Why Do Americans Buy Guns? Crime, Partisanship, and Firearm Demand in California

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Abstract

This paper examines how crime and political context shape firearm purchases. We combine geocoded administrative records of all licit handgun purchases in California (2008–2020), crime reports from law enforcement agencies, and mortality records, allowing us to observe the link between crime and handguns at a granular level over a long temporal span. Across identification strategies, we find a weakly positive relationship, but the effect is substantively small, unstable, and driven by individuals who already own at least one handgun. While violent crime clusters near city centers, handgun purchases occur in suburban and rural peripheries, and have shifted in ways that track home ownership and political partisanship rather than crime. Americans cite protection from crime as the primary reason for gun purchases. Our findings refute the claim that day-to-day experience of crime constitutes a primary driver of firearm consumption, instead highlighting the likely importance of perceived threats and politics.

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1 Introduction

Americans are buying handguns more than ever. Roughly one million Americans become new firearm owners each year (Wertz et al. 2018), and in California, the setting of our study, annual handgun purchases more than doubled between 2008 and 2020. Because gun ownership raises the risk of suicide, homicide, and accidental death (Miller, Azrael, and Hemenawy 2002; Miller, Azrael, and Hemenway 2002; Anglemyer, Horvath, and Rutherford 2014; Studdert et al. 2020), this growth carries potentially large social costs. In 2024, the Surgeon General declared firearm violence a national public health crisis (Office of the Surgeon General 2024). Policy responses to mitigate firearm harm have focused on regulating access and use through background checks, waiting periods, red-flag laws, safe storage, assault rifle bans, and carry permits standards. These policies take the desire to own a gun as given, which raises a prior question of why so many Americans want to buy guns in the first place.

The dominant answer, in both public debate and academic research, is crime. Personal safety is by far the most frequently cited reason for buying a gun in public opinion surveys (Newton and Zimring 1969; Swift 2013; Dimock, Doherty, and Christian 2013; Ward et al. 2024; Alsan, Schwartzstein, and Stantcheva 2026). A large literature argues that higher crime motivates protective purchases (Kleck and Patterson 1993; Southwick 1997; Kleck and Kovandzic 2009; T. Kovandzic, Schaffer, and Kleck 2012, 2013; Kleck 2015), which have also raised concerns about a feedback loop in which crime begets guns, which in turn beget suicide, homicide, and accidental death (Kleck 1979; McDowall and Loftin 1983; Studdert et al. 2022). If this view is correct, it implies that estimates of the effect of gun prevalence on crime, a central empirical question in the economics of crime (Duggan 2001), may be contaminated by reverse causality.

Aggregate trends suggest that this link may not be so straightforward. Between 2000 and 2021, the share of gun owners citing protection against crime as a reason for ownership rose from

67 to 88 percent (Jones 2021). Over the same period, violent crime declined both nationally and in California, while handgun purchases rose steadily (Figure 1).[1] The rising demand for protection amid falling crime suggests one of two things. Either buyers are responding to something other than the actual crime they experience, or aggregate data is too coarse to detect a genuine local response. To distinguish between these, it is necessary to know whether people buy guns when crime increases where they live.

In this paper, we test whether local crime increases handgun purchases by using administrative data that measure both at fine geographic and temporal resolution. Existing tests of the protective purchasing hypothesis rely on self-reported ownership, indirect proxies such as firearm suicides or magazine subscriptions, or data aggregated to states and counties. We instead use the universe of lawful handgun transactions in California from 2008 to 2020, roughly 6.7 million purchases recorded at the point of sale by the California Department of Justice. Because all firearm transfers in California, including private sales, must pass through a licensed dealer, these records capture when each handgun was bought and where the buyer lives, and they allow us to distinguish first-time buyers from existing owners. We link these purchases to crime at two geographic scales. At the city level, we use annual violent crime counts reported by each law enforcement agency (LEA). Because no statewide map of LEA jurisdictions exists, we also construct new boundary files that track jurisdictional changes over time. At the neighborhood level, we assign geocoded homicides recorded in state-wide mortality records to a census tract. We supplement these with street-level records of all violent crime incidents from the Los Angeles County Sheriff's Department and the Long Beach Police Department.

We exploit variation in the location and timing of crime within places at both scales. At the

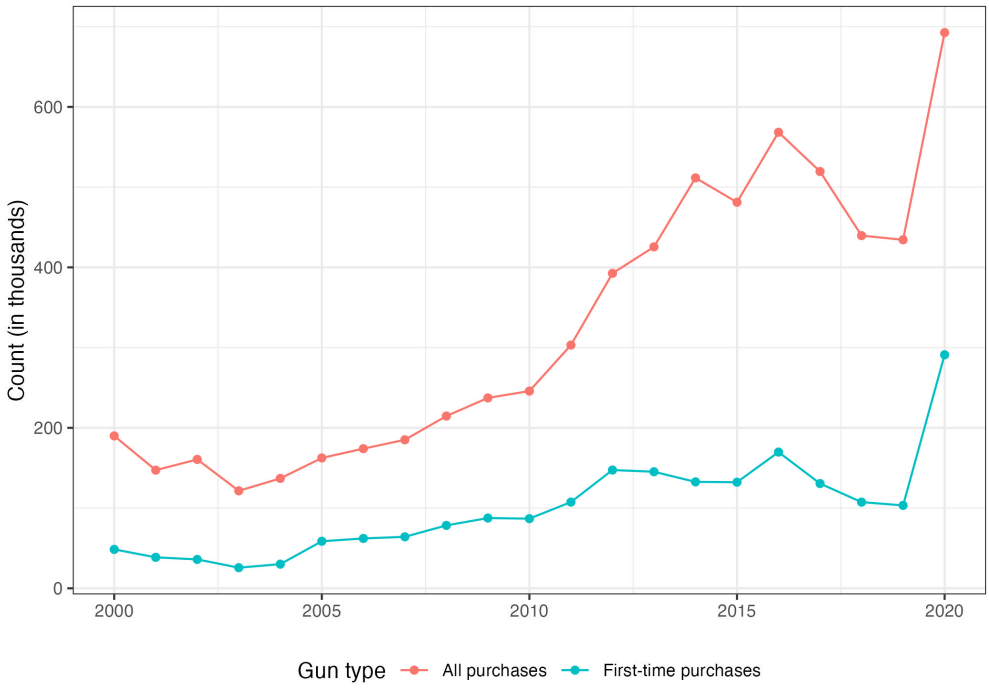
1. Data reported to the FBI by law enforcement agencies exclude unreported crimes. However, the downward trend in violent crime is also evident in the National Crime Victimization Survey, both nationally (Lauritsen and Lopez 2025) and in California (Tinney and Thompson 2025), as well as in the California mortality records we use to measure homicides.

