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

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January 17, 2025

Abstract

The availability of firearms in the United States is thought to be a significant cause of homicide and suicide in the United States. However, due to data limitations, many consider the relationship an open question. Using data from multiple sources, we find a strong, consistent positive relationship between firearms and firearm-related outcomes using a variety of methods. We also present evidence of a negative relationship between active policing and homicide. Last, we show that active policing will mitigate firearm-related harms, but only those related to criminal activity.

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

*We thank William King, Jeffrey Fagan, and attendees of the Middle Tennessee State University Seminar series for valuable comments and suggestions.

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1 Introduction

With its high rate of gun-related deaths, America stands out among developed countries. Some argue this is attributed to the widespread availability of firearms in the United States and others contend that individual factors such as mental health issues or high crime rates. In this study, we examine the impact of changes in firearm prevalence on gun deaths, encompassing both homicides and suicides, while also exploring how changes in policing affect gun-related deaths. We use a novel measure of firearm availability in a shift-share-inspired design to accurately document exogenous changes to the local firearm supply. This approach is innovative because the challenge of establishing a direct link between gun prevalence and fatal gun outcomes has largely stemmed from data quality issues. In short, we lack a standardized registry or comprehensive record of firearm sales or purchases. Furthermore, even in regions with stringent gun laws mandating registration or notification to authorities of transfers, firearms are durable goods that can be easily transported or sold through secondary and tertiary parties.

The local gun supply estimate we use is based on the number of gun dealers – or Federal Firearm Licenses (FFLs) – already in an area and national trends in gun manufacturing. These data come from a unique dataset documenting changes in the number of licensed gun dealers (Type 1 and Type 2 FFLs) at a narrow geographic region and weights gun dealers by the inverse of national new gun production (augmented by imports and exports) per gun dealer. Importantly, this measure of gun prevalence is exogenous to changes in local crime rates through its construction. This is for two reasons: first, national production is independent of local crime rates, and, second, the existence of short-run firm entry barriers. Even if one were to accept the notion that gun dealer presence changed as a result of new demand caused by changes in observed or perceived local crime rates, this would not occur contemporaneously. The weighting of gun dealers by gun manufacturing also adjusts for the changes in the nature of gun dealers throughout the panel. Possibly more importantly, we show that the weighted gun dealer measure (our estimated new gun sales) is an accurate measure of observed transactions (sales, concealed carry permits, and thefts).

Our work contributes to existing research on firearms and firearm deaths in several meaningful ways. Most prior studies on firearm prevalence and fatal outcomes rely on proxies of firearm ownership to measure the relationship between gun prevalence and fatal outcomes. These proxies include the proportion of gun suicide (Cook and Ludwig, 2006), the number of gun shows in a county (Gobaud et al., 2022), the number of background checks (Lang, 2016), and the number of federally licensed firearm dealers (Johnson and Robinson, 2024; Stansfield et al., 2021; Steidley et al., 2017). These studies almost universally find that greater firearm prevalence is associated with worsening fatal outcomes, suggesting a net social cost of firearm ownership. This is, in part, due to gun spillovers into illegitimate uses (Phillips et al., 2013; Johnson et al., 2024; Billings, 2023). It is difficult to say with certainty how these guns are leaking into illegitimate uses, but Billings (2023) shows that concealed handgun permit holders face increased property crime victimization, with the interesting finding that firearms are often stolen. Furthermore, Billings (2023) finds that violent crime involving firearms in neighborhoods with concealed handgun permit holders increases with the total number of these permits, suggesting that stolen guns spillover into neighborhood crime. Both Kahane (2020) and Khalil (2017) support this notion, finding a

positive impact of increased stolen firearms on firearm-aggravated assaults, homicides, and robberies in the future, and fail to find a relationship between firearm flows and non-firearm offenses, indicating that this is not a spurious correlation. Moreover, Khalil (2017) finds a time lag between when a gun is stolen and when firearm-aggravated assaults, homicides, and robberies change. They even show that contemporaneous gun theft is not related to any of the prior outcome measures. Our work here supports the notion that there is a time lag between when gun supply changes and when gun death outcomes change.

Changes in firearm prevalence are not the sole driving factor in determining changes in homicides. We also consider the mitigating effect police have on crime. The national discourse surrounding the role of police in communities has prompted extensive research on the effects of police force size, effort, and the likelihood of criminal charges based on the severity of the crime committed. This conversation takes place alongside discussions of gun access and ease of purchase following tragic events involving multiple fatalities in a public space. Most gun homicides are not national news events (e.g., school shootings), however, and greater police presence at the margin could reasonably lower homicide rates, especially in areas where gun access is high. Our paper contributes to this ongoing policy debate by investigating the effect that greater police presence/effort has on gun homicides in areas where there has been a large positive shock to crime gun supply.

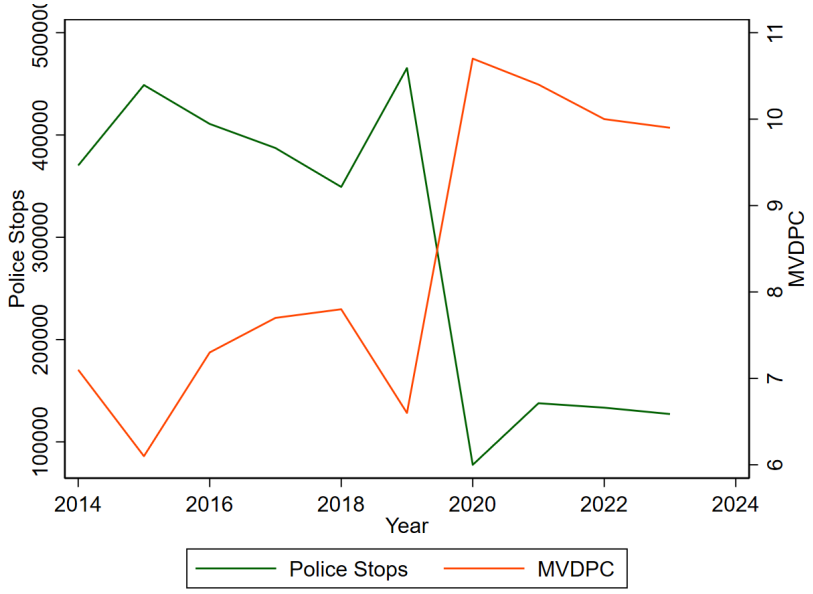
We account for changes in policing using several proxy variables of police force size and effort. Prior work has shown greater police presence or larger police force sizes lead to reductions in most serious crimes, especially homicides (Mello, 2019; Chalfin et al., 2022). This is important for obvious reasons, but in part because people rightly place a significant value on policing minimizing their risk of falling victim to homicide. For instance, Chalfin and McCrary (2018) estimate that the likelihood of someone experiencing motor vehicle theft is 46 times higher than their likelihood of being a victim of murder. However, despite the rarity of murder, the cost associated with murder is 27 times greater than motor vehicle theft. A more refined picture of gun presence, particularly crime gun presence, can help show where the marginal value of policing may be greatest.

Our primary proxy for police presence is the number of motor-vehicle deaths per capita, which is documented by the Centers for Disease Control and Prevention (CDC). The general mechanism is, all else equal, greater police presence reduces speeding, driving under the influence (DUI), and reckless driving – all of which contribute to motor vehicle deaths. Moreover, in that process of making elective stops, police officers will confiscate weapons; some of which, sans an intervention, would be involved in a murder. We also discuss alternative measures of policing. We use various index crime clearance rates and the DUI arrest rate in place of the motor vehicle death rate – with the latter being especially interesting because of the documented link between alcohol consumption and homicide (c.f., Greenfeld, 1998). Overall, these alternative measures lead to conclusions similar to those we discuss throughout the work. Finally, we also consider alternative outcome variables that should have different relationships with changes in gun presence: gun assaults and knife assaults.

The main measure of police presence is the motor vehicle deaths per capita. The main benefit of this measure is that it is based on death certificate data and is consistently reported. The drawback is that it is not obvious why it is a useful measure of policing. We detail why it probably is later in the paper, but as a first anecdotal example of this relationship, we show the number of police stops against the number of motor vehicle deaths

for Philadelphia County, Pennsylvania in Figure 1. It is evident that there is an inverse relationship between the level of policing as captured by the number of stops and the number of vehicular deaths. Peaks for one year in police stops are matched by valleys in vehicle deaths. Indeed, this relationship persisted past the 2020 pandemic.

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).

Our results can be summarized as follows: overall, we find a positive relationship between the estimated number of new guns sold and gun-related outcomes (homicides and suicides). On average, we find a 1 % increase in new gun sales increases gun homicides and gun suicides by about 0.15% using our preferred specification with county-level data. Using some back-of-the-envelope calculations based on averages, our estimates suggest the surge in gun demand that occurred after the Sandy Hook tragedy led to roughly 500 additional gun homicides, 800 additional gun suicides, and 50 additional gun accidents, with the latter being nearly identical to estimates reported in Levine and McKnight (2017). We also find that all types of homicides are significantly increasing in the motor vehicle death rate and, with the exception of non-gun homicide, significantly decreasing in the clearance rate and the arrest rate for driving under the influence (DUI). We find that gun assaults are increasing in the estimated number of guns sold and decreasing in the clearance rate while knife assaults are not.

Adding confidence to our results is the fact that police presence has the strongest impact on gun-related homicides when we interact police presence with gun presence. We find that the effect of new guns sold on homicide, gun homicide, and gun assault is increasing in the motor vehicle death rate and decreasing in the clearance rate (index crime, index property, and index violent crime) and the DUI arrest rate. The effect of gun sales on suicide (both gun suicide and suicide) is, conversely, consistent across policing levels when using *any*

measure of policing (i.e., clearance rates, motor vehicle deaths, or the DUI arrest rate). Similarly, the effect of gun sales on non-gun suicide and non-gun homicide is consistently zero across all levels of policing. Last, we find the results involving interactions usually extend to assaults with the only exception being that knife assaults are declining in gun sales when the clearance rate is high.

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 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.[1] Consistent with our main results, these seizures most often occurred during traffic stops.

