Guns, Law Enforcement, and Death in the United States of America*

December 5, 2025

Abstract

Firearm availability is believed to increase gun deaths, but causal links are hard to establish due to poor data. To fill this gap, we develop a shift-share inspired measure of local firearm supply, validated against background checks and permits. Using 1995–2019 county-level data, we find that a 1% increase in gun availability raises gun homicides and suicides by about 0.15%. Police presence reduces the firearm-homicide link but not suicides, suggesting law enforcement mitigates criminal violence but not self-harm. These results indicate that gun control and policing are complementary policies rather than substitutes.

Key Words: Firearms; Death; Punishment Probability
JEL Code: D0, I12, I18, Z18; K4; K14

*We thank William King, Jeffrey Fagan, Devika Hazra, Jamein Cunningham, Joshua Robinson, attendees of the Middle Tennessee State University Seminar, University of Texas Crime, Law, and Policy Workshop attendees, attendees of 2025 Canadian Economic Association Conference, attendees of 2025 Midwestern Economic Association Conference, and attendees of the 2025 College of Charleston Seminar series for valuable comments and suggestions.

1 Introduction

The United States exhibits exceptionally high rates of firearm-related deaths relative to other developed nations. This pattern has generated substantial debate over whether widespread firearm availability drives these outcomes or whether other factors—such as mental health issues or local crime conditions—are primarily responsible. This paper examines the causal relationship between changes in local firearm supply and gun deaths, while simultaneously investigating how police presence can mitigate these effects.

We develop a novel shift-share identification strategy that exploits variation in local firearm dealer presence weighted by national gun production trends. This approach addresses a fundamental challenge in the firearms literature: the absence of comprehensive data on gun sales and ownership. Our method leverages the number of Federal Firearm License (FFL) holders in each county, weighted by the inverse of national gun production per dealer. This weighting accounts for temporal changes in dealer productivity while ensuring that our firearm availability measure is orthogonal to local crime conditions through two key features. First, national production levels are usually independent of local crime rates. Second, short-run barriers to FFL entry prevent contemporaneous responses to local demand shocks. This second reason is consistent with recent evidence showing that U.S. firearm markets (gun dealer presence) exhibit low-dimensional structure where most variation stems from stable spatial components rather than time-varying local factors (Topaz and Johnson, 2025).[1]

Our identification strategy makes three primary contributions to the literature. First, we provide causal evidence on the firearm-mortality relationship using a 25-year panel (1995-2019) that spans a longer time horizon than previous studies. Second, we show that police presence significantly moderates the relationship between firearm availability and gun deaths, with stronger effects for homicides than suicides. Third, our novel firearm sales measure accurately predicts observed gun-related transactions. We show this by **validating** our measure using National Instant Criminal Background Check System (NICS) checks, reported gun thefts, and Google searches at the national, state level background checks (explicitly for a gun), and county level gun sales. Using county-level data, we find that a 1% increase in estimated new gun sales leads to approximately a 0.15% increase in both gun homicides and suicides. Importantly, we also find evidence that police presence (measured primarily by motor vehicle deaths per capita as a proxy for enforcement intensity) significantly reduces the impact of increased firearm availability on gun deaths. This moderating effect is strongest for homicides, with no measurable impact on the firearm-suicide relationship.

Our results suggest that the harmful effects of increased firearm availability are substantially amplified in areas with limited police presence, though these represent net effects that warrant careful interpretation.[2] We acknowledge that firearms serve defensive purposes and

[1]It is also debatable that gun sales itself has a large crime-related endogenity problem . Like gun dealers, Johnson and Topaz (2025) show that gun sales and concealed carry permits are also low dimensional. For example, a single stable spatial component explains 95 % of the variance in gun sales.

[2]It is plausible there are other mechanisms at play. For example, our proxy for policing may be capturing local crime. In this case, the interaction effects could suggest gun presence increases the intensity of a local crime or people are more likely to carry their weapons when crime is high which would present more opportunities of theft (c.f., Billings, 2023).

may prevent some homicides, particularly in high-crime areas where such defensive uses are most common (Kleck and Bordua, 1983). However, our estimates capture the net impact of increased gun availability, and this net effect is unambiguously an increase in gun deaths. Importantly, even in areas with strong police presence, we find no evidence that increased firearm availability reduces homicides through deterrence. The persistence of positive effects on suicide across all policing levels further reinforces this interpretation. Since firearm availability increases suicide risk regardless of police presence, even hypothetical deterrent effects on homicide would be offset by increased self-harm. These results suggest that the social costs of expanded firearm access operate through multiple channels that police presence can only partially mitigate.

Our findings connect two important policy debates: gun control and police funding. The results suggest that areas experiencing increases in firearm availability may particularly benefit from enhanced police presence, as the marginal value of law enforcement appears greatest where gun access is highest. This interaction has direct implications for resource allocation in public safety. Rather than viewing gun control and policing as substitute policies, our evidence indicates they may be complements—with police effectiveness in reducing gun violence increasing precisely where firearm availability is expanding. From a policy perspective, this suggests that communities unable or unwilling to restrict gun access may find particular value in investing in law enforcement capacity. Conversely, areas considering reductions in police presence should account for the potentially amplified consequences in neighborhoods with high gun availability. The heterogeneous effects we document also imply that optimal policing strategies may vary systematically across jurisdictions based on local firearm prevalence. Areas with limited gun access may achieve public safety goals with lower police intensity, while areas with widespread firearm availability may require more intensive enforcement to achieve comparable outcomes. These insights are particularly relevant for policymakers facing budget constraints, as they suggest that the return on investment in policing may be higher in areas where gun availability is expanding, potentially justifying differential resource allocation across communities with varying firearm access levels.

The rest of the paper is as follows: in Section 2 we discuss some of the relevant background literature, in Section 3 we introduce the data and discuss the primary assumptions, in Section 4 we present the results, and last, in Section 5 we offer a brief conclusion.

2 Background

Most empirical work on firearm prevalence and mortality relies on proxy measures of gun ownership, as comprehensive firearm registries do not exist in the United States.[3] Common proxies include the share of suicides/accidents committed with firearms (Azrael et al., 2004; Cook and Ludwig, 2006; Siegel et al., 2013; Edwards et al., 2018; Chalak et al., 2022), background check/concealed carry volumes (Lang, 2013, 2016; Edwards et al., 2018; Depew and Swensen, 2019; Johnson et al., 2024), magazine subscriptions (Duggan, 2001), self-reported surveys (e.g., Mocan and Tekin, 2006; Moody and Marvell, 2005), the number of

[3]There are exceptions. Johnson et al. (2024), for example, uses county-level gun sales to measure gun sales.

federally licensed firearm dealers (Wiebe et al., 2009; Steidley et al., 2017; Stansfield et al., 2021; Johnson and Robinson, 2024), and some combination of the different proxies (Siegel et al., 2014; Schell et al., 2020; Morral et al., 2024).

These studies consistently find positive associations between firearm prevalence and fatal outcomes, suggesting net social costs of firearm ownership – though there are exceptions (c.f., Moody and Marvell, 2005). Using the percentage of suicides completed with a gun as a proxy for gun ownership, Cook and Ludwig (2006), Siegel et al. (2013), and Chalak et al. (2022) all find a positive relationship between gun ownership and homicide. A similar relationship emerges in Duggan (2001), which links subscriptions to *Guns & Ammo* (a popular gun-interest magazine) to higher homicide rates. However, because of the mechanical relationship between the proportion of gun suicides and overall suicide rates, researchers often use other measures to explore the link between gun presence and suicide. For example, Lang (2013) identifies a positive relationship between firearm background checks and firearm suicides, while Duggan (2001) finds a similar pattern using magazine subscriptions. It is important to note, however, that many of these proxy variables are only available at the state level, and others—such as magazine subscriptions—are less suitable today.[4]

The mechanism, when the outcome is homicide, underlying these relationships appears to involve spillovers from legitimate to illegitimate gun use. Billings (2023) documents that concealed handgun permit holders face increased property crime victimization, with firearms frequently stolen. The study further shows that violent firearm crime increases in neighborhoods with more permit holders, suggesting that stolen guns contribute to local violence. This spillover hypothesis receives additional support from Kahane (2020) and Khalil (2017), who find positive relationships between gun thefts and subsequent firearm-related assaults, homicides, and robberies. Importantly, Khalil (2017) demonstrates temporal separation between gun theft and violent crime, with no contemporaneous relationship but significant lagged effects. This temporal pattern aligns with our finding that changes in local gun supply affect mortality outcomes with a lag. The spillover mechanism helps explain why increases in legitimate gun ownership can paradoxically increase gun violence. Phillips et al. (2013) and Johnson et al. (2024) provide additional evidence for this channel, showing how legal gun purchases can inadvertently supply illegal markets through theft and diversion. Our work builds on these findings by providing a cleaner identification strategy for measuring the causal effects of firearm availability changes.

The theoretical mechanism linking police presence to firearm-related mortality operates through multiple channels that become particularly important as local gun availability increases. First, police enforcement can directly interdict firearms before they are used in crimes through traffic stops, searches incident to arrest, and proactive policing in high-crime areas. This effect becomes more valuable as the local gun supply expands, since a larger pool of firearms creates more opportunities for diversion to criminal use. Second, police presence may deter criminal activity generally, but this deterrent effect should be strongest for crimes requiring specific tools—such as firearms—that can be seized during police encounters. Third, police can disrupt the secondary markets through which stolen or diverted firearms reach potential perpetrators, with this disruption becoming more critical as the number of firearms available for theft or diversion increases. The interaction between

[4] There are exceptions; all of the studies that use gun dealers as a proxy for guns are county-level.

gun availability and police presence is therefore not merely additive but multiplicative: areas with both high gun availability and low police presence may experience disproportionately high rates of firearm violence as guns flow unimpeded into criminal use, while areas with high gun availability but strong police presence may see these flows substantially reduced through active interdiction and deterrence.

A substantial literature examines how police force size and effort affect crime outcomes. Chalfin and McCrary (2018) argue that U.S. cities are "substantially under-policed" after adjusting for measurement error, suggesting significant potential benefits from increased police presence. Much of this research exploits variation from Community Oriented Policing Services (COPS) grants, which provided federal funding for police hiring and equipment. Studies using COPS grant variation consistently find crime-reducing effects of additional police officers. Mello (2019) shows that cities receiving COPS funding experienced 3.2% increases in police force size and 3.5% reductions in victimization cost-weighted crimes. Weisburst (2019) finds that 10% increases in police employment are associated with 13% reductions in violent crimes and 7% reductions in property crimes. Evans and Owens (2007) demonstrates that COPS-funded officers reduce auto thefts, burglaries, robberies, and aggravated assaults. Chalfin et al. (2022) extends this work by examining differential effects across racial groups, finding that homicide reductions particularly benefit Black Americans, though larger police forces also increase arrests for low-level offenses.

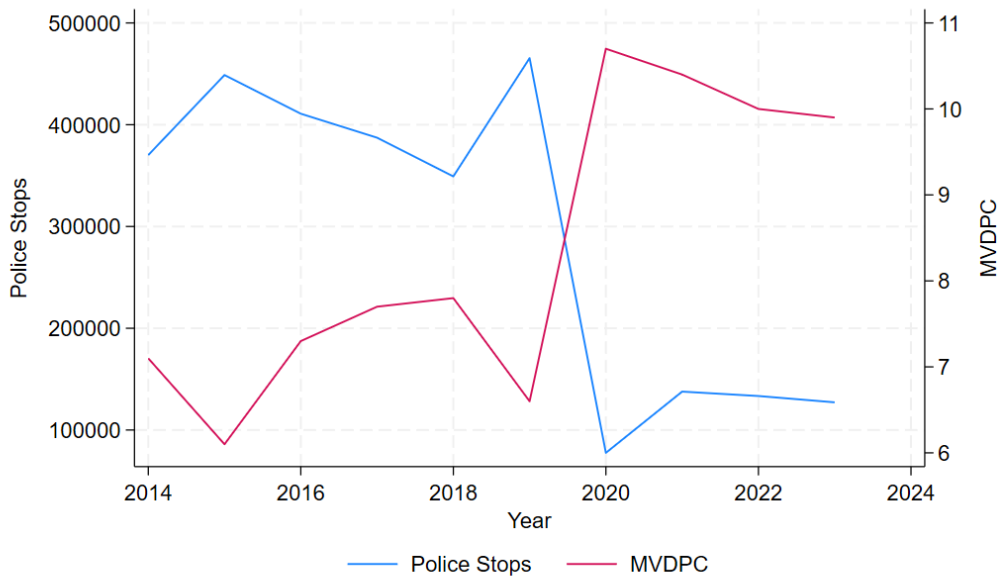
Beyond force size, police effort on the intensive margin also affects crime outcomes. Chalfin and Goncalves (2023) shows that officers make fewer but higher-quality arrests near shift end, when the opportunity cost of overtime is highest. Cho et al. (2022) examines how officer fatalities affect colleague behavior, finding broad reductions in arrest activity driven primarily by decreased enforcement of low-level offenses. These studies highlight how officer discretion and incentives shape policing outcomes.

Our approach of measuring policing activity differs from this literature by using motor vehicle deaths per capita as a proxy for police presence and effort. We choose this measure because it improves on both data quality and data availability over time and at smaller geographic units. First, motor vehicle death data provides longer time series coverage across more geographic units than traditional crime data. Second, death certificate data is more reliable than Uniform Crime Report data, which suffers from inconsistent voluntary reporting by law enforcement agencies (Loftin and McDowall, 2010).

Figure 1 shows anecdotal evidence of the relationship between active policing and decreased motor vehicle deaths in Philadelphia County, Pennsylvania. While illustrative, we validate our motor vehicle death measure using multiple approaches. We show consistent positive correlations between motor vehicle deaths and homicides at national, yearly, monthly, and weekly frequencies. We demonstrate that motor vehicle deaths decrease with DUI arrests, consistent with the measure capturing police enforcement intensity. Finally, we replicate our main findings using direct policing measures, including various crime clearance rates and DUI arrest rates, providing confidence that our results are not artifacts of our specific proxy choice. The connection between traffic enforcement and broader policing is theoretically and empirically plausible. Traffic stops provide opportunities for weapons seizures and can deter criminal activity through increased police visibility.

Our work could also be considered a large-scale empirical replication of the "Kansas City Gun Experiment" which was a Bureau of Justice Assistance funded experiment in which

Figure 1: Police Stops and Motor Vehicle Deaths Per Capita in Philadelphia County, PA



Notes: Total number of police stops in Philadelphia County, PA (left-hand axis); Number of motor vehicle deaths per capita (right-hand axis).

police made an explicit effort to seize guns in a gun crime hot spot (Sherman et al., 1995). They did so by increasing police presence, for 29 weeks, in an area that had a homicide rate many times the national average (nearly 200 per 100 thousand people). During the experiment, gun crimes in the targeted hot spot decreased by nearly 50% while a control hot spot increased by about 4%. This is thought to be at least partly driven by the increase in gun seizures which grew by 65% relative to the previous year.[5] Consistent with our main results, these seizures most often occurred during traffic stops. This pattern isn't unique. On August 11, 2025, President Donald Trump invoked the Home Rule Act to assume temporary control of the D.C. Police Department, prompting the deployment of federal law enforcement officers and National Guard troops to curb rising crime. The impact was swift and multifaceted: the city recorded its fewest motor vehicle accidents since the COVID-19 pandemic, alongside a rise in gun recoveries, and a drop in homicides.

The theoretical foundation for our approach rests on the idea that police presence can mitigate the harmful effects of increased firearm availability. While guns may flow into illegitimate uses through theft and diversion, active policing can interdict these weapons before they are used in crimes. Traffic enforcement is particularly relevant because it provides a mechanism for discovering and seizing illegal weapons. Our empirical strategy allows us to test whether this interaction effect is quantitatively important and whether it varies across different types of gun violence. We ultimately find that more active police does in fact lower firearm-related mortality, but that this is not sufficient to fully offset the effect of increased gun purchases.

[5]Whether or not this is "big" or not is in the eye of the beholder. Only 76 guns were recovered but the hot spot was also quite small (80 square blocks).

3 Data

Summary statistics for the variables of interest are in Table 1. Because of the COVID-19 pandemic, we focus on the years 1993 to 2019.[6] We start with the mortality data, which involves combining different International Classification of Diseases (ICD) coding systems. For 1999-2020, we use ICD-10 codes; for 1993-1998, we use ICD-9 codes. The CDC warns against comparing data across coding systems, but we argue the discontinuity will be minimal for deaths as clear as homicides, suicides, and motor vehicle deaths. Total Homicide (ICD codes X85-Y09 for years 1999-2020 and E960-E969 for years 1993-1998) are the total number of homicides in year t and Gun Homicide (ICD codes X93, X94, and X95 for years 1999-2020 and E965.0, 965.1, E965.2, E965.3, E965.4 for years 1993-1998) are the number of gun homicides in year t in a given county. Non-gun Homicide is the total number of non-gun homicides in year t and is found by subtracting gun homicides from total homicides.[7] Total Suicide (ICD codes X60-X84 for years 1999-2020 and E50-E959 for years 1993-1998) and Gun Suicide (ICD codes X72, X73, X74 for years 1999-2020 and E955.0, 955.1, E9565.2, E955.3, E955.4 for years 1993-1998) are the total number of suicides and gun suicides in a given county in year t. As with non-gun homicide, to find non-gun suicide, we subtract gun suicides from suicides. Finally, MVD is motor vehicle deaths (Injury Mechanism Motor Vehicle Traffic for all years). All deaths are assigned to the county where the decedent lived.

Table 1: Summary Statistics

		Mean	Std. Dev.	Min	Max
	Year	2006	7.79	1993	2019
	Homicide	7.40	21.5	0	203
	Suicide	16.2	28.9	0	178
	Gun Homicide	4.96	16.2	0	159
	Gun Suicide	7.65	12.8	0	93
	Non-Gun Homicide	2.45	5.90	0	56
t	Non-Gun Suicide	8.56	17.1	0	120
	Dealer & Pawn	33.6	54.6	1	874
	Businesses	3419.2	6916.0	79	40767
	Population	130388.6	259305.2	2216	1671329
	MVD	15.9	22.3	0	133
	Black Pop.	14108.9	35730.2	0	243875
	Emp/Pop	0.30	0.11	0	0.60
	MVD PC	19.8	15.6	0	162.8
	Guns Sold	2971.2	3935.3	56.4	32373.2

Notes: Summary statistics for the variables of interest at the county level.

Population and Black population are the total population and Black population in a given year. This data is from the US Census Bureau and runs from 1993-2019. Businesses are the total number of businesses and EMP/Pop is the employment-to-population ratio.

[6]We are able to extend into post-pandemic years but for this analysis, we limit to pre-pandemic years because of the structural break that occurred in 2020 that is associated with COVID-19 and the murder of George Floyd.

[7]We also note that the CDC classifies the 9-11 terrorist attack victims as homicide victims. We do not include these deaths and identify them using the victim list found on Wikipedia. This list does not include the county where the victim resided but does include the city and state in which the victim lived. We geocode (using Geocodio) to the best guess of the victim's county of residence based off of this address. Geocodio link: https://www.geocod.io/upload/

This data is from the County Business Pattern Survey and runs from 1993-2019.[8]

Finally, a key contribution of this paper is the introduction of a new dataset on firearm sales that is at a finer resolution and spans further back in time than previous measures.[9] These estimated sales are based on gun dealer counts. Gun dealer data is from three different sources. The first is Johnson and Robinson (2024) which runs from 2003 to 2019. The second is from Wiebe et al. (2009) and runs from 1993-1999. Finally, the third source is three separate lists. The first two are technically from Johnson and Robinson (2024) but were not used in the original study. This is because one of the FFL lists (2002) did not include the FFL number which made it difficult to pair with the remaining data, and the second one (2000) had an FFL number that could not be matched with other data. Connecting these two years with the rest of the data was done with a donated FFL list from 2001. Bridging the datasets was not difficult as the Johnson and Robinson (2024) data and the donated list have information on the license holder (name and address) and the license number. With this information, we can infer the missing license number in the 2002 list which completes the panel that now runs from 1993-2019. Gun Sales are our estimated gun sales which we will detail in Section 3.2.1. To produce gun sales estimates we combine these dealer counts with national supply trends, which is manufacturing minus exports plus imports. This data is from the 2021 Firearm Commerce Report published by the Bureau of Alcohol, Tobacco, and Firearms (ATF).[10]

Though not the main focus of the paper, we also present estimates that use data from the Federal Bureau of Investigation's (FBI) Uniform Crime Report UCR. These data are from Kaplan (2018). Here we use county-years that reported some index crime and whose absolute difference of the crime of interest from the mean crime of interest was not more than 3 times the mean over the panel.[11] We also drop data from New York and Illinois due to inconsistent reporting and observations that are missing one of the variables of interest (e.g., DUI). The general idea here is that we want to preserve as many observations as possible but also want to avoid including obvious errors. Given these problems, however, we emphasize that we consider all estimates involving UCR data as robustness checks or of only peripheral interest. Summary statistics for these variables are in Table 2. Variable descriptions are as follows: DUI Arrests are the number of DUI arrests, % Clear are the number of crimes cleared (or solved) divided by the number of crimes reported, Law Enforcement Employees are the number of employees (officers and civilians), and Gun/Knife Assaults are the number of assaults involving a gun/knife. These data are all at the county-year level.

3.1 Trends in Gun Prevalence Measures and Concerns

There are two approaches one could take when analyzing trends in firearm prevalence. The first approach focuses on market-based activity and measures changes in trends using variables like concealed carry permits, gun sales, gun thefts, and FFL counts. This is our approach. The second takes a survey-based approach that estimates the proportion of households with a firearm; the most well-known example being the estimated Household

[8] This data is found here: https://www.census.gov/programs-surveys/cbp.html.

[9] Data and code will be made public upon acceptance.

[10] Found here: https://www.atf.gov/firearms/docs/report/2021-firearms-commerce-report/download

[11] E.g., $|DUI_{t,i} - D\bar{U}I_i| < 3 * D\bar{U}I_i$

Table 2: Summary Statistics (FBI Data)

	Mean	Std. Dev.	Min	Max
DUI Arrests	365.5	1418.3	0	58974
% Cleared	0.24	0.14	0	3
% Cleared (Violent)	0.57	0.25	0	8
% Clear (Property)	0.20	0.14	0	2.33
Law Enforcement Employees	313.0	1175.3	0	37936
Gun Assault	59.6	339.3	0	23749
Knife Assault	47.8	217.8	0	10356

Notes: Summary statistics for the variables of interest at the county level. Uniform Crime Report data (1993-2019).