Figure 1: Trends of guns and crime

(a) All crime



(b) Gun purchasing



Note: The top panel plots trends in all crime types, violent crime (including murder and nonnegligent manslaughter, rape, robbery, and aggravated assault), and homicides, assembled from FBI reports. Each time series is normalized by their 2000 value to facilitate comparisons across differing scales. The bottom panel plots the number of first-time gun purchases and repeat purchases, as reported by the California DOJ.

neighborhood level, homicides are rare: only 18 percent of tract-years record one. This allows a difference-in-differences design that compares purchases in tracts experiencing a homicide to their predicted level absent one, using counterfactual imputation estimators that accommodate reversible treatment (Liu, Wang, and Xu 2024; Borusyak, Jaravel, and Spiess 2024). Neighborhoods may be the wrong scale, however, if residents learn about crime through local news and social networks that span a city. We therefore also estimate first-difference models at the LEA level, where most jurisdictions correspond to a city with its own police department. At this scale, endogeneity is a greater concern: in large cities, trends in development, income, and migration may cause crime and gun purchases to move together. Our preferred city-level design therefore focuses on jurisdictions with fewer than 35,000 residents, the median city size in California, where homicides are rare and sporadic. It compares cities that experience a homicide to others in the same metropolitan area and year, conditional on controls. In these smaller cities, a homicide is also likely to be locally salient, which makes this a setting where a protective response should be easiest to detect. Across designs, we test for pre-trends using lagged purchases as a placebo outcome and check for confounding mortality trends using pneumonia deaths.

Finally, to place the role of crime in context, we document how the geography of gun purchases differs from the geography of crime, and we estimate repeated cross-sectional regressions that compare crime against neighborhood demographics, home ownership, and two-party vote share as predictors of purchasing within LEAs. These exercises are descriptive, but they help benchmark and contextualize the estimated magnitude of the crime effect.

Our first finding is that local crime has, at most, a small effect on handgun purchases. Across seven specifications, point estimates are generally positive but mostly indistinguishable from zero, and the estimates that are statistically significant come from city-level designs that show evidence of pre-trends. The designs that pass our placebo tests imply small magnitudes. In the city-level

first-difference model, each additional homicide is associated with 1.4 additional handgun purchases (SE = 0.8); in small cities, where homicides are rare and salient, the estimate is 1.1 (SE = 1.0). These estimates allow us to rule out responses larger than about three handguns per homicide. With roughly 2,000 homicides per year in California, even this upper bound implies that homicides account for about one percent of the state's handgun purchases. Estimates using violent crime are larger. The city-level first-difference estimate implies that violent crime accounts for about five percent of purchases, and roughly one in ten at the upper end of its confidence interval. This estimate is likely biased upward, however, since our event-study estimates show purchases rising in the year before crime increases. At the neighborhood level, a homicide in a tract has no detectable effect on purchases by its residents.

Second, these small and inconsistent effects are driven almost entirely by people who already own a gun. In all but one specification, the estimated effect of crime is larger for repeat purchasers than for first-time buyers; the exception is small cities, where homicides modestly increase first-time purchases. If guns are bought for protection, the first gun should matter far more than the second or third, so a response concentrated among existing owners fits poorly with a protective motive. The response is also asymmetric—purchases rise when violent crime increases but do not fall when it declines. THis pattern is likely produced because handguns are durable goods, but it also means that crime can at most ratchet the stock of guns upward; it cannot explain why purchases grew during a period when crime fell. Finally, we find no response in neighborhoods with high income or home ownership, where residents can most easily afford a purchase.

Next, we conduct several exercises that place these findings in context and clarify some possible explanations for the minimal role of local crime in gun purchases. First, we demonstrate that gun purchases and crime occur in systematically different places. Within cities, homicides are concentrated near the urban core, while purchases per capita rise steadily with distance from the

city hall; across distance quintiles, the two are almost perfectly negatively correlated (ρ = -0.96). This pattern holds in every year of our sample, and it holds when we measure distance within commuting zones rather than cities.

The geography of increasing gun purchases is closely linked to the geography of partisanship. In repeated cross-sections comparing tracts within the same city, Republican vote share is by far the strongest predictor of purchases per capita, and its association roughly tripled between 2008 and 2016. Home ownership is the only other predictor whose association grew over the period. Conditional on these characteristics, homicide rates have essentially no association with purchasing. Grouping tracts by partisanship makes the contrast stark. Republican neighborhoods buy handguns at roughly three times the rate of Democratic ones, a gap that widened over time. Within each partisan group, by contrast, tracts with high and low homicide risk follow nearly identical trajectories. These patterns are descriptive and do not identify a causal effect of partisanship, but they are consistent with work in political science arguing that gun ownership has become bound up with partisan identity (Lacombe 2019, 2021).

Why do gun owners overwhelmingly cite protection against crime when their purchases barely respond to the crime around them? A possibility that we cannot rule out is misreporting, in that protection against crime is a socially acceptable justification for a purchase made for other reasons. However, the strong link between partisanship and purchases suggests a link to research suggesting that political behavior is often better explained by symbolic or sociotropic motives rather than individual material self interest (Sears and Funk 1990). Vote choice tracks perceptions of the national economy more closely than personal finances (Kinder and Kiewiet 1979). Support for tax cuts, including repeal of the estate tax, is only loosely tied to whether respondents stand to benefit (Bartels 2005). White opposition to busing was driven by racial predispositions rather than by having children in affected schools (Sears, Hensler, and Speer 1979). Attitudes toward immigra-

tion are better explained by sociotropic and cultural concerns than by labor-market competition (Citrin et al. 1997; Hainmueller and Hiscox 2007, 2010), and preferences for redistribution reflect beliefs about deservingness more than expected payoffs (Fong 2001). Economists have formalized related ideas as identity-based preferences (Akerlof and Kranton 2000).

Most of this existing evidence, however, concerns attitudes rather than behavior, and a common objection is that symbolic motives dominate only because expressing an opinion is costless; self-interest should reassert itself when behavior is consequential. Consistent with this view, personal stakes did not predict whites' attitudes toward busing, but they did predict whether whites actively protested against it (Green and Cowden 1992), and vested interest predicts behavior more strongly than attitudes (Crano 1997). Handgun purchasing then, is a hard case for symbolic accounts on exactly these grounds. It is costly, requiring several hundred dollars, a background check, a ten-day waiting period, and the ongoing risks of ownership. That even this behavior is largely decoupled from the local crime it is meant to address, while tightly linked to political partisanship, suggests an important role for symbolic rather than purely instrumental explanations of behavior.

Our paper contributes to three bodies of literature. First, we contribute to research on the relationship between gun prevalence and crime. Much of this work aims to estimate the effect of guns on crime and treats the reverse channel, from crime to guns, as a source of endogeneity (Kleck 1979; Southwick 1997; Duggan 2001; Kovandzic, Schaffer, and Kleck 2013). Because gun ownership is rarely observed directly, these studies rely on proxies such as magazine subscriptions (Duggan 2001), firearm suicides (Kleck and Patterson 1993; T. Kovandzic, Schaffer, and Kleck 2012, 2013), or the share of crimes committed with guns (Kleck and Patterson 1993), measured for the nation as a whole (Kleck 1979; Southwick 1997; Douglas C Bice and D. D. Hemley 2002), for states (Miller, Hemenway, and Azrael 2007; Siegel, Ross, and King III 2013), or for counties (Duggan

2001; T. Kovandzic, Schaffer, and Kleck 2012, 2013). Measuring the reverse channel directly with transaction-level data, we find that it is small, which bounds the scope for simultaneity bias in this literature.

