
Effect for new guns (SE)	0.05 (0.11)	-0.01 (0.01)	2.51 (1.30)
Effect for repeat guns (SE)	0.15 (0.12)	0.00 (0.01)	5.43 (5.65)
Placebo test using lagged outcome (SE)	0.04 (0.12)	0.05 (0.03)	-3.72 (2.18)
Implied % of outcome explained	7.84%	Total correlation is negative	0.09%

Notes: Results from seven models estimating the effect of crime on gun purchasing. Each model is presented in a separate column. Panel A reports results with LEAs as the unit of analysis, while Panel B uses Census tracts. In all regressions, standard errors are clustered at the unit level. $p < 0.05$, $p < 0.01$, $p < 0.001$ (two-tailed tests).

the number of observed crimes by the estimated effect of crime on guns, and divide this predicted number of guns purchased by the actual number of guns purchased in the sample time period. This calculates the expected percent of the actual gun purchasing as a result of crime.

While the empirical patterns are broadly consistent with the theory that crime influences gun purchasing, they do not support the claim that crime is a large or primary driver.

First, although there is generally a positive correlation between crime and gun purchases, the relationship is weak. Across seven main specifications, only two coefficients are statistically significant (see row 5). None of the tract-level estimates reach significance, and in the Los Angeles Sheriff Department's jurisdiction the estimated correlation between violent crime and gun purchases is negative.

Second, even among the statistically significant estimates (columns 1–2 of Table 1a), only the LEA-level first-differences specification using violent crime (column 2) plausibly isolates a causal effect. The two-way fixed effects model in column 1 fails the "pre-trend" test: when regressing the placebo outcome of lagged gun purchases, the estimated effect is positive and significant, indicating upward bias from unobserved confounding. Appendix B2 further shows a clear upward pre-trend in the LEA-level two-way fixed effects results.

Third, while the first-differences estimates in column 2 of Table 1a do not display statistically significant pre-trends, their interpretation still requires caution. As Figure B2 illustrates, although this specification reduces visible pre-trends, the estimate for the period immediately prior to treatment is larger than the contemporaneous effect itself. This suggests that the estimated treatment effect may be upwardly biased by confounding or is not much larger than natural variation in the outcome. Thus, even this specification yields results that are neither robust nor substantively large.

Fourth, in all but one specification (Table 1a, column 4), the estimated effect of crime is larger for repeat gun purchasers rather than first-time buyers. This suggests that, insofar as crime influences gun demand, it primarily encourages existing owners to acquire additional firearms rather than motivating new individuals to purchase their first gun. Crime leads to statistically significant increases in first-time purchasing in only two specifications, one of which (Table 1a, column 1) suffers from the pre-trends problem described above.

Taken together, across seven research designs only one—the LEA-level first-differences estimate—yields a statistically significant correlation that survives basic pre-trend checks, suggesting that homicides explain about 5.1% of observed gun purchases in California during the study period. The two estimates that suggest the largest effect size (column 1 of Table 1a and column 1 of Table 1b) both exhibit an upwards-sloping pre-trend, suggesting an positive bias in the estimate. The remaining models suggest a much more modest effect sizs of less than 1%. Of the four other specifications that also pass pre-trend tests (columns 3–4 of Table 1a and columns 2–3 of Table 1b; see Appendix B for pre-trends figures) all yield estimates statistically indistinguishable from zero. Moreover, insofar as crime is correlated with gun purchasing, it is correlated with repeat purchases rather than first-time acquisitions. Collectively, these results cast doubt on the claim that local crime is a primary driver of American gun demand.

Finally, we document several patterns of heterogeneity that further probe whether people buying guns to increase personal safety, which we report in Table 2. Since guns are expensive and generally more prevalent in areas with high levels of home ownership, we estimate the same tract-level homicides events study for tracts in Table 1b column 3, but for tracts that are in the top tercile of income and home-ownership according to census demographics. In California, these are places where the median household income level is more than $87,000 and where more than 70% of respondents report owning a home. We find that the occurence of a homicide has no effect on gun purchasing in these areas.

We also show that consumers may react to acute instances of increased crime, but not to decreases. This conclusion is based on an extension of the aggregate LEA-level first difference model (Table 1, column 2), where we interact the violent crime measure with an indicator for whether the change is positive or negative. We find that consumers purchase more guns only during years where there is an increase in crime, and there is no corresponding decline in response to decreases in crime. This is consistent with a ratchet effect, albeit as described above, a rather small one.

Overall, the pattern of estimates in this section is consistent with the idea that positive shocks to crime induce some non-negative response in gun purchasing, but that the magnitudes of the effect are unstable and small, while decreases in crime are not associated with reduced sales. While it appears that increases in crime do cause some already-existing gun owners to purchase more guns, it would be difficult to conclude from our data that fear of crime rooted in exposure to actual incidents is a significant driver of firearm demand.

Table 2: Effects of gun availability on crime outcomes

(a) Panel A: Event study for wealthy areas

Model/Design	*Events study (imputation method)*
Unit & Sample	Tracts in the top tercile of median income and homeownership (2008–2020)
Sample size	21211
Coefficient of interest	Homicide occurrence (binary)
Total effect (SE)	-1.29 (12.73)
Effect for new guns (SE)	1.03 (4.67)
Effect for repeat guns (SE)	-2.32 (10.64)
Placebo test using lagged outcome (SE)	-18.39 (12.31)
Implied % of outcome explained	Total correlation is negative

(b) Ratcheting effect

	Change guns
Change violent crime	0.07
	(0.08)
Positive shift	21.42
	(12.21)
Change violent crime × Positive shift	0.25**
	(0.09)
N	5374
Year FE	✓

Panel A: Event-study estimates of the effect of a homicide occurring in a tract within the top tercile of income and homeownership in California. This model corresponds to Table 1b, column 3, but uses a subset of the data. Panel B: LEA-level first-difference regression results, identical to Table 1a, column 2, but interacting the treatment with an indicator for whether the change was positive or negative. $p < 0.05$, $p < 0.01$, $p < 0.001$ (two-tailed tests).

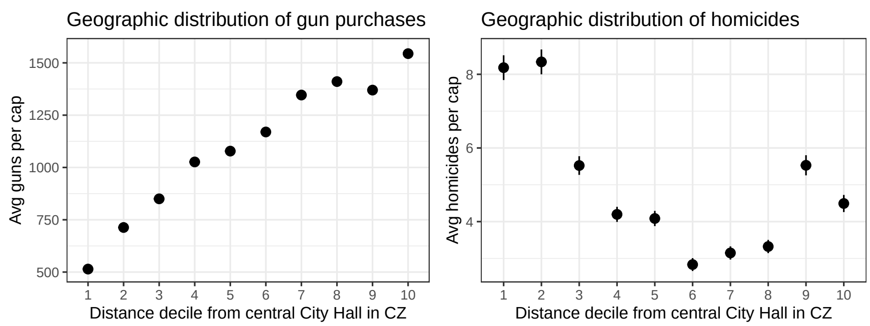
5 The geographies of crime and guns are different

Why is the estimated relationship between crime and guns so weak in statistical models using panel data? The remainder of this paper answers that question by exploring the evolving geography of crime and gun purchases in a more descriptive way, focusing first on variation within Law Enforcement Areas and the larger metro areas in which they are embedded, and then on variation across Law Enforcement Areas.