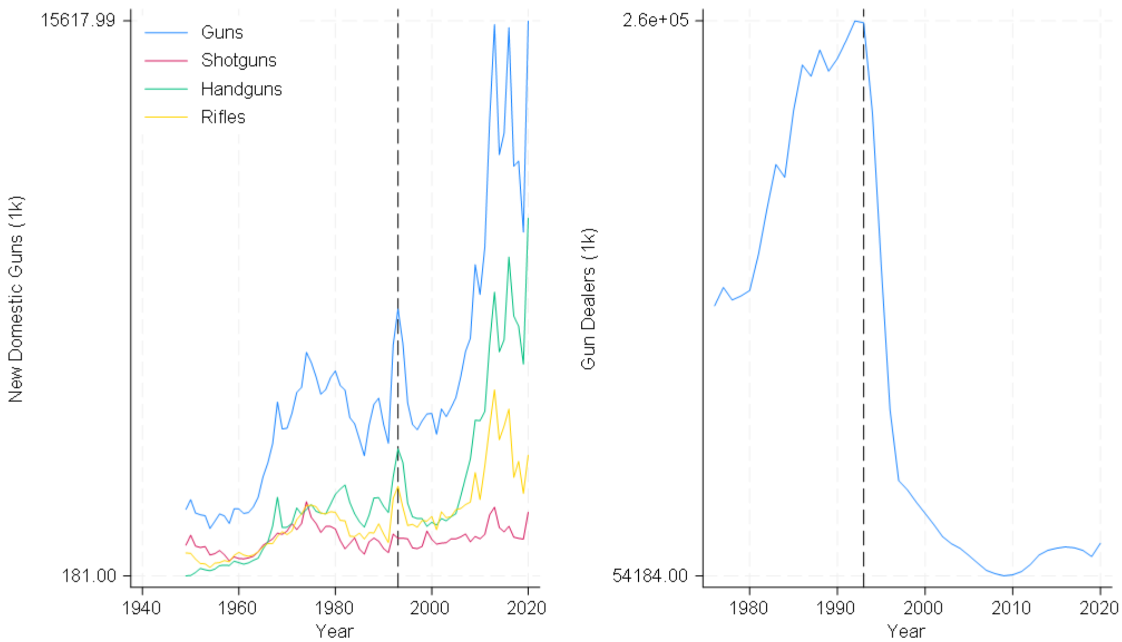
Firearm Ownership Rate (HFR) (Schell et al., 2020; Morral et al., 2024). This second approach does not measure changes in the number of new guns in an area but rather the share of households with a firearm. There are two main problems with this approach. First, they are only available at the state-level and, second, they rely on survey respondents answering questions honestly. Honesty is often a big assumption when questions are related to sensitive topics (e.g., height, weight, racial attitudes, and alcohol consumption). We do not think guns are an exception. For example, Cook and Ludwig (1998) show that estimates of defensive gun use based on survey data are likely upwardly biased, meaning defensive gun use is over-reported, other work (Miller et al., 2022) documents misreporting of the cause of gun injuries (accident vs assault) by hospital patients, and Bond et al. (2024) shows that a large portion of survey respondents may be lying about their gun ownership status.[12]

We build our gun sales estimates using gun dealers and new gun manufacturing. Trends in both of these variables are found in Figure 2. Before 1975, the gun supply in the United States was remarkably similar across the three types of guns - though there was growth in all three. This changed in the mid-70s and early 80s when handgun supply began to outpace rifles and shotguns. The gun supply remained fairly constant up until the mid-90s, during which there was large, temporary growth in the domestic supply of handguns and rifles. This increase is generally attributed to various gun legislation that was introduced contemporaneously, after which gun production fell back to past levels. This changed around 2010 when gun production in the United States exploded, with the largest growth occurring in handguns and rifles. The growth in handgun and rifle supply is attributed to multiple factors, but three primary reasons are arguably the most accepted. First, veterans returning from the Second Gulf War wanted a rifle similar to the one they used in combat (the M4 to the AR-15 platform).[13] Second, some of this growth is attributed to fears related to a second Obama term (e.g., increased gun control), which is also similar to what happened

[12] In the 2020 United States presidential election, 91% (79 %) of Black female (male) voters voted for Joe Biden (see `https://www.nbcnews.com/politics/2020-elections/exit-polls/`). Additionally, in South Carolina, the number of concealed carry permits issued to Black/African American females in 2016 was 8,820, and in 2019, almost 16,000 permits were issued. Similar growth in this demographic is also seen in Connecticut and Nevada. Moreover, concealed carry permit growth for the Black/African American community as a group is also observed in Texas, Oklahoma, New Mexico, and Utah (see Figure A1 of the Appendix). This data was originally collected by Trent Steidley and was recently used/discussed in (Stansfield et al., 2023). The website that originally hosted the data seems to have been removed but the data and documentation can be obtained from the corresponding author.

[13] This phenomenon is not uncommon and is thought to have occurred after the Vietnam War (cf., the Mini-14).

Figure 2: Gun Manufacturing and Gun Dealers



Notes: Left is the new domestic gun supply (1000s). Right is gun dealers (1000s). Vertical line indicates the start of our county level data.

in the mid-90s due to new gun control legislation, and third, the Assault Weapons Ban expired on September 13, 2004.

Gun dealers also have trends. Gun dealers peaked in 1992 but plummeted soon after. This decline was caused by the Brady Handgun Violence Prevention Act (effective February 1994) which increased the dealer licensing fee to 200 dollars for three years from ten dollars per year. Three years after the passage of this bill – also, the length of time an FFL is valid – the number of FFLs had nearly halved. While the number of gun dealers is quite small today relative to the number seen in the early 90s, there were major differences in the characteristics of gun dealers in the early 90s to those today. According to ATF reports and contemporary writing, many gun dealers at the time were not selling guns but obtained the FFL for their own personal benefit (c.f., Schwarzkopf, 1993; Langley, 2006). For example, one of the main benefits of a Federal Firearm License is that the license holder can receive a gun in the mail. The accuracy of this position depends on the source. Others (e.g., Sugarmann and Rand, 1992), characterized many of these sellers as "kitchen table dealers" who would haphazardly transfer firearms. The recent increase in the number of gun dealers today is likely a response to the increased demand for firearms.

3.2 Primary Assumptions

Our work relies on two primary assumptions. First, our measure of gun sales accurately captures year-to-year variation in new gun sales. Second, police presence and/or effort is negatively associated with the motor vehicle death rate and can be used as a measure of police activity. In the subsequent subsections, we document trends in these measures and discuss potential critiques of these assumptions.

3.2.1 Gun Sales Estimates

Using gun dealer and manufacturing data, we construct county-level yearly estimates of new gun sales in a shift-share-inspired design. This estimate is important because the effect of a gun store's presence in an area over time is not constant. Increases in the number of new guns sold per gun dealer will increase the local supply of guns, and simply accounting for the number of gun stores in an area, as in Johnson and Robinson (2024), would provide biased results. In practical terms, without this adjustment, in years with lots of new guns (i.e., later in the panel) the effect of gun stores on local firearm deaths would be lower than the actual effect and the reverse would be the case earlier in the panel. This problem would be exacerbated in the early 90s when gun dealers were at their highest. Thus, as referenced above, the change in sales in our shift-share based measure is caused by institutional changes and firearm demand shocks.[14]

Our assumption is the number of new guns in a specific geography is:

$$Gun\ Sales_{i,t} = (\frac{GM_t - Ex_t + Im_t}{\Sigma Gun\ D_t}) * Gun\ D_{i,t} \tag{1}$$

The left-hand side of the formula represents the average number of new domestic guns (GM - Ex + Im, or guns manufactured minus exports plus imports) in per gun dealer (Gun D) in year t (this is a national average) and the right-hand side represents the number of gun dealers in the geography.[15] These estimates are very accurate. When we aggregate our estimated new gun sales to the yearly level, we find they closely match NICS checks that are explicitly for gun sales (handgun, long-gun, multiple, or other) and the value of reported stolen guns. This is presented in Figure 3 (scaled by 2019). The overall correlation between the estimated new gun sales and yearly NICS checks (value of reported stolen guns) is .87 (.90).[16] One additional point to note is that the gun sales estimates are likely fairly in the early 90s. While value of reported stolen guns is fairly noisy for a number of reasons (subjective value, inflation, and selective reporting), there still appears to be a local peak in both it and our estimates in the early 90s.[17]

This high correlation is not due to a single state. When we aggregate our estimates of gun

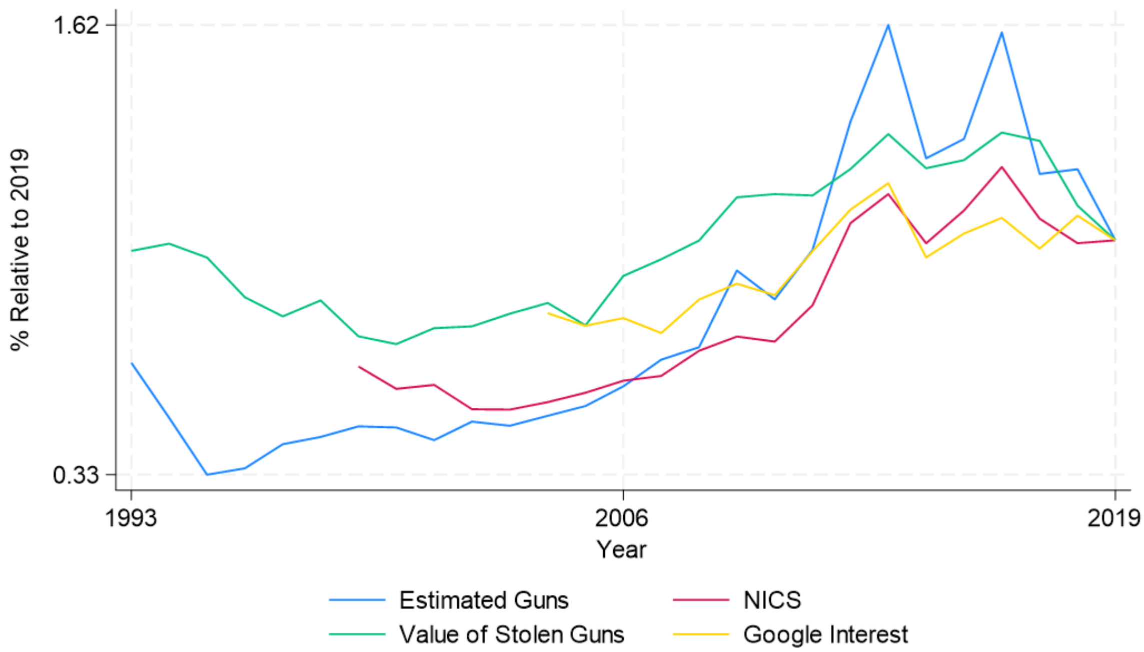
[14] To illustrate, in Figure A2 (Appendix), we present the change in the new domestic gun supply (manufactured guns plus net imports) per gun dealer. Before 2000, the average gun dealer sold 30 new guns per year, but after 2000, sales per FFL increased to 150. This change was possibly due to three main factors: i) the number of gun dealers decreased after the Brady Handgun Violence Prevention Act of 1993 (institutional change), ii) gun manufacturing exploded after the sun-setting of the Assault Weapons Ban (institutional change), and iii) the run-up to the 2004 Presidential Election (demand shock).

[15] Readers may also note that in the replication file, we technically use the lead of local gun dealers when constructing the local new gun sales estimates. This is because most of the FFL data used in Johnson and Robinson (2024) is from January or February, so it is closer to the year-end sum of gun manufacturing (from the previous year) than the manufacturing of the same year.

[16] Gun sales estimates are also highly correlated with Google searches with the word "Gun" ($\rho = .94$).

[17] This is also observed in New York concealed carry permits which are presented, by county, in Figure A5.

Figure 3: Estimated Gun Sales, NICS Checks, and Value of Reported Stolen Guns



Notes: Estimated new gun sales, NICS checks (1999-2019), value of reported stolen guns (1993-2019), and Google Searches with the word "Gun" (2004-2019). NICS checks include only background checks that were explicitly for a firearm (handgun, long-gun, other, or multiple). No adjustments are made for differences in state classifications. Blue is estimated sales, red is NICS checks, and green is stolen guns. Yellow is the percent of Google searches with the word "gun" averaged across all states. All variables scaled by their 2019 maximum.

sales to the state-year level, the estimates closely match NICS checks with the correlation between the two is 0.85. The value of reported stolen guns is also highly correlated with our new gun estimates (ρ=.70). This is shown visually in Figure A3 and Figure A4 (Appendix), which compares state level NICS checks, the value of reported stolen guns, and Google searches with the word "Gun" to our estimated gun sales.[18] Most of these correlations are quite high. Moreover, when they do fail, it is often in places we would expect.[19]

The state-level panels are illustrative of the generality of our shift-share gun measure. However, to finalize this section we next discuss the final step in creating the county-level estimated new gun sales. We adjust the new gun sales estimate to allow for seepage across county lines. The way we do this is by using the number of gun dealers in county i and the

[18] To be clear, this is searches **with** the word "Gun" – meaning searches like "buy gun", "by gun", "get gun", etc. are all included. Arguably, a more restrictive search would be preferable but these more restrictive searches are more prone to missing data due to the search shares being determined by a sample of searches in the geography-time window.

[19] A good example of this is Iowa. Here, we note a negative correlation between our estimates of new gun sales and NICS checks explicitly for a gun. However, this is often due to how the state classifies NICS checks. Another notable failure occurred in Connecticut in 2000. The number of NICS checks explicitly for a gun fell from 45,303 in 1999 to 12 in 2000. It appears that, in this case, NICS checks for permits and handguns were all classified as permit checks for that year and then delineated again after 2000.

number of gun dealers in the counties adjacent to county i instead of gun dealers in county i only - which is similar to Johnson and Robinson (2024)'s "Halo" measure. While this will certainly over-count the number of guns, it will do so in a manner that is a level shift and would be absorbed into the fixed effect (i.e., will not affect marginal effects).

We take an approach similar to Johnson and Robinson (2024) where the original measure was as follows:

$$GD_{i,t} = \frac{\left(D_{i,t} + PW_{i,t} + \sum_{j=1}^{m}(D_{j,t} + PW_{j,t})\right)}{A_i + \sum_{j=1}^{m}(A_j)} \tag{2}$$

where $\mathrm{GD}_{i,t}$ is gun dealer density and is a function of the number of gun dealers – Dealer or type 1 FFL ($\mathrm{D}_{i,t}$) and Pawn or type 2 FFL ($\mathrm{P}_{i,t}$) FFLs – in county i and the counties surrounding i, divided by the total land area of county A_i and the adjacent counties A_j. This measure is slightly adjusted. Instead of dividing by the land area, we multiply it by the average number of guns sold per dealer which is the left-hand part of Equation 1.

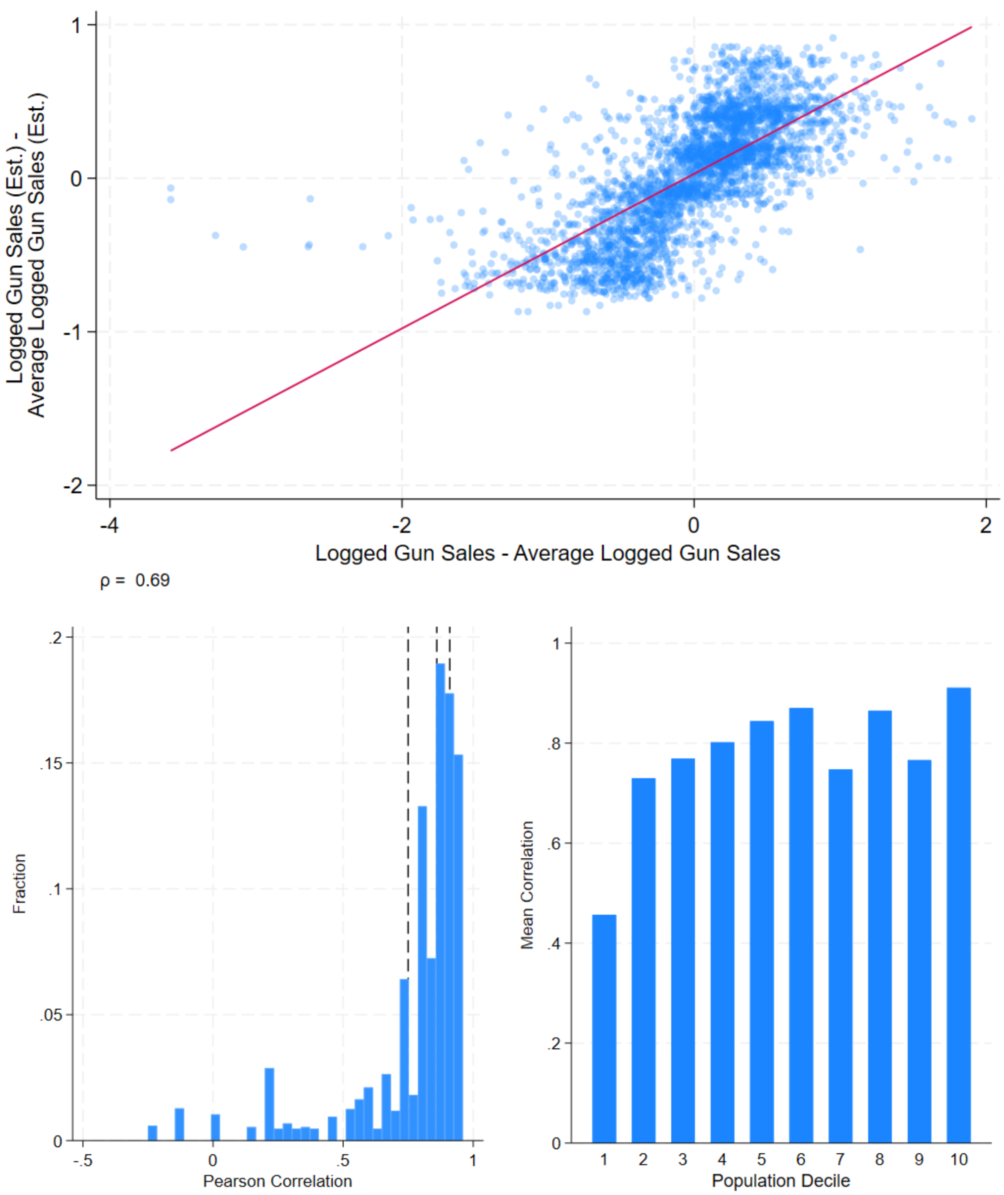
Despite the incompleteness of the gun sales measure (e.g., it approximates new gun sales and not *all* sales and assumes all gun dealers sell an equal number of guns), we believe that the estimate is accurate and closely reflects the year-to-year variation in all sales – which is more important in a fixed effects context. To illustrate, in Figure 4 (top panel), we plot the county mean differenced natural log of our gun sales estimates (i.e.,$ln(Gun\ Sales_{i,t}) - \overline{ln(Gun\ Sales_i)}$) against the county mean differenced natural log of gun sales. [20] Visually, there is a significant positive correlation between the gun sales and estimated gun sales. Comparisons for every county are found in Figures A7 (California), A8 (Massachusetts), A9 (Pennsylvania), and A10 (Utah).[21]

Overall the correlation between demeaned sales and sales estimates (both logged) is high (ρ=.69) and this generally true across counties. Figure 4 (bottom - left panel) presents a histogram of the correlations (each county) with 25^{th}, 50^{th}, and 75^{th} percentile indicated by dashed lines. Clearly, the vast majority of correlations are very high. Bottom right panel of Figure 4 demonstrates where the estimates usually fail (i.e., where the correlation is low). As expected, it is in counties with low populations (i.e., bottom decile or an average county population around 6 thousand people). This is expected for a two main reasons. First, and most obviously, it is completely reasonable for a county with less than a thousand people to have no gun sales in a given years (or even many years). In these cases, because of the construction of our sales estimates, there will still positive estimates that will vary depending the number of gun dealers in the surrounding counties and manufacturing that

[20] These counts are from a series of Freedom of Information Act Requests and publicly available data. The state included are: California, Pennsylvania, Utah, and Massachusetts. Note: California is only handgun sales. We do not think this is an issue. Handgun and rifle manufacturing are nearly parallel after roughly 2006. Data is available upon request from Author 1.

[21] An earlier version of this manuscript validated the our estimates with concealed carry data. This data is available upon request from Author 1. We do not use this data because guns are not concealed carry permits – though the two are highly correlated (c.f., Johnson et al., 2024). Put simply, we want to demonstrate that our measure "does what it says on the tin." Note also that in these figures we include more recent years (2020 ,2021, 2022, and 2023) to demonstrate that the pattern also holds in more recent years; meaning the post-COVID years did not break the pattern – though in this case there is likely some reverse causality concerns with guns sales and crime.

Figure 4: Estimated Gun Sales vs Gun sales



Notes: (Top panel) Demeaned estimated gun sales and actual gun sales. County level data from the following states: California, Pennsylvania, Utah, and Massachusetts. Red line indicates best fit line. Pearson correlation, ρ, is found in the bottom left corner. (Bottom left panel) Histogram of county level correlations (demeaned) and average correlation across county population deciles.

year. Second, Johnson et al. (2024) note that gun dealers in rural areas tend to sell fewer guns than counterparts in urban areas.

The similarities are *not* driven by population. Visually, this can be seen throughout all of the plots as the gun sales estimates and gun sales have up and down years unlike population which tends to increase in a more linear fashion. Furthermore, when estimating logged gun sales using our logged gun sales estimates and controlling for logged population, we find that a one percent increase in our gun sales estimates is associated with 1.05 % (t-stat: 22.02) increase in guns sales and the 95% confidence interval includes 1 (i.e., .96 - 1.15).[22] This result also mostly holds when taking a first-difference approach though here the coefficient falls by half.