Second, we contribute to work on why Americans buy guns. Early survey evidence established protection as the dominant stated motive for ownership (Cook and Ludwig 1996), and recent work argues that owners and non-owners share a common safety objective but disagree about whether firearms achieve it (Alsan, Schwartzstein, and Stantcheva 2026). A separate literature in political science and sociology emphasizes guns as markers of political identity and citizenship (Carlson 2015; Joslyn 2020; Lacombe 2021). In economics and public health, studies of gun demand have largely focused on responses to salient national events: purchases rose ahead of the 2008 presidential election in anticipation of stricter regulation (Depetris-Chauvin 2015) and after mass shootings (Levine and McKnight 2017; Studdert et al. 2017). We provide the most granular test to date of whether purchases respond to locally experienced crime. Taken together with this prior work, our results suggest that gun demand responds to highly publicized and symbolic events far more than to crime in buyers' own neighborhoods.

Third, our findings connect to research distinguishing acute fear of local crime from a diffuse belief that the world is becoming dangerous (Stroebe, Leander, and Kruglanski 2017). Such diffuse fears and generalized mistrust predict protective gun ownership far better than specific fears of crime or perceived risk of victimization (Warner and Thrash 2020). Gun ownership also correlates with perceptions of economic decline and social disorder (Carlson 2015; Jackson 2006; Britto 2013; Hirtenlehner and Farrall 2013), with declining confidence in government's capacity to provide public safety (Jiobu and Curry 2001), and with the COVID-19 pandemic (Miller, Zhang, and Azrael 2022; Lacombe et al. 2024; Rosenberg et al. 2025). This work relies largely on surveys; our behavioral evidence complements it by showing that if purchases are protective, the threat buyers

respond to is not the one recorded in local crime statistics.

The remainder of the paper proceeds as follows. Section 2 describes the data, and Section 3 presents our empirical strategy. Section 4 estimates the effect of local crime on handgun purchases, including heterogeneity by buyer type and the direction of crime changes. Section 5 examines where handguns are purchased in California and how purchasing correlates with neighborhood characteristics. Section 6 concludes.

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: Gun regulation in California is among the strictest nationally. Handgun purchasers must be 21 or older, and the state enforces a 10-day waiting period before they can take possession. All firearm transfers and purchases, including private sales, must go through a dealer with a Federal Firearm License and undergo a formal background check. The California Department of Justice (CA DOJ) records these transactions in the Dealer Record of Sale (DROS) database.

We analyze geocoded handgun transactions in DROS 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 include variables such as the purchaser's home address and the time of acquisition, which we use to geocode transactions and aggregate into per-capita counts at various geographic levels 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. 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 specific types of crime incidents per capita. In the context of our paper, we focus on homicides and total violent crime (homicides, rape, robbery and aggravated assault). We focus on homicides because we find them to be most strongly correlated with gun purchasing, out of the all crime types in a simple levels regression. 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 (2001) and D. C. Bice and D. Hemley (2002) 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.

Panel 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:

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

This specification assumes that, after accounting for 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 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, Wang, and Xu 2024; Borusyak, Jaravel, and Spiess 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 Limited Effects of Crime on Gun Purchasing

Table 1 summarizes the results and the statistical methods used to explore the relationship between the threat of crime and gun purchasing using the full panel data. Although the empirical patterns are broadly consistent with the idea that crime influences gun purchasing, they do not support the claim that crime is a large or primary driver.

Table 1a presents results for the LEA-level analysis and Table 1b presents results for the tract-

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)	1.10 (1.01)
Effect for new guns (SE)	0.062* (0.03)	0.037 (0.04)	0.35 (0.44)	0.76* (0.32)
Effect for repeat guns (SE)	0.17*** (0.05)	0.14*** (0.03)	1.00 (0.60)	0.34 (0.83)
Placebo: lagged outcome (SE)	0.28** (0.11)	0.23 (0.21)	0.28 (1.05)	-0.13 (1.43)
Placebo: pneumonia deaths (SE)	-0.00 (0.01)	-0.00 (0.01)	0.10 (0.07)	0.00 (0.11)
Implied % of outcome explained	6.98%	5.13%	0.411%	0.331%

(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: lagged outcome (SE)	0.04 (0.12)	0.05 (0.03)	-3.72 (2.18)
Placebo: pneumonia deaths (SE)	-0.03 (0.02)	0.00 (0.00)	-0.12 (0.22)
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).

level analysis. The first three rows of each table provides a summary of the different designs, estimation methods, and samples. The fourth row indicates the 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 on total gun purchasing, row six on first-time gun purchases, and row seven on 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. We check this using two placebo outcomes. One implication is that contemporaneous crime shocks should not affect previous gun purchasing values, so we expect the coefficient from regressing the pre-treatment outcome on crime to be close to zero and statistically insignificant. Row eight shows the effect on this placebo outcome: gun purchasing in the immediately preceding time period. Row 9 shows effects for an alternative placebo: pneumonia deaths. Second, since crime trends should not affect number of pneumonia deaths in a local area, a statistically significant effect here would suggest unmeasured confounding correlated with other trends in causes of deaths.

Finally, row ten provides a summary statistic to interpret the effect sizes across designs. We multiply the observed number of crimes by the estimated effect of crime on gun purchasing, then divide this predicted number of guns purchased by the actual number purchased during the sample period. This yields an estimate of the expected share of actual gun purchasing attributable to crime.

Overall, the relationship between crime and gun purchases is generally positive but weak and imprecise. Across seven specifications, only two coefficients are statistically significant (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. For all specifications, the effect of crime on pneumonia deaths is statistically insignificant and close to zero, providing some reassurance the results are not driven by omitted variables correlated with broader mortality trends.

However, we hasten to add that the two statistical significance estimates from the LEA analysis warrant caution. Compared to tract-level estimates, LEA-level estimates are more susceptible to bias from broader developmental, income, or migration trends that independently affect both crime and gun purchasing. Indeed, the estimate in column 1 fails the "pre-trend" test, meaning that when using lagged gun purchases as a placebo outcome, the estimated effect is positive and significant, indicating upward bias. Appendix Figure B2 confirms a clear upward pre-trend in the LEA-level two-way fixed effects results. In a similar vein, the specification in column 2 substantially reduces pre-trends, but the estimate for the period immediately prior to treatment is larger than the contemporaneous effect itself. This suggests that the estimated treatment effect may also be upwardly biased by confounding or is not larger than the baseline variation in the outcome.

Moreover, 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.

Across seven empirical settings only one—the LEA-level first-differences estimate—yields a statistically significant correlation that potentially survives pre-trend checks. This specification suggests that violent crime 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 a positive bias in the estimate.