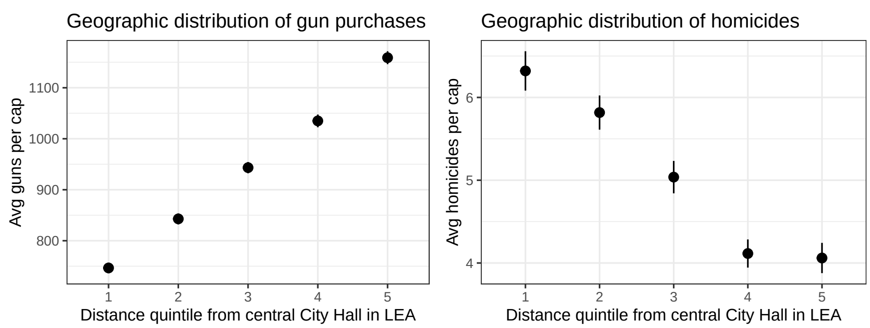
5.1 Variation Within Law Enforcement Areas

Figure 2: Gun purchasing by distance from city hall

(a) Within commuting zones



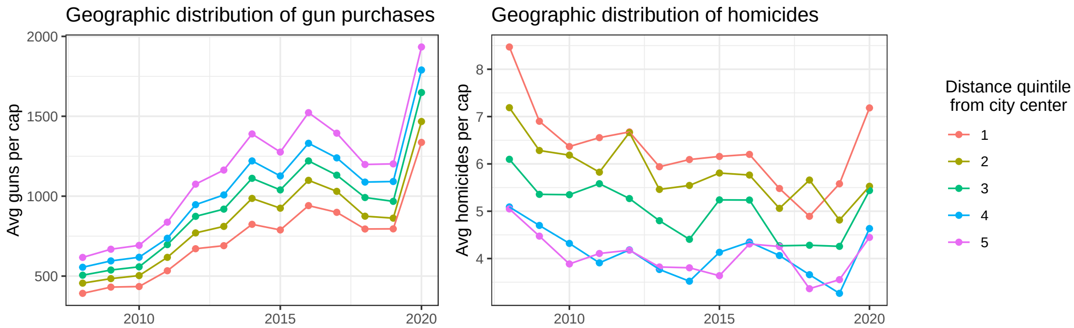
(b) Within LEA



Note: The figure plots the average number of guns purchased per capita, with tracts categorized by distance from the central City Hall. Panel A shows results aggregated at the Commuting Zone level, with tracts divided into deciles, while Panel B shows results aggregated at the Law Enforcement Area (LEA) level, with tracts divided into quantiles. Error bars represent 95% confidence intervals.

An important first observation is that within California's metro areas, guns are not purchased in the same places where crime occurs. Figure 2 contrasts the geographic location of gun purchases and crime. Borrowing a technique from urban economics [Glaeser et al., 2008], we first categorize each tract in California by how far it is from the center of its commuting zone or its law enforcement area. To do so, we locate the city hall of every LEA jurisdiction, or the city hall of the primary city in a commuting zone, and calculate the distance of the tract centroid from the city hall coordinates. We take the distribution of distances and split the tracts into distance quantiles. For every quantile, we plot the average gun purchasing volume per capita and the average number of homicides per capita. Panel A shows these results for commuting zone deciles, and Panel B for LEA quintiles. Note that commuting zones are aggregations of counties meant to capture the metropolitan areas

Figure 3: City hall graphs over time



Note: The figure plots the average number of guns purchased per capita over time, with tracts categorized by distance from the central City Hall within each LEA. This measure is the same as in Figure 2b, but shown over time.

within which people tend to spend much of their time and commute for work. There are currently 70 commuting zones in California. LEAs are much smaller geographic entities, and there are currently 517[3] in California.

Regardless of whether we look within the larger commuting zone—or within the smaller LEA—we find that more guns are purchased in the periphery. On the other hand, the geographic distribution of homicides shows that crime tends to happen closer to the city center. If we plot gun purchasing against homicides for every quantile, the correlation is -0.75 at the commuting zone level, and -0.96 for LEA-level analysis.

This geographic relationship is stable over time as well. In Figure 3, we show the same quantity over time for law enforcement areas. From the 2008 financial crisis to the beginning of the COVID-19 pandemic, time trends across distance quantiles show common shocks in gun purchasing, such as the "Trump slump" in purchasing after President Trump's first election in 2016. However, the relative ordering remains the same; more guns are purchased further away from city centers, whereas inner city areas are home to the lowest rates of gun purchasing per capita across time.

On the other hand, homicides consistently occur in tracts located closest to the city center. Although there is some year-to-year variation in the exact locations of homicides, this central pattern remains generally stable over time. The stability in the over-time geographic relationship in gun purchasing is further highlighted by comparing it with that of homicides. These visualizations simultaneously suggest the existence of A) a time-varying common factor that has continued to

[3]The number of LEAs we use in our analysis in 2020. There are more LEAs that are smaller that are responsible for specific facilities like government buildings or public transit routes. We exclude these from our analysis.

drive gun purchasing regardless of geographic location and B) a time-invariant feature of American life that correlates with distance from the city center that creates baseline differences in the level of gun purchasing across space.