The motivation behind this modification is outlined in Johnson et al. (2024) and Johnson and Robinson (2024) but the short version is that dense urban areas have fewer gun stores than one would expect but will often have many just outside the county lines. There are many reasons why this occurs ranging from local ordinances preventing the establishment of gun dealers to increases in the cost of rental space.[23] Regardless, this pattern creates an illusion that there are few gun dealers – or guns available – in the area when in actuality they just moved outside of an arbitrary border. This is often seen in crime gun recovery. For example, Johnson et al. (2024) demonstrates that when a gun is recovered during a criminal investigation and the gun is successfully traced, it is generally found that the gun was last sold in the county it was recovered or an adjacent county. According to ATF records, a similar pattern happens at the state level where recovered guns are generally traced to dealers in the same state they were originally sold or a bordering state.[24] Figure 5 illustrates the practical difference between a measure that accounts for county seepage and one that does not, with the top sub-figure being the area measure and the bottom sub-figure being the county-specific one.

The reason why our gun sales measure is accurate is due to gun interest being highly synchronized across geographies. This is demonstrated in Figure 6 which plots monthly standardized NICS checks for all 50 states (each dot is a state-month observation) from 2001 to 2025. Visually it is easy to see each state has a similar peak and to reinforce this characteristic of NICS checks, we also highlight two states that are 2 thousand miles apart: Maine and New Mexico. As with the other states, there are obvious shared shared peaks and trends. Similar results are found when using monthly Digital Marketing level data (searches with the word "Gun").[25] These results are found in Figure A6 of the Appendix. In this Figure, we also highlight Kansas City and two other randomly selected DMAs (Boise and Miami). As with NICS checks, search interest is highly synchronized across states.

One potential concern is that national crime trends may be reflected in local crime, which in turn could increase local gun sales. If so, gun sales would not be locally exogenous but would only appear so because of shared national crime dynamics. Recently, Johnson et al. (2025) show this is likely not the case. Using monthly NICS checks and Google search

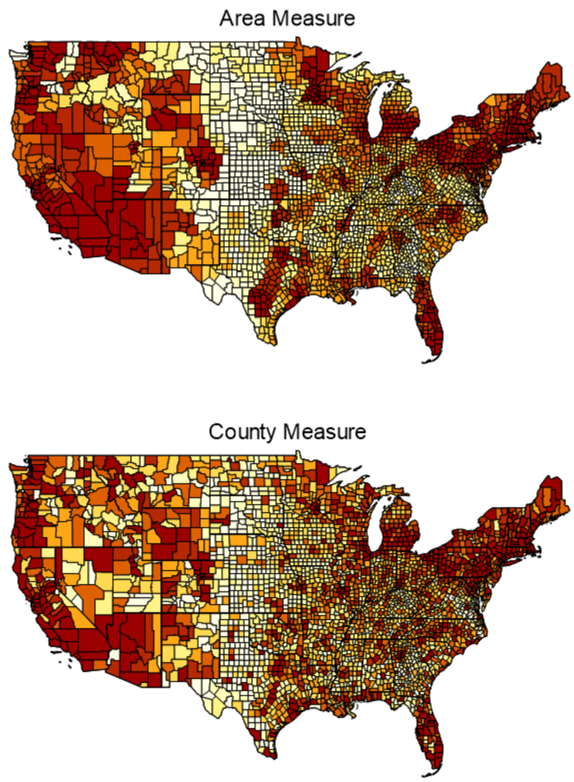
[22] Model details: linear fixed effects panel (county fixed effects) and county clustered standard errors. 166 counties and 2,786 observations. Adjusted $R^2 = 0.96$.

[23] For example, in 1995 the city of Minneapolis enacted a moratorium that prevented gun dealers from opening in the city.

[24] See, for example, https://www.atf.gov/resource-center/firearms-trace-data-2021

[25] There are 209 Digital Marketing Areas.

Figure 5: Local Area Measure vs County Measure (1995)



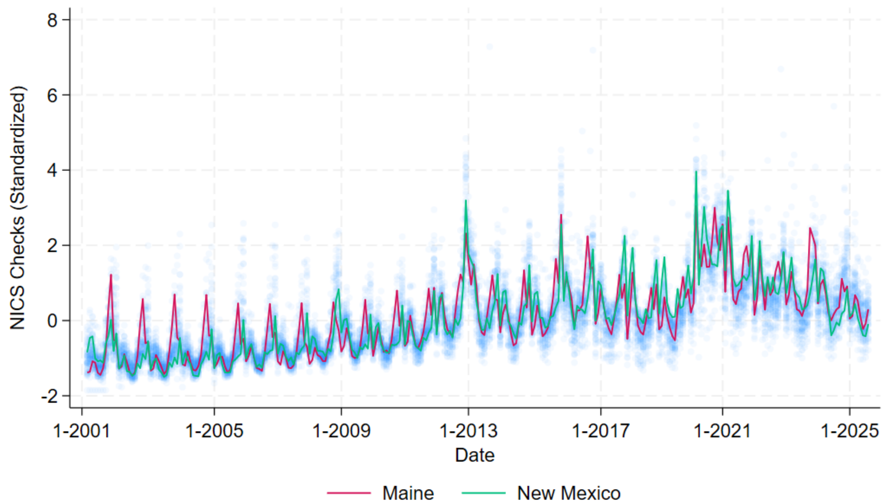
Notes: Local gun supply measure (top) and the number of new guns per gun dealer times the number of dealers in the county (bottom) using 1995 data.

data, Johnson et al. (2025) show a high level of synchronization of gun interest/transactions across states. They do so by estimating standardized NICS checks and search data in state i using search/search data from state j (e.g., gun interest in Maine to predict interest in New Mexico). Overall, they find the average correlation of gun interest across the i-j pairs to be .86 when using search data and .74 when using NICS checks. This result does not depend on the inclusion of other variables such as national homicide or murders in the same state. Moreover, these variables generally have no or only a small effect on gun interest.[26] Johnson et al. (2025) argue that this result is due to gun interest being driven primarily by national events (e.g., changes in federal laws, presidential elections and mass shootings) that are unrelated to local crime conditions.

A potential concern is that local dealer counts respond to local crime. However, Topaz and Johnson (2025), using principal components analysis (PCA), demonstrate that firearm licensing exhibits strong spatial clustering and remarkably stable temporal patterns, with only 3 % of variance attributable to year-to-year changes. This stability that persisted

[26]One exception is large national events, such as the unrest following the murder of George Floyd, which modestly increased gun interest. This falls outside our study period.

Figure 6: Standardized Monthly NICS Checks (2001-2025)



Notes: Monthly standardized NICS checks (2001-2025). Each dot represents a state-month observation. Red line is Maine specifically; green is New Mexico.

persisting through the 2008 financial crisis and subsequent gun sales surges indicates that the spatial organization of gun commerce is highly persistent and unresponsive to short-term local shocks. Recent work extending the PCA framework to actual gun sales and concealed carry permits provides additional validation. Johnson and Topaz (2025) analyze 168 counties across four states and find that a single principal component explains 95 % of variance, with this component being largely time-invariant. Similarly, concealed carry permits across 1,111 counties exhibit 78% of variance from a stable first component, with temporal variation driven primarily by national political cycles rather than local factors. These findings have direct implications for our identification strategy. First, the dominance of time-invariant components (78-95% of variance) means that county fixed effects absorb most of the endogenous variation, mitigating concerns about reverse causality. Second, the small temporal components align with national events (Sandy Hook, presidential elections), supporting our assumption that changes in local gun availability are driven by national production trends rather than local demand.

3.2.2 Motor Vehicle Deaths as a Measure of Active Policing

We now move to the measure of police activity: motor vehicle deaths per capita. Motor vehicle deaths have generally declined since 1968, a trend attributed to increased vehicle safety, improved medical care, and ride-sharing. These fatalities are also influenced by road safety, which can depend on the presence and efforts of police. For example, according to the National Highway Traffic Safety Administration's Fatality Analysis Reporting System, in 2023, 44 % of fatal motor vehicle crashes involved a driver who was speeding, distracted, or tested positive for alcohol. Additionally, a little less than half of all drivers involved in fatal accidents had at least one traffic violation (e.g., driving while intoxicated or speeding) or wreck.[27] These deaths are somewhat preventable, and police efforts (e.g., DUI checkpoints) play a role in reducing them. Furthermore, guns are inevitably confiscated during traffic stops, as was often the case during the Kansas City Gun Experiment.

Establishing a potential link between motor vehicle deaths and homicide is not difficult to visually show and is presented in Figure 7. In this figure, we plot the yearly, monthly, and weekly motor vehicle deaths along with the gun homicides. The relationship is strongest in the most recent years but even in the years before 2000, there are shared ebbs and flows. Moreover, the relationship between the two is weakest in the late 80s and early 90s, which is where we would expect it. This is because during this period there was the widespread adoption of safety features (e.g., airbags) and legislation (e.g., mandatory seatbelts), which reduced traffic fatalities (c.f., Zador and Ciccone, 1993; Anderson et al., 2024) in a complementary way independent of law enforcement.[28]

While both figures suggest an underlying connection, we formally test the relationship in several ways. In the Appendix, using a time series approach we demonstrate a positive contemporaneous relationship between motor vehicle deaths and gun homicides when the data is yearly (see Figure B2). However, when examining a narrower measure (monthly, Figure B3 or weekly, Figure B4), we observe that the relationship is lagged and primarily unique to gun homicides. Exploring the relationship between motor vehicle deaths and non-gun homicides, gun suicides, and non-gun suicides, we generally find no significant relationship, and when present, the magnitude is small. This suggests that active policing not only saves lives by reducing drunk driving accidents but also leads to crime guns being removed from the streets and/or the incarceration of individuals willing to use firearms in criminal activities. However, this policing does not affect suicide rates.

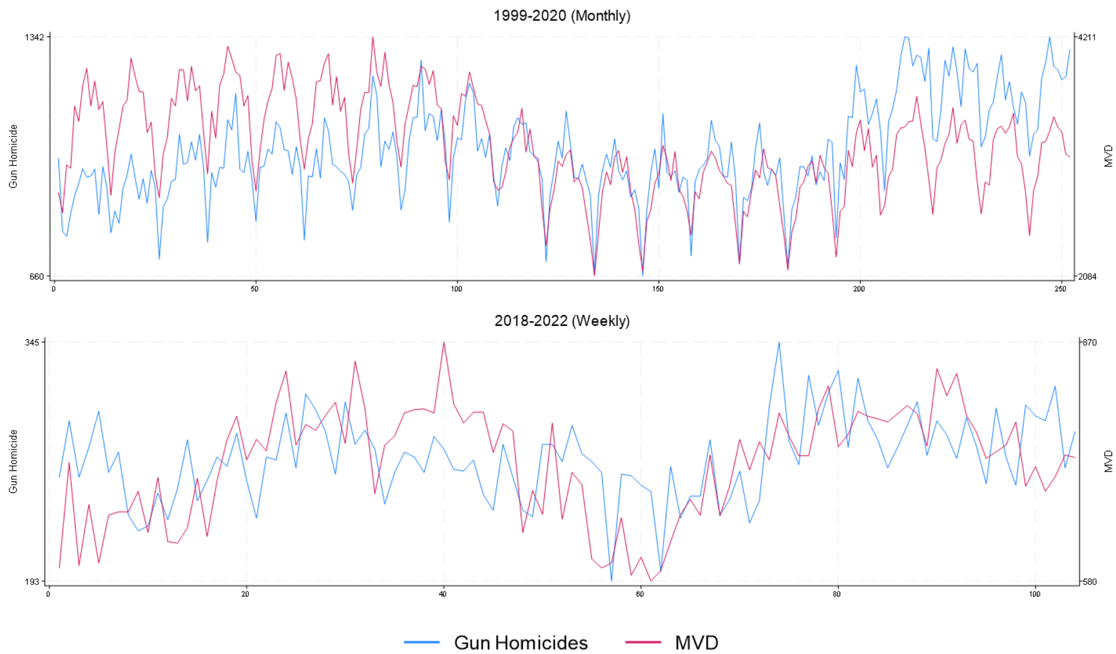
We also provide evidence of the shared relationship between DUI arrests per capita (which are a function of the number of elective stops) the motor vehicle death rate in a first-stage type regression in Table 3 (top panel). Here, we present the coefficient estimate of the natural log of DUI arrests on the motor vehicle death rate (population is the exposure variable) using a fixed effects Poisson and county-clustered standard errors.[29] After accounting for *any* time trend (e.g., state, county, national, year fixed effects), we find that there is a negative relationship between DUI arrests and motor vehicle deaths that is consistent across trend control approaches. Overall, we find that a 1 % increase in DUI arrests decreases the motor vehicle death rate by 0.02 %. A similar relationship exists between the property crime clearance rate and the motor vehicle death rate (bottom panel Table 3) with 1% increase in the property crime clearance rate decreasing the motor vehicle death rate by .2%. Ultimately, we choose to focus on motor vehicle deaths because the county-level DUI arrest data and clearance data is from the Federal Bureau of Investigation's Uniform Crime Report, which is known to be of questionable quality. In contrast, motor vehicle deaths are reported to the CDC by medical examiners who determine the cause of death, and this reporting is mandatory. Therefore, we consider motor vehicle deaths to be a more reliable proxy for local law enforcement activities.

[27] Of the 504,337, traffic fatalities from 2009-2023, roughly 28 % involved a driver that was speeding.

[28] We also note that the relationship between MVD and homicide goes back even further. In Figure B1 we present a plot of the number of motor vehicle deaths and homicides from 1948 to the present and note similar trends. However, the further back you go, the lag between motor vehicle deaths and homicides increases.

[29] DUI data is from the UCR arrest data.

Figure 7: Motor Vehicle Deaths and Gun Homicide through the Years



Notes: Motor vehicle deaths and gun homicide by year, month, and week. Top panel uses yearly data from 1980 to 2020, middle panel (monthly) from 1999-2020, bottom panel (weekly) from 2018-2022.

4 Results

We divide our results into sections. In Section 4.1, we demonstrate a statistically significant positive relationship between firearm estimates and fatal outcomes. We then explore the relationship between firearms and non-fatal outcomes. In Section 4.2, we expand on these findings and show how the effects presented are augmented by the level of law enforcement. In both of these sections, we perform several robustness checks using alternative measures of policing and outcome variables. We consider the results presented in Section 4.1 to be mostly causal, but acknowledge the results in Section 4.2 might have some bias due to reverse causality (e.g., policing increases due to high crime) and/or alternative channel (e.g., decrease in policing increases the supply of crime guns).[30]

4.1 Firearms and Firearm-Related Outcomes

We begin by estimating the relationship between per capita homicides, gun homicides, non-gun homicides, suicides, gun suicides, and non-gun suicides in county i in year t. Our main variable of interest is the natural log of estimated gun sales lagged 3 years. The three-year lag is selected based on results detailed in Johnson et al. (2024), which demonstrates the average time-to-crime of recovered crime guns is not an informative measure of the time between when a firearm is last sold and when it is involved in a crime. This is because the

[30]For example, increases in theft – particularly from vehicles – will likely result in more stolen guns.

Table 3: DUI Rates and MVD (County Level)

	Motor Vehicle Death					
	County FE	County FE + Trend	County FE + Year FE	County FE + State - Year FE	County FE + State Trends	County FE + County Trend
DUI Arrests per 100K	0.0231***	-0.0172***	-0.00943**	-0.00827*	-0.0167***	-0.0190***
	(5.21)	(-5.07)	(-2.82)	(-2.39)	(-4.74)	(-4.82)
Pop	-0.829***	-0.365***	-0.360***	-0.397***	-0.389***	-0.0793
	(-28.95)	(-12.19)	(-12.12)	(-14.96)	(-13.77)	(-0.89)
% Black	-0.120***	-0.0234***	-0.0255***	0.000290	0.00309	0.0393**
	(-18.19)	(-3.65)	(-3.85)	(0.04)	(0.42)	(2.77)
Year		-0.0131***				
		(-25.50)				
Constant	0.842*	22.07***	-4.415***	-3.900***	-3.943***	-7.608***
	(2.33)	(25.28)	(-12.28)	(-12.04)	(-11.54)	(-7.00)
OBS	67224	67224	67224	67219	67224	67224
Counties	2807	2807	2807	2807	2807	2807
R2	0.815	0.817	0.819	0.822	0.818	0.820

	Motor Vehicle Death					
	County FE	County FE + Trend	County FE + Year FE	County FE + State - Year FE	County FE + State Trends	County FE + County Trend
% Clear (Property)	-0.275***	-0.177***	-0.0812***	-0.0533**	-0.175***	-0.195***
	(-10.18)	(-7.29)	(-3.78)	(-2.58)	(-7.26)	(-7.13)
Pop	-0.820***	-0.349***	-0.349***	-0.403***	-0.389***	-0.0758
	(-31.11)	(-12.66)	(-12.78)	(-16.27)	(-14.88)	(-0.90)
% Black	-0.120***	-0.0257***	-0.0270***	0.000829	0.00139	0.0254
	(-19.24)	(-4.13)	(-4.19)	(0.11)	(0.19)	(1.90)
Year		-0.0123***				
		(-25.50)				
Constant	0.980**	20.16***	-4.569***	-3.835***	-3.985***	-7.745***
	(2.97)	(25.25)	(-13.88)	(-12.70)	(-12.49)	(-7.46)
OBS	72016	72016	72016	72014	72016	72016
Counties	2883	2883	2883	2883	2883	2883
R2	0.824	0.826	0.828	0.830	0.826	0.829

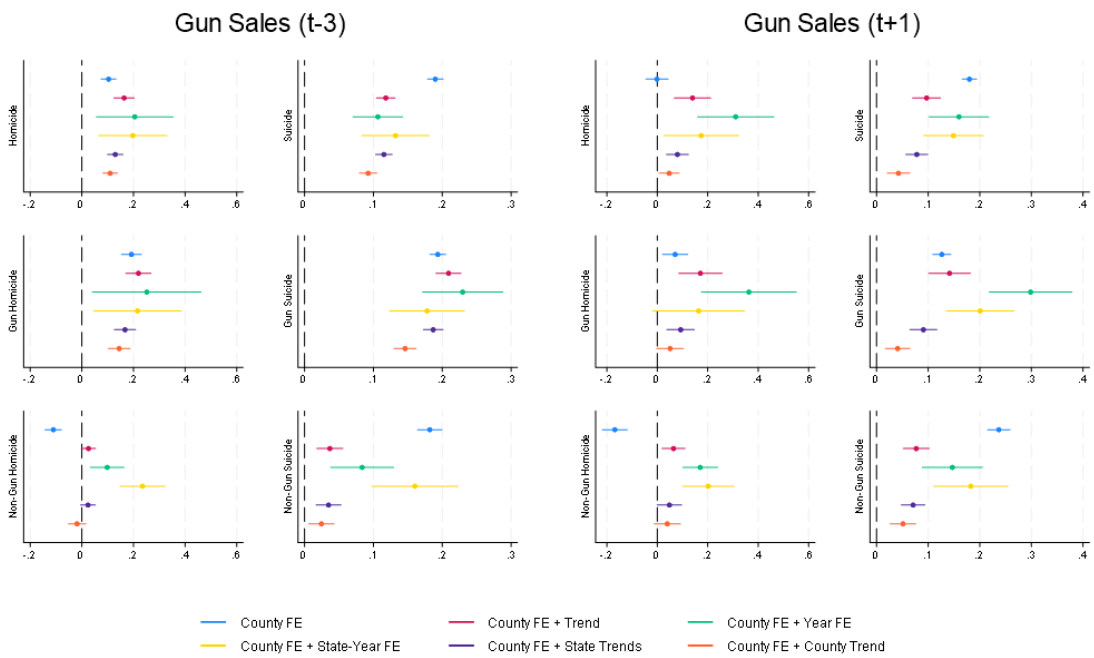
Notes: Fixed effects Poisson regression results. Time controls are indicated by column labels. T-statistics from county clustered standard errors are in parentheses; * p<0.05, ** p<0.01, *** p<0.001.

average is driven up by a small number of very old guns. In fact, the majority of crime guns are less than five years old (c.f., Johnson et al., 2024) and according to the ATF, in 2019, 42 % of recovered guns were reported to be less than three years old.[31]. We also verify the appropriateness of the lag choice using several approaches.

The long time horizon coupled with a large number of heterogeneous panel units creates complications. The primary ones are the treatment of zeros and trend selection. To that end, we use a fixed effects Poisson estimator with logged explanatory variables. We start by considering six different specifications which toggle fixed effects and trend variables for robustness: 1) county fixed effects, 2) county fixed effects with linear time trend, 3) county fixed effects and year fixed effects, 4) county fixed effects with state-specific year fixed effects, 5) county fixed with state-specific linear time trends, and 6) county fixed effects with county-specific time trends. All models use county-clustered standard errors. Keeping with Johnson and Robinson (2024), the model with county-fixed effects with county-specific time trends is our preferred specification which will be our focus. In all models, we control for the percent of the county that identifies as Black or African American, the total number of businesses in the county and adjacent counties, county population, county adjacent population, and the employment-to-population ratio. All variables are in natural logs so that the coefficient estimates are all elasticities. We also include the county population as an exposure variable

[31] https://www.atf.gov/resource-center/firearms-trace-data-2019

Figure 8: Estimated Relationship Between Gun Measures and Gun Deaths (County Level Panel)



Notes: Coefficient estimates for estimated gun sales. The outcome variable is indicated by y-axis label. Figure heading indicates the time of the sales. The outcome is indicated by the y-label in the sub-figures. Model specification indicated in figure legend with FE indicating some form of fixed effects (county or year). Control variables are the percent of the county that identifies as Black or African American, the total number of businesses in the county and adjacent counties, county population, county adjacent population, and the employment-to-population ratio. Error bars indicate the 95% confidence interval using county-clustered standard errors. All models use the Poisson estimator.

so that the outcome is a rate.

The simplified estimating equation is shown below.

$$Y_{i,t} = exp(\beta_0 + \Gamma GunSales_{i,t-3} + X_{i,t}\beta + \psi_i + \psi_i \cdot t + ln(Pop_{i,t}) + \varepsilon_{i,t}) \tag{3}$$

where $Y_{i,t}$ is one of the various fatal outcomes; Γ captures the elasticity of lagged gun prevalence to fatal outcome; and $X_{i,t}$ are a vector of controls followed by our various fixed effects and trend specifications. The control variables are kept the same across all the models that we discuss but estimates of models without the control variables (i.e., controlling only for motor vehicle death rate and gun sales) are found in Tables D1 (homicides) and D2 (suicides).