The remaining majority of models all pass basic pre-trend tests (columns 3–4 of Table 1a and columns 2–3 of Table 1b), but yield effect size of less than 1% and many of the estimates statistically indistinguishable from zero. Insofar as crime is correlated with gun purchasing, it is correlated with repeat purchases rather than first-time acquisitions. Collectively, while the correlation between crime and gun purchasing is positive, the 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 buy guns in response to local crimes, which we report in Table C1. 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 1a, 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.

Finally, in Appendix Table C2, we re-estimate the tract-level model from Table 1b, column 3, but allow for potential spillover effects of homicides. Specifically, we code tracts that share

a border with a tract experiencing a homicide as treated and re-run the difference-in-differences analysis. Although the estimated effects are larger than those in Table 1b, column 3, they remain statistically insignificant.

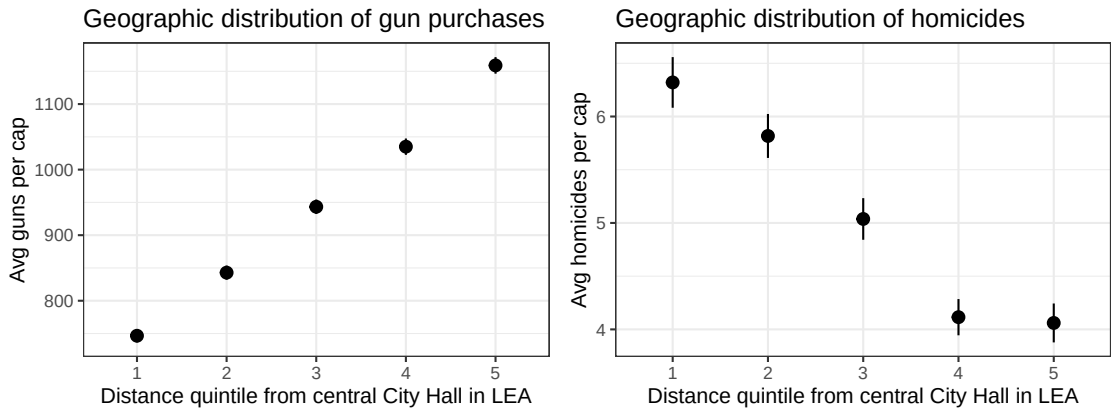
Overall, the pattern of estimates in this section is consistent with the idea that positive shocks to crime induce some non-negative response in gun purchasing, but that the magnitudes of the effect are unstable and small, while decreases in crime are not associated with reduced sales. While it appears 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.

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.

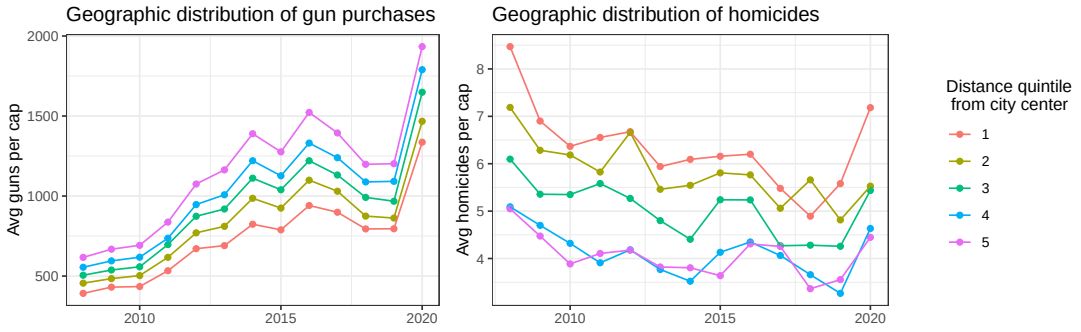
An important first observation is that within California's metro areas, guns are not purchased in the same places where crime occurs. Figure 2 contrasts the geographic location of gun purchases and crime. Borrowing a technique from urban economics (Glaeser, Kahn, and Rappaport 2008), we first categorize each tract in California by how far it is from the center of 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 quintiles. For every quintile, we plot the average gun purchasing volume per capita and the average number of homicides per capita. Note that there there are currently roughly 510 LEAs for each year in our sample in

Figure 2: Geographic distribution of gun purchasing and homicides by distance from city hall



Note: The figure plots the average number of guns purchased per capita, with tracts categorized by distance quintiles from their local City Hall. The figure shows results averaged across Law Enforcement Area (LEA) level. Error bars represent 95% confidence intervals.

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 2, but shown over time.

California. [2]

We find that more guns are purchased in the periphery while crime tends to happen closer to the city center. If we plot gun purchasing against homicides for every quantile the correlation is -0.96. This geographic relationship is stable over time as well. In Figure 3, we show the same quantity over time. 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

2. There are more LEAs that are responsible for specific facilities like government buildings or public transit routes. We exclude these from our analysis.

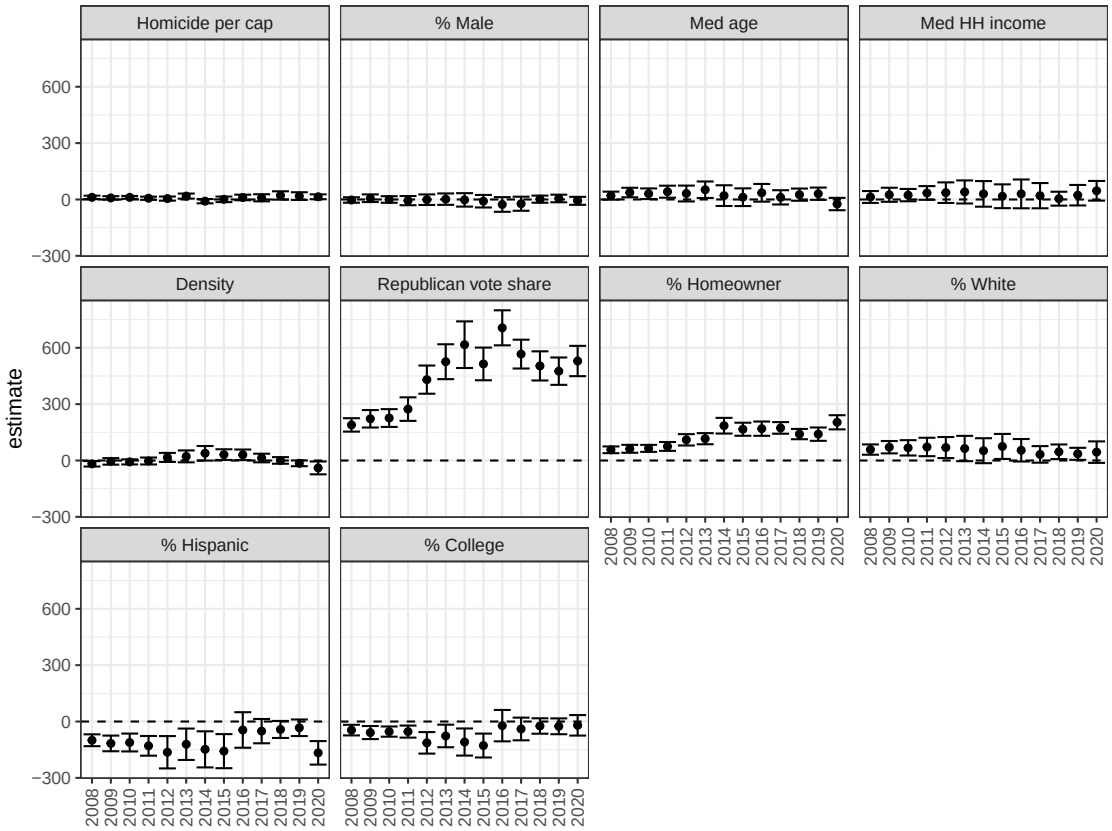
remains the same; more guns are purchased further away from city centers, whereas inner city areas are home to the lowest rates of gun purchasing per capita across time.