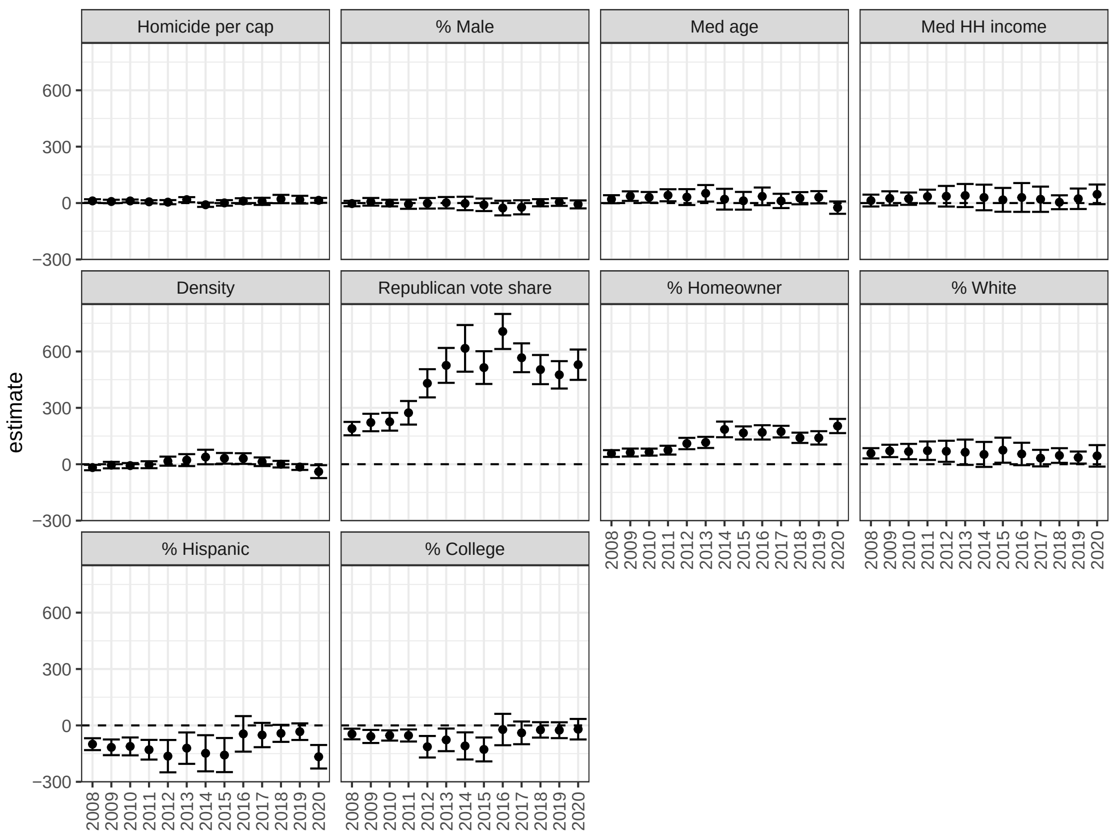
If crime does not explain the geography of gun purchases within Law Enforcement Areas, then what does? To shed more light on the role of crime in relation to other factors shaping the geography of gun purchases, we collect ten key crime, demographic, political, and economic covariates from various data sources to conduct a "horse-race." We standardize all variables to have a mean of zero and a standard deviation of one, and we regress gun purchasing levels per capita on the ten variables, weighting by population. We fit a tract-level cross-sectional model for every year in our sample, including fixed effects for LEAs, and plot the coefficient on each variable over time.

Each point in Figure 4 can be interpreted as the partial correlation of a demographic variable in a given year; in other words, within each LEA, how much change in gun purchasing would we expect after a one-standard deviation change in the predictor holding other factors constant? The coefficients for homicides per capita are indistinguishable from zero, verifying the lesson from the city hall graphs above: gun purchases and homicides are not concentrated in the same neighborhoods. Two predictors of neighborhood-level gun purchases have strengthened substantially over time, and both are highly correlated with distance from the city center: home ownership and above all, Republican partisanship [Rodden, 2019]. In the appendix, we verify that the results look very similar if we focus on violent crime rather than homicides using our street-level crime data from Long Beach and Los Angeles.

The growing geographic correspondence between partisanship and gun purchases in Figure 3 is difficult to ignore. Consistent with the work of Lacombe et al. [2019], and Dyck and Pearson-Merkowitz [2023] partisanship clearly dominates all other predictors of gun purchases. Consistent with Lacombe et al. [2024] and Rosenberg et al. [2025] we do not find evidence that the pandemic purchasing spike in 2020 was associated with a change in the geography, demographics, or partisanship of gun purchases in California.

The geography of partisanship vis-a-vis crime can be appreciated in Figure 5, which divides census tracts into Democratic (60 percent Democratic and above), Republican (40 percent Democratic and below), and "purple" neighborhoods (between 40 percent and 60 percent Democratic),

Figure 4: Repeated cross-sections within LEAs



Note: This figure plots results from regressing gun purchasing levels on ten covariates and LEA fixed effects. A unit of analysis is a Census tract. Each year corresponds to a separate model, and the figure shows the partial correlation of each covariate after standardizing all variables to have a mean of zero and a standard deviation of one. Error bars represent 95% confidence intervals.

Figure 5: Gun Purchases, partisanship, and homicides

Note: This figure plots the average number of guns purchased per capita over time, by two-party vote share and homicide risk. A "Purple" tract is defined as any neighborhood with a Democratic two-party vote share between 40% and 60%. Democratic and Republican tracts are defined analogously, as neighborhoods with vote shares above or below this range, respectively. Relative homicide risk is measured by dividing tracts into terciles based on the number of homicides within each LEA, capturing the intensity of homicides occurrence in a tract relative to other tracts in the same LEA.

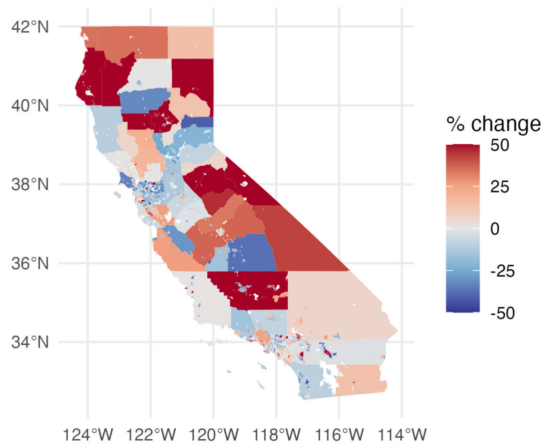
with separate plots of average gun purchases per capita for low, medium, and high levels of homicide risk. Regardless of homicide risk, the upward trend in California gun purchases has been dominated by Republican neighborhoods.