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

There is a large literature on the effects of police force size and the marginal benefit of an additional police officer. In fact, Chalfin and McCrary (2018) show that, after adjusting for measurement error, U.S. cities are "substantially under-policed." Much of the research on police force size and crime outcomes makes use of the policy experiment setting that the Community Oriented Policing Services (COPS) grants have on crime reduction through the allocation of block grants for hiring and non-hiring aspects of policing. These studies find robust evidence that the marginal hiring of a police officer does reduce crime. Mello (2019) show that cities scoring just above the threshold to receive money through COPS grants have an average 3.2% increase in police force size and a 3.5% reduction in victimization cost-weighted crimes. Similarly, Weisburst (2019) show that in municipalities that received COPS hiring grants, a 10% change in police force employment is associated with a 13% reduction in violent crimes and a 7% reduction in property crimes. Evans and Owens (2007) make use of the size of a COPS grant that a locality receives and find that the additional officers hired through the grant reduce auto thefts, burglaries, robberies, and aggravated assaults. Chalfin et al. (2022) extend this work by gauging how an additional officer changes crime and victimization outcomes by race. Specifically, they find that benefits in terms of homicide reductions accrue especially to Black Americans, but that larger police force sizes lead to more arrests for low-level offenses. We could also consider the heightened presence of police a showing of police force size increase (even though it is not a literal increase in police force size). Draca et al. (2011) show that when police were strategically placed throughout London after the July 2005 terror attack, a 10% increase in police activity led to a 3-4% reduction in crime.

[1] 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).

"Effort" is more difficult to document credibly, though several studies offer well-identified evidence of how policing on the intensive margin affects crime outcomes. Chalfin and Goncalves (2023) find that near the end of officers' shifts, fewer arrests are made, but the quality of arrests increases. The authors note that this occurs because an arrest near the end of an officer's shift likely results in extra time at work, and that this trade-off may only be made for more serious crimes.[2] Officer discretion is, in fact, an important determinant in the amount and types of crimes that are pursued. Cho et al. (2022) examine how police change their arresting behavior after another officer in their department has been killed in the line of duty. These authors find a meaningful reduction for all arrest types following the death of a colleague and also find that these reductions are largely driven by changes in arrests for lower-level offenses. In essence, these authors show that the salience of a death in the line of duty adjusts the internal calculus for police officers when considering the marginal costs and marginal benefits of pursuing an arrest.

In our model, we use motor vehicle deaths per capita as our proxy for police presence and effort. We do this, in part, because there is a longer time horizon of available data, a higher number of individual units (counties), and a higher quality of death data relative to offense data. Offense data from the FBI's Uniform Crime Report (UCR) is based on records voluntarily reported by Law Enforcement Agencies (LEAs). This data is unfortunately full of errors and inconsistent reporting (c.f., Loftin and McDowall, 2010). However, our alternative – motor vehicle deaths per capita – is much more consistent. We believe this is a useful proxy for three main reasons. First, we document a consistent positive relationship between motor vehicle deaths and homicide at the national level. This relationship is observed yearly, monthly, and weekly. Second, we demonstrate a plausible mechanism for this relationship. Specifically, we show that motor vehicle deaths are decreasing in DUI arrests, which we argue is a marker of increased police presence or effort. This is similar to the Kansas City Gun Experiment. Finally, we note that our connection between motor vehicle deaths and gun deaths is probably not unique. Papers from the early nineties (e.g., Fingerhut et al., 1994; Farchi et al., 1994) juxtapose gun deaths with motor vehicle deaths, suggesting a potential connection. While a direct link between law enforcement, gun deaths, and motor vehicle deaths is not explicitly made, the fact that these deaths are so often discussed in tandem suggests researchers were at least close to making a similar connection.

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.[3]. 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 E60-E969 for years 1993-1998) are

[2]Indeed, this is the opposite finding of what some expect in the "collars for dollars" hypothesis that police might increase arrest activity near the end of a shift in order to receive extra compensation.

[3]If there is interest, we will expand our analysis to the post-pandemic years in the future.

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.[4] 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	2006.0	7.79	1993	2019
Homicide	5.94	31.9	0	1988
Suicide	11.7	31.7	0	1075
Gun Homicide	4.12	24.5	0	1577
Gun Suicide	6.20	15.2	0	565
Non-Gun Homicide	1.82	7.84	0	411
Non-Gun Suicide	5.48	17.6	0	647
Dealer & Pawn	26.6	56.6	0	3813
Businesses	2351.5	7984.4	1	285383
Population	95846.4	308987.3	55	10170292
MVD	12.8	31.0	0	1156
Black Pop.	12840.1	57990.9	0	1437837
Emp/Pop	0.27	0.12	0	2.02
MVD PC	22.4	19.7	0	877.2
Guns	7838.2	3540.6	4117	15519
Guns Sold	2422.6	4114.9	0	157105.0

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. This data is from the County Business Pattern Survey and runs from 1993-2019.[5]

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.[6] 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.

[4] 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/

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

[6] XX Put a link to the data here XX

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, Guns, 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).[7]

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.[8] 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.

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
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).

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/focused approach with the most well-known application being the estimated Household Firearm Ownership Rate (HFR) (Schell et al., 2020; Morral et al., 2024).[9] As we will discuss, there are major issues with the most recent HFR and, to a lesser extent, survey data more generally, which challenges the efficacy of this approach. Namely, they are completely unmoored from measures not based on self-reports;

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

[8] E.g., $|DUI_{t,i} - \bar{DUI}_i| < 3 * \bar{DUI}_i$

[9] We focus on this metric specifically because it is an amalgamation of several other measures (e.g., General Social Survey, the proportion of firearm suicides, Pew, etc) and is considered the frontier of gun prevalence measurement.

this includes gun manufacturing, sales, thefts, and concealed carry permits. Because of these problems and the fact that the HFR estimates are state-level, we focus our analysis on the county-level data. However, we replicate estimates using our gun sales data aggregated to the state-level, and compare these state-level estimates to analogous estimates using the HFR. These estimates and discussions are found in Appendix E.

Trends in the domestic supply of firearms as shown by the 2024 HFR (Morral et al., 2024) and the other gun-related variables are found in Figure 2. The top left-hand panel is the national population-weighted average HFR (line) along with the General Social Survey average (dots). We note that this poorly matches observed NICS checks (bottom right) as well as the production of new domestic guns (top right). Digging deeper, what is most concerning are the discontinuities in 1997 and 2002 (see Figure B1 in the Appendix) which indicate possible bad polling data and/or a problem with the algorithm that was used to create the HFR estimate (Bayesian additive regression tree).[10] We say this because these discontinuities appear in *every* state and are similar in magnitude (roughly a 4 % drop). It is difficult to say why this occurred and maybe it does represent a real drop in ownership, but during this period there was probably more than normal levels of government mistrust – particularly in the gun community.[11] Questionable reporting is not limited to surveys and is known to be unreliable when 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 and other work (Miller et al., 2022) documents misreporting of the cause of gun injuries (accident vs assault) by hospital patients.[12]

Accuracy concerns for the HFR are not limited to the late 90s and early 2000s. For example, among demographic groups with a history of generally supporting the Democratic Party, carrying a concealed handgun has become more common.[13] 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 B3 of the Appendix).[14] Further concerns related to the

[10]For example, according to the GSS, conservative gun ownership fell from 46 % in 1996 to 37.8 % in 2002. It then rose to 50.6 % in 2004 and dropped, again, to 41 % (see Figure B2 of the Appendix), all while gun manufacturing increased. This general result is also discussed in Urbatsch (2019) in much more detail.

[11]For instance, the Oklahoma City bombing occurred in 1995 and, over the course of the investigation, it was found that the building was selected by Timothy McVeigh (the perpetrator) because it housed, among other federal government offices, a Bureau of Alcohol, Tobacco, and Firearms' (ATF) office. Furthermore, the bombing itself was motivated by incidents in Waco and Ruby Ridge. It is also important to note that in 1997 the Supreme Court of the United States upheld most of the Brady Act and by 1998 NICS was online. The drop is also probably not related to the disposal of guns in response to a national tragedy (e.g., Sandy Hook) as the Columbine tragedy did not happen until 1999.

[12]More recently, Azevedo et al. (2023) find that the value of reported stolen guns fell significantly after the 2016 US presidential election and that this drop was increasing in support for the Republican presidential candidate, suggesting selective reporting of gun thefts.

[13]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/).

[14]This data was originally collected by Trent Steidley and was recently used/discussed in (Stansfield

accuracy of the HFR are also found in Figure B4, Figure B5, and Figure B6. These figures are all found in the Appendix. Here we note that the HFR has an almost inverse relationship (especially after 2000) to the value of reported stolen guns per capita (Figure B4) and the number of issued concealed carry permits (Figure B6).

The second panel in the top row of Figure 2 shows the trend of new domestic gun supply, and the bottom left panel shows pawn and gun dealer licenses. 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, duzing 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).[15] 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 in the mid-90s due to new gun control legislation, and third, the Assault Weapons Ban expired on September 13, 2004. Finally, the bottom right panel of Figure 2 shows NICS checks and shares the same basic pattern as the new domestic gun supply.

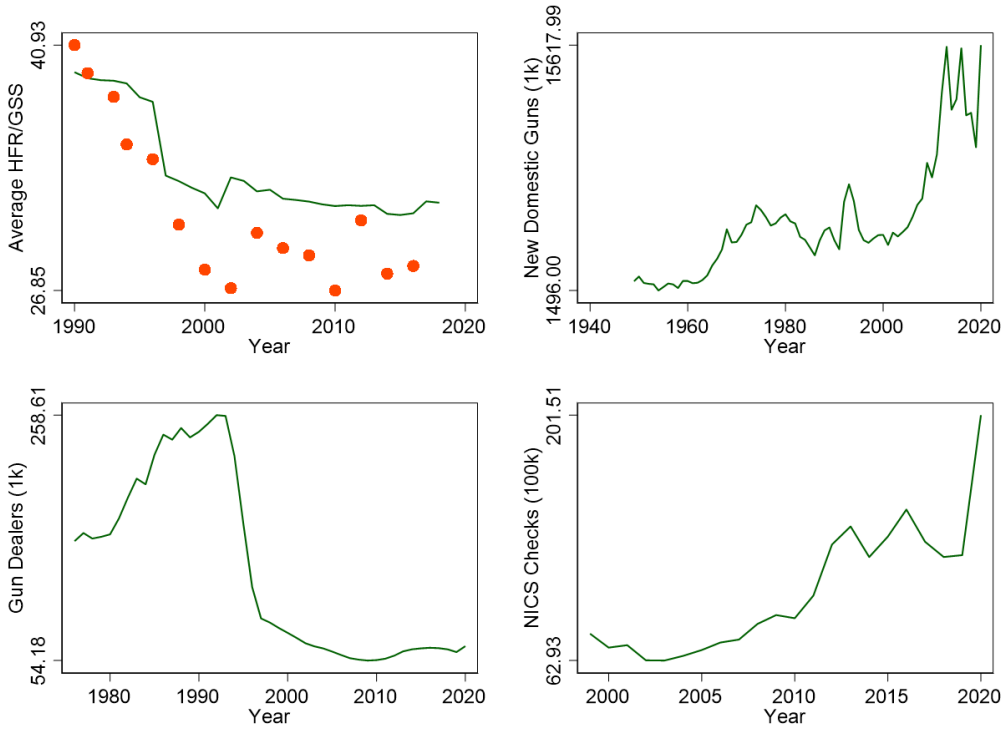
Gun dealers also have trends. Gun dealers peaked in 1992 but plummeted soon after. This decline was caused by the Brady Handgun Violence Prevention Act (effective February 1994) which increased the dealer licensing fee to 200 dollars for three years from ten dollars per year. Three years after the passage 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. Changes in the number of gun dealers also seem to have the same variation as household ownership. This is shown in Figure 2 at the national level and replicated for each state in Figure B7 (Appendix).