Homicides also consistently occur in tracts located closest to the city center. Although there is more year-to-year variation in the exact locations of homicides, this central pattern remains generally stable over time. The stability of 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 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. In Appendix D we repeat this analysis, analyzing tracts within commuter zones instead of LEAs, and find the same patterns. Note that commuting zones are much larger aggregations of counties meant to capture the metropolitan areas within which people tend to spend much of their time and commute for work.

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.

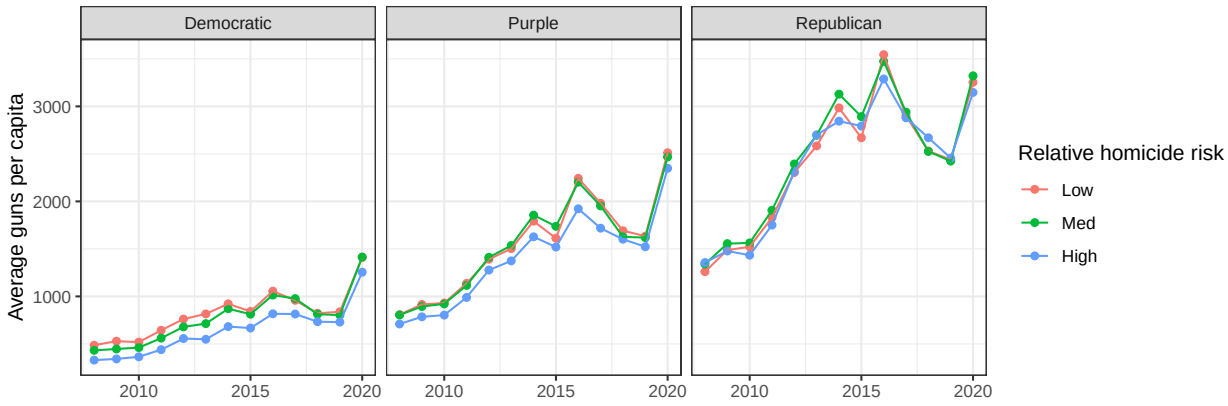
Each point in Figure 4 can be interpreted as the partial correlation of a demographic variable in a given year; in other words, within each LEA, how much change in gun purchasing would we expect after a one-standard deviation change in the predictor holding other factors constant? The

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.

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 4 is difficult to ignore. Consistent with the work of Lacombe, Howat, and Rothschild (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), with separate plots of average gun purchases per capita for low, medium, and high levels of homicide risk within each city. Risk of homicide explains very little of the variation in gun purchasing: within each partisan category, tracts with low, medium, and high homicide risk exhibit nearly identical purchasing trends. While gun purchases per capita have risen across all types of neighborhoods, Republican neighborhoods consistently purchase handguns at substantially higher rates than Democratic neighborhoods, and this gap has widened over time.

6 Discussion and Conclusion

Personal safety is the most frequently cited reason Americans give for buying a gun, yet little direct evidence links gun purchases to the crime buyers actually experience. Using the universe of lawful handgun transactions in California, we find that local crime has at most a small effect on purchasing. Our preferred estimates allow us to rule out responses large enough for homicides to account for more than about one percent of handgun purchases. The response we do detect is concentrated among existing owners. If anything, purchases rise slightly when crime increases but do not fall when it declines. Meanwhile, guns and crime occur in systematically different places. Purchases are concentrated in suburban and rural areas where crime is rare, and their geography aligns closely, and increasingly, with the geography of partisanship.

Why do a growing share of gun owners cite protection against crime when local crime is such a weak predictor of purchasing? While one possibility is that protection is a socially acceptable justification for a purchase made for other reasons, a more plausible explanation is that buyers respond to a diffuse perception of danger, filtered through social and political context, rather than to the crime around them. Diffuse fears and generalized mistrust predict protective gun ownership

far better than specific fears of victimization, and their predictive power falls sharply once partisanship is taken into account (Warner and Thrash 2020). Perceptions of crime are also themselves strongly partisan. Republicans report far more worry about crime than Democrats and are much more likely to believe it is rising (Brenan 2022, 2024), a pattern also documented in California (Bonner 2022).[3] Our own data show that within cities, more Republican neighborhoods experience fewer homicides. Salient but non-local events like mass shootings or violent civil unrest may prime this more generalized sense of fear, and survey items that ask about perceptions of crime or fear of victimization may partially reflect these diffuse threats.

Our results have broader implications. First, they call into question the notion of a vicious cycle in which crime begets guns, which in turn beget suicide, homicide, and accidental death. The first link in that cycle appears to be weak, which also limits the scope for simultaneity bias in studies estimating the effect of gun prevalence on crime. Second, policy debates over gun violence have focused on regulating access and use, but our findings suggest that demand for firearms is shaped by beliefs that operate upstream of these regulations. If, as recent survey evidence suggests, owners and non-owners share a goal of safety but disagree about how to achieve it (Alsan, Schwartzstein, and Stantcheva 2026), then information about risks and alternatives may be a policy lever in its own right.

Our analysis has limitations and leaves opportunities for future work. We study a single state with unusually strict gun laws, observe only lawful handgun purchases, and measure crime annually. Nor can our administrative data directly measure the beliefs that might drive demand. Future work could link purchasing data to measures of media exposure, such as partisan news consumption, or to local social networks. Understanding these demand-side forces may prove as consequential for policy as the study of gun laws themselves.

3. A large literature in psychology examines the relationship between belief in a dangerous world and political ideology. For a recent review, see Clifton and Kerry (2023).