5.2 Variation Across Law Enforcement Areas

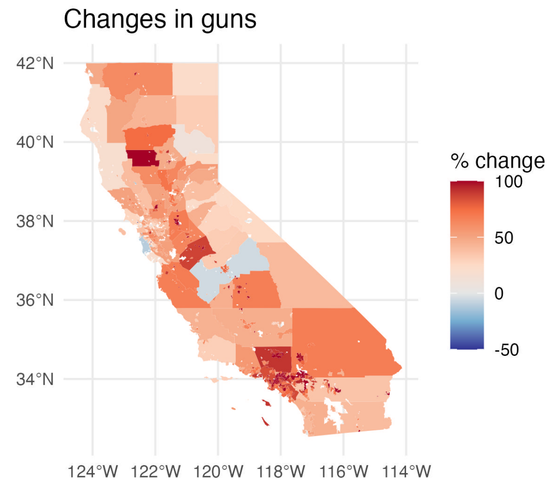
Zooming out to draw comparisons across LEAs, we again see that the geographies of gun purchases and crime have evolved very differently over time. The left-hand panel in Figure 6 colors the jurisdictions of California Law Enforcement Agencies by change in violent crime per capita, comparing the average for 2008 to 2013 with the average for 2014 to 2020. While much of the state has become safer, including most urban and suburban areas in Northern California and the Lake Tahoe area as well as most suburban areas in Southern California, other areas have experienced increases in violent crime– including parts of urban Los Angeles, a number of troubled smaller cities and towns in the Southern and Central part of the state, and several rural areas scattered throughout the state. California thus resembles the United States as a whole, where data from both the FBI Uniform Crime Statistics and the National Crime Victimization Survey demonstrate that as metro-area crime falls, the traditional urban-rural disparity in violent crime is declining [Berg and Lauritsen, 2016, Brazeal, 2024].

Figure 6: Choropleth map of changes in crime and gun purchases

(a) Changes in violent crime



(b) Changes in gun purchasing



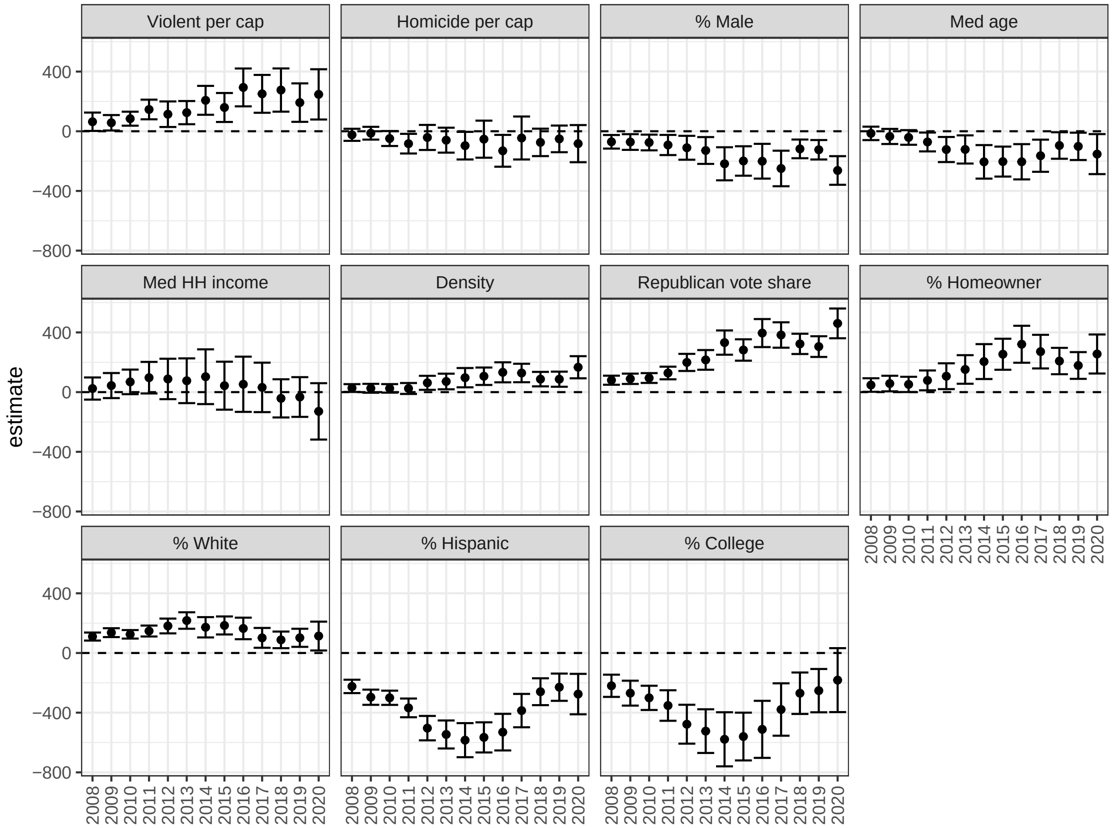
Note: These maps show the change in violent crime per capita and gun purchases per capita, comparing the average from 2008–2013 with the average from 2014–2020. Each unit plotted represents a LEA.

The right-hand panel in Figure 6 shows that over a similar time period, per capita gun purchases have increased throughout California, but the largest increases have been in suburban and exurban areas around Los Angeles and the cities and especially rural areas of the Central Valley.

In Figure 7, we present results from repeated cross-section regressions using LEAs rather than tracts as the units of analysis. Similar to the more disaggregated tract-level results, Republican vote share and homeownership show an increasing correlation with gun purchasing levels over time. In contrast, the top-left panel indicates that, holding other factors constant, LEA-level violent crime is a weak but statistically significant predictor of guns purchased per capita, with the strength of this association growing over time. In the remainder of this section, we examine this correlation more closely and argue that it does not represent a meaningful relationship in the context of California cities.

First, to address the most obvious concern, the absence of a tract-level correlation is not an artifact of using homicides in the tract analysis and violent crime at the LEA level. As we have already demonstrated, in Appendix C, we show that when using violent crime at the tract level with data from Long Beach and the Los Angeles County Sheriff's Department, the correlation with gun

Figure 7: Repeated cross-sections across LEAs



Note: This figure plots results from regressing gun purchasing levels on ten covariates each year. A unit of analysis is a LEA. Each year corresponds to a separate model, and the figure shows the partial correlation of each covariate after standardizing all variables to have a mean of zero and a standard deviation of one. Error bars represent 95% confidence intervals.

purchasing remains close to zero and statistically insignificant.

Second, at the LEA level all 11 measured covariates are significantly correlated with gun purchasing, with several stronger than violent crime. This stands in contrast to the tract-level results, where only Republican vote share and homeownership consistently predict gun purchasing. Demographic and crime patterns have varied considerably across California cities in recent decades [Allen and Turner, 2011, Lofstrom and Martin, 2013]. At this larger geographic scale, correlations between violent crime and gun purchasing are more likely to reflect unobserved confounding than a direct causal effect.