Due to the large number of models, we present results graphically in Figure 8. The left panel uses lagged gun sales while the right uses the lead and serves as a robustness check. The outcome is indicated by the y-axis title. Coefficient estimates for estimated gun sales and motor vehicle deaths for all specifications are found in Tables D3 (homicides) and D4 (suicides) of the Appendix. Overall, we find a positive relationship between lagged gun sales and homicides. This relationship is driven primarily by gun homicides. Gun sales do

Table 4: Estimated Relationship with Preferred Specification

	Homicides	Gun Homicides	Non-Gun Homicides	Suicides	Gun Suicides	Non-Gun Suicides
L3.Local Guns Sales	0.110***	0.144***	-0.0186	0.0924***	0.146***	0.0245**
	(7.48)	(6.71)	(-1.06)	(14.29)	(17.54)	(2.60)
Adj. Pop	-1.249***	-1.559***	-0.189	-0.190	-0.496***	0.217
	(-3.33)	(-3.38)	(-0.73)	(-1.72)	(-3.44)	(1.53)
% Black	0.0130	0.0162	0.174***	-0.0419***	-0.0553***	0.0222
	(0.33)	(0.26)	(3.62)	(-3.29)	(-3.50)	(0.96)
Local Establishments	1.251***	1.641***	0.365	-0.155*	-0.389***	0.190
	(6.78)	(7.41)	(1.90)	(-2.40)	(-5.03)	(1.84)
Emp/Pop	0.0400	-0.0287	0.0791	-0.112***	-0.00805	-0.265***
	(0.56)	(-0.31)	(1.02)	(-4.12)	(-0.24)	(-5.96)
Pop	-0.813***	-0.980***	-0.303	-0.303***	-0.286**	-0.197
	(-6.51)	(-7.10)	(-1.72)	(-4.16)	(-2.70)	(-1.71)
MVD PC	0.0656***	0.0788***	0.0366**	0.00302	0.00671	-0.00161
	(5.47)	(4.84)	(2.73)	(0.69)	(1.21)	(-0.24)
Constant	3.799	5.791	-7.880*	-1.610	3.025	-11.43***
	(0.78)	(0.92)	(-2.25)	(-1.14)	(1.77)	(-5.41)
OBS	65210	63518	63165	66796	66688	66205
Counties	2893	2784	2768	3032	3018	2968
R2	0.886	0.876	0.747	0.865	0.763	0.835

Notes: Estimated effects using a fixed effects Poisson estimator. All models include county fixed effects and county-specific time trends. The outcome is indicated by the column label. T-statistics from county clustered standard errors are in parentheses; * p<0.05, ** p<0.01, *** p<0.001.

not affect non-gun homicides. Suicides also tend to have a positive relationship with gun sales though these relationships are more sensitive to the handling of trends. All models find a positive relationship between gun sales and suicides, but this relationship is driven by changes in the amount of gun suicides.

Full results using the preferred specification are in Table 4. We find that gun sales have a positive and statistically significant relationship with the expected outcome variables – those involving homicides, especially gun homicides. A 1 % increase in sales increases homicides and gun homicides by about 0.1 to .14 %; similar relationships occur with suicides and gun suicides. To give an idea of how big this effect is, in December of 2012 there were almost 820,000 "extra" NICS checks due to the Sandy Hook tragedy. This is about 4 % of the number of checks that occurred in 2012.[32] Assuming each check represents a new gun in public, this translates to about 520 additional gun homicides or about .00065 per "extra" gun. Gun sales have no relationship with non-gun homicides and a small relationship with non-gun suicides. A 1 % increase in the motor vehicle death rate is associated with a 0.07 % increase in homicide and gun homicide but only about a 0.04 % increase in non-gun homicide. Our measure of police presence does not have a significant relationship with the number of suicides which is expected.

Finally, to further justify our lag choice, we estimate the relationship between our outcome variables of interest and 8 lags and leads of our gun sales estimates. These estimates are shown in Figure C1 of the Appendix, along with a brief discussion of the different estimates. Overall, we find results suggesting that our original approach is reasonable, although it requires some detective work. Estimates involving suicide are fairly consistent, but the same cannot be said for homicide estimates. Initially, the lagged effect of gun sales on gun homicides is consistently positive or near zero, but the leads are very noisy and often statistically significant. The leads of gun sales are also statistically significant when the

[32] Extra background checks are determined by the error generated from an Autoregressive (AR) regression with a 1 and 12-month lag.

outcome variable is non-gun homicides. Although significant leads in Figure C1 could suggest endogeneity, we demonstrate that these patterns are better explained by trends in the outcome variable. When we estimate first-difference and Anderson-Hsiao models, the lead coefficients attenuate substantially, providing reassurance that our primary specification is not capturing anticipatory effects or reverse causality. These estimates are found in Figures C2 and C3 of the Appendix.

4.1.1 Estimates using FBI Data

As a robustness check, in Table 5 we consider alternative measures of law enforcement effort: the clearance rate (crimes cleared divided by total reported crime) for index property crime (burglary, larceny-theft, motor vehicle theft, and arson), the DUI arrest rate, and law enforcement employees. These estimates are found in Table 5. The outcome variable is indicated in the column label. With the drawbacks of the UCR data in mind, we estimate models similar to those presented in Table 4 but switch the natural log of the motor vehicle death rate for the clearance rate of index property crime (top panel of Table 5), the natural log of the DUI arrest rate (middle panel of Table 5), and the natural log of law enforcement employees (bottom panel of Table 5). We focus on property crime here to avoid having homicide on both sides of the equation. We also note that property crimes are generally considered less serious than violent crimes, which gives investigators more discretion regarding the effort invested in an investigation. We also consider two additional outcomes: gun assaults and knife assaults, with the expectation that gun sales will have a positive relationship with gun assaults but not with knife assaults.

In all panels of Table 5, we find a relationship between lagged gun sales that is similar to what is presented in Table 4. A 1 % increase in gun sales increases homicides (gun homicides) by about .11 (.14) %. Suicides (gun suicides) increase by about .1 (.15) % in response to a 1 % increase in gun sales. Non-gun suicides and non-gun homicides are not affected by changes in gun sales or the effect is small (possibly suggesting some omitted variable when the outcome is suicide). Last, a 1 % increase in gun sales increases gun assaults by about .18 % and does not significantly affect knife assaults. We find a negative relationship between the property crime clearance rate and homicide, gun homicide, and gun assaults that is statistically significant. A 1 % increase in the property crime clearance rate decreases gun homicides by about .2 %. A similar increase in the property crime clearance rate reduces gun assaults by about .4 %. The relationship between suicide and knife assaults and property crime clearance rates is not statistically significant. Estimates using the alternative clearance rates are found in Table D5 of the Appendix. The DUI arrest rate has a similar effect on outcomes that is like the other measures of policing. A 1 % increase in the DUI arrest rate decreases homicide and gun homicide by about .03 % – which ex post is not surprising considering over 40 % of those convicted of homicide reported drinking when they committed their crime (Greenfeld, 1998). Gun and knife assaults are however increasing in the DUI arrest rate – and may reflect problems in the data, omitted variables, and/or endogeneity. Last, a 1 % increase in law enforcement personnel decreases homicide and gun homicide by about .05 % and reduces suicides (gun suicide) by about .02 (.04) %. Law enforcement employment has no significant relationship with gun or knife assault.

Table 5: Robustness checks

	Homicides	Gun Homicides	Non-Gun Homicides	Suicides	Gun Suicides	Non-Gun Suicides	Gun Assaults	Knife Assaults
L3.Local Guns Sales	0.113***	0.144***	-0.00967	0.0919***	0.151***	0.0162	0.175***	-0.0504
	(7.55)	(6.60)	(-0.50)	(13.68)	(17.43)	(1.67)	(6.25)	(-1.58)
% Clear (Property)	-0.158**	-0.218**	-0.0994	0.0428	0.0265	0.0416	-0.438**	0.0757
	(-2.67)	(-2.64)	(-1.36)	(1.72)	(0.86)	(1.01)	(-3.17)	(0.68)
OBS	62457	60703	60062	64436	64317	63837	63677	63878
Counties	2748	2658	2630	2878	2867	2825	2822	2828
R2	0.880	0.869	0.732	0.866	0.771	0.831	0.966	0.961

	Homicides	Gun Homicides	Non-Gun Homicides	Suicides	Gun Suicides	Non-Gun Suicides	Gun Assaults	Knife Assaults
L3.Local Guns Sales	0.118***	0.150***	-0.00761	0.104***	0.163***	0.0254*	0.208***	-0.00143
	(6.78)	(5.92)	(-0.35)	(15.09)	(18.19)	(2.36)	(6.43)	(-0.04)
DUI Arrests per 100K	-0.0296***	-0.0337***	-0.00900	0.00394	-0.000769	0.0120*	0.0474***	0.0801***
	(-5.03)	(-4.52)	(-1.01)	(1.14)	(-0.18)	(2.22)	(3.52)	(7.50)
OBS	58347	56625	55913	60195	60085	59636	59462	59707
Counties	2667	2572	2532	2799	2789	2742	2735	2747
R2	0.879	0.868	0.730	0.860	0.762	0.826	0.966	0.961

	Homicides	Gun Homicides	Non-Gun Homicides	Suicides	Gun Suicides	Non-Gun Suicides	Gun Assault	Knife Assault
L3.Local Guns Sales	0.108***	0.139***	-0.0107	0.0900***	0.146***	0.0175	0.173***	-0.0458
	(7.21)	(6.38)	(-0.55)	(13.34)	(16.98)	(1.79)	(6.25)	(-1.44)
Law Enforcement Emp.	-0.0555***	-0.0619**	-0.00526	-0.0237**	-0.0407***	0.00579	-0.0600	0.0359
	(-3.38)	(-2.89)	(-0.24)	(-2.79)	(-3.77)	(0.48)	(-1.79)	(1.79)
OBS	61616	59854	59394	63116	62960	62542	62440	62585
Counties	2759	2658	2635	2877	2861	2818	2829	2836
R2	0.879	0.869	0.732	0.865	0.770	0.831	0.966	0.961

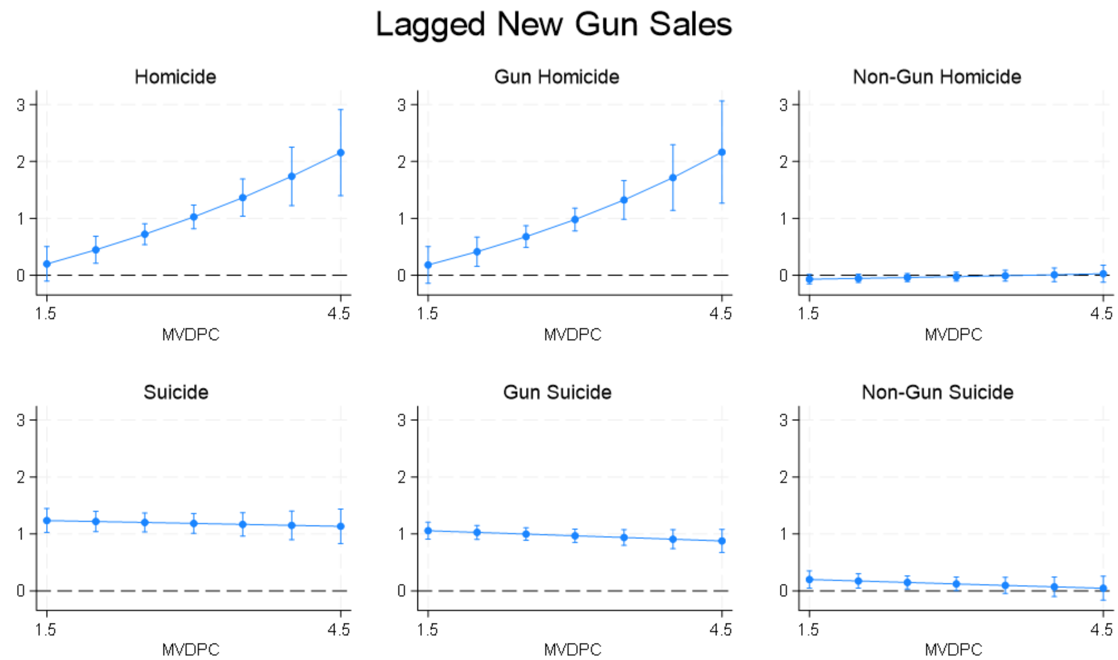
Notes: Estimated effects using a fixed effects Poisson estimator. All models include county fixed effects and county-specific time trends. The outcome is indicated by the column label. T-statistics from county clustered standard errors are in parentheses; * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

4.2 Interaction of Gun Prevalence and Policing

We now explore the interaction between estimated gun sales and motor vehicle deaths at the county level. We expect that the effect of lagged gun sales will increase with the motor vehicle death rate. To test this, we run a specification similar to that presented in Equation 3 but include the interaction of gun sales and the motor vehicle death rate. Point estimates of interest are in Table D6 of the Appendix. In all models involving homicide or gun homicide, the interaction is positive and statistically significant, suggesting active policing reduces homicide and, in particular, gun homicide.

In Figure 9, we present the elasticity of gun sales on various outcomes at different log levels of motor vehicle deaths. We find, at lower log levels of motor vehicle deaths per capita, gun sales have virtually no effect on homicide or gun homicide. However, as motor vehicle fatalities per capita rise, the effect of guns increases but only for these crime-related deaths. This suggests active policing is a mitigating factor in the homicide rate. Additionally, we find that the effect of active policing on suicide is consistent across suicide, gun suicide, and non-gun-related suicides. This observation virtually rules out medical care-related explanations. If this relationship were due to region-specific changes in medical care, we would expect a similar relationship involving suicide and non-gun-related deaths.

Figure 9: Estimated Relationship Between Gun Sales and Deaths (Motor Vehicle Deaths)



Notes: Estimated effect of guns by motor vehicle death rate. The figure heading label indicates outcome variable. Control variables are the percent of the county that identifies as Black or African American, the total number of businesses in the county and adjacent counties, county population, county adjacent population, and the employment-to-population ratio. Error bars indicate the 95% confidence interval using county-clustered standard errors. All models use the Poisson estimator.

4.2.1 Estimates using FBI Data

As a robustness check, we also explore how the effect of new guns changes with different levels of law enforcement, now proxied by the index property crime clearance rate, the DUI arrest rate, and the number of law enforcement employees. The general idea here is that the impact of new guns should decrease with the property crime clearance rate, the DUI arrest rate, and the number of law enforcement employees. Additionally, the effect of new guns on suicide should not be influenced by these variables. Results are presented in Figure 10 and with point estimates found in Table D7 of the Appendix.[33] As before, all models use fixed effects Poisson estimators with county fixed effects and county-specific linear time trends.

We find that the effect of new guns on homicide and gun homicide decreases as the clearance rate increases. The clearance rate does not affect the relationship between new guns and non-gun homicide, suicide, gun-suicide, or non-gun suicide. DUI arrests and law enforcement employment similarly mitigate the impact of firearms on fatal crime-related

[33]Recall that estimates using UCR data drop counties with unreliable information which means the data used to produce estimates here is different from what is used in Section 4.1. To show that the results presented there are not dependent on these dropped observations, we also include estimates that interact gun sales with motor vehicle deaths per capita (column one in Figure 10).

outcomes, with increases in the DUI arrest rate/law enforcement employment decreasing the effect of firearms on homicide and gun homicide while not altering the effect on suicides and non-gun homicides. Similar results are found in Table D8 (Appendix) when using the index crime clearance rate and violent crime clearance rate.

As an additional robustness check, we also explore the interaction between gun sales and law enforcement on knife and gun assaults. These estimates are presented graphically in Figure 11 and point estimates are found in Table D9 in the Appendix. Results are similar to those discussed above. The effect of guns on gun assaults is increasing in the motor vehicle death rate. Increases in the clearance rate and DUI arrest rate reduce the effect of firearms on gun assaults but do not affect knife assaults or have a relatively smaller joint effect.

5 Conclusions

The general perception of gun violence research is that there is a lack of available data and that any conclusions drawn are based on scant or circumstantial evidence. While the level of available data may not be ideal, in reality, there is more data than the public perceives. In this work, we aim to enhance public understanding of how changes in firearm prevalence relate to fatal outcomes, such as homicides and suicides, by using a novel measure of gun prevalence based on federally licensed firearm dealer counts and gun manufacturing. Additionally, we trace these relationships over a longer period and at finer geographic levels than previous studies. Our gun sales measure is validated not only by high correlations with NICS checks, Google Searches, and the value of reported stolen gun values, but also by principal component analysis showing that gun sales, concealed carry permits, and dealer presence all exhibit remarkably low-dimensional structure (Topaz and Johnson, 2025; Johnson and Topaz, 2025). With 95% of gun sales variation arising from stable, spatially clustered components rather than local temporal shocks, county fixed effects effectively absorb most endogenous variation, supporting causal interpretation of our results

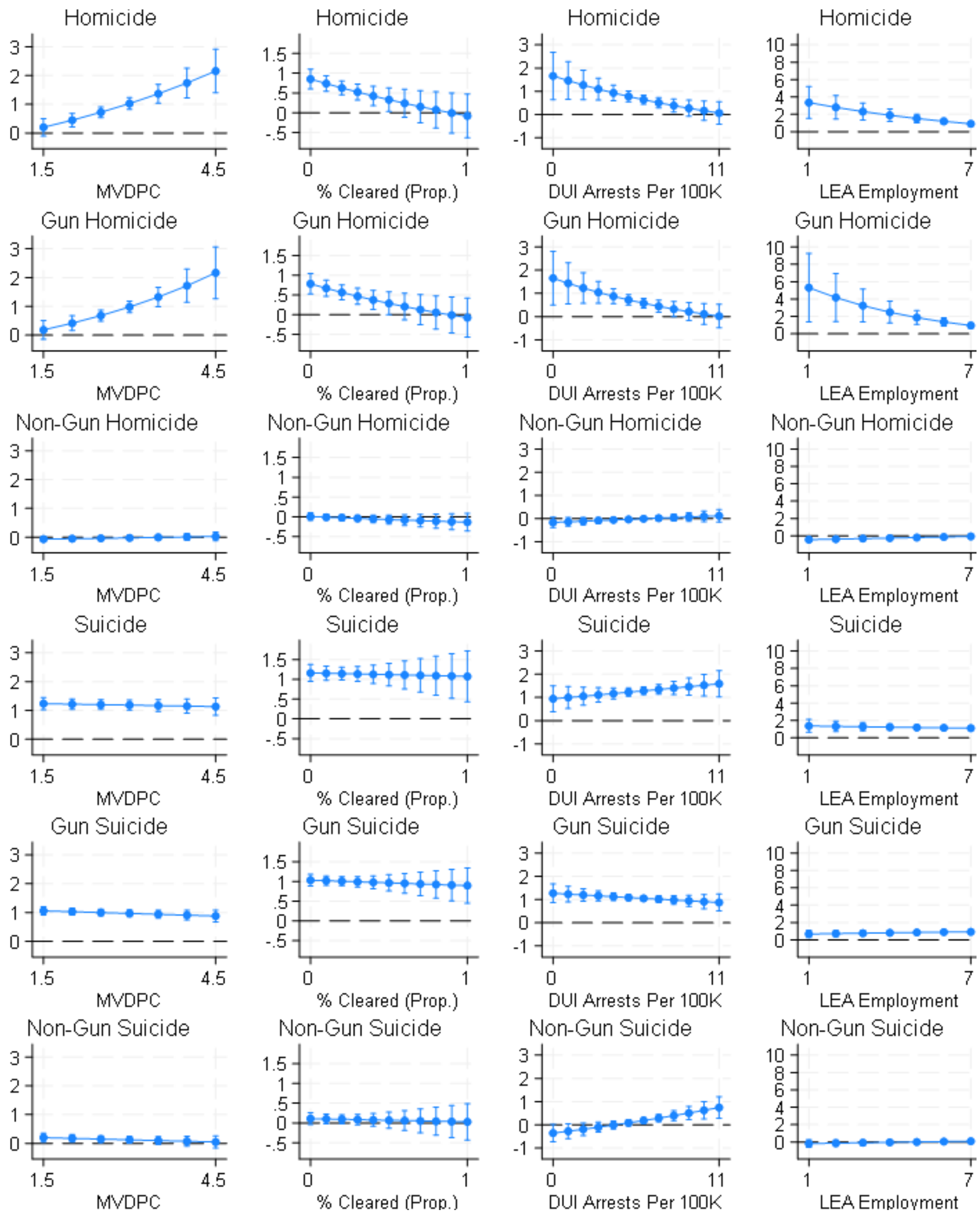
Our analysis finds that increases in the gun supply are associated with increases in both gun-related homicides and suicides in subsequent years. The impacts are statistically significant and substantial. Notably, our findings demonstrate that higher police presence and enforcement efforts, proxied by vehicular fatality rates, clearance rates, DUI arrest rates, and law enforcement employees, can meaningfully mitigate some of the elevated homicide risks associated with increased firearm access. This underscores that attributing fatal gun outcomes solely to the presence of firearms is overly simplistic. By documenting the net effect of firearms and police presence, we conclude that while additional firearms lead to more fatal outcomes, enforcing existing laws, not necessarily related to firearms, could lead to significant improvements. This result is consistent with Sherman et al. (1995). Additionally, we do not find similar patterns for non-gun homicides and non-gun suicides, suggesting the effects are specific to changes in firearm access and prevalence.

While some might expect there to be similar effects across non-gun and gun homicides, this expectation is probably incorrect. Gun homicides and non-gun homicides are quite different in terms of where they occur and the victim. For example, according to the CDC, from 2018-2022 roughly 29% of non-gun homicides occurred in the victim's home (including hospice and nursing homes) while only about 17 % of gun homicides occurred in the victim's home. Moreover, roughly 33% of all non-gun homicides are female while only 15 % of gun homicides are female. This suggests that non-gun homicides are different in kind from gun homicides with the latter being more likely to be affected by increased police presence. Additionally, we find no interactive effect between police presence and gun presence when the outcome variable is suicide-related. Given that over 50% of murders had a conviction record (Reaves, 2006) – which is well above suicide victims (Webb et al., 2011) – it makes sense that interaction with law enforcement would have a larger impact on criminal-related deaths.