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Supporting Information

"Why Do Americans Buy Guns? Crime, Partisanship, and Firearm Demand in California"

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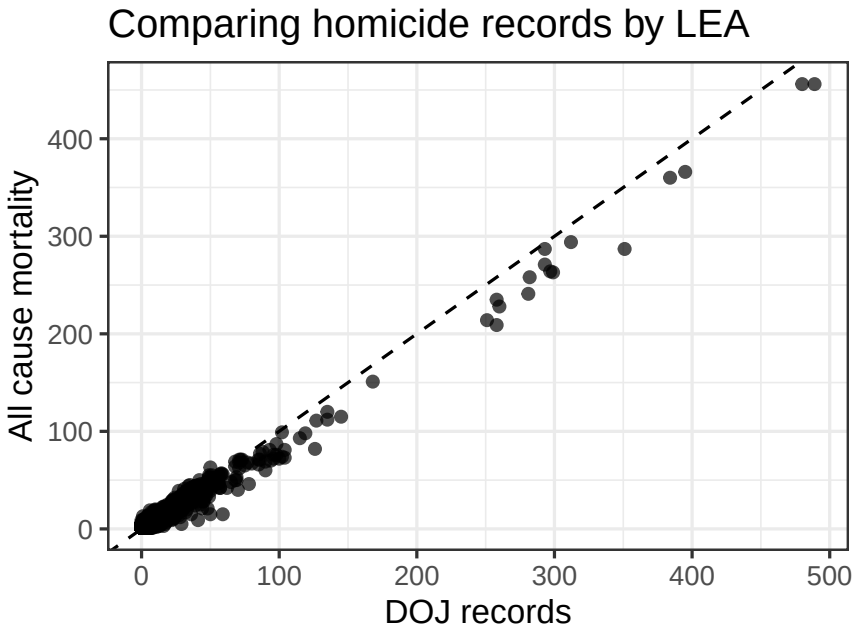
D Additional analyses for descriptive patterns of crime and guns A7

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.

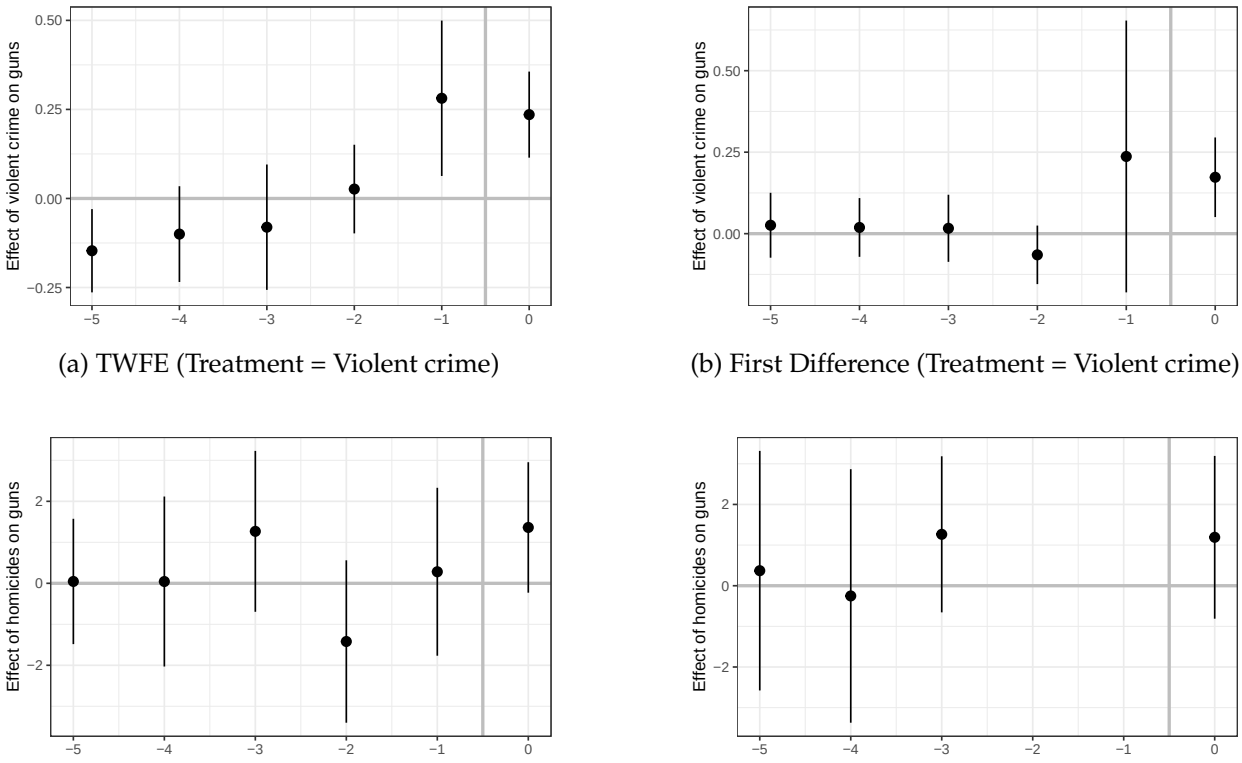
Figure A1: Comparing mortality data against DOJ data



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)

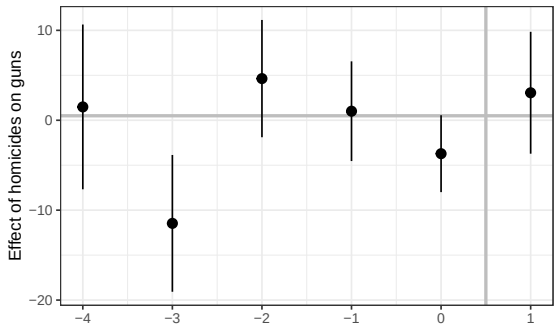
(b) First Difference (Treatment = Violent crime)

(c) First Difference (Treatment = Homicides)

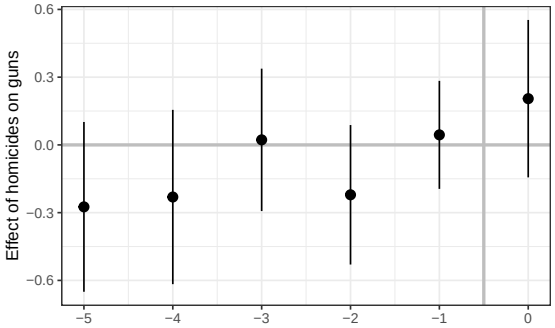
(d) Random Homicide Location

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.

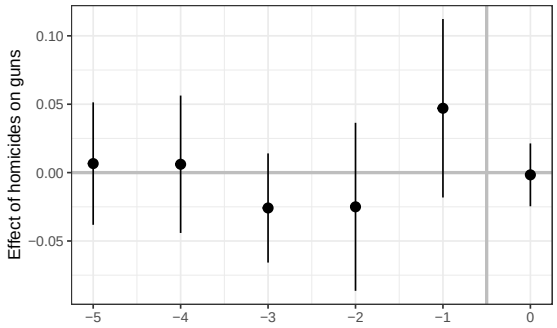
Figure B3: Tract-level analysis pretrends



(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 panel analyses of crime and guns

Table C1: 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).