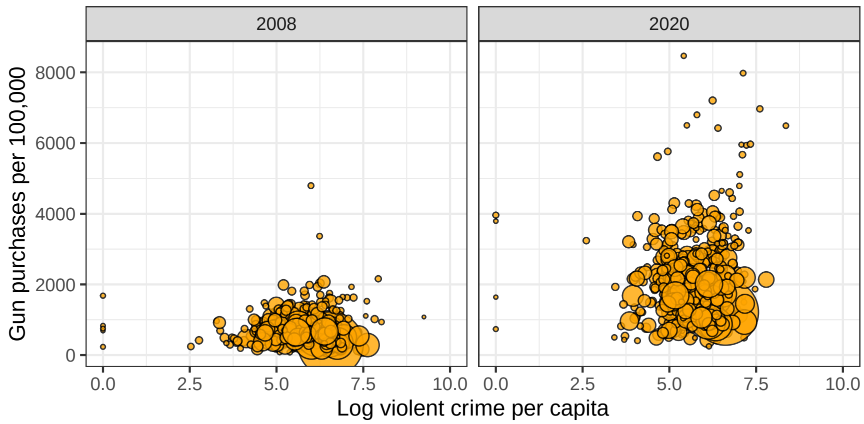
Third, the positive partial correlation between violent crime and gun purchasing is sensitive to the model specification. Figure 8 provides bivariate scatterplots of gun purchases per 100,000 residents against the log of violent crime per 100,000 residents for the first and last years in our dataset– 2008 and 2020– where the size of the data marker corresponds to the population of the LEA. In both years, the relationship is quite weak. In fact, the bivariate population-weighted correlation is weakly negative for each year in the data set, but the coefficient only becomes positive and statistically significant when the rich set of control variables in Figure 7 is included– partisanship in particular. Figure 8 also makes clear that gun purchases have increased dramatically in safe and dangerous LEAs alike.

Descriptively, the growing coefficients for violent crime in Figure 7 are driven by two dynamics. First, a handful of low-population, rural, and small-town LEAs like Susanville, Oroville, Arvin, Selma, Fortuna, Escalon, and Yreka have emerged as outliers (the small dots in the upper right corner of the Figure 8a), where violent crime rates have surged in recent years, in part due to the prevalence of the illicit drug trade, and gun purchases have also increased dramatically. Second, violent crime has fallen in a number of relatively urban and especially suburban LEAs where gun purchases were low to begin with, and where purchases have grown more slowly than in the rest of the state (e.g. Irvine, Orinda, Mill Valley, Tiburon, and Palos Verdes Estates). Finally, it is worth noting that the coefficients for violent crime are driven primarily by repeat purchases: they are less than half the size when the analysis is restricted to first-time buyers.

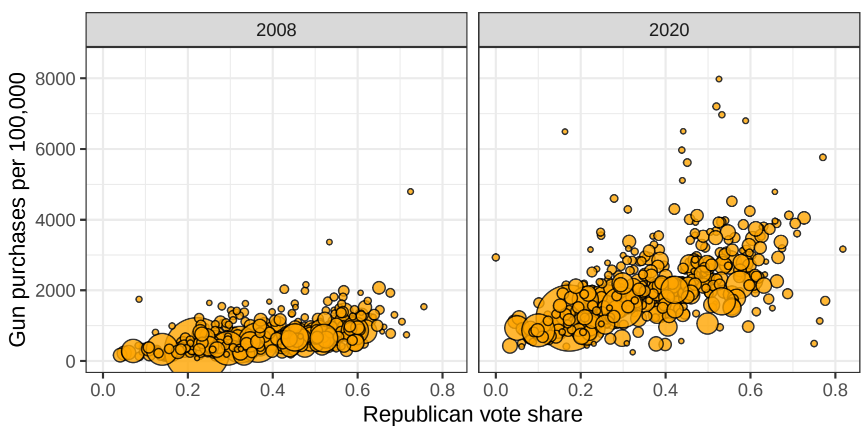
Figure 8 also provides a contrast with the role of partisanship, demonstrating that while the correlation between Republican vote share and gun purchasing was already pronounced in 2008, it grew as guns were increasingly and disproportionately purchased in Republican-leaning LEAs. Figures 7 and 8 demonstrate that while crime might play a small role after accounting for voting behavior and demographics, as in the tract-level analysis, the geography of gun purchases across LEAs is best explained by partisanship.

Figure 8: Cross-section relationship with gun purchasing

(a) Violent crime



(b) Republican vote share



Note: This figure plots the bivariate relationship between gun purchasing and crime (Panel A) and between gun purchasing and Republican vote share (Panel B) for the years 2008 and 2020. The unit of analysis is a LEA. The size of the point is determined by the population of the LEA.

6 Conclusion

Fear of crime is frequently cited as one of the leading reasons Americans buy firearms, yet there has been little direct evidence to examine this claim. In this paper, we offer a comprehensive analysis of the link between an increase in crime and an increase in gun purchases, and our research gives a nuanced answer. While the directional correlation and the patterns of heterogeneity support the hypothesis that acute shock from a dangerous crime in a neighborhood may cause some residents to purchase guns, most of the people who purchase these guns have already purchased guns in the past. Moreover, the overall magnitude of the estimates strongly reject the idea that exposure to actual crime is an important consideration. We also document that the places where people buy guns and places where crimes occur are systematically different, while the geography of gun purchases closely and increasingly resembles the geography of partisanship.

Our work builds upon previous studies in public health, criminology, economics, and political science, and is intended to be a stepping stone for future work. Why do an increasing portion of survey respondents attest that protection against crime is the primary reason for owning a gun when local crime is such a weak predictor of gun purchases? One hypothesis is that fear of crime may be a socially acceptable reason for justifying the purchase of a controversial but strongly desired consumer item. Another hypothesis is that the perceived threat of criminal violence is linked to a broader set of fears and anxieties about social change and disorder, and that these fears have become increasingly correlated with factors we document, such as homeownership, suburban and rural residence, or above all, Republican partisanship. Indeed, crime is a popular and sensational subject for news outlets as well as for politicians on the campaign trail, despite the fact that the U.S. is significantly safer today than it was three decades ago. Our study suggests that when it comes to gun purchases, perceptions and diffuse fears may be more important than acute threats.

These findings might facilitate several avenues of further research. First, there is clearly much more to learn about the complex relationship between partisanship, ideology, gun ownership, and perceptions about crime and threats of disorder. Second, our results call into question the notion that there is a vicious cycle in which crime begets gun purchases, which then beget accidental death, suicide, and crimes of passion. The initial phase of this proposed cycle appears to be surprisingly weak. This is a useful insight for those wishing to empirically estimate the impact of gun prevalence on other outcomes. Rather than crime shocks, future studies seeking exogenous sources of variation in gun purchases might consider the role of political events like elections or mass protests.