We show that some of the negative consequences of gun prevalence are increasing in lax policing, but it is important to point out that these are net effects. We have no doubt that firearms can be used defensively and may prevent some homicides and that such incidents are likely more common in high-crime areas (Kleck and Bordua, 1983). Yet, on net, our results suggest that an influx of new guns results in more gun homicides. Moreover, even in areas with a high police presence, there is little evidence of a deterrence effect. That said, this also overlooks the effect on suicide, which is independent of police presence, statistically significant, and positive. Therefore, even if there were a deterrent effect, our results indicate these benefits would be offset by a rise in suicides.

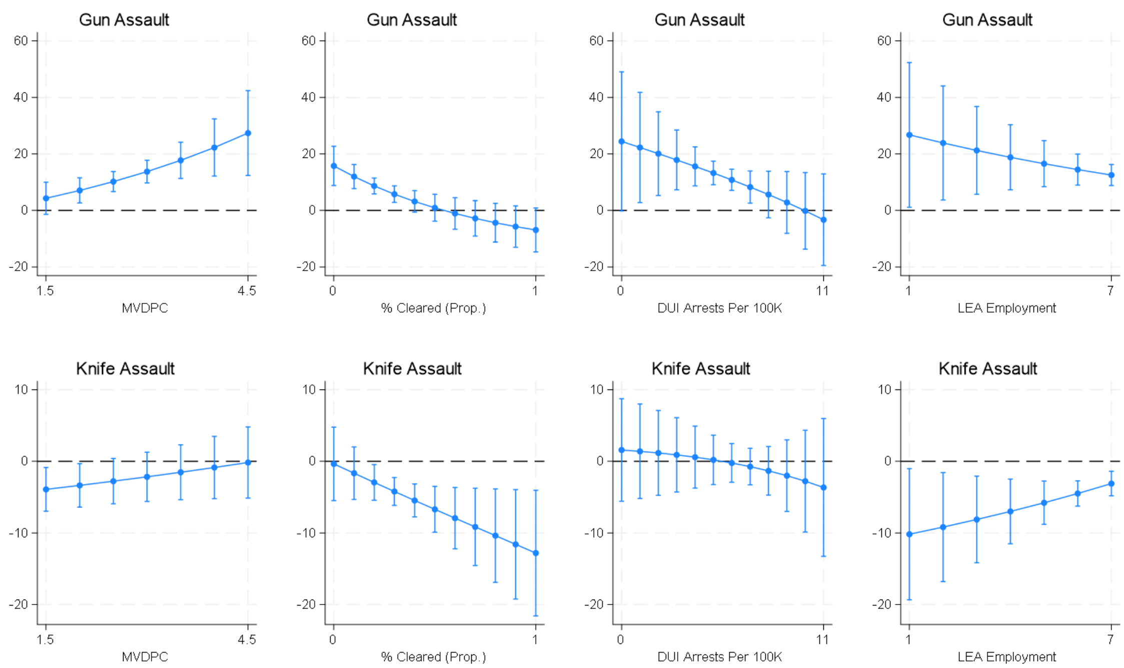
The results also suggest that policies aimed at reducing straw purchases, burglaries of gun owners, illegal firearm trafficking from the regulated dealer market, and mandating the reporting of stolen weapons could provide substantial public health benefits by limiting firearm access among individuals prone to violence or self-harm. Our results could also suggest that increased police presence may have larger negative effects on homicide in urban areas. While both results are politically contentious, efforts to constrict the hazardous spillovers from legal gun markets may offer a path forward in communities plagued by gun violence while preserving Second Amendment rights. Additionally, our novel measure of gun prevalence opens the door for further research, which is constrained by the lack of a local measure of firearms. For instance, Siegel et al. (2017) shows that from 1990 to 2020 there were nearly 150 different state gun laws in the United States. Unfortunately, the effects of these laws cannot often be rigorously tested at the state level because of a lack of variation across states. Time series methods could arguably be used as a substitute, but the problem here is the limited number of years. We believe county-level data opens up many research possibilities in the field of law and economics; however, it would naturally require some local measure of gun prevalence. We think our measure of changes in gun prevalence (or an improved version of it) could do just that.

Figure 10: Estimated Relationship Between Gun Sales and Deaths (by Law Enforcement Proxy)



Notes: Estimated effect of guns by law enforcement proxy (motor vehicle deaths per capita, property crime clearance rate, DUI arrest rate, and LEA employment) using a fixed effects Poisson estimator. All models include county-fixed effects and county-specific time trends. Outcome variable is indicated by the sub-figure heading label. Control variables are the percent of the county that identifies as Black or African American, the total number of businesses in the county and adjacent counties, county population, county adjacent population, and the employment-to-population ratio. Error bars indicate the 95% confidence interval using county-clustered standard errors.

Figure 11: Estimated Relationship Between Gun Sales and Assault



Notes: Estimated effect of guns by DUI arrest rate, clearance rate and motor vehicle death rate using a fixed effects Poisson estimator. All models include county-fixed effects and county-specific time trends. Outcome variable is indicated by the sub-figure heading label. Control variables are the percent of the county that identifies as Black or African American, the total number of businesses in the county and adjacent counties, county population, county adjacent population, and the employment-to-population ratio. Error bars indicate the 95% confidence interval using county-clustered standard errors.

References

Anderson, D. M., Y. Liang, and J. J. Sabia (2024). Mandatory seatbelt laws and traffic fatalities: A reassessment. *Journal of Applied Econometrics 39*(3), 513–521.

Azrael, D., P. J. Cook, and M. Miller (2004). State and local prevalence of firearms ownership measurement, structure, and trends. *Journal of Quantitative Criminology 20*(1), 43–62.

Billings, S. B. (2023, June). Smoking gun? Linking gun ownership to crime victimization. *Journal of Public Economics 222*, 104874.

Bond, A. E., A. T. Karnick, D. W. Capron, and M. D. Anestis (2024). Predicting potential underreporting of firearm ownership in a nationally representative sample. *Social psychiatry and psychiatric epidemiology 59*(4), 715–723.

Chalak, K., D. Kim, M. Miller, and J. Pepper (2022). Reexamining the evidence on gun ownership and homicide using proxy measures of ownership. *Journal of Public Economics 208*, 104621.

Chalfin, A., B. Hansen, E. K. Weisburst, and M. C. Williams Jr. (2022, June). Police Force Size and Civilian Race. *American Economic Review: Insights 4*(2), 139–158.

Chalfin, A. and J. McCrary (2018, March). Are U.S. Cities Underpoliced? Theory and Evidence. *The Review of Economics and Statistics 100*(1), 167–186.

Chalfin, A. and F. Goncalves (2023). The Professional Motivations of Police Officers.

Cho, S., F. Gonçalves, and E. Weisburst (2022). Do Police Make Too Many Arrests? The Effect of Enforcement Pullbacks on Crime. *IZA Discussion Paper Series*.

Cook, P. J. and J. Ludwig (1998). Defensive gun uses: New evidence from a national survey. *Journal of Quantitative Criminology 14*, 111–131.

Cook, P. J. and J. Ludwig (2006, January). The social costs of gun ownership. *Journal of Public Economics 90*(1), 379–391.

Depew, B. and I. D. Swensen (2019). The decision to carry: the effect of crime on concealed-carry applications. *Journal of Human Resources 54*(4), 1121–1153.

Duggan, M. (2001). More guns, more crime. *Journal of political Economy 109*(5), 1086–1114.

Edwards, G., E. Nesson, J. J. Robinson, and F. Vars (2018). Looking down the barrel of a loaded gun: The effect of mandatory handgun purchase delays on homicide and suicide. *The Economic Journal 128*(616), 3117–3140.

Evans, W. N. and E. G. Owens (2007, February). COPS and crime. *Journal of Public Economics 91*(1), 181–201.

Greenfeld, L. A. (1998). *Alcohol and crime: An analysis of national data on the prevalence of alcohol involvement in crime.* US Department of Justice, Office of Justice Programs, Bureau of Justice ....

Johnson, D. B., B. Dooley, and B. Stickle (2025). Triggering animal spirits? national trends drive state-level gun interest. *Available at SSRN 5354540*.

Johnson, D. B. and J. J. Robinson (2024). Gun dealer density and its effect on homicide. *The Journal of Law and Economics 67*(1), 1–30.

Johnson, D. B., J. J. Robinson, D. Semenza, and A. Thompson (2024). Where are the guns? evaluating gun prevalence measures and their connection with homicides using gun sales data. *Journal of Criminal Justice*.

Johnson, D. B. and C. Topaz (2025). Concealed carry permits and gun sales are also low dimensional. *Available at SSRN 5715443*.

Kahane, L. H. (2020, March). State gun laws and the movement of crime guns between states. *International Review of Law and Economics 61*, 105871.

Kaplan, J. (2018). Jacob kaplan's concatenated files: Uniform crime reporting (ucr) program data: Property stolen and recovered (supplement to return a) 1960-2022. *Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor] pp*, 12–29.

Khalil, U. (2017, January). Do more guns lead to more crime? Understanding the role of illegal firearms. *Journal of Economic Behavior & Organization 133*, 342–361.

Kleck, G. and D. J. Bordua (1983). The factual foundation for certain key assumptions of gun control. *Law & Policy 5*(3), 271–298.

Lang, M. (2013). Firearm background checks and suicide. *Economic Journal 123*(12), 1085–1099.

Lang, M. (2016). State Firearm Sales and Criminal Activity: Evidence from Firearm Background Checks. *Southern Economic Journal 83*(1), 45–68. Publisher: Southern Economic Association.

Langley, M. (2006). An analysis of the decline in gun dealers: 1994 to 2005.

Loftin, C. and D. McDowall (2010). The use of official records to measure crime and delinquency. *Journal of quantitative criminology 26*, 527–532.

Mello, S. (2019, April). More COPS, less crime. *Journal of Public Economics 172*, 174–200.

Miller, M., D. Azrael, R. Yenduri, C. Barber, A. Bowen, E. MacPhaul, S. J. Mooney, L. Zhou, E. Goralnick, and A. Rowhani-Rahbar (2022). Assessment of the accuracy of firearm injury intent coding at 3 us hospitals. *JAMA Network Open 5*(12), e2246429–e2246429.

Mocan, H. N. and E. Tekin (2006). Guns and juvenile crime. *The Journal of Law and Economics 49*(2), 507–531.

Moody, C. E. and T. B. Marvell (2005). Guns and crime. *Southern Economic Journal*, 720–736.

Morral, A. R., R. Smart, T. L. Schell, B. Vegetabile, and E. Thomas (2024). Geographic and demographic differences in the proportion of individuals living in households with a firearm, 1990-2018. *JAMA Network Open 7*(2), e240562–e240562.

Phillips, C. D., O. Nwaiwu, D. K. McMaughan Moudouni, R. Edwards, and S.-h. Lin (2013, January). When Concealed Handgun Licensees Break Bad: Criminal Convictions of Concealed Handgun Licensees in Texas, 2001–2009. *American Journal of Public Health 103*(1), 86–91.

Publisher: American Public Health Association

Reaves, B. A. (2006). *Violent felons in large urban counties.* US Department of Justice, Office of Justice Programs, Bureau of Justice ....

Schell, T. L., S. Peterson, B. G. Vegetabile, A. Scherling, R. Smart, and A. R. Morral (2020). *State-level estimates of household firearm ownership*, Volume 10. RAND Santa Monica, CA.

Schwarzkopf, N. (1993). *It Doesn't Take a Hero: The Autobiography of General Norman Schwarzkopf.* Bantam.

Sherman, L. W., J. W. Shaw, and D. P. Rogan (1995). *The Kansas City Gun Experiment.* US Department of Justice, Office of Justice Programs, National Institute of ....

Siegel, M., M. Pahn, Z. Xuan, C. S. Ross, S. Galea, B. Kalesan, E. Fleegler, and K. A. Goss (2017). Firearm-related laws in all 50 us states, 1991–2016. *American Journal of Public Health (ajph).*

Siegel, M., C. S. Ross, and C. King III (2013). The relationship between gun ownership and firearm homicide rates in the united states, 1981–2010. *American journal of public health 103*(11), 2098–2105.

Siegel, M., C. S. Ross, and C. King III (2014). Examining the relationship between the prevalence of guns and homicide rates in the usa using a new and improved state-level gun ownership proxy. *Injury Prevention 20*(6), 424–6.

Stansfield, R., D. Semenza, and I. Silver (2023). The relationship between concealed carry licenses and firearm homicide in the us: a reciprocal county-level analysis. *Journal of urban health 100*(4), 657–665.

Stansfield, R., D. Semenza, and T. Steidley (2021, July). Public guns, private violence: The association of city-level firearm availability and intimate partner homicide in the United States. *Preventive Medicine 148*, 106599.

Steidley, T., D. M. Ramey, and E. A. Shrider (2017). Gun Shops as Local Institutions: Federal Firearms Licensees, Social Disorganization, and Neighborhood Violent Crime. *Social Forces 96*(1), 265–298. Publisher: Oxford University Press.

Sugarmann, J. and K. Rand (1992). *More gun dealers than gas stations: A study of federally licensed firearms dealers in America.* Violence Policy Center Washington, DC.

Topaz, C. and D. B. Johnson (2025). Low dimensional structure in us firearm markets. *Available at SSRN 5332841*.

Webb, R. T., P. Qin, H. Stevens, P. B. Mortensen, L. Appleby, and J. Shaw (2011). National study of suicide in all people with a criminal justice history. *Archives of general psychiatry 68*(6), 591–599.

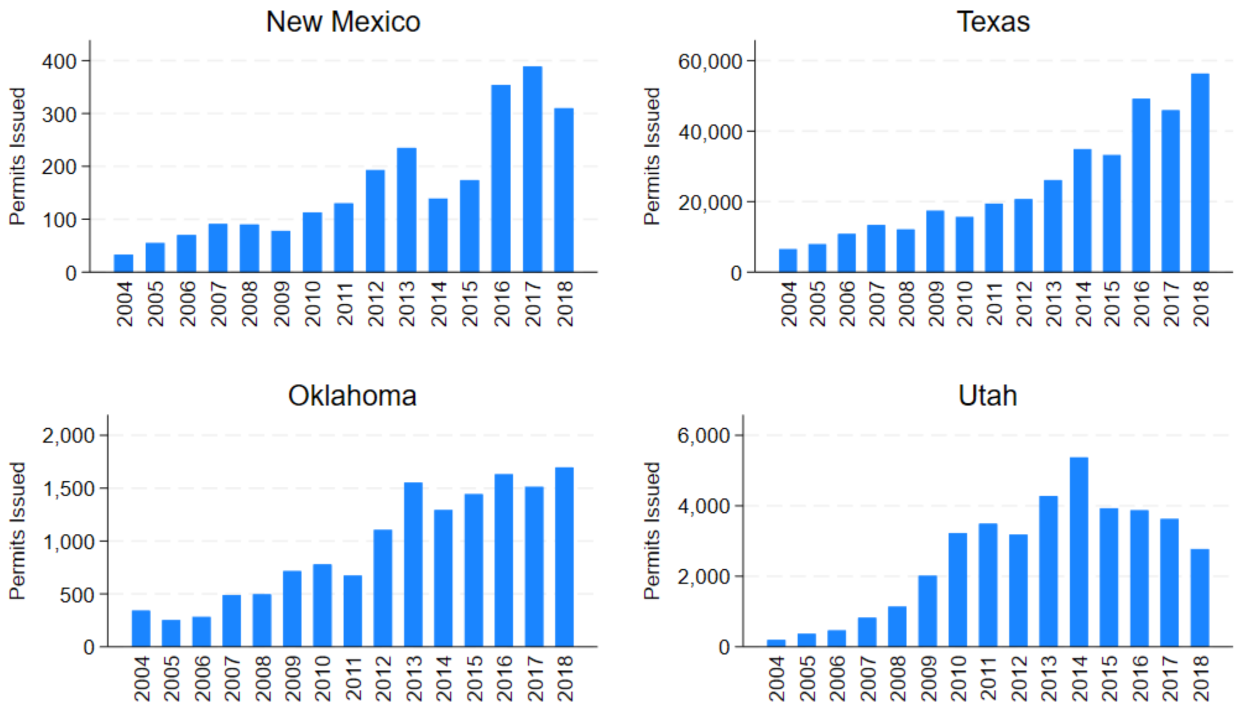
Weisburst, E. K. (2019, May). Safety in Police Numbers: Evidence of Police Effectiveness from Federal COPS Grant Applications. *American Law and Economics Review 21*(1), 81–109.

Wiebe, D. J., R. T. Krafty, C. S. Koper, M. L. Nance, M. R. Elliott, and C. C. Branas (2009). Homicide and geographic access to gun dealers in the united states. *BMC Public Health 9*, 1–10.

Zador, P. L. and M. A. Ciccone (1993). Automobile driver fatalities in frontal impacts: air bags compared with manual belts. *American Journal of Public Health 83*(5), 661–666.

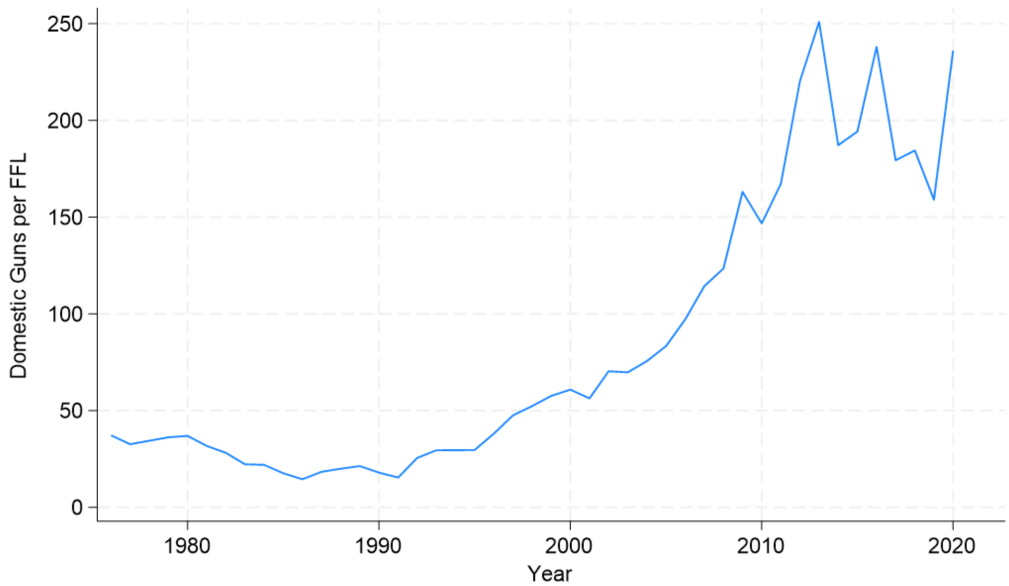
A Trends

Figure A1: Number of Concealed Carry Permits Issued to Black/African Americans (2004-2018)



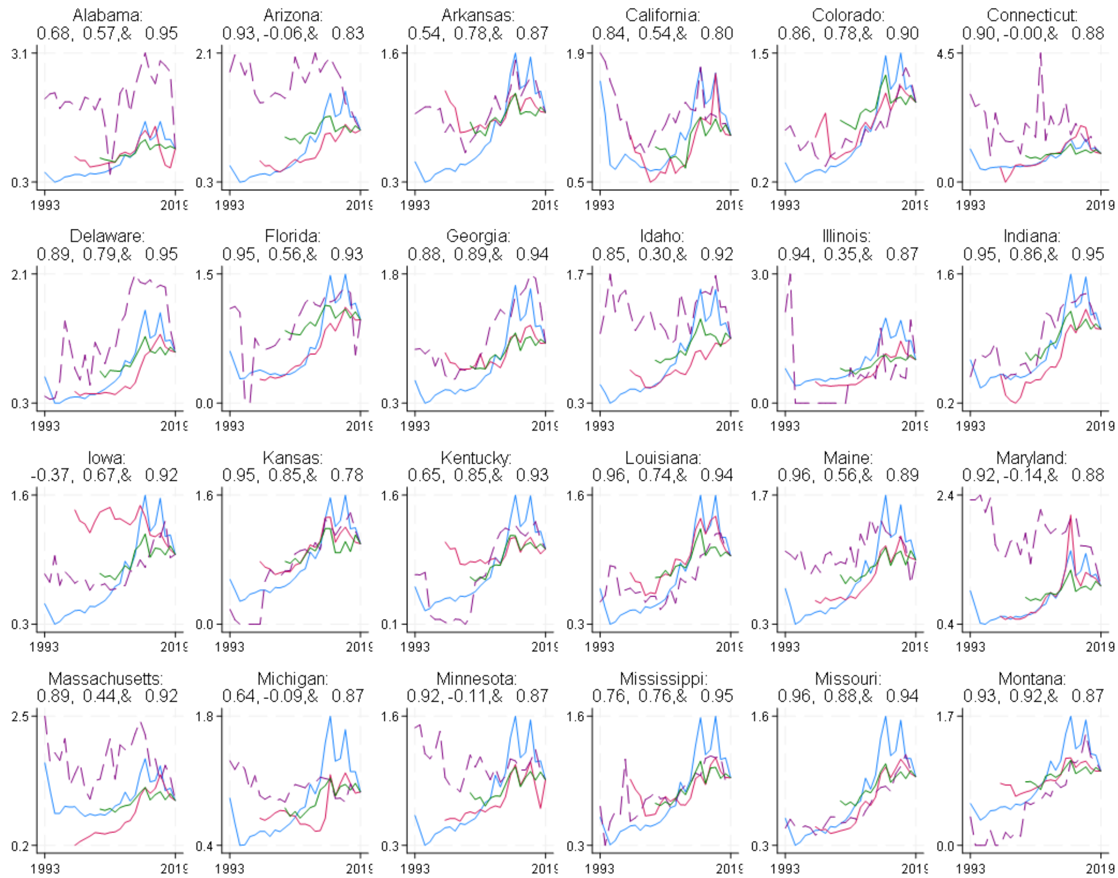
Notes: Concealed carry permits/handgun permits issued to Black residents of Texas, Utah, New Mexico, and Oklahoma.