Ultimately, gun dealers and gun manufacturing are the variables we use to construct the new gun sales estimate. We do so by combining the local exposure to gun dealers (bottom left) and the amount of new gun manufacturing (top right) which, in the end, produce estimates that closely parallel county-level concealed carry permits, county-level gun

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.

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

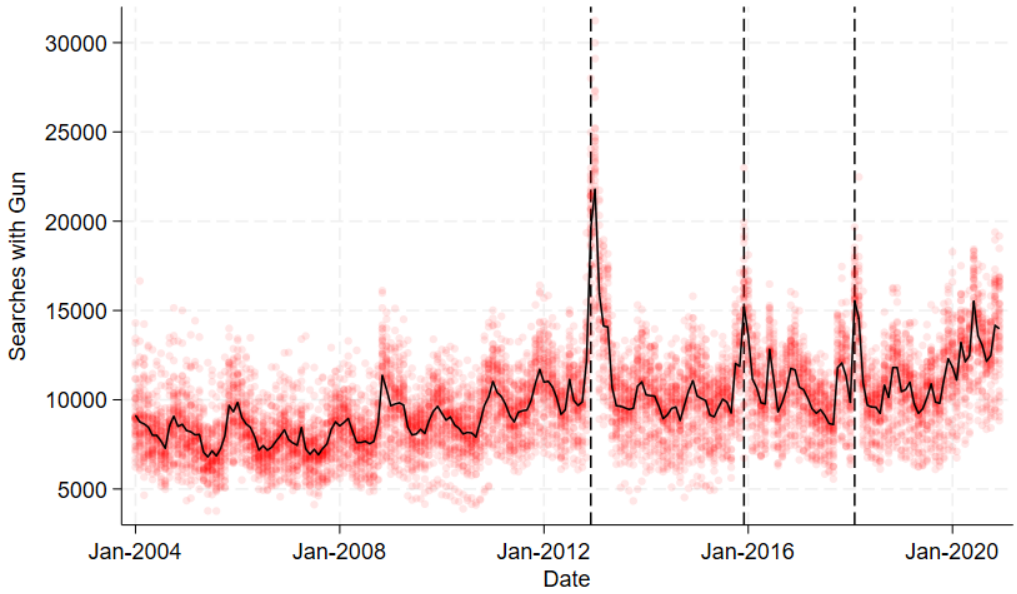
Figure 2: Gun Manufacturing and Gun-Related Proxy Variables



Notes: Top left is the average HFR from Morral et al. (2024) (line) and General Social Survey household ownership rate (dot). Both are weighted by the state population. Top right is the new domestic gun supply (1000s). The bottom left is gun dealers (1000s), and, last, the bottom right is NICS checks for firearms (100s).

sales, state-level NICS checks, state-level background checks, and gun theft. To understand why/how these estimates are accurate, in Figure 3 we present the percent (technically percent multiplied by a large constant) of monthly Google searches containing the word "gun" for each state from January 2004 to December 2020. Broadly speaking the popularity of guns, as measured by Google search interest, has increased over the last seventeen years in the panel. Additionally, the pattern of searches is very similar across all states and is largely seasonal. The large peaks are also shared and often occur at the same time as mass shootings (e.g., Sandyhook, San Bernardino, and Parkland).

Figure 3: Google Searches with the Word "Gun"



Notes: Percent of all Google searches with the word "gun." Each dot is the percent in state i, in month m, and in year t. Dashed lines indicate the Sandyhook Massacare, the San Bernardino Terrorist Attack, and the Parkland Massacare.

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 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. 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 measure is caused by institutional changes and firearm demand shocks.[16]

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

$$Gun\ Sales_{i,t} = (\frac{GM - Ex + Im}{\Sigma Gun\ D_t}) * Gun\ D_{i,t} \tag{1}$$

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) per gun dealer (Gun D) in time t (this is a national average) and the right-hand side represents the number of gun dealers in the geography.[17] These estimates are very accurate. When we aggregate our estimated new gun sales to the yearly level, we find they closely match National Instant Criminal Background Check System (NICS) checks that are explicitly for gun sales (handgun, long-gun, multiple, or other). This is presented in Figure 4. The overall correlation between the estimated new gun sales and yearly NICS checks is 0.8149.

This high correlation is not due to a single state. When we aggregate our estimates of gun sales to the state-year level, the estimates closely match NICS checks with the correlation between the two is 0.88. This is shown visually in Figure B9 (Appendix), which compares state level NICS checks to our estimated gun sales. Most of these correlations are quite high. Moreover, when they do fail, it is often in places we would expect.[18] Finally, this is not driven entirely by shared trends. The correlation between the first difference of state-level estimated new gun sales and the first difference of NICS checks explicitly for a gun is 0.57.

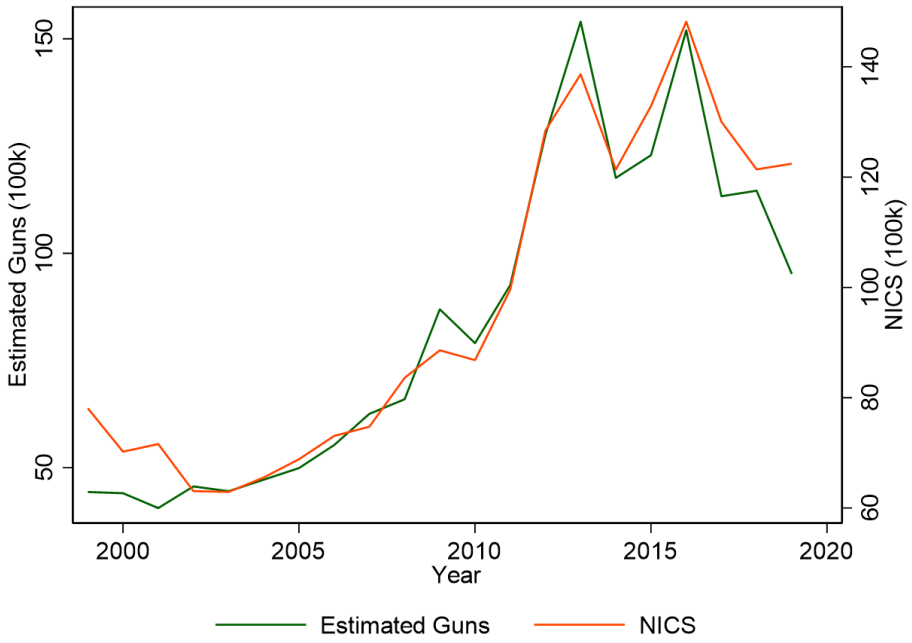
The state-level panels are illustrative of the generality of our shift-share gun measure. However, to finalize this section we next discuss the final step in creating the county-level estimated new gun sales. We adjust the new gun sales estimate to allow for seepage across county lines. The way we do this is by using the number of gun dealers in county i and the 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 overcount the number of guns, it will do so in a manner that is a level shift and will be absorbed into the fixed effect (i.e., will not affect marginal effects).

[16] To illustrate, in Figure B8 (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 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).

[17] 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.

[18] 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.

Figure 4: Estimated Gun Sales vs NICS Checks



Notes: Estimated new gun sales and NICS checks (1999-2019). 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. Green is estimated sales, orange is NICS checks.

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 $GD_{i,t}$ is gun dealer density and is a function of the number of gun dealers – Dealer or type 1 FFL ($D_{i,t}$) and Pawn or type 2 FFL ($P_{i,t}$) 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 5, we plot the mean differenced natural log of our gun sales estimates[19] against the mean differenced natural log of newly issued concealed carry permits (right) and the first difference of the natural log of our gun sales estimates[20] against the mean first difference natural log of newly issued concealed carry permits (left) for each county in the 18 US states that have data available on concealed carry permits (more than 20,000 county-year observations).[21] Visually, there is a significant positive correlation using both fixed-effects canceling approaches. The correlation between estimated gun sales and concealed carry permits using the mean differenced approach is 0.68 and the correlation using the first difference is 0.44. We consider both of these quite high because, while many people will apply for a concealed carry permit in the same year as purchasing a new firearm, some will be renewing their permit and will not purchase a new gun to go with it. Additionally, some people will buy a used gun instead of a new one. In sum, inconsistency is expected.

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/city 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.[22] 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.[23] Figure

[19] i.e.., $ln(Gun\ Sales_{i,t}) - \overline{ln(Gun\ Sales_i)}$

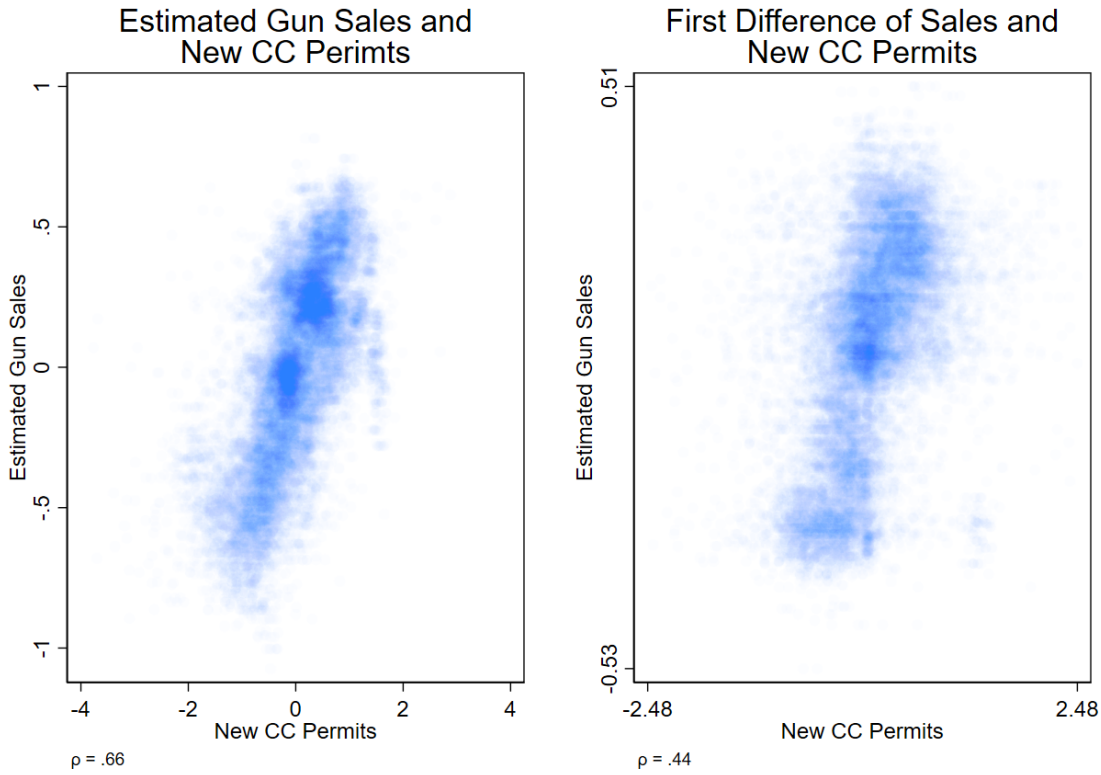
[20] i.e.., $ln(Gun\ Sales_{i,t}) - ln(Gun\ Sales_{i,t-1}$

[21] These counts are from a series of Freedom of Information Act Requests and publicly available data. Data is available upon request from Author 1.