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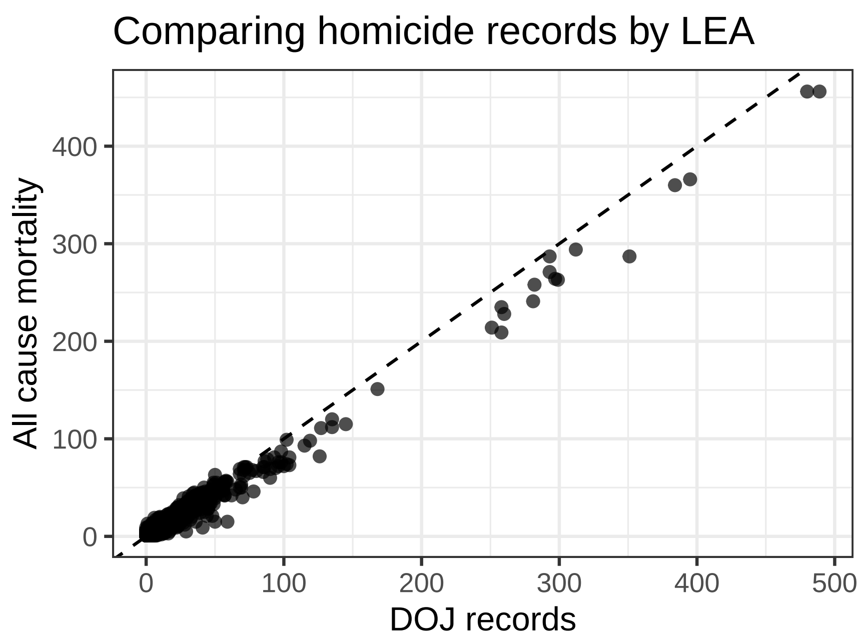
Appendix

A Validation of all-cause mortality data set

One concern of our tract-level analysis is whether the measure of homicide deaths reported by mortality records correspond to actual incidents. As one check, we aggregate the number of deaths reported in the statewide mortality records from each tract for every LEA and compare these totals to the number of homicides reported by the corresponding LEA. We find that the two sources of homicide data are highly correlated ($\rho = 0.99$), though mortality records generally undercount deaths relative to LEA-reported homicides. This suggests that our local crime measure may slightly undercount homicides. However, the points closely hug the 45-degree line, indicating that the absolute magnitude of this bias is likely small.

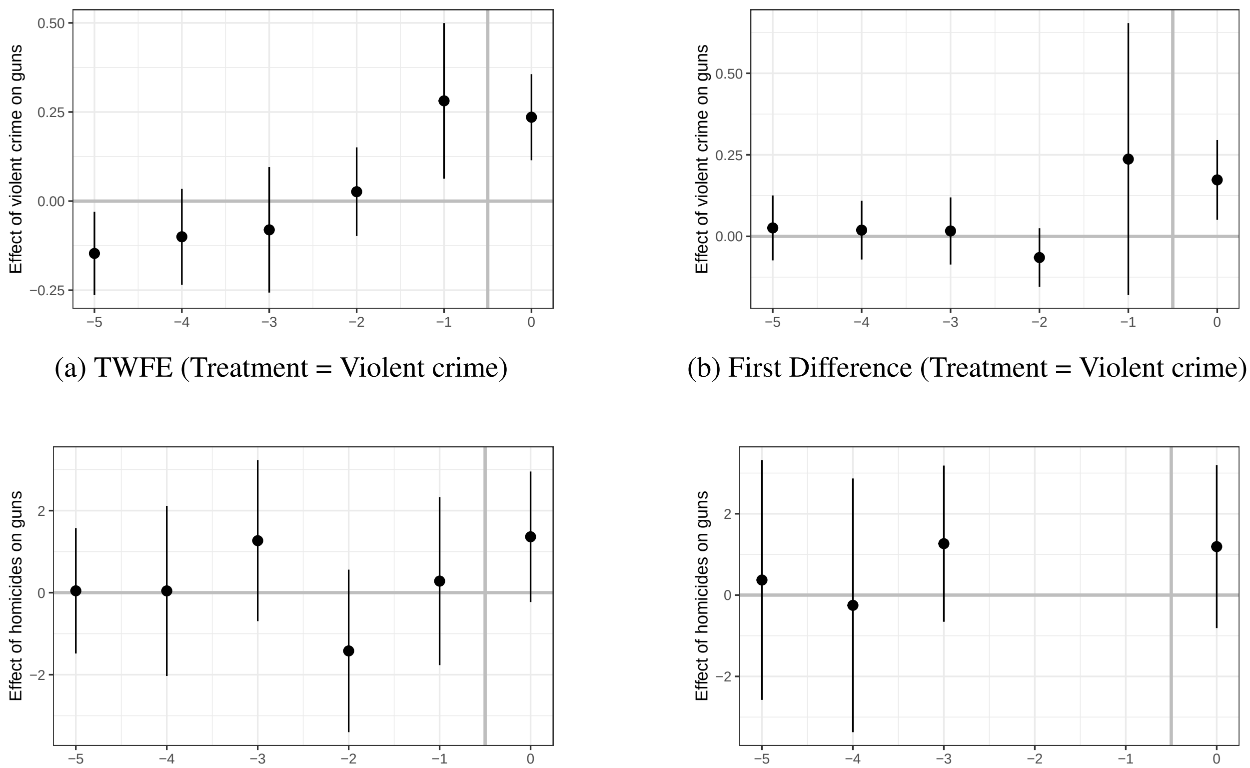
Figure A1: Comparing mortality data against DOJ data



Note: The figure plots the number of homicides recorded by the California Department of Justice against the number of homicide deaths reported in mortality records. Each point represents a LEA-year. The dotted line indicates the 45-degree line.

B Pretrends analysis

Figure B2: LEA-level analysis pretrends



(a) TWFE (Treatment = Violent crime)

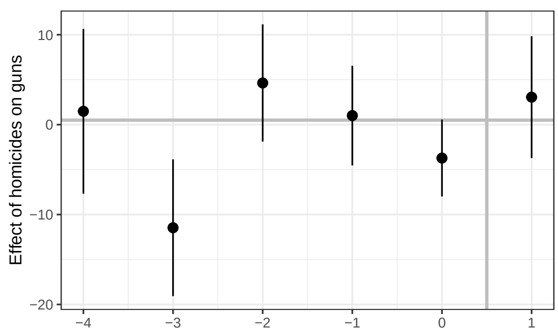
(b) First Difference (Treatment = Violent crime)

(c) First Difference (Treatment = Homicides)

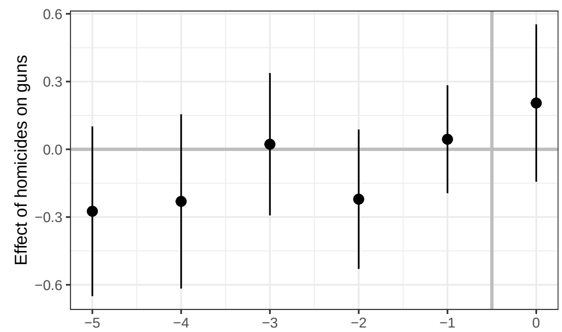
(d) Random Homicide Location

Note: Each panel plots the pre-trends analysis corresponding to a column in Table 1a. Points are estimated by regressing lagged handgun purchases per capita on the contemporaneous crime measure. An observation is a Law Enforcement Area (LEA). Standard errors are clustered at the LEA level. In subfigure (d), pre-trends for the two most recent periods cannot be estimated because they are included as controls.