Table C2: Effect allowing for spillovers of tract-level crime

Model/Design	*Events study (imputation method)*
Unit & Sample	All Tracts in CA (2008–2020)
Sample size	21211
Coefficient of interest	Homicide occurrence (binary)
Total effect (SE)	10.17 (5.28)
Effect for new guns (SE)	5.12 (1.71)
Effect for repeat guns (SE)	5.05 (4.56)
Placebo test using lagged outcome (SE)	-2.03 (4.42)
Implied % of outcome explained	0.18

Notes: Results from models estimating the effect of crime on gun purchasing. Tracts are coded as treated if they experience a homicide or share a border with a tract that does. The treatment indicator equals 1 under these conditions and 0 otherwise. Standard errors are bootstrapped. $p < 0.05$, $p < 0.01$, $p < 0.001$ (two-tailed tests).

D Additional analyses for descriptive patterns of crime and guns

In this section, we conduct additional analyses to supplement that of Section 5.

First, we repeat the analyses in Figure 2 and Figure 3, but categorize tract by their distance from the city hall associated with the central city in the commuting zone. We break the distance from the central city hall into deciles and plot the average levels of gun purchases and homicides. We find the same takeaways: that the geogrpahic distribution of gun purchases and homicides are inversely correlated and this relationship is stable over time.

Next, 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 D7 and D6 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 D8 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 D4: Distribution of gun purchases and homicides within commuting zones

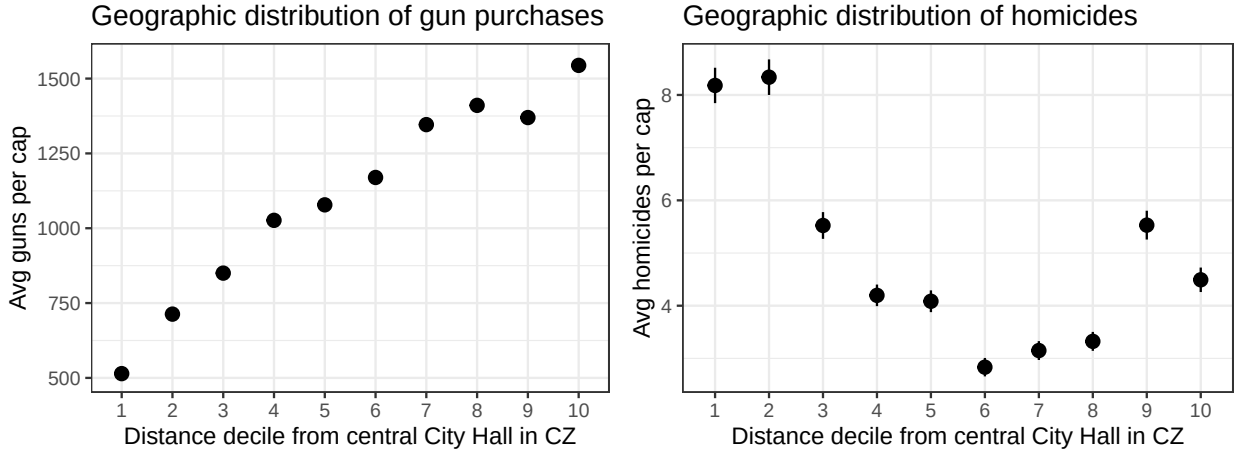


Figure D5: City hall graphs over time (using commuting zones)

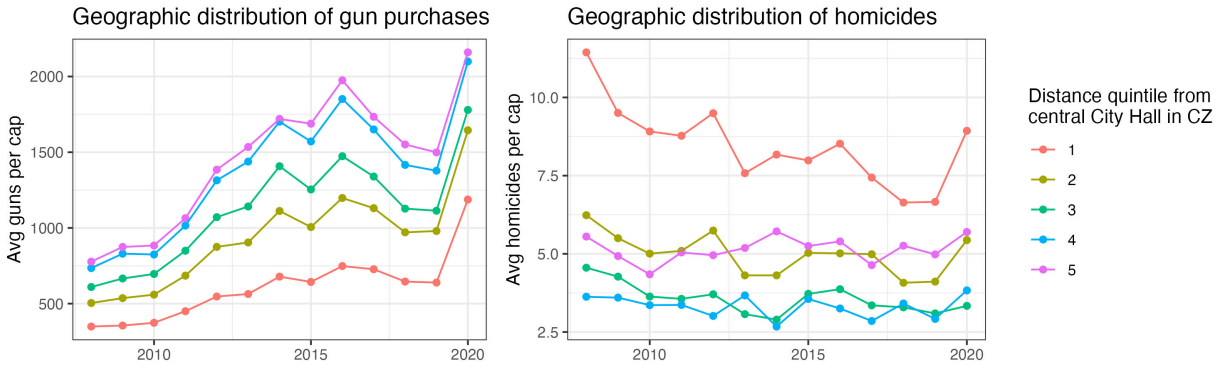
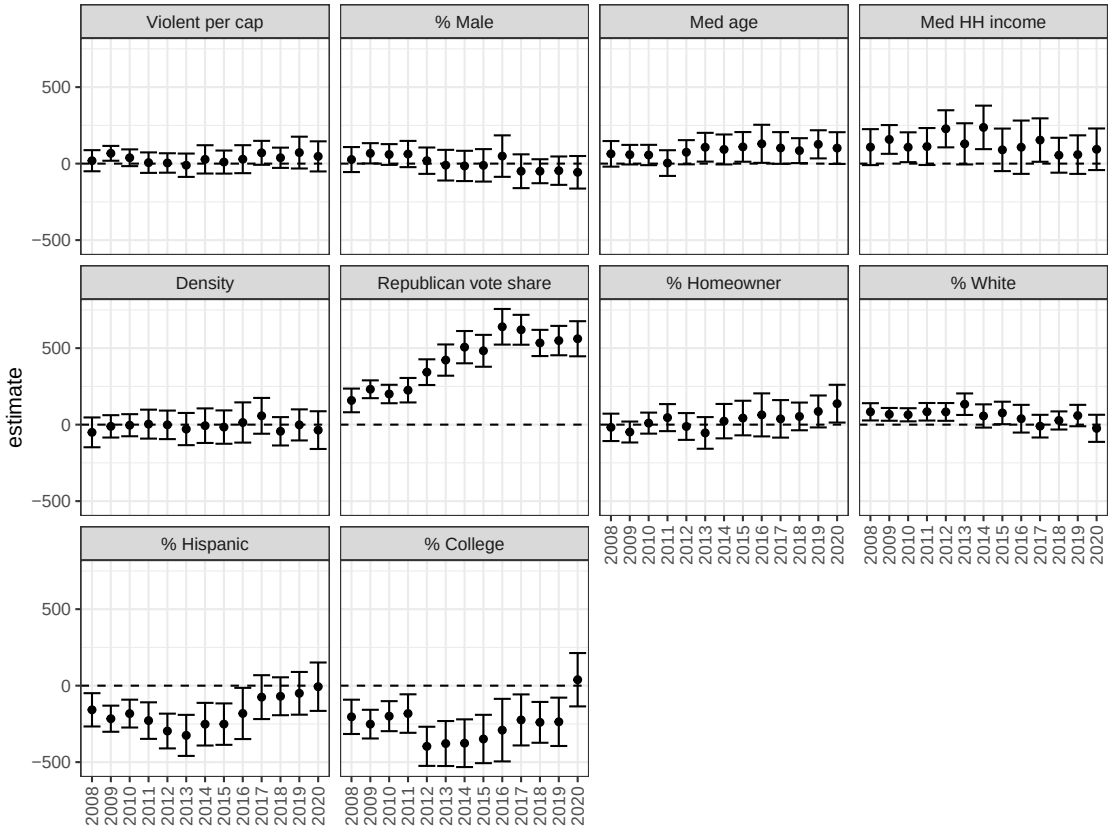
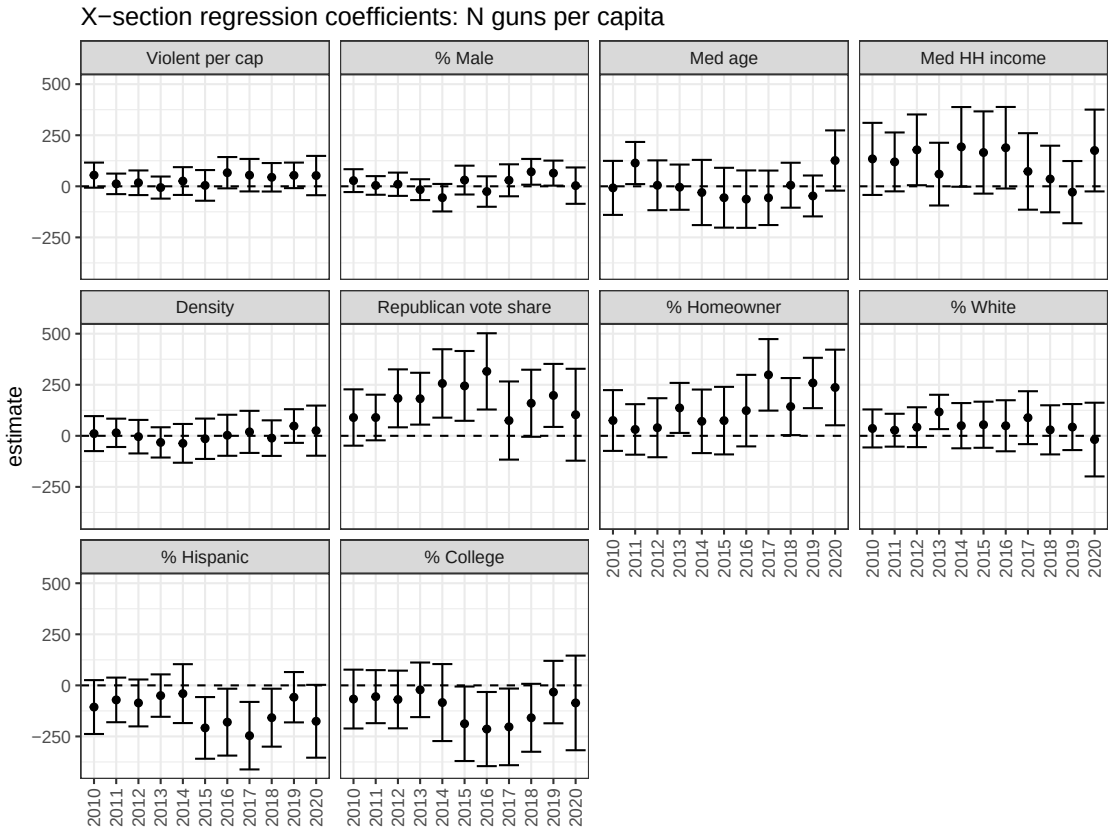