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

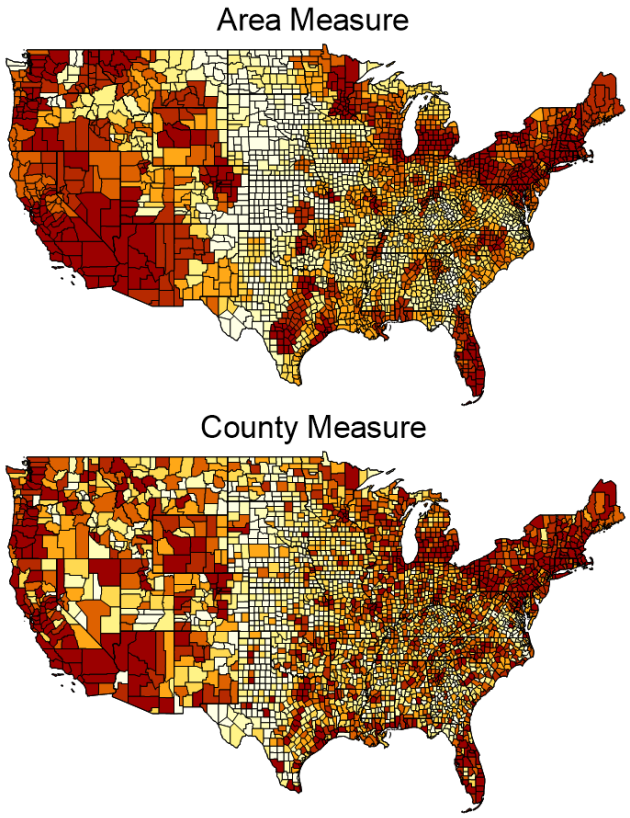
[23] See, for example, https://www.atf.gov/resource-center/firearms-trace-data-2021

Figure 5: Estimated Gun Sales vs New CC Permits



Notes: Estimated gun sales and newly issued concealed carry permits. Left panel is the mean differenced correlation of the natural log of estimated gun sales vs the mean differenced correlation of the natural log of newly issued concealed carry permits. Right panel is the first difference of the natural log of gun sales and the first difference of newly issued concealed carry permits. States included, with dates, are as follows: Colorado (2003-2019), Connecticut (1999-2019), Florida (1993-2019), Indiana (2012-2019), Iowa (1996-2019), Massachusetts (2006-2019), Minnesota (2005-2019), North Carolina (1997-2019), North Dakota (2002-2019), New Mexico (2004-2019), New York (1993-2019), Nevada (2002-2019), Ohio (2004-2019), Oklahoma (2002-2019), Oregon (2008-2019), Pennsylvania (2001-2019), Tennessee (2008-2019), and Texas (2001-2019). Pearson correlation, ρ, is found in the bottom left corner.

Figure 6: 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.

6 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.

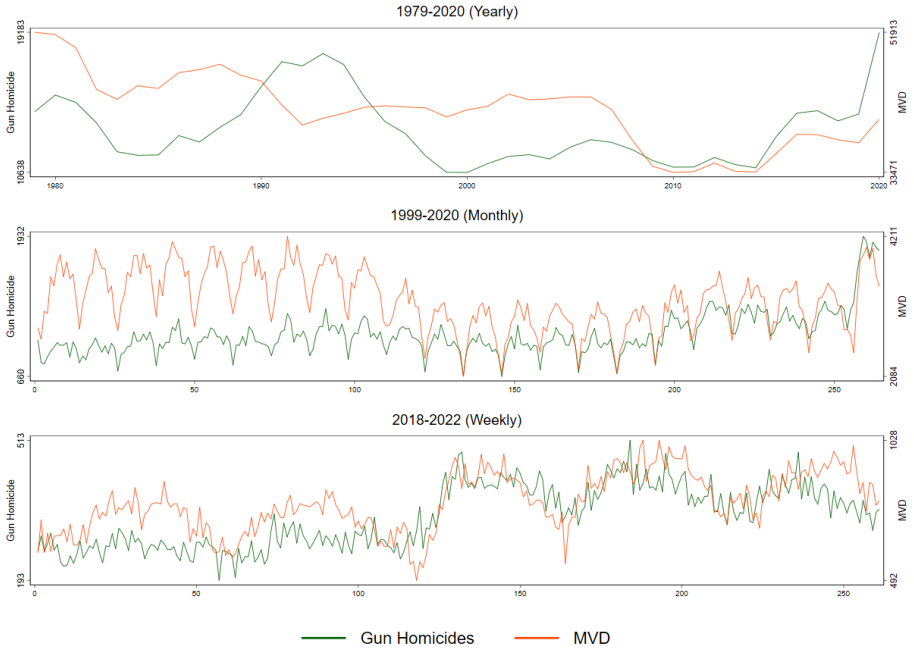
3.2.2 Motor Vehicle Deaths as a Measure of Active Policing

We now move to the measure of police activity: motor vehicle deaths per capita. Motor vehicle deaths have generally trended down since 1968, due to increased vehicle safety, improved medical care, and ride-sharing. That said, these deaths are also largely determined by the general safety of roads, which can depend on the presence of police. 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 seat-belts), which reduced

traffic fatalities (c.f., Zador and Ciccone, 1993; Anderson et al., 2024) in a complementary way independent of law enforcement.[24]

While both figures are indicative of an underlying connection, we test the relationship formally 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 C2). However, when examining a narrower measure (monthly, Figure C3 or weekly, Figure C4), 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.

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.

We also provide evidence of the shared relationship between DUI arrests per capita (which are a function of the number of elective stops) and the motor vehicle death rate in a first-stage type regression in Table 3. Here, we present the coefficient estimate of the natural log of DUI arrests on the motor vehicle death rate (population is the exposure variable)

[24] We also note that the relationship between MVD and homicide goes back even further. In Figure C1 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.

using a fixed effects Poisson and county-clustered standard errors.[25] 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 %. Ultimately, we choose to focus on motor vehicle deaths because the county-level DUI arrest 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.

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	0.0230***	-0.0171***	-0.00941**	-0.00828*	-0.0166***	-0.0188***
	(5.22)	(-5.06)	(-2.82)	(-2.41)	(-4.73)	(-4.78)
Pop	-0.828***	-0.364***	-0.359***	-0.396***	-0.388***	-0.0755
	(-28.98)	(-12.16)	(-12.09)	(-14.91)	(-13.73)	(-0.85)
% Black	-0.120***	-0.0234***	-0.0255***	0.000299	0.00310	0.0382**
	(-18.28)	(-3.66)	(-3.86)	(0.04)	(0.42)	(2.70)
Year		-0.0132***				
		(-25.56)				
Constant	0.833*	22.07***	-4.429***	-3.916***	-3.960***	-7.659***
	(2.31)	(25.33)	(-12.33)	(-12.09)	(-11.59)	(-7.06)
OBS	67571	67571	67571	67564	67571	67571
Counties	2815	2815	2815	2814	2815	2815
R2	0.815	0.817	0.820	0.822	0.818	0.820

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

4 Results

We break 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) or alternative channel (e.g., decrease in policing increases the supply of crime guns).[26]

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

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

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

main variable of interest is the natural log of the gun prevalence measures: estimated gun sales (Equation 2) 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 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).[27]

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. We also include the county population as an exposure variable so that the outcome is a rate.

The simplified estimating equation is shown below.

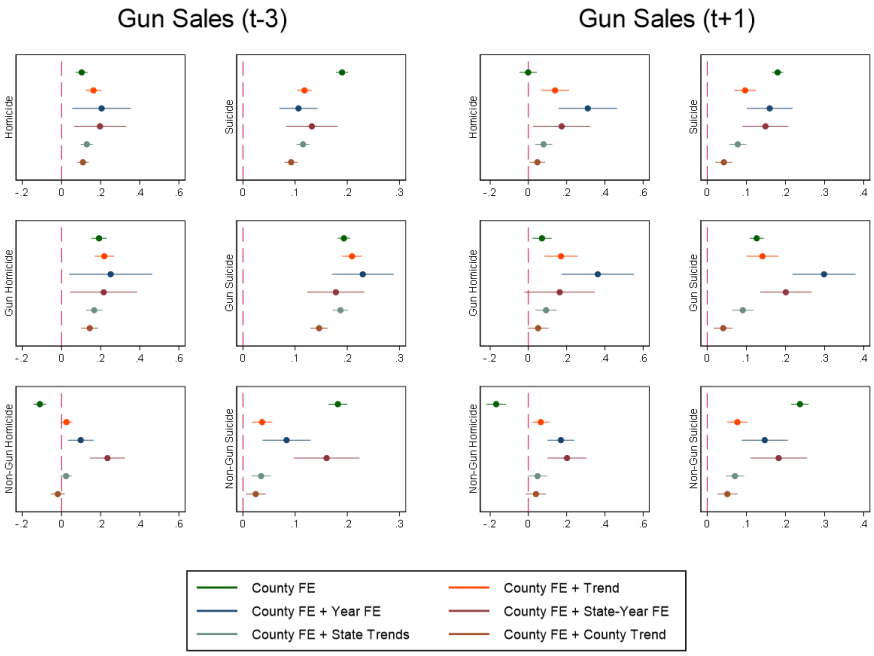
$$Y_{i,t} = exp(\beta_0 + \Gamma GunMeasure_{i,t-3} + X_{i,t}\beta + \psi_i + \psi_i \cdot t + \varepsilon_{i,t}) \tag{3}$$

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 these results graphically in Figure 8. The left panel uses lagged gun 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 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. Leads of gun sales do not affect contemporaneous deaths (c.f., much smaller t-statistics) or have a relatively smaller effect relative to the

[27] According to the ATF, in 2019, 42 % of recovered were reported to be less than three years old (https://www.atf.gov/resource-center/firearms-trace-data-2019).