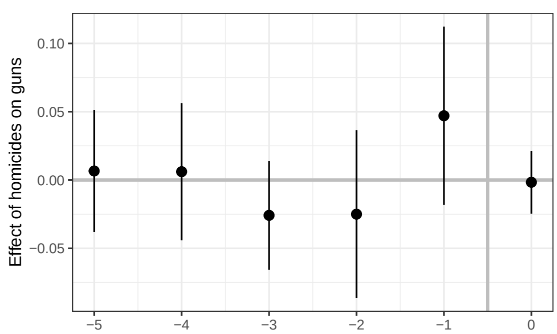
Figure B3: Tract-level analysis pretrends



(a) All CA Tracts Events Study
(Treatment = Homicides)



(b) First Difference using Long Beach tracts
(Treatment = Violent crime)



(c) First Difference using LA Sheriff tracts
(Treatment = Violent crime)

Note: Each panel plots the pre-trends analysis corresponding to a column in Table 1b. Panel (a) shows an events study plot generated using the imputation method. Points in Panel (b) and (c) are estimated by regressing lagged handgun purchases per capita on the contemporaneous crime measure. An observation is a tract. Standard errors are clustered at the tract level.

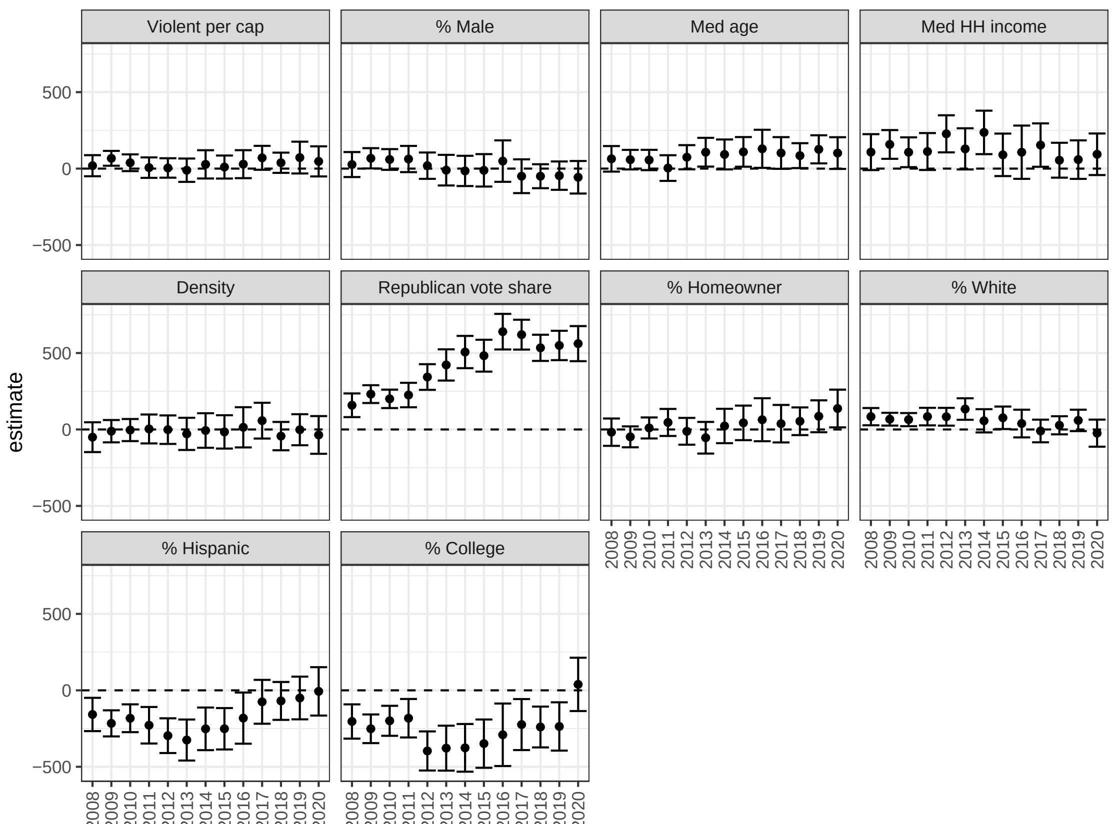
C Additional repeated cross sections

In this section, we conduct additional repeated cross-sectional regressions, identical to those in Figures 4 and 7 of the main text but using different outcomes and subsets of the data.

Figures C5 and C4 use FOIA-obtained data from the Long Beach Police Department and the Los Angeles County Sheriff's Department to run regressions identical to Table 4. Instead of using homicides per capita as our measure of crime, we use violent crime per capita. The predictive strength of Republican vote share is not affected by the specific crime variable chosen.

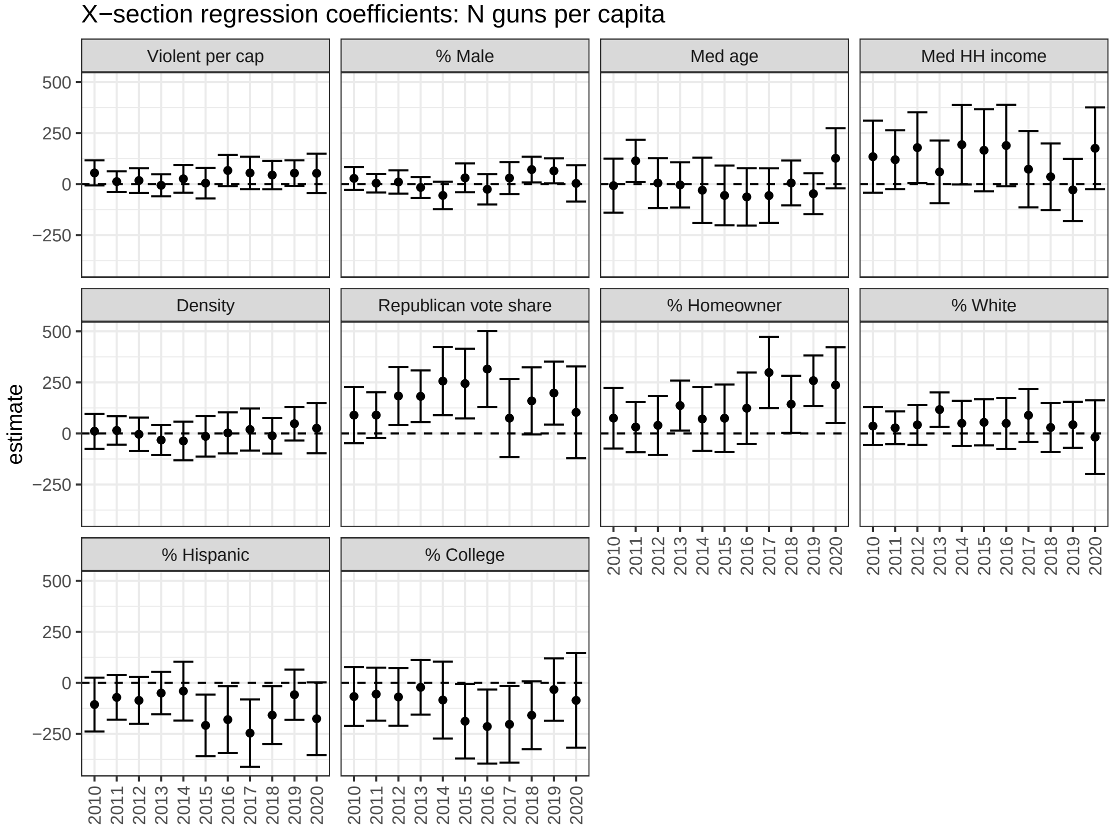
Figure C6 repeats the exercise in Figures 4 and 7, using crime per capita as the outcome. These results show that the types of variables predicting the location of crime have remained largely consistent over the study period.