Figure D6: Repeated cross-sections within Los Angeles Sheriff



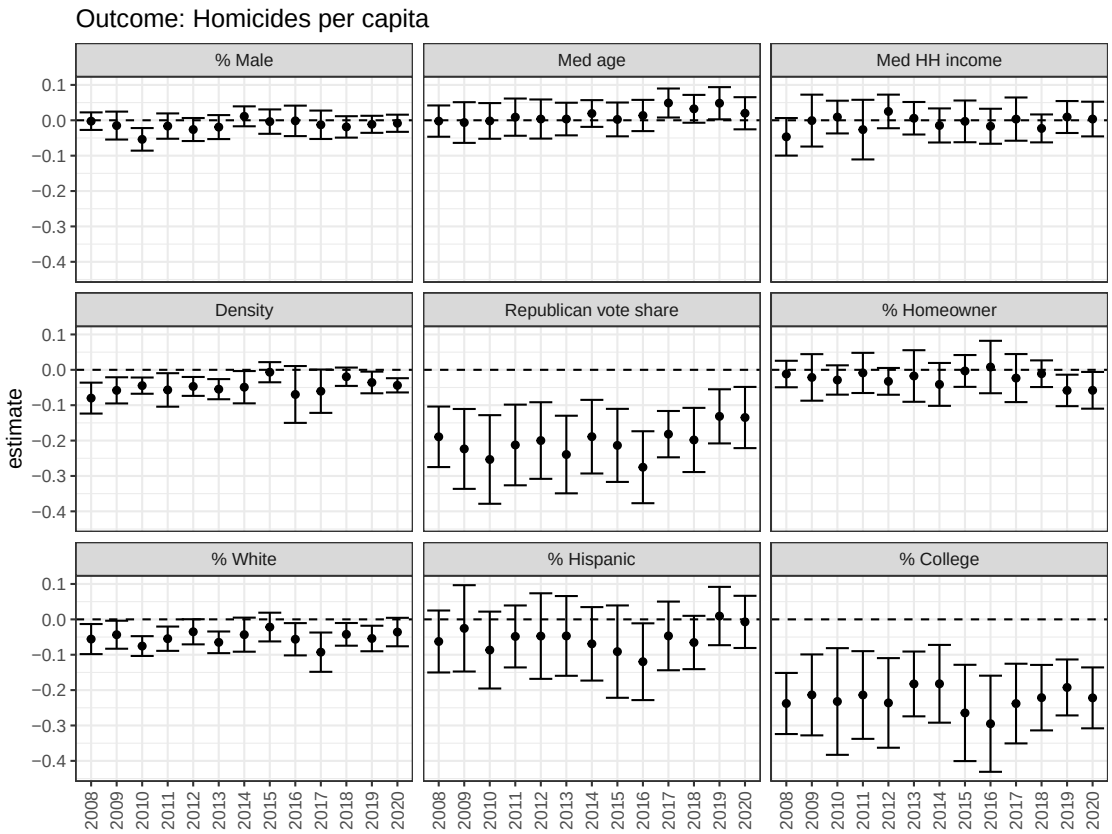
Note: This figure plots results from regressing gun purchasing levels on ten covariates each year, using Census tracts within the Los Angeles Sheriff's jurisdiction as the unit of analysis. Each year corresponds to a separate model, and the figure shows the partial correlation of each covariate after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

Figure D7: 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 D8: Repeated cross sections predicting crime



Note: This figure plots results from regressing crime levels on nine covariates each year. The unit of analysis is a Census tract, the outcome is homicides per capita, and LEA fixed effects are residualized. 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.