Figure 8: Estimated Relationship Between Gun Measures and Homicide (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. 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.

lagged counterpart but generally only when using time trends.[28]

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
States	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 panel label. County clustered standard errors are in parentheses; * p<0.05, ** p<0.01, *** p<0.001.

Full results using the preferred specification are in the top pane of 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 one % 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.[29] 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.

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) and the DUI arrest rate. 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) and the natural log of the DUI arrest rate

[28] This is also observed in Tables D5 and D6 of the Appendix.

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

(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 usually considered less serious than violent crimes so investigators have a little more discretion regarding how much effort to invest 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 knife assaults.

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

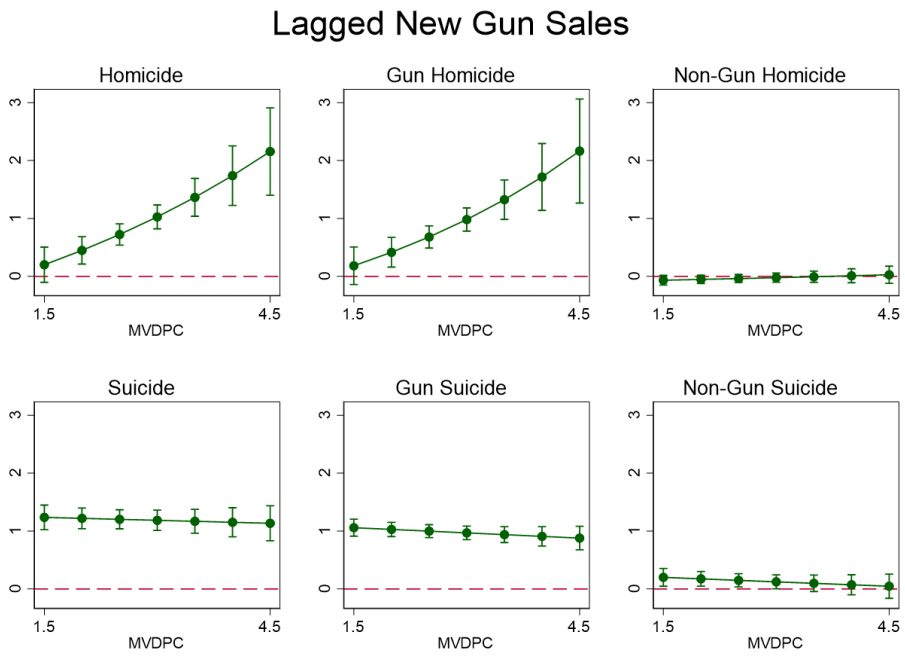
Notes: Estimated effects using a fixed effects Poisson estimator. All models include county fixed effects and county-specific time trends. The outcome is indicated by the panel label. County clustered standard errors are in parentheses; * p<0.05, ** p<0.01, *** p<0.001.

In both 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 one % 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 one percent 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 D7 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.

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 be increasing in the motor vehicle death rate. To test this, we run a specification similar to what is presented in Equation 3 but include the interaction of gun sales and the motor vehicle death rate. Point estimates of interest are in Table D8 of the Appendix. In all models involving homicide or gun homicide, the interaction is positive and statistically significant, suggesting active policing will reduce homicide and, in particular, gun homicide.

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.

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 constant for suicide, gun suicide, and suicides that do not involve a gun. Note that this virtually rules out medical care-related explanations. If this relationship is due to region-specific improvements (or probably dis-improvements) in medical care we would expect to observe a similar relationship involving suicide and non-gun related deaths.

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 that we now proxy using the index property crime clearance rate and the DUI arrest rate. The general idea here is that the effect of new guns should be decreasing in the property crime clearance rate and the DUI arrest rate. Additionally, the effect of new guns on suicide should not be affected by the property crime clearance rate or the DUI arrest rate. Results are found in Figure 10 and with point estimates found in Table D9 and D10 of the Appendix. As before, all models are fixed effects Poisson with county fixed effects and county specific linear time trends.

We find that the effect of new guns on homicide and gun homicide is decreasing in the clearance rate and the clearance rate does not affect the relationship between new guns and non-gun homicide, suicide, gun-suicide, and non-gun suicide. DUI arrests have a similar mitigating effect on the impact of firearms on fatal outcomes, with increases in the DUI arrest rate decreasing the effect of firearms on homicide and gun homicide while not changing the effect of firearms on suicides – all types – and non-gun homicides. Similar results are found in Tables D11 and D12 when using the index crime clearance rate and violent crime clearance rate.

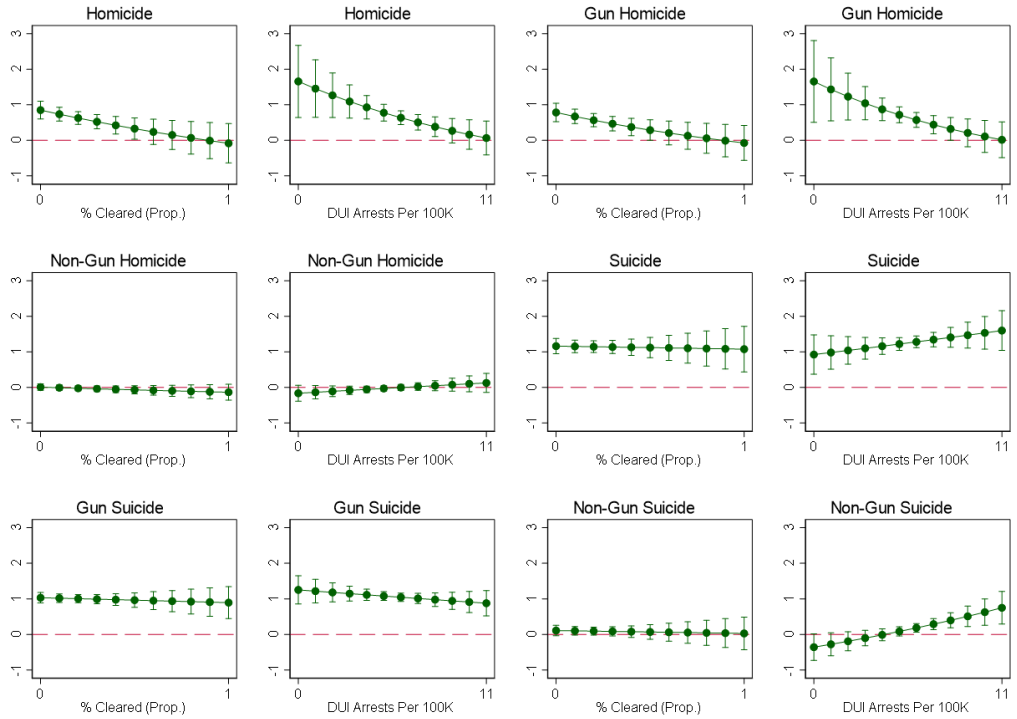
As a final robustness check, we also explore the interaction between gun sales and law enforcement on knife and gun assaults. These estimates are presented graphically in Figure 11 and point estimates are found in Table D13 in the Appendix. Results are similar to those discussed above. The effect of 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 there certainly is a less-than-ideal level of data, in reality, there is more data available than public perception would lead one to believe. In this work, we seek to expand public understanding of how changes in firearm prevalence relate to fatal outcomes involving firearms, such as homicides and suicides, by utilizing a novel measure of gun prevalence based on federally licensed firearm dealer counts and gun manufacturing. Additionally, we are able to trace these relationships over a longer time period and at finer geographic levels than prior work on the subject.

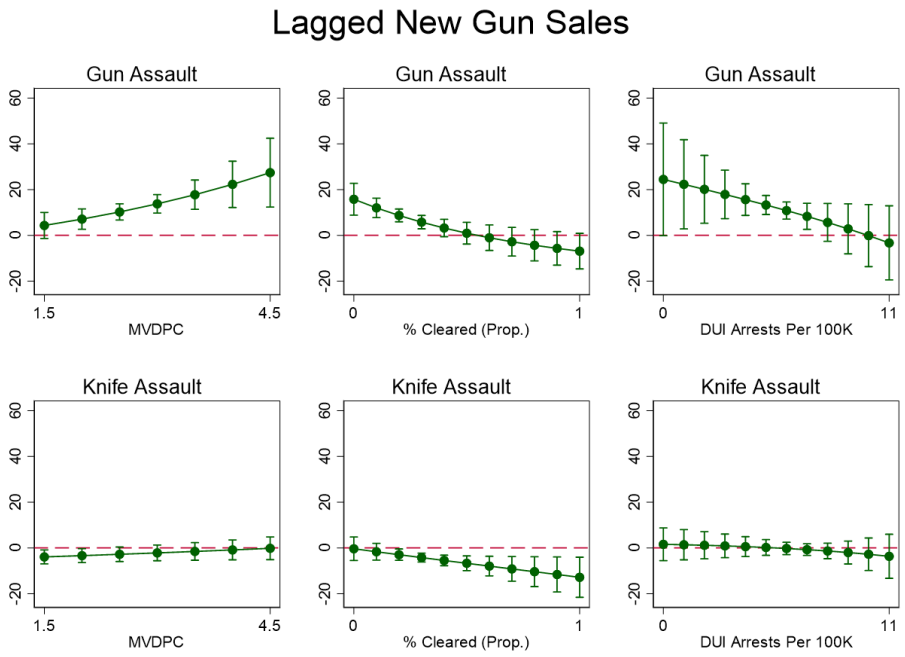
Our analysis finds that increases in the gun supply are associated with increases in both homicides and suicides committed with guns in subsequent years. The impacts are both statistically significant and substantive in magnitude. Notably, our findings also demonstrate that higher police presence and enforcement efforts, as proxied by vehicular fatality rates and clearance rates, can meaningfully mitigate some of the elevated homicide risks associated with increased firearm access. This underscores that a naive policy prescription attributing fatal gun outcomes solely to the presence of firearms is insufficient. By documenting the net effect of firearms and police force presence, we arrive at the conclusion that while additional firearms are resulting in more fatal outcomes, enforcement of existing laws

Figure 10: Estimated Relationship Between Gun Sales and Deaths (Clearance Rate & DUI Arrests)



Notes: Estimated effect of guns by clearance rate and DUI arrest 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.