Figure C4: Repeated cross-sections within Los Angeles Sheriff



Note: This figure plots results from regressing gun purchasing levels on ten covariates each year, using Census tracts within the Los Angeles Sheriff's jurisdiction as the unit of analysis. Each year corresponds to a separate model, and the figure shows the partial correlation of each covariate after standardizing all variables to have a mean of zero and a standard deviation of one. Error bars represent 95% confidence intervals.

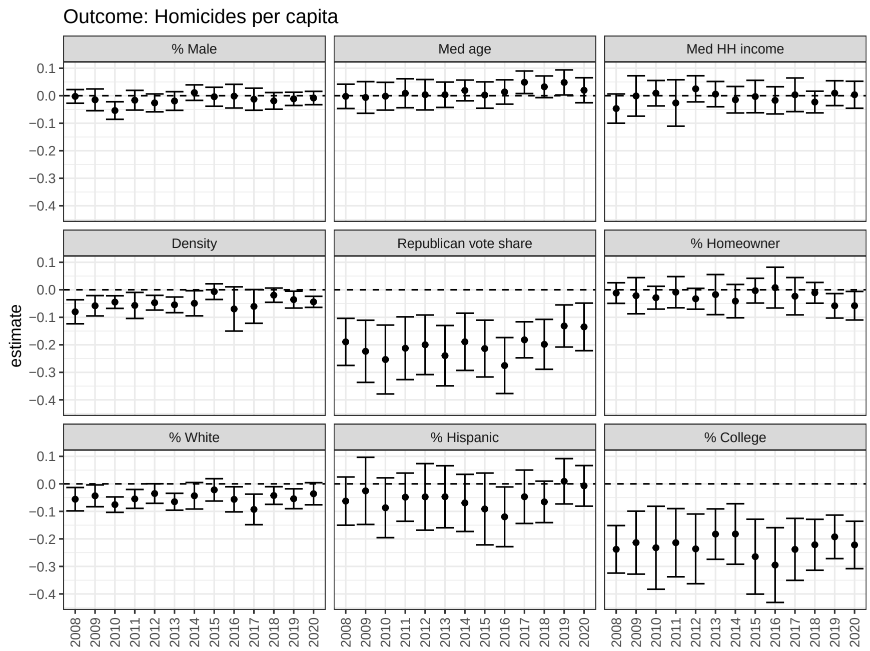
Figure C5: Repeated cross-sections within Long Beach



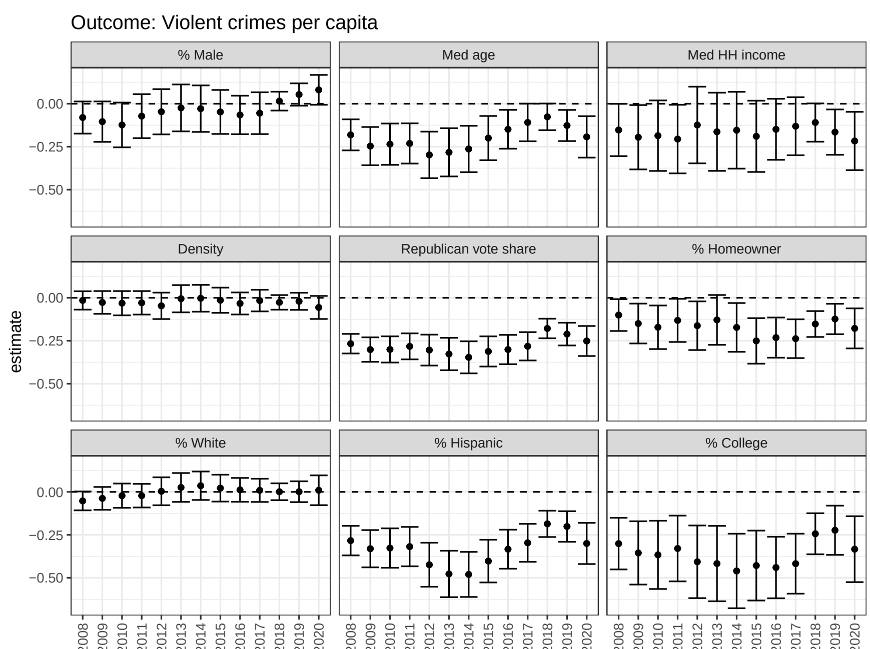
Note: This figure plots results from regressing gun purchasing levels on ten covariates each year, using Census tracts within the Long Beach LEA as the unit of analysis. Each year corresponds to a separate model, and the figure shows the partial correlation of each covariate after standardizing all variables to have a mean of zero and a standard deviation of one. Error bars represent 95% confidence intervals.

Figure C6: Repeated cross sections predicting crime

(a) Within LEA



(b) Across LEA



Note: This figure plots results from regressing crime levels on nine covariates each year. In Panel A, the unit of analysis is a Census tract, the outcome is homicides per capita, and LEA fixed effects are residualized. In Panel B, the unit of analysis is a LEA and the outcome is violent crimes per capita. Each year corresponds to a separate model, and the figure shows the partial correlation of each covariate after standardizing all variables to have a mean of zero and a standard deviation of one. Error bars represent 95% confidence intervals.