Figure A2: New Domestic Guns per FFL



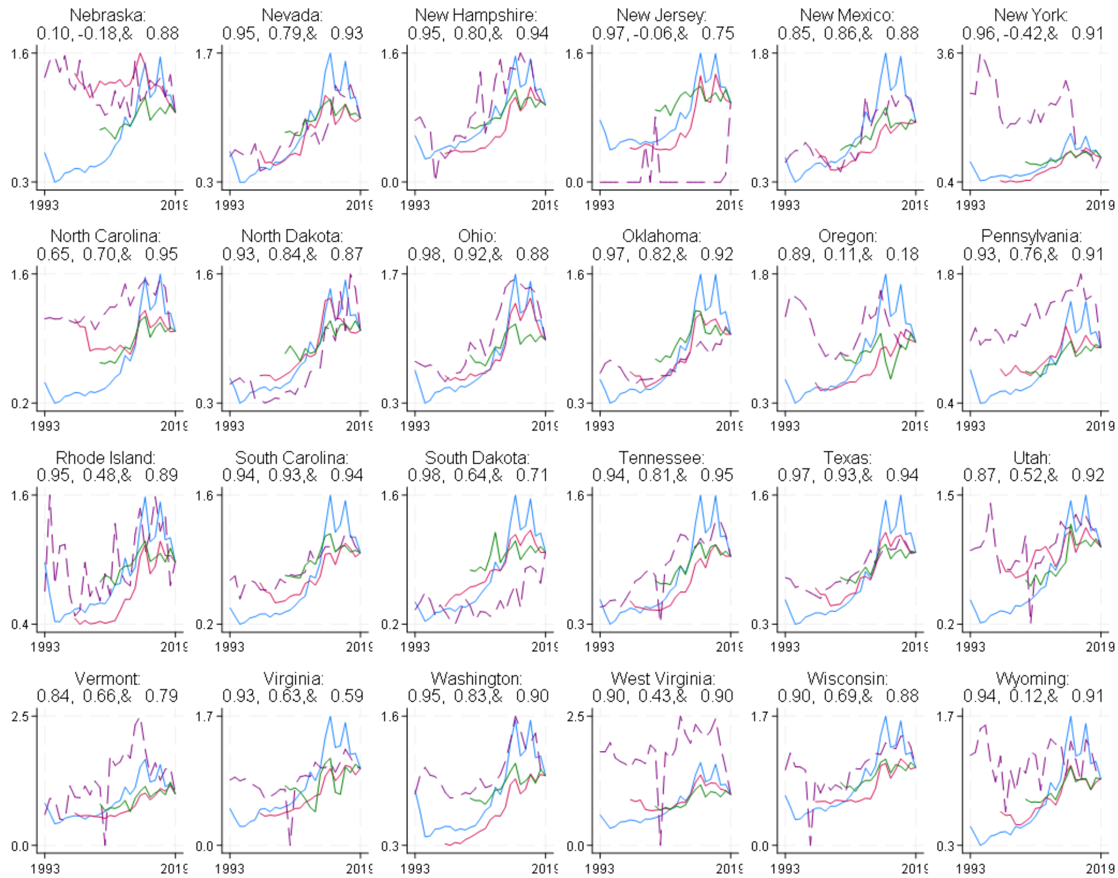
Notes: New Domestic Guns per gun dealer. Yearly figures are found by summing Manufacturing and net exports and dividing it by the number of dealer or pawn licenses (Type 1 or Type 2 FFL). The figure is generated using data from two separate Firearm Commerce Reports: 2002 and 2001. Reports are found at https://osf.io/qk6gu and https://osf.io/4skga.

Figure A3: Estimated Gun Sales, NICS Checks, Value of Reported Stolen Guns, Gun Searches



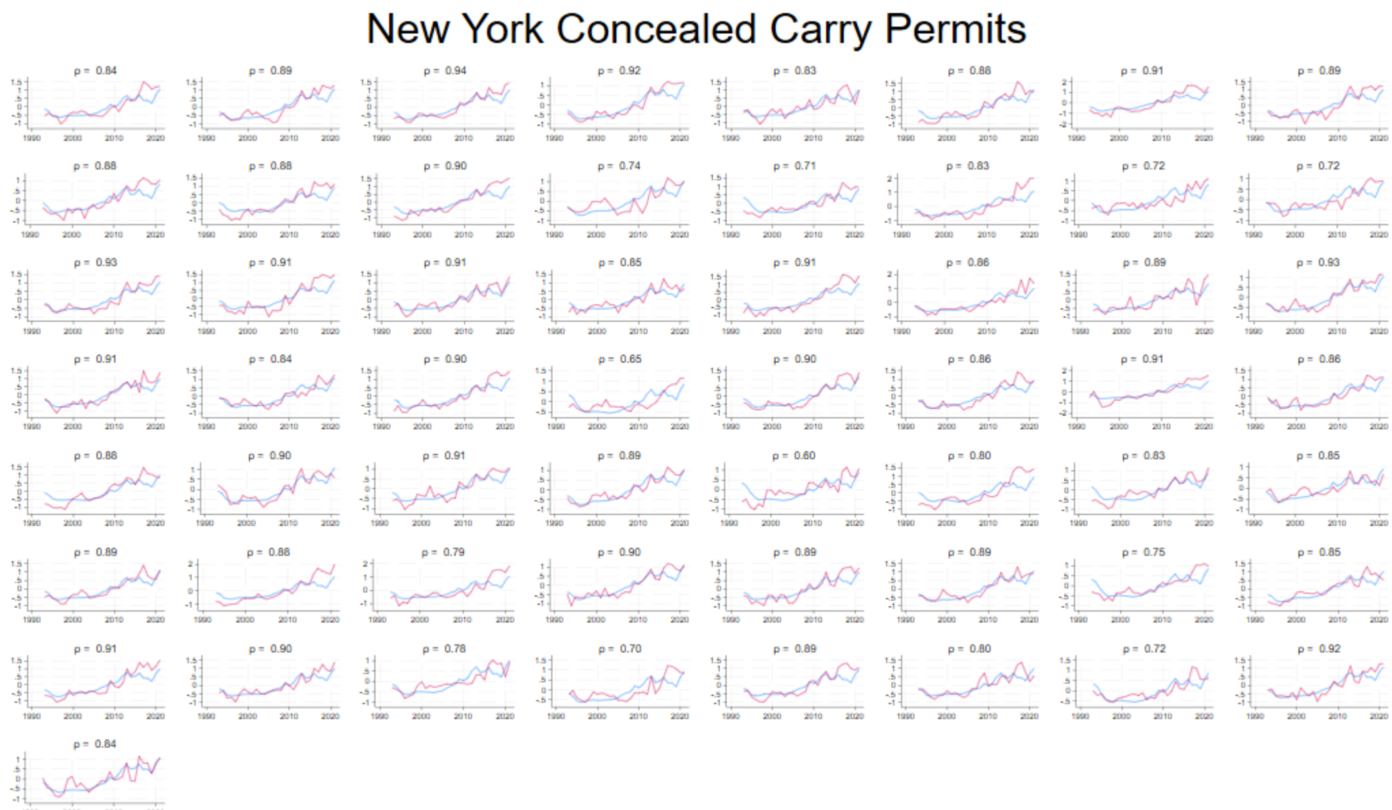
Notes: Estimated gun sales (blue), NICS checks (red), value of reported stolen guns (dashed purple), and Google Searches with the word "gun" (green line). State indicated in panel title along with the correlation between estimated gun sales and NICS Checks followed by the correlation between gun sales and the value of reported stolen guns, and, last, the correlation between estimated gun sales and Google search interest.

Figure A4: Estimated Gun Sales, NICS Checks, Value of Reported Stolen Guns, Gun Searches



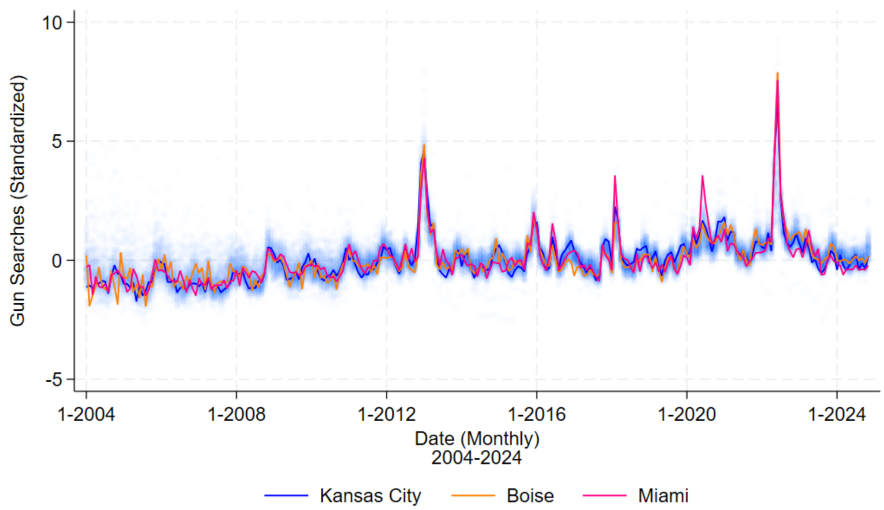
Notes: Estimated gun sales (blue), NICS checks (red), value of reported stolen guns (dashed purple), and Google Searches with the word "gun" (green line). State indicated in panel title along with the correlation between estimated gun sales and NICS Checks followed by the correlation between gun sales and the value of reported stolen guns, and, last, the correlation between estimated gun sales and Google search interest.

Figure A5: Estimated Gun Sales vs Concealed Carry Perimts



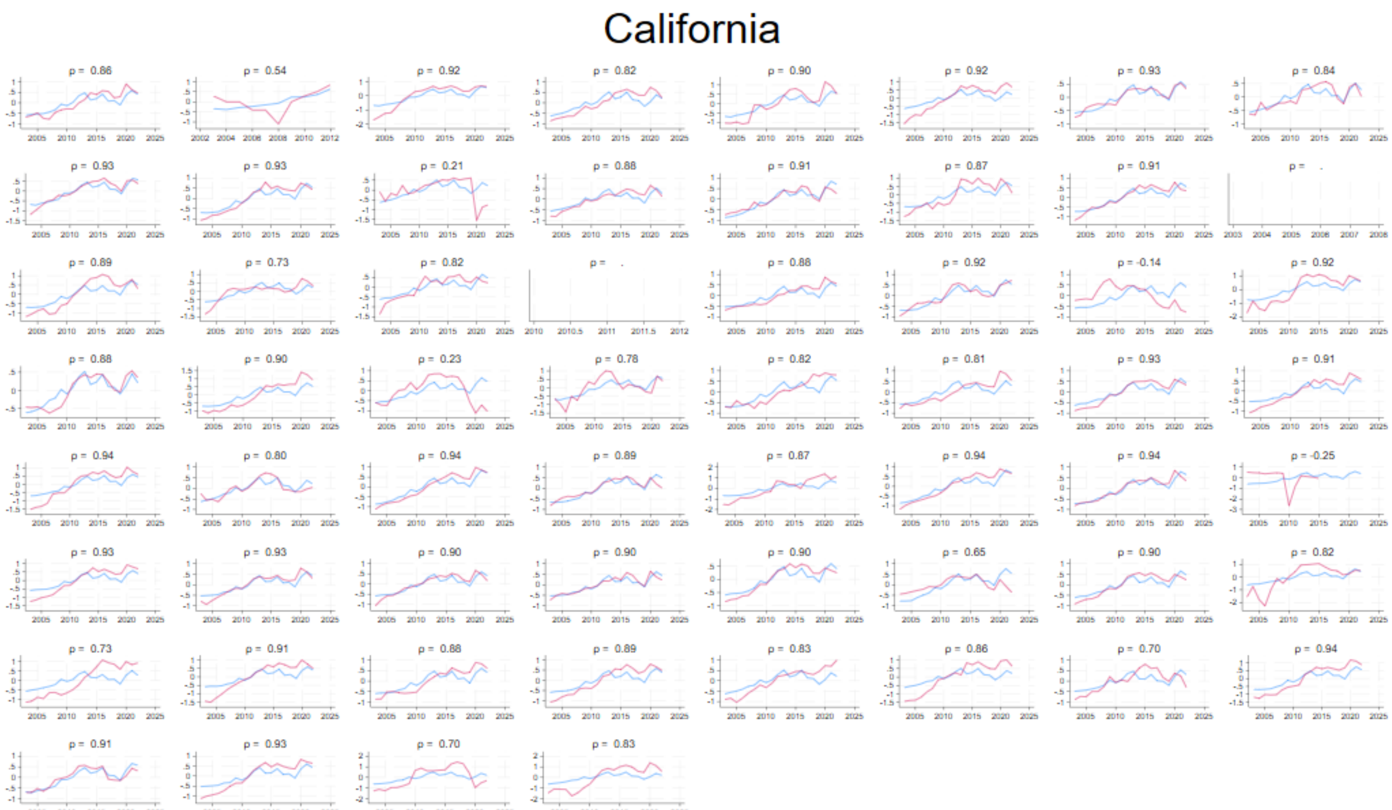
Notes: Demeand estimated gun sales and issued concealed carry permits. Pearson correlation, ρ, is found in the title.

Figure A6: Standardized Monthly Google Searches with the word "Gun"



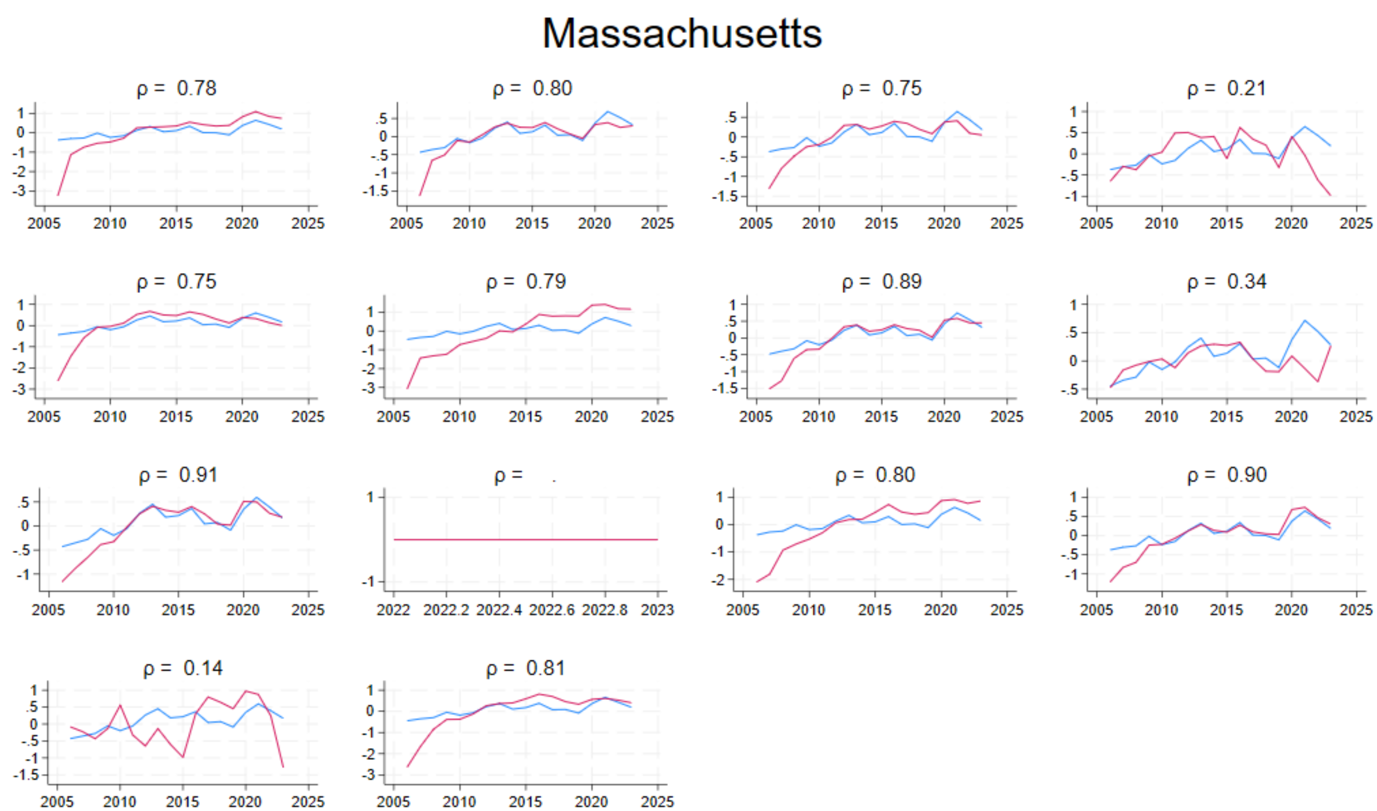
Notes: Monthly standardized share of searches with the word "Gun" (2004-2024). Each dot represents a DMA-month observation. Blue line is the Kansas City DMA, orange is Boise, and pink is Miami.

Figure A7: Estimated Gun Sales vs Gun sales



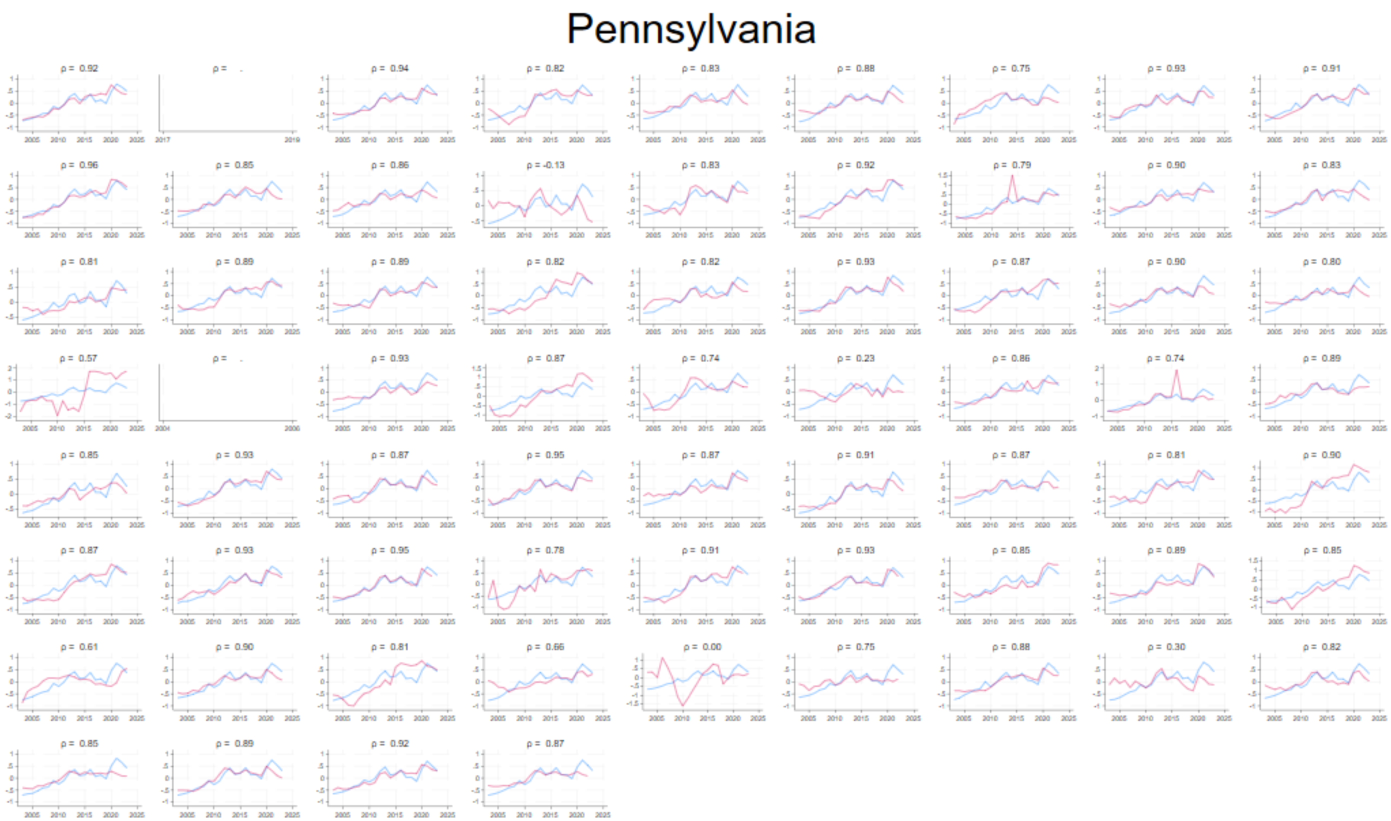
Notes: Demeand estimated gun sales and actual gun sales. Pearson correlation, ρ, is found in the title.