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.

and gun regulations 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 position 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 ignores the effect on suicide, which is independent of police presence, statistically significant, and positive. Therefore, even if there was a deterrent effect, our results indicate these benefits would be offset by the 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 will have larger negative effects on homicide in urban areas. While both of these results are politically contentious, such efforts to constrict the hazardous spillovers from legal gun markets may offer a path forward in communities plagued by gun violence while also 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) suggests 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 could do just that.

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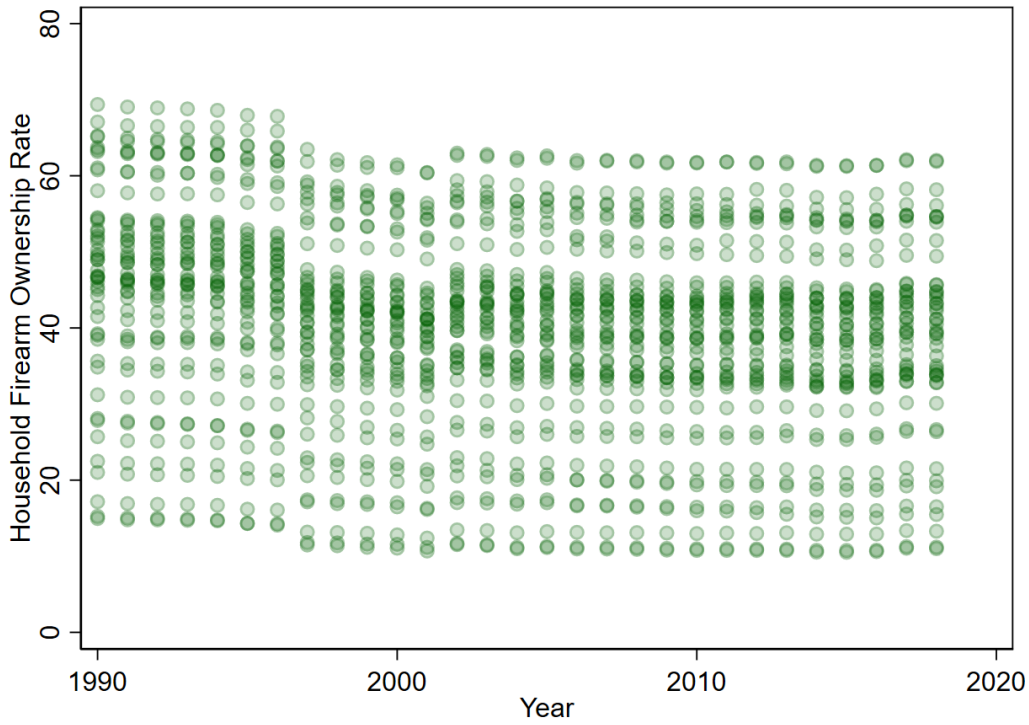
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A Main Appendix: State-Level Analysis

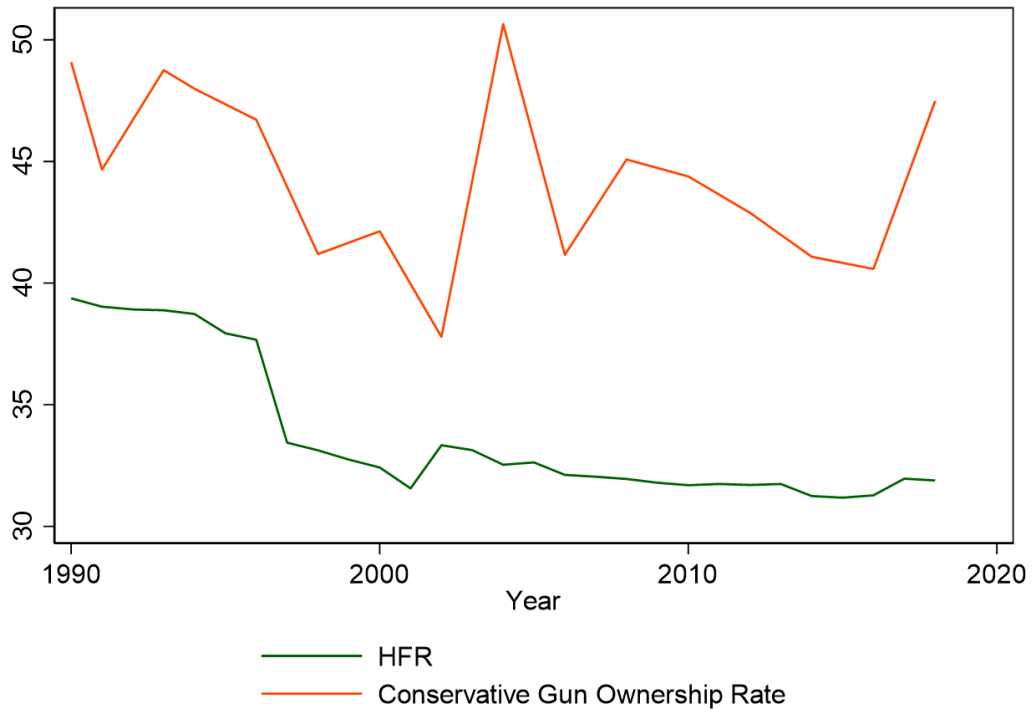
B Ownership and Trends

Figure B1: Household Firearm Ownership for the Contiguous US States



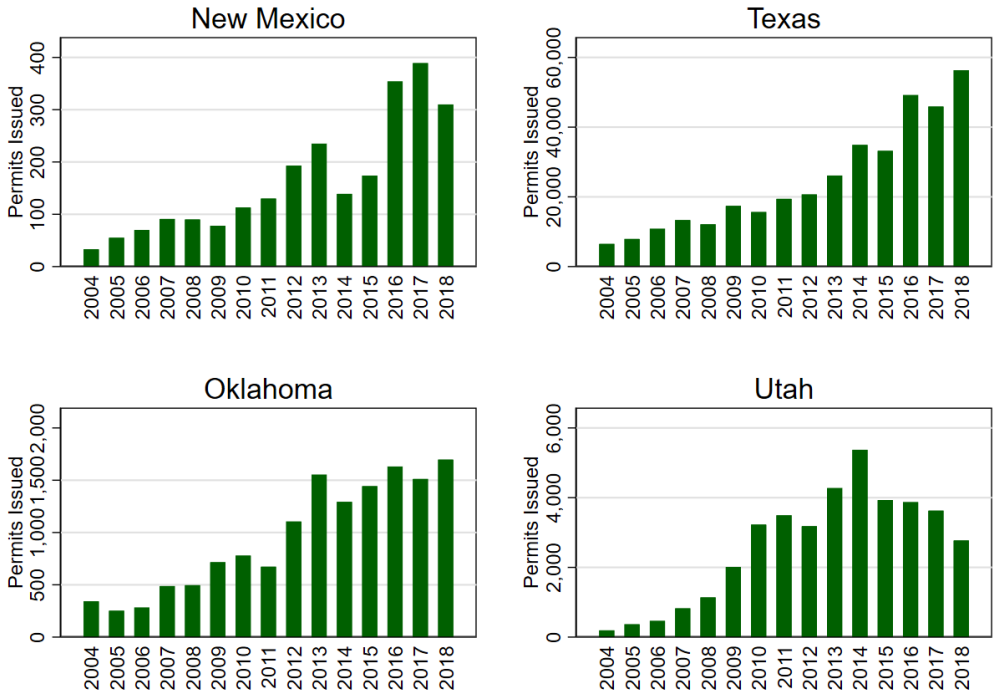
Notes: Estimated HFR for each of the US contiguous states from 1990-2018.

Figure B2: Conservative Gun Ownership vs HFR



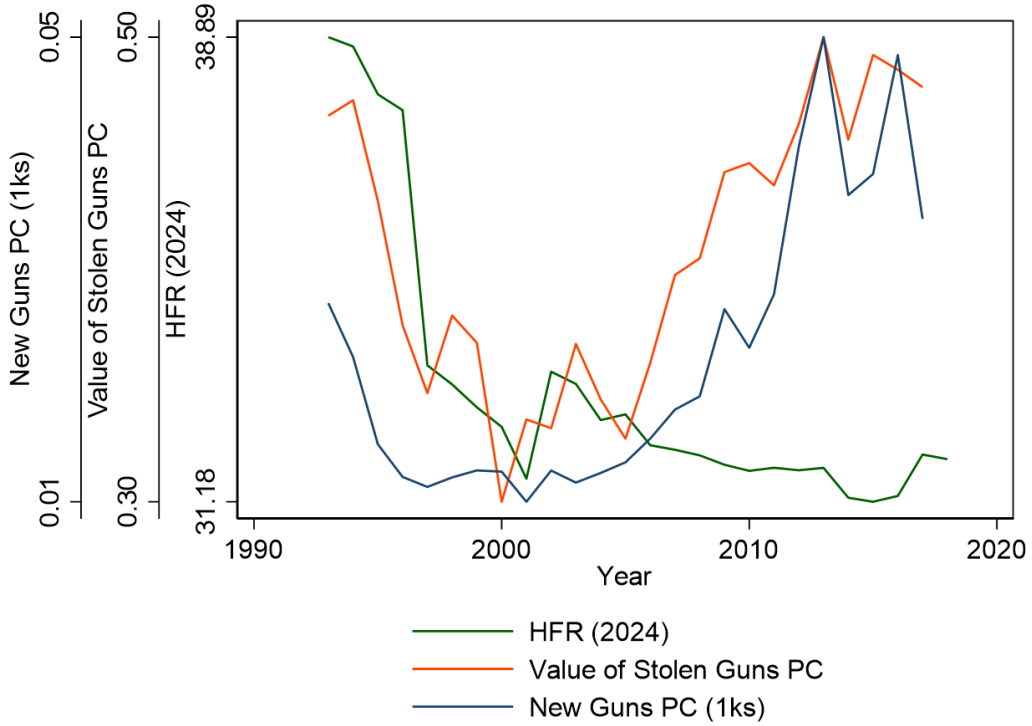
Notes: Conservative gun ownership and estimated gun ownership by year. Data is from the GSS and the HFR published by the Rand corporation.