Figure A8: Estimated Gun Sales vs Gun sales



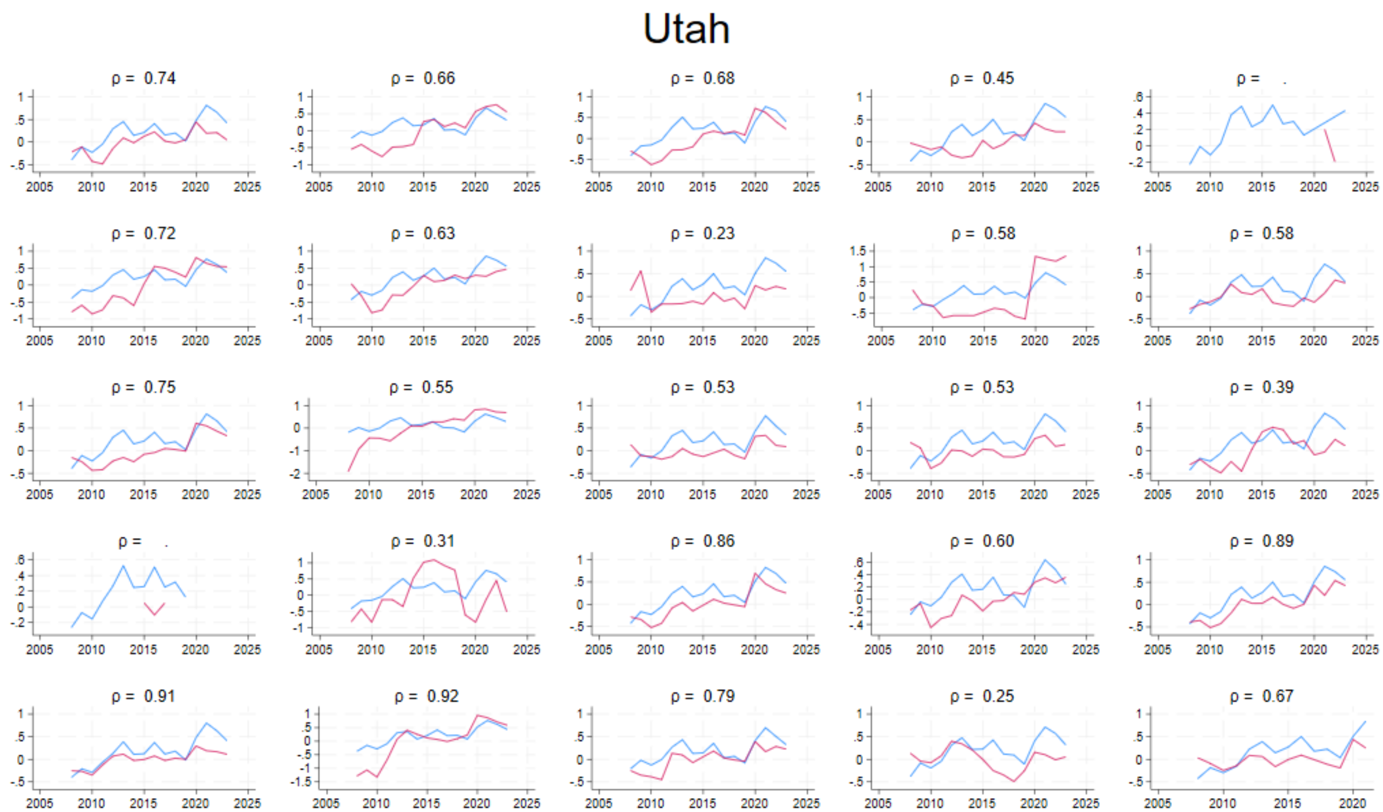
Notes: Demeand estimated gun sales and actual gun sales. Pearson correlation, ρ, is found in the title.

Figure A9: Estimated Gun Sales vs Gun sales



Notes: Demeand estimated gun sales and actual gun sales. Pearson correlation, ρ, is found in the title.

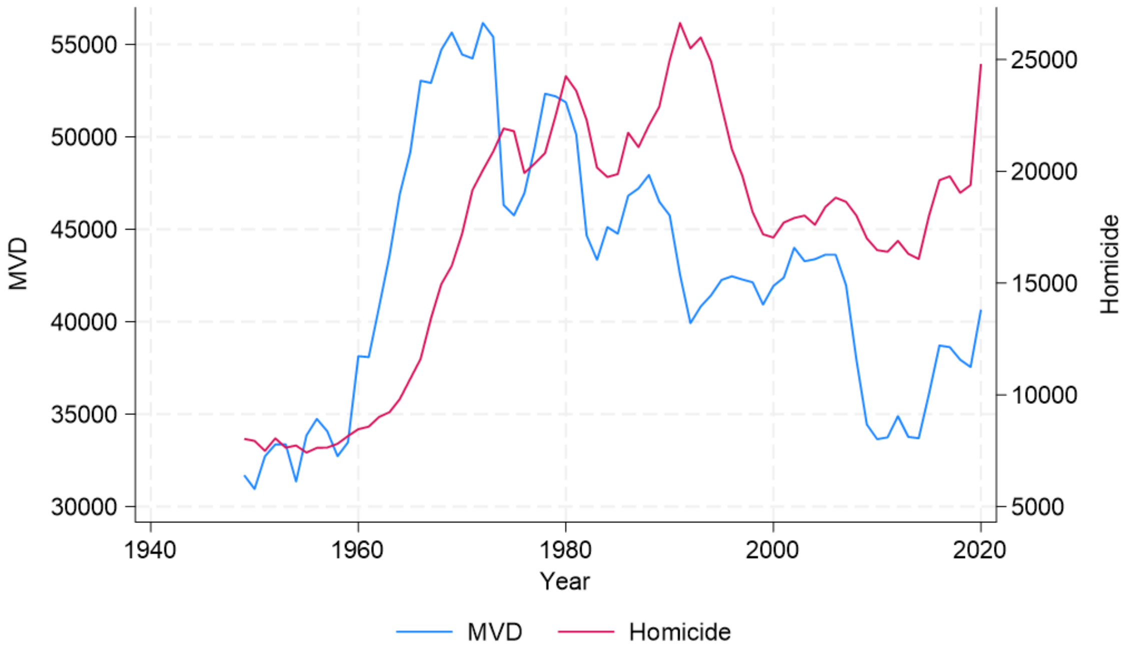
Figure A10: Estimated Gun Sales vs Gun sales



Notes: Demeand estimated gun sales and actual gun sales. Pearson correlation, ρ, is found in the title.

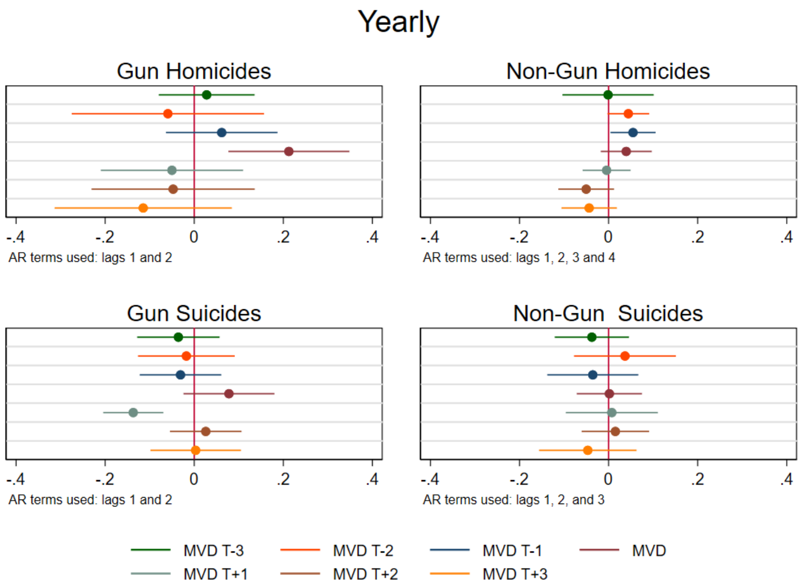
B MVD as Law Enforcement

Figure B1: Motor Vehicle Deaths and Homicide through the Years



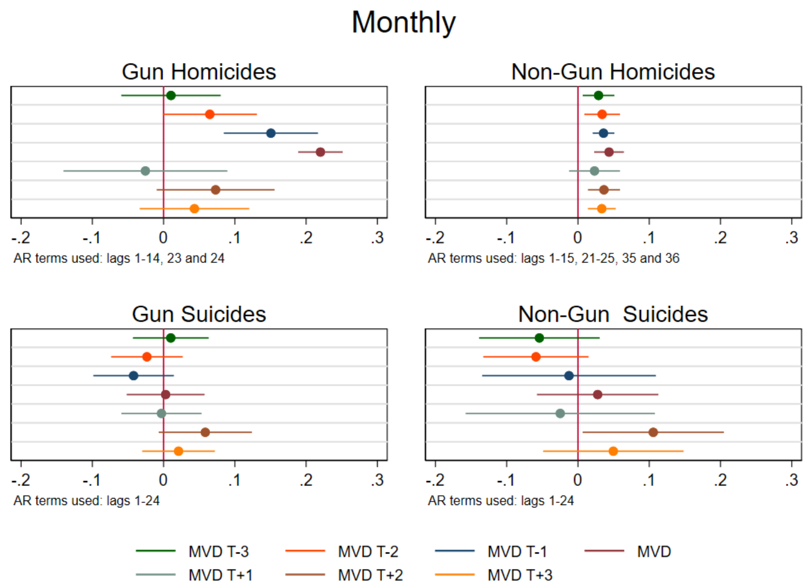
Notes: Motor vehicle deaths and homicide. Years: 1948-2019.

Figure B2: Estimated Relationship between Yearly Motor Vehicle Deaths and Yearly Deaths



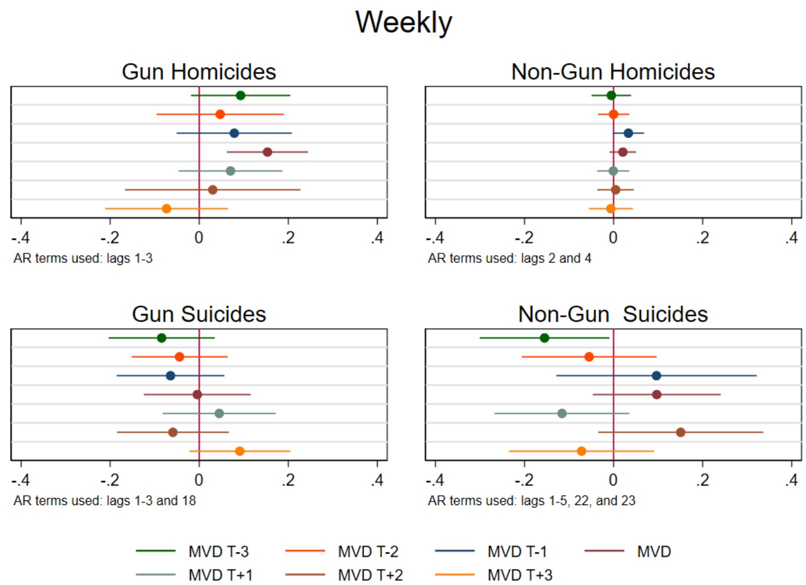
Notes: Estimated relationship between various lags of motor vehicle deaths and deaths. All models are Autoregressive models with the auto-regressive term selected based on the correlograms. Chosen lags are indicated in the subplot notes. Years: Years: 1979-2019

Figure B3: Estimated Relationship between Monthly Motor Vehicle Deaths and Monthly Deaths



Notes: Estimated relationship between various lags of motor vehicle deaths and deaths. All models are Autoregressive models with the auto-regressive terms selected based on the correlograms. Chosen lags are indicated in the subplot notes. Years: 1999-2019

Figure B4: Estimated Joint Relationship of Motor Vehicle Deaths and Violent Deaths (Weekly)



Notes: Estimated relationship between various lags of motor vehicle deaths and deaths. All models are Autoregressive models with the auto-regressive terms selected based on the correlograms. Chosen lags are indicated in the subplot notes. Years: 2018-2019

C Lag Choice Verification

Here, we explore how varying lags and leads of gun sales affect outcomes. We begin with the estimation results presented in Figure C1, which shows coefficient estimates from 17 models that differ by the lag/lead of the natural log of gun sales. All models are estimated using fixed effects Poisson with county-clustered standard errors. Control variables include the percent of the population identifying as Black or African American, the total number of businesses in the county and adjacent counties, county population and adjacent county population, and the employment-to-population ratio. Essentially, the only difference between these models and those presented in Table 4 is the lag structure applied to gun sales. These estimates partly support our choice of a 3-year lag. Gun homicides respond consistently and positively to past gun sales, but the lead coefficients are noisy and significant, which is concerning. Also concerning are the significant leads in non-gun homicide-related regressions. In contrast, gun suicides and non-gun suicides show minimal sensitivity to gun sales leads, and non-gun suicides display no consistent relationship with either leads or lags, except with very deep lags.

While the positive and significant leads for gun homicides in Figure C1 may initially raise concerns about reverse causality or anticipatory behavior, we argue these patterns are more plausibly explained by trends in the outcome variable. To evaluate this more directly, we estimate three alternative specifications using first-difference models: (1) a standard first-difference dynamic panel, (2) an Anderson-Hsiao estimator that instruments the lagged outcome using its second lag, and (3) an Anderson-Hsiao IV approach that additionally instruments first-differenced gun sales using year fixed effects. These specifications aim to eliminate persistent trends that might otherwise create spurious correlations between future gun sales and past violence. It should also be noted that in these alternatives, the outcome variable is the first difference of the natural log of gun homicides, non-gun homicides, gun suicides, or non-gun suicides.

The baseline first-difference dynamic panel takes the form:

$$\Delta Y_{i,t} = \beta_0 + \beta_1 \Delta \text{Gun Sales}_{i,t+j} + \beta_2 \Delta Y_{i,t-1} + \beta_3 \Delta X_{i,t} + \varepsilon_{i,t} \tag{4}$$

where $Y_{i,t}$ is the natural log of the outcome of interest, $\text{Gun Sales}_{i,t+j}$ is the log of gun sales at lead or lag $j \in \{-8, \ldots, +8\}$, and $X_{i,t}$ is the vector of control variables (also in logs). Standard errors are clustered at the county.

To address potential Nickell bias from including a lagged dependent variable in first differences, we implement the Anderson-Hsiao approach, with the first stage estimating:

$$\Delta Y_{i,t-1} = \beta_0 + \pi Y_{i,t-2} + \beta_1 \Delta \text{Gun Sales}_{i,t+j} + \beta_2 \Delta X_{it} + u_{i,t} \tag{5}$$

and the second stage using the predicted value $\widehat{\Delta Y}_{i,t-1}$:

$$\Delta Y_{i,t} = \beta_0 + \alpha \widehat{\Delta Y}_{i,t-1} + \beta_1 \Delta \text{Gun Sales}_{i,t+j} + \beta_2 \Delta X_{i,t} + \varepsilon_{i,t} \tag{6}$$

Coefficient estimates for the various leads and lags of gun sales are presented in Figure C2, with the top panel displaying the dynamic panel model and the bottom panel showing results from the Anderson-Hsiao estimator. These results reveal that once common trends are removed, the problematic gun sales leads in the Poisson model attenuate toward zero, while the third lag—our preferred specification—remains stable and statistically significant for gun homicides. Importantly, gun sales continue to show no effect on non-gun homicides in any specification involving changes in non-gun homicide rates.

As a final step, we implement a fully instrumented version of the Anderson-Hsiao estimator to account for potential endogeneity in both the lagged outcome and gun sales. Specifically, we instrument $\Delta Y_{i,t-1}$ with $Y_{i,t-2}$ (as before) and $\Delta \text{GunSales}_{i,t+j}$ with a full set of year fixed effects:

$$\text{First Stage 1:} \quad \Delta Y_{i,t-1} = \beta_0 + \pi_1 Y_{i,t-2} + \beta_1 \Delta X_{i,t} + u_{it} \tag{7}$$

$$\text{First Stage 2:} \quad \Delta \text{Gun Sales}_{i,t+j} = \beta_0 + \pi_2 \text{YEAR} + \beta_1 \Delta X_{i,t} + u_{it}^{(2)} \tag{8}$$

The second stage then becomes:

$$\Delta Y_{it} = \beta_0 + \alpha \widehat{\Delta Y}_{i,t-1} + \gamma \Delta \widehat{\text{Gun Sales}}_{i,t+j} + \beta_1 \Delta X_{it} + \varepsilon_{it} \tag{9}$$

As before, we estimate 17 different specifications that vary by j for each outcome and present the coefficients graphically. X is the same set of county-level controls, and standard errors are again clustered at the county level. Results are shown in Figure C3 and closely resemble those presented in Figure C2. Visually, all three models confirm that once persistent trends are accounted for, the apparent anticipatory effects of gun sales disappear, while the three-year lag remains a consistent and robust predictor of gun-related fatalities.

Figure C1: Estimated Relationship between Gun Sales Estimates (various lags) and Deaths

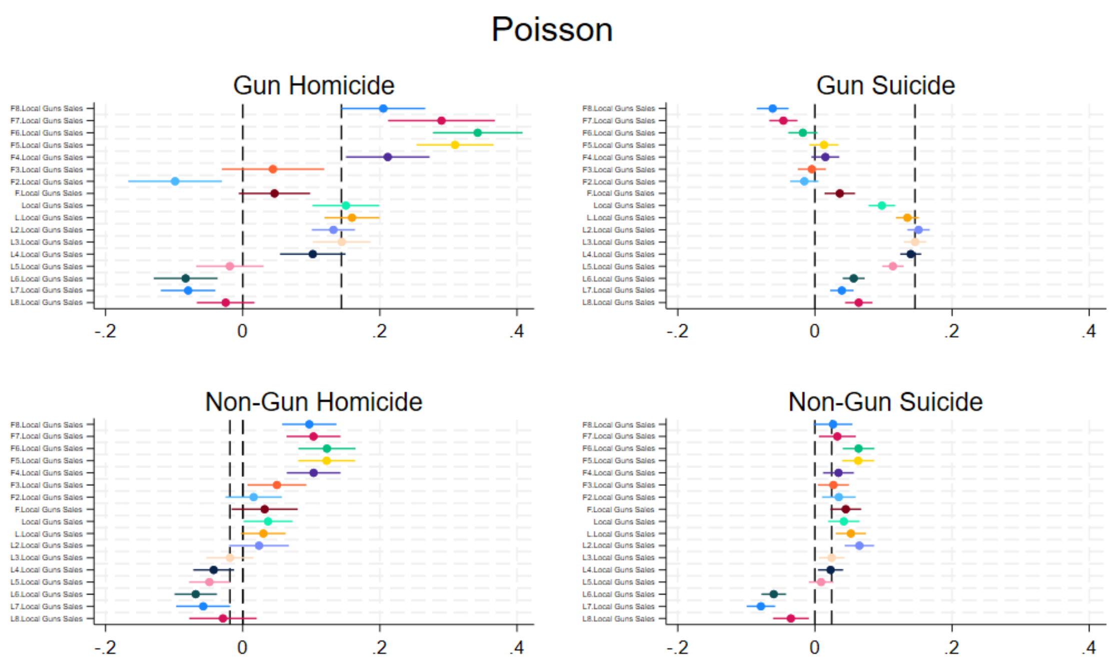


Figure C2: First Difference and Anderson-Hsiao Estimator: Estimated Relationship between Gun Sales and Deaths

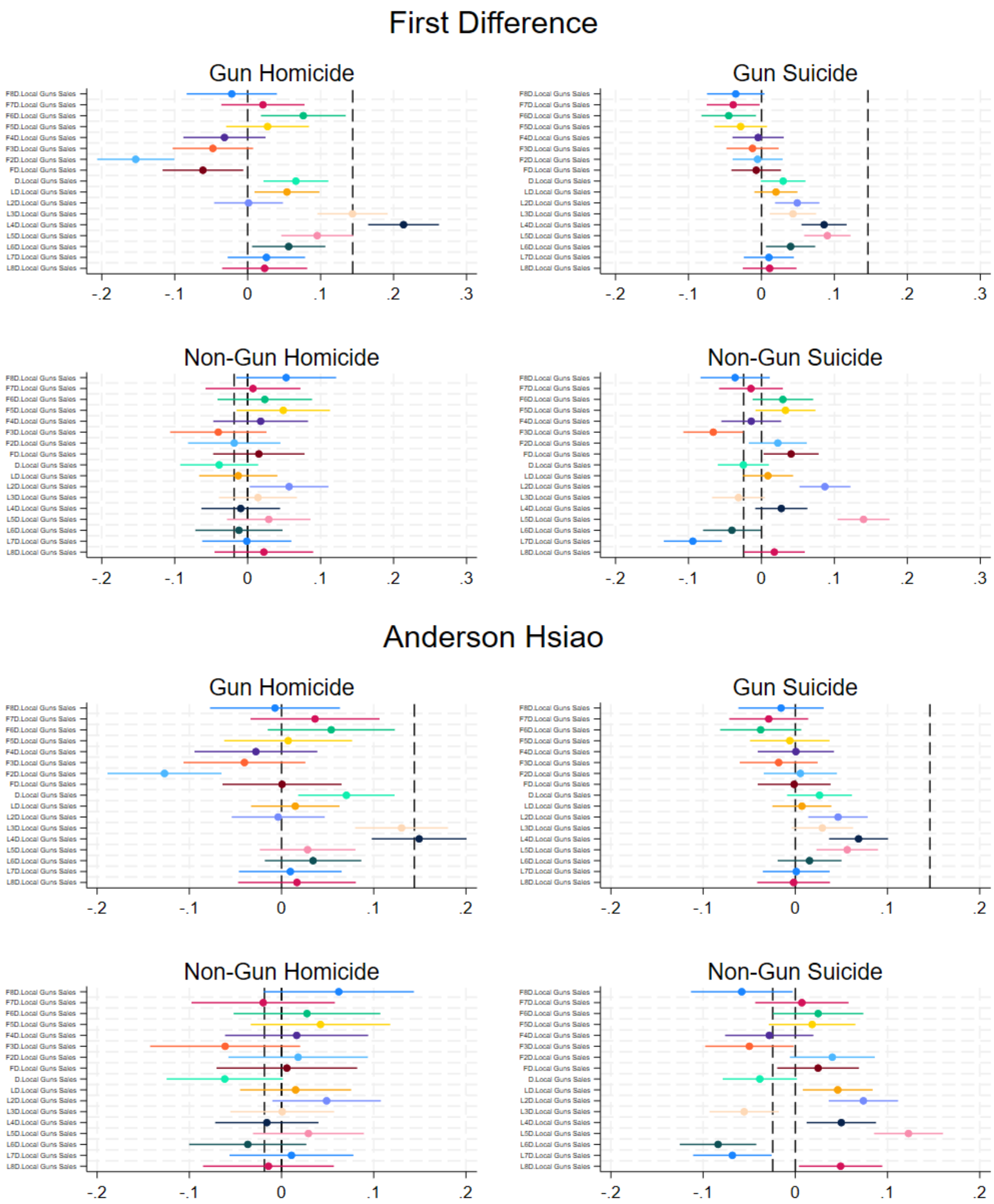
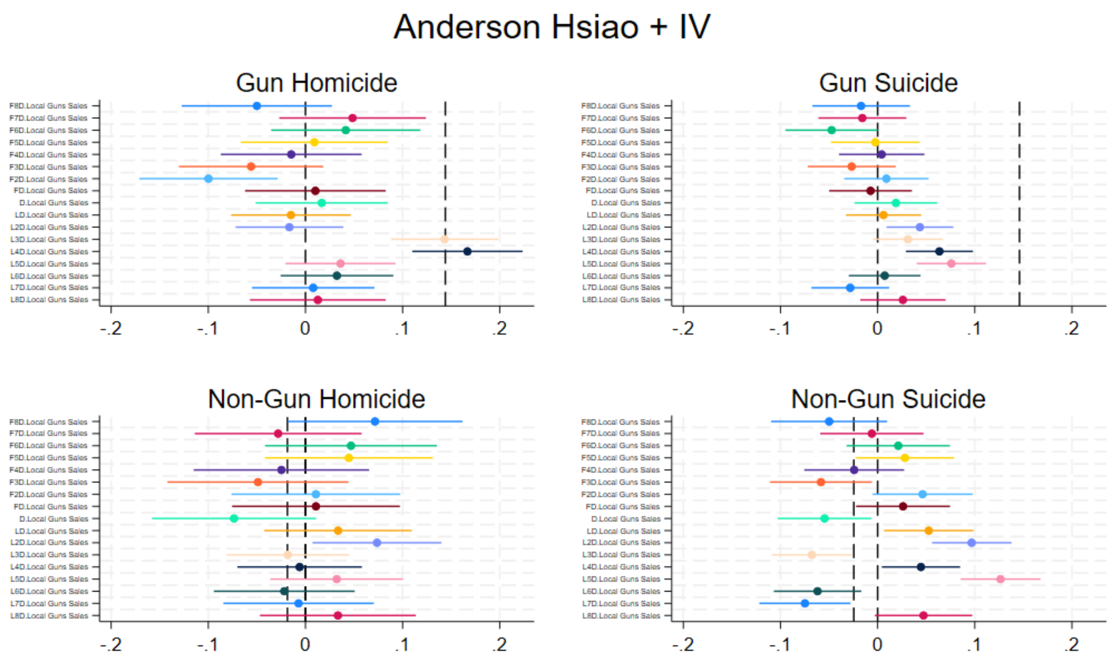