Figure B3: Number of Concealed Carry Permits Issued to Black/African Americans (2004-2018)



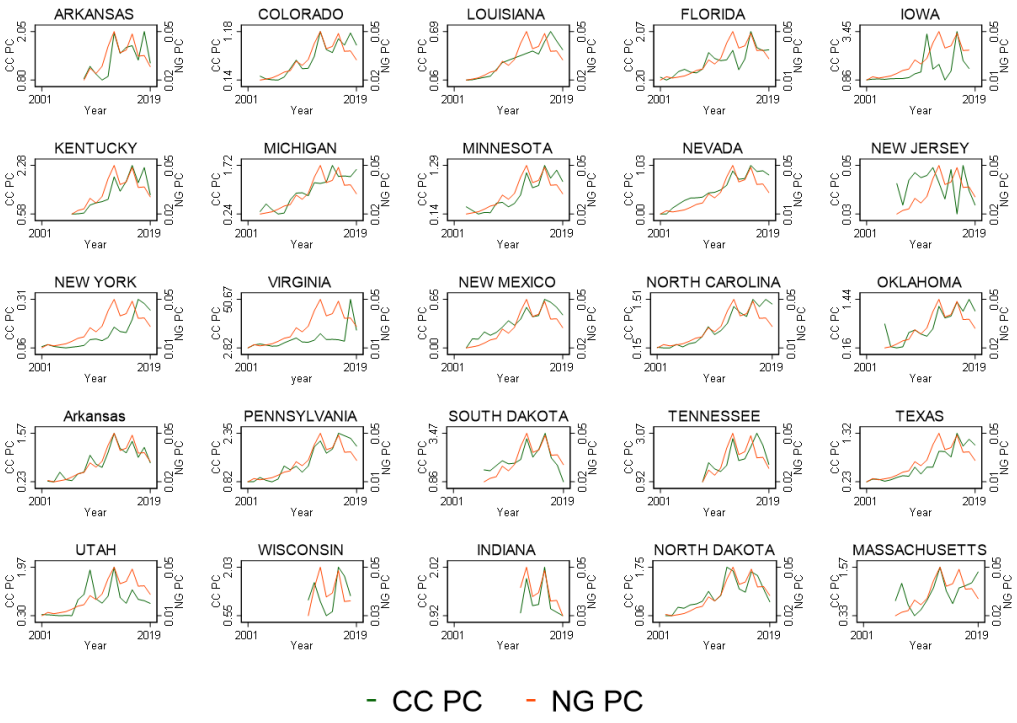
Notes: Concealed carry permits/handgun permits issued to Black residents of Texas, Utah, New Mexico, and Oklahoma.

Figure B4: Household Firearm Ownership, Value of Reported Stolen Guns, and New Domestic Gun Supply (1993-2018)



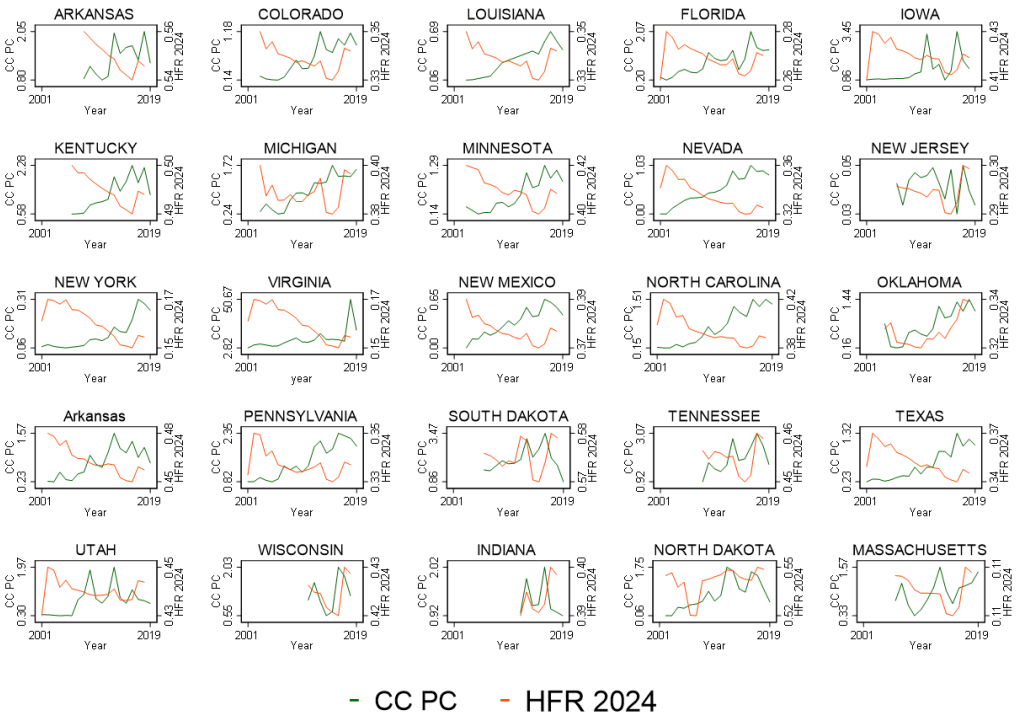
Notes: Household Firearm Ownership Rate (Morral et al., 2024), Value of Reported Stolen Guns Per Capita, and New Guns per 1,000 people.

Figure B5: New Domestic Gun Supply and Concealed Carry Permits



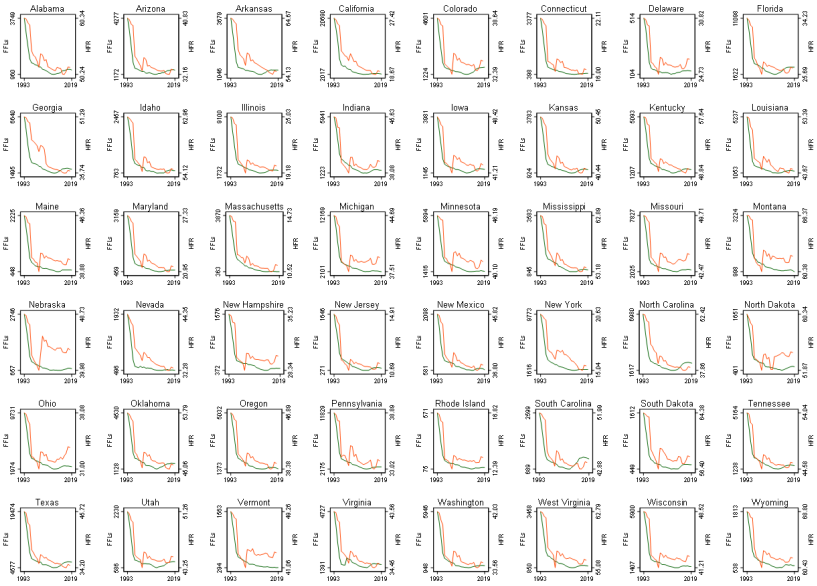
Notes: New domestic gun supply (manufacturing minus exports plus imports) per capita and issued concealed carry permits per capita. Data orginally collected by Trent Steidley.

Figure B6: Household Firearm Ownership and Concealed Carry Permits



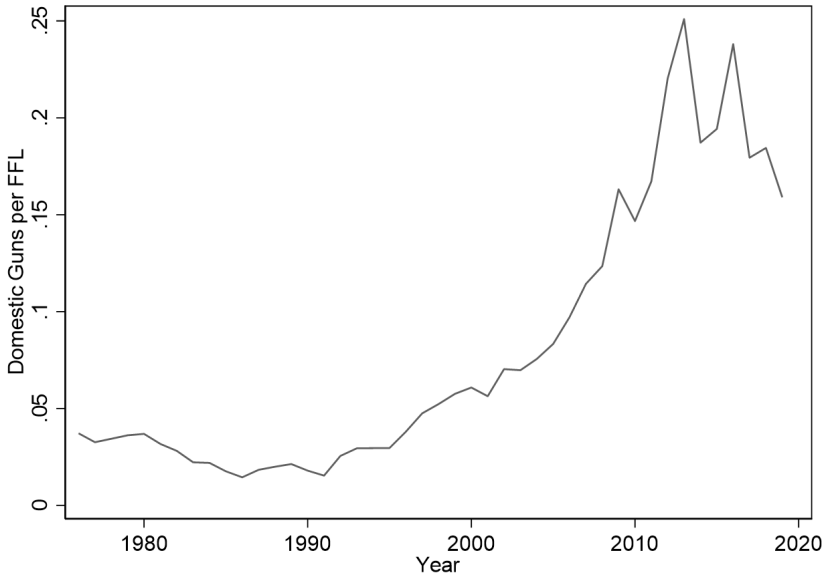
Notes: Household Firearm Ownership Rate (Morral et al., 2024) and issued concealed carry permits per capita. Data was collected originally by Trent Steidley.

Figure B7: Gun Dealers and Household Firearm Ownership



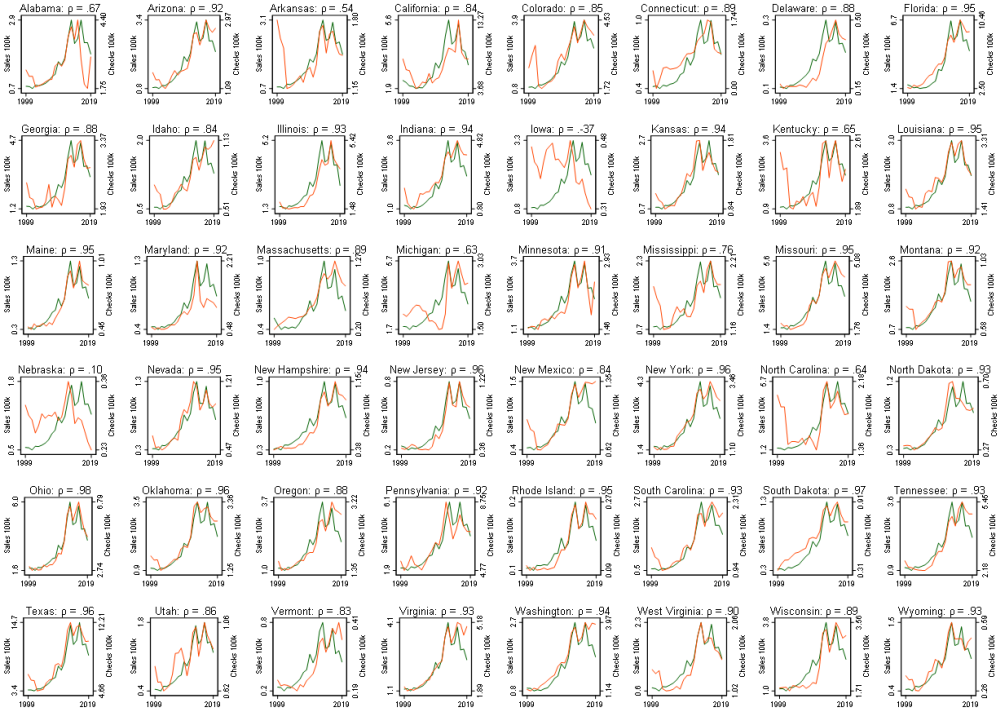
Notes: Trends in Household Firearm Ownership and Gun Dealers. Green line indicates the total number of gun dealers in the state. Orange is the household firearm ownership rate.

Figure B8: 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.