Figure C3: Anderson-Hsiao IV (Two-Stage) Estimator: Estimated Relationship between Gun Sales and Deaths



D Main Estimates

Table D1: Estimated Relationship Between Gun Sales and Homicide (County Level Panel with No Control Variables)

	Homicide					
	County FE	County FE + Trend	County FE + Year FE	County FE + State - Year FE	County FE + State Trends	County FE + County Trend
L3.Local Guns Sales	0.0242	0.164***	0.184	0.129	0.134***	0.113***
	(1.76)	(5.49)	(1.59)	(1.58)	(8.53)	(6.41)
MVD PC	0.236***	0.155***	0.114***	0.0723***	0.135***	0.106***
	(8.10)	(9.43)	(6.62)	(6.92)	(9.84)	(7.17)
OBS	65890	65890	65890	65890	65890	65890
Counties	2907	2907	2907	2907	2907	2907
R2	0.880	0.880	0.881	0.884	0.882	0.886
	Gun Homicide					
	County FE	County FE + Trend	County FE + Year FE	County FE + State - Year FE	County FE + State Trends	County FE + County Trend
L3.Local Guns Sales	0.118***	0.220***	0.227	0.127	0.175***	0.149***
	(6.23)	(5.90)	(1.41)	(1.21)	(8.50)	(5.91)
MVD PC	0.249***	0.186***	0.130***	0.0818***	0.165***	0.132***
	(7.52)	(8.42)	(5.47)	(5.90)	(8.92)	(6.64)
OBS	64080	64080	64080	64055	64080	64080
Counties	2798	2798	2798	2798	2798	2798
R2	0.867	0.868	0.869	0.873	0.870	0.876
	Non-Gun Homicide					
	County FE	County FE + Trend	County FE + Year FE	County FE + State - Year FE	County FE + State Trends	County FE + County Trend
L3.Local Guns Sales	-0.193***	0.0327	0.116*	0.219***	0.0298	-0.0288
	(-16.98)	(1.71)	(2.54)	(4.40)	(1.91)	(-1.77)
MVD PC	0.198***	0.0821***	0.0720***	0.0472***	0.0665***	0.0482***
	(6.57)	(5.10)	(4.39)	(3.46)	(4.97)	(3.65)
OBS	63692	63692	63692	63670	63692	63692
Counties	2777	2777	2777	2777	2777	2777
R2	0.736	0.738	0.738	0.742	0.739	0.747

Notes: Fixed effects Poisson regression results. The outcome variable is indicated by the panel heading. Time controls are indicated by column labels. T-statistics from county clustered standard errors are in parentheses; * p<0.05, ** p<0.01, *** p<0.001.

Table D2: Estimated Relationship Between Gun Sales and Suicide (County Level Panel with No Control Variables)

	Suicide					
	County FE	County FE + Trend	County FE + Year FE	County FE + State - Year FE	County FE + State Trends	County FE + County Trend
L3.Local Guns Sales	0.203***	0.119***	0.0816**	0.114***	0.122***	0.113***
	(37.07)	(13.96)	(3.09)	(4.01)	(18.77)	(19.04)
MVD PC	-0.0244***	0.00264	0.00973*	0.00829	0.00303	-0.00245
	(-4.95)	(0.55)	(2.02)	(1.87)	(0.70)	(-0.57)
OBS	67584	67584	67584	67584	67584	67584
Counties	3043	3043	3043	3043	3043	3043
R2	0.862	0.862	0.863	0.864	0.863	0.865
	Gun Suicide					
	County FE	County FE + Trend	County FE + Year FE	County FE + State - Year FE	County FE + State Trends	County FE + County Trend
L3.Local Guns Sales	0.163***	0.213***	0.205***	0.159***	0.197***	0.186***
	(29.56)	(18.28)	(5.08)	(5.11)	(26.26)	(26.45)
MVD PC	0.0232***	0.00988	0.0171**	0.0158**	0.00959	0.00202
	(3.80)	(1.64)	(2.88)	(2.90)	(1.77)	(0.37)
OBS	67472	67472	67472	67472	67472	67472
Counties	3030	3030	3030	3030	3030	3030
R2	0.759	0.759	0.760	0.762	0.760	0.764
	Non-Gun Suicide					
	County FE	County FE + Trend	County FE + Year FE	County FE + State - Year FE	County FE + State Trends	County FE + County Trend
L3.Local Guns Sales	0.249***	0.0369***	0.0623*	0.135***	0.0373***	0.0146
	(33.01)	(3.32)	(2.40)	(3.81)	(3.88)	(1.62)
MVD PC	-0.0875***	-0.00351	0.00451	0.00112	-0.00262	-0.00788
	(-10.33)	(-0.50)	(0.62)	(0.16)	(-0.39)	(-1.21)
OBS	66978	66978	66978	66978	66978	66978
Counties	2984	2984	2984	2984	2984	2984
R2	0.830	0.831	0.832	0.833	0.832	0.835

Notes: Fixed effects Poisson regression results. The outcome variable is indicated by the panel heading. Time controls are indicated by column labels. T-statistics from county clustered standard errors are in parentheses; * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table D3: Estimated Relationship Between Gun Sales and Homicide (County Level Panel)

	Homicide					
	County FE	County FE + Trend	County FE + Year FE	County FE + State - Year FE	County FE + State Trends	County FE + County Trend
L3.Local Guns Sales	0.103***	0.164***	0.205**	0.197**	0.129***	0.110***
	(6.92)	(8.06)	(2.69)	(2.92)	(8.40)	(7.48)
MVD PC	0.180***	0.146***	0.102***	0.0611***	0.118***	0.0656***
	(7.14)	(8.85)	(6.77)	(6.27)	(8.49)	(5.47)
OBS	65210	65210	65210	65210	65210	65210
Counties	2893	2893	2893	2893	2893	2893
R2	0.881	0.881	0.882	0.884	0.882	0.886
	Gun Homicide					
	County FE	County FE + Trend	County FE + Year FE	County FE + State - Year FE	County FE + State Trends	County FE + County Trend
L3.Local Guns Sales	0.192***	0.219***	0.251*	0.216*	0.167***	0.144***
	(9.66)	(8.75)	(2.34)	(2.49)	(7.90)	(6.71)
MVD PC	0.193***	0.177***	0.117***	0.0702***	0.145***	0.0788***
	(6.76)	(8.44)	(5.75)	(5.49)	(7.64)	(4.84)
OBS	63518	63518	63518	63496	63518	63518
Counties	2784	2784	2784	2784	2784	2784
R2	0.868	0.868	0.869	0.874	0.870	0.876
	Non-Gun Homicide					
	County FE	County FE + Trend	County FE + Year FE	County FE + State - Year FE	County FE + State Trends	County FE + County Trend
L3.Local Guns Sales	-0.111***	0.0255	0.0982**	0.235***	0.0235	-0.0186
	(-6.77)	(1.78)	(2.95)	(5.22)	(1.60)	(-1.06)
MVD PC	0.146***	0.0771***	0.0669***	0.0390**	0.0594***	0.0366**
	(5.14)	(4.40)	(3.94)	(2.89)	(4.37)	(2.73)
OBS	63165	63165	63165	63143	63165	63165
Counties	2768	2768	2768	2768	2768	2768
R2	0.737	0.738	0.739	0.742	0.739	0.747

Notes: Fixed effects Poisson regression results. The outcome variable is indicated by the panel heading. Time controls are indicated by column labels. Control variables are the percent of the county that identifies as Black or African American, the total number of businesses in the county and adjacent counties, county population, county adjacent population, and the employment-to-population ratio. T-statistics from county clustered standard errors are in parentheses; * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table D4: Estimated Relationship Between Gun Sales and Suicide (County Level Panel)

	Suicide					
	County FE	County FE + Trend	County FE + Year FE	County FE + State - Year FE	County FE + State Trends	County FE + County Trend
L3.Local Guns Sales	0.190***	0.118***	0.106***	0.132***	0.115***	0.0924***
	(32.76)	(16.96)	(5.75)	(5.29)	(18.59)	(14.29)
MVD PC	-0.0188***	0.00156	0.00725	0.00661	0.00344	0.00302
	(-3.90)	(0.34)	(1.58)	(1.51)	(0.78)	(0.69)
OBS	66796	66796	66796	66796	66796	66796
Counties	3032	3032	3032	3032	3032	3032
R2	0.862	0.862	0.863	0.864	0.863	0.865
	Gun Suicide					
	County FE	County FE + Trend	County FE + Year FE	County FE + State - Year FE	County FE + State Trends	County FE + County Trend
L3.Local Guns Sales	0.194***	0.209***	0.230***	0.178***	0.187***	0.146***
	(32.82)	(22.46)	(7.69)	(6.38)	(25.33)	(17.54)
MVD PC	0.0129*	0.00953	0.0142*	0.0143**	0.0104	0.00671
	(2.21)	(1.67)	(2.52)	(2.64)	(1.91)	(1.21)
OBS	66688	66688	66688	66688	66688	66688
Counties	3018	3018	3018	3018	3018	3018
R2	0.759	0.759	0.760	0.761	0.760	0.763
	Non-Gun Suicide					
	County FE	County FE + Trend	County FE + Year FE	County FE + State - Year FE	County FE + State Trends	County FE + County Trend
L3.Local Guns Sales	0.182***	0.0366***	0.0836***	0.160***	0.0348***	0.0245**
	(20.03)	(3.77)	(3.58)	(5.02)	(3.75)	(2.60)
MVD PC	-0.0559***	-0.00182	0.00292	-0.00211	-0.00235	-0.00161
	(-6.85)	(-0.26)	(0.40)	(-0.30)	(-0.34)	(-0.24)
OBS	66205	66205	66205	66205	66205	66205
Counties	2968	2968	2968	2968	2968	2968
R2	0.830	0.831	0.831	0.833	0.831	0.835

Notes: Fixed effects Poisson regression results. The outcome variable is indicated by the panel heading. Time controls are indicated by column labels. Control variables are the percent of the county that identifies as Black or African American, the total number of businesses in the county and adjacent counties, county population, county adjacent population, and the employment-to-population ratio. T-statistics from county clustered standard errors are in parentheses; * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table D5: Estimated Relationship with Preferred Specification (Clearance Rate)

	Homicides	Gun Homicides	Non-Gun Homicides	Suicides	Gun Suicides	Non-Gun Suicides	Gun Assaults	Knife Assaults
L3.Local Guns Sales	0.113***	0.144***	-0.00947	0.0919***	0.151***	0.0163	0.175***	-0.0490
	(7.57)	(6.61)	(-0.49)	(13.73)	(17.49)	(1.67)	(6.34)	(-1.55)
% Cleared	-0.151*	-0.246**	-0.0293	0.0493*	0.0460	0.0260	-0.156	0.381**
	(-2.45)	(-2.76)	(-0.43)	(2.10)	(1.58)	(0.66)	(-1.08)	(3.18)
OBS	62559	60787	60180	64534	64405	63927	63752	63946
Counties	2751	2661	2634	2880	2868	2827	2823	2828
R2	0.880	0.869	0.733	0.866	0.771	0.831	0.966	0.961

	Homicides	Gun Homicides	Non-Gun Homicides	Suicides	Gun Suicides	Non-Gun Suicides	Gun Assaults	Knife Assaults
L3.Local Guns Sales	0.109***	0.138***	-0.00886	0.0921***	0.150***	0.0171	0.171***	-0.0420
	(7.35)	(6.43)	(-0.45)	(13.65)	(17.36)	(1.75)	(6.36)	(-1.36)
% Cleared (Violent)	-0.138***	-0.226***	0.0167	0.0205	0.0231	0.0133	-0.215*	0.201*
	(-3.89)	(-4.48)	(0.46)	(1.73)	(1.58)	(0.67)	(-2.30)	(2.42)
OBS	61388	59696	59211	62852	62704	62340	62465	62591
Counties	2744	2647	2622	2856	2840	2804	2823	2827
R2	0.880	0.869	0.732	0.865	0.769	0.831	0.966	0.961

Notes: Estimated effects using a fixed effects Poisson estimator. All models include county fixed effects and county-specific time trends. The outcome is indicated by the panel label. T-statistics from county clustered standard errors are in parentheses; * p<0.05, ** p<0.01, *** p<0.001.

Table D6: Interaction Effects (Local Gun Sales)

	Homicides	Gun Homicides	Non-Gun Homicides	Suicides	Gun Suicides	Non-Gun Suicides
L3.Local Guns Sales	-0.0804	-0.106	-0.0569	0.0993***	0.170***	0.0439*
	(-1.63)	(-1.47)	(-1.50)	(7.18)	(9.40)	(2.04)
MVD PC	-0.672***	-0.898***	-0.111	0.0298	0.0948	0.0774
	(-4.25)	(-4.10)	(-0.84)	(0.64)	(1.60)	(1.05)
L3.Local Guns Sales × MVD PC	0.0763***	0.101***	0.0154	-0.00276	-0.00916	-0.00807
	(4.55)	(4.33)	(1.11)	(-0.58)	(-1.48)	(-1.07)
OBS	65210	63518	63165	66796	66688	66205
Counties	2893	2784	2768	3032	3018	2968
R2	0.886	0.876	0.747	0.865	0.763	0.835

Notes: Estimated interaction effects using a fixed effects Poisson estimator. All models include county-fixed effects and county-specific time trends. Control variables are the percent of the county that identifies as Black or African American, the total number of businesses in the county and adjacent counties, county population, county adjacent population, and the employment-to-population ratio. The outcome is indicated by the panel label. T-statistics from county clustered standard errors are in parentheses; * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table D7: Estimated Relationship with Preferred Specification

	Homicides	Gun Homicides	Non-Gun Homicides	Suicides	Gun Suicides	Non-Gun Suicides
L3.Local Guns Sales	0.226***	0.299***	-0.0853	0.0804***	0.192***	-0.0700
	(3.84)	(3.88)	(-1.34)	(3.40)	(6.76)	(-1.83)
DUI Arrests	0.157	0.226	-0.142	-0.0368	0.0479	-0.154*
	(1.71)	(1.75)	(-1.29)	(-0.95)	(1.03)	(-2.48)
L3.Local Guns Sales × DUI Arrests	-0.0193*	-0.0266*	0.0138	0.00420	-0.00506	0.0170**
	(-2.03)	(-1.99)	(1.19)	(1.07)	(-1.06)	(2.68)
OBS	58347	56625	55913	60195	60085	59636
Counties	2667	2572	2532	2799	2789	2742
R2	0.879	0.869	0.730	0.860	0.762	0.826

	Homicides	Gun Homicides	Non-Gun Homicides	Suicides	Gun Suicides	Non-Gun Suicides
L3.Local Guns Sales	0.136***	0.173***	0.00386	0.0937***	0.155***	0.0187
	(6.97)	(6.23)	(0.16)	(10.82)	(14.40)	(1.46)
Clearance Rate	1.310*	1.661	0.704	0.141	0.244	0.179
	(2.03)	(1.92)	(0.90)	(0.51)	(0.70)	(0.40)
L3.Local Guns Sales × Clearance Rate	-0.153*	-0.196*	-0.0843	-0.0102	-0.0227	-0.0141
	(-2.23)	(-2.13)	(-1.02)	(-0.35)	(-0.62)	(-0.31)
OBS	62457	60703	60062	64436	64317	63837
Counties	2748	2658	2630	2878	2867	2825
R2	0.880	0.869	0.732	0.866	0.771	0.831

	Homicides	Gun Homicides	Non-Gun Homicides	Suicides	Gun Suicides	Non-Gun Suicides
L3.Local Guns Sales	0.349***	0.552***	-0.297***	0.0984***	0.0823**	-0.0379
	(7.44)	(8.61)	(-4.07)	(3.62)	(2.81)	(-0.89)
LEA Employees	0.225***	0.404***	-0.356***	-0.0119	-0.111**	-0.0855
	(3.56)	(4.79)	(-3.51)	(-0.31)	(-2.60)	(-1.48)
L3.Local Guns Sales × LEA Employees	-0.0297***	-0.0510***	0.0378***	-0.00113	0.00875*	0.00764
	(-4.89)	(-6.29)	(3.73)	(-0.29)	(2.05)	(1.30)
OBS	56406	54607	54121	57904	57768	57344
Counties	2747	2637	2610	2872	2856	2812
R2	0.880	0.869	0.735	0.863	0.766	0.828

Notes: Estimated interaction effects using a fixed effects Poisson estimator. All models include county fixed effects and county-specific time trends. The outcome is indicated by the panel label. T-statistics from county clustered standard errors are in parentheses; * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table D8: Estimated Relationship with Preferred Specification (Other Clearance Rates)

	Homicides	Gun Homicides	Non-Gun Homicides	Suicides	Gun Suicides	Non-Gun Suicides
L3.Local Guns Sales	0.156***	0.207***	-0.000237	0.0912***	0.151***	0.0184
	(7.45)	(7.48)	(-0.01)	(10.16)	(13.06)	(1.43)
Clearance (All Crime)	2.001**	2.881**	0.409	0.0274	0.0716	0.0997
	(2.99)	(3.03)	(0.54)	(0.10)	(0.21)	(0.23)
L3.Local Guns Sales × Clearance (All Crime)	-0.225**	-0.326**	-0.0452	0.00197	-0.00345	-0.00719
	(-3.13)	(-3.20)	(-0.56)	(0.07)	(-0.10)	(-0.16)
OBS	61388	59696	59211	62852	62704	62340
Counties	2744	2647	2622	2856	2840	2804
R2	0.880	0.869	0.732	0.865	0.769	0.831

	Homicides	Gun Homicides	Non-Gun Homicides	Suicides	Gun Suicides	Non-Gun Suicides
L3.Local Guns Sales	0.180***	0.242***	-0.0121	0.0970***	0.162***	0.0138
	(8.22)	(8.67)	(-0.49)	(10.84)	(13.33)	(1.11)
Clearance (Violent)	1.489***	2.197***	-0.0559	0.122	0.258	-0.0565
	(4.64)	(4.92)	(-0.14)	(0.93)	(1.51)	(-0.26)
L3.Local Guns Sales × Clearance (Violent)	-0.169***	-0.250***	0.00757	-0.0105	-0.0246	0.00719
	(-4.95)	(-5.28)	(0.18)	(-0.77)	(-1.38)	(0.33)
OBS	61388	59696	59211	62852	62704	62340
Counties	2744	2647	2622	2856	2840	2804
R2	0.880	0.869	0.732	0.865	0.769	0.831

Notes: Estimated interaction effects using a fixed effects Poisson estimator. All models include county fixed effects and county-specific time trends. The outcome is indicated by the panel label. T-statistics from county clustered standard errors are in parentheses; * p<0.05, ** p<0.01, *** p<0.001.

Table D9: Interaction Effects (Local Gun Sales and Various Assaults)

	Gun Assaults				Knife Assaults			
	MVDPC	Clearance Rate	DUI Arrest Rate	LEA Employees	MVDPC	Clearance Rate	DUI Arrest Rate	LEA Employees
L3.Local Guns Sales	-0.0487	0.242***	0.480***	0.350***	-0.119**	-0.00733	0.0539	-0.295*
	(-0.50)	(5.08)	(3.58)	(3.30)	(-2.89)	(-0.13)	(0.45)	(-2.28)
Police Level	-0.738*	3.851*	0.519*	0.166	-0.219	2.712	0.175	-0.295
	(-2.40)	(2.41)	(2.31)	(1.18)	(-1.72)	(1.72)	(1.07)	(-1.48)
L3.Local Guns Sales × Police Level	0.0844**	-0.443**	-0.0482*	-0.0207	0.0257	-0.274	-0.00981	0.0327
	(2.59)	(-2.61)	(-2.05)	(-1.44)	(1.92)	(-1.61)	(-0.57)	(1.68)
OBS	59653	63677	59462	57295	59742	63878	59707	57416
Counties	2789	2822	2735	2825	2797	2828	2747	2832
R2	0.965	0.966	0.966	0.966	0.960	0.961	0.961	0.962

Notes: Estimated interaction effects using a fixed effects Poisson estimator. All models include county-fixed effects and county-specific time trends. Control variables are the percent of the county that identifies as Black or African American, the total number of businesses in the county and adjacent counties, county population, county adjacent population, and the employment-to-population ratio. The outcome is indicated by the panel label. T-statistics from county clustered standard errors are in parentheses; * p<0.05, ** p<0.01, *** p<0.001.