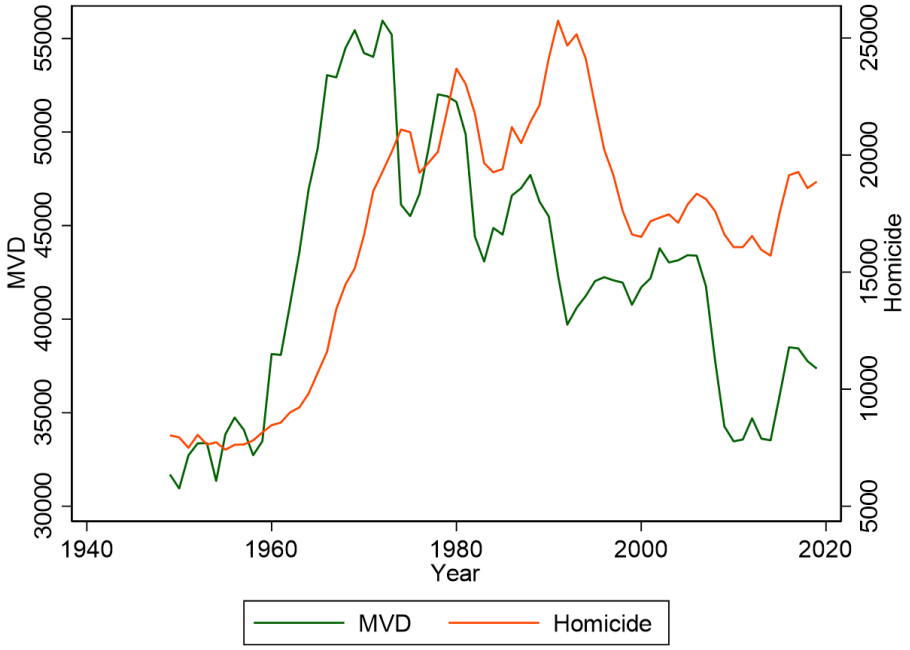
Figure B9: Estimated Gun Sales vs NICS Checks



Notes: Change in estimated gun sales (Green Line) vs NICS Checks (Orange Line). Gun sales data is from Johnson et al. (2024).

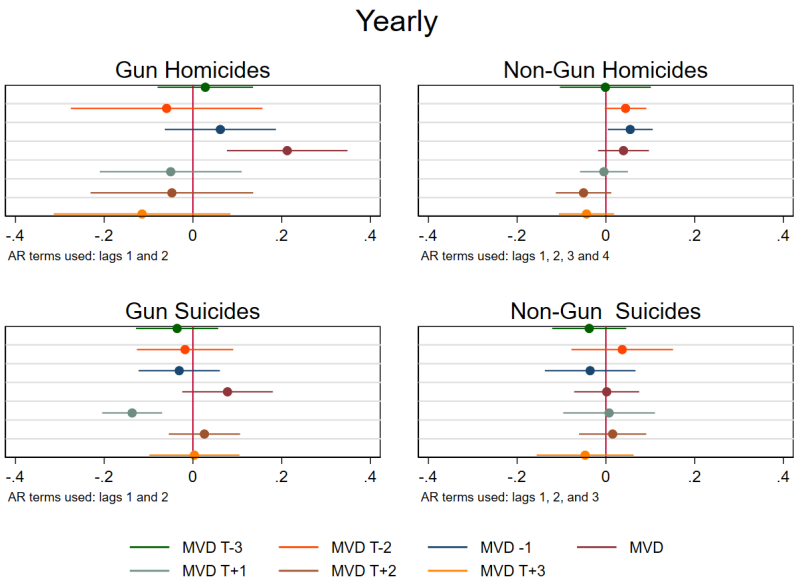
C MVD as Law Enforcement

Figure C1: Motor Vehicle Deaths and Homicide through the Years



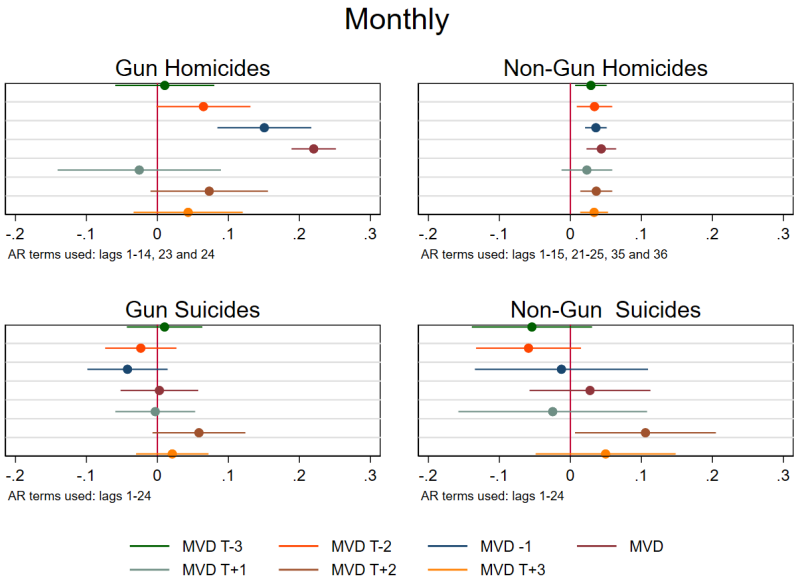
Notes: Motor vehicle deaths and homicide. Years: 1948-2019.

Figure C2: Estimated Relationship between Yearly Motor Vehicle Deaths and Yearly Deaths



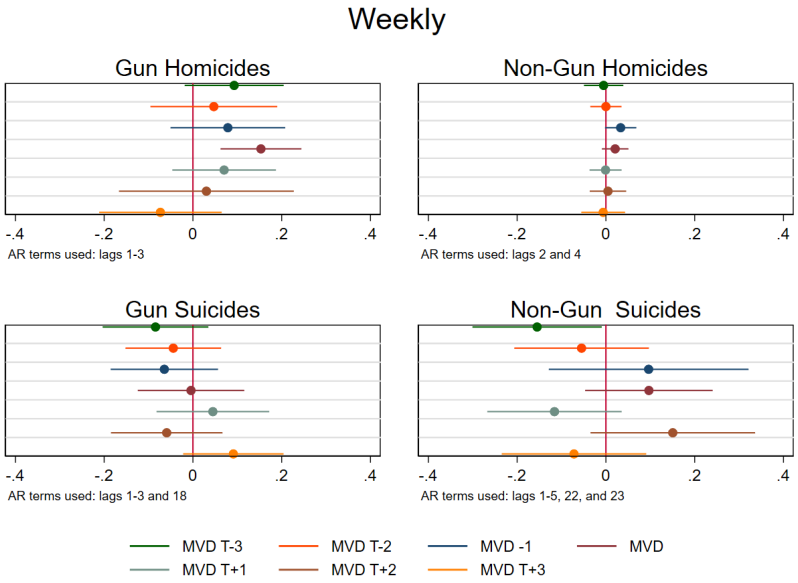
Notes: Estimated relationship between various lags of motor vehicle deaths and deaths. All models are Autoregressive models with the auto-regressive term selected based on the correlograms. Chosen lags are indicated in the subplot notes. Years: Years: 1979-2019

Figure C3: 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 C4: 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

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. 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. 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. 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. County clustered standard errors are in parentheses; * p<0.05, ** p<0.01, *** p<0.001.

Table D5: Estimated Relationship Between Gun Sales Leads 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
F.Local Guns Sales	-0.00119	0.140***	0.311***	0.174*	0.0797***	0.0470*
	(-0.05)	(3.78)	(4.02)	(2.29)	(3.56)	(2.34)
MVD PC	0.254***	0.213***	0.135***	0.0837***	0.162***	0.0981***
	(5.78)	(7.19)	(6.46)	(7.69)	(7.72)	(5.97)
OBS	70565	70565	70565	70565	70565	70565
Counties	2896	2896	2896	2896	2896	2896
R2	0.884	0.884	0.887	0.890	0.886	0.891
	Gun Homicide					
	County FE	County FE + Trend	County FE + Year FE	County FE + State - Year FE	County FE + State Trends	County FE + County Trend
F.Local Guns Sales	0.0708**	0.171***	0.363***	0.164	0.0926***	0.0507
	(2.77)	(3.88)	(3.78)	(1.76)	(3.30)	(1.85)
MVD PC	0.298***	0.267***	0.156***	0.0977***	0.203***	0.121***
	(5.58)	(6.87)	(5.53)	(6.96)	(7.60)	(5.78)
OBS	68835	68835	68835	68813	68835	68835
Counties	2789	2789	2789	2789	2789	2789
R2	0.871	0.871	0.874	0.879	0.873	0.880
	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
F.Local Guns Sales	-0.168***	0.0645**	0.170***	0.202***	0.0480	0.0396
	(-6.59)	(2.75)	(4.80)	(3.89)	(1.92)	(1.48)
MVD PC	0.167***	0.110***	0.0902***	0.0544***	0.0849***	0.0514***
	(5.32)	(5.52)	(5.07)	(4.09)	(5.60)	(3.59)
OBS	68491	68491	68491	68469	68491	68491
Counties	2774	2774	2774	2774	2774	2774
R2	0.748	0.750	0.750	0.754	0.751	0.758

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. County clustered standard errors are in parentheses; * p<0.05, ** p<0.01, *** p<0.001.

Table D6: Estimated Relationship Between Gun Sales Leads 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
F.Local Guns Sales	0.180***	0.0970***	0.160***	0.149***	0.0781***	0.0425***
	(24.98)	(6.97)	(5.40)	(5.05)	(7.15)	(3.84)
MVD PC	0.0133*	0.0235***	0.0128**	0.0113*	0.0218***	0.0146**
	(2.26)	(3.85)	(2.66)	(2.54)	(4.03)	(3.10)
OBS	72116	72116	72116	72116	72116	72116
Counties	3029	3029	3029	3029	3029	3029
R2	0.859	0.859	0.861	0.862	0.860	0.862
	Gun Suicide					
	County FE	County FE + Trend	County FE + Year FE	County FE + State - Year FE	County FE + State Trends	County FE + County Trend
F.Local Guns Sales	0.127***	0.141***	0.299***	0.201***	0.0908***	0.0409***
	(14.05)	(6.86)	(7.29)	(6.01)	(6.75)	(3.35)
MVD PC	0.0363***	0.0351***	0.0210***	0.0189***	0.0273***	0.0189***
	(4.53)	(4.89)	(3.76)	(3.55)	(4.52)	(3.39)
OBS	71999	71999	71999	71999	71999	71999
Counties	3014	3014	3014	3014	3014	3014
R2	0.756	0.756	0.759	0.761	0.757	0.762
	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
F.Local Guns Sales	0.237***	0.0769***	0.147***	0.183***	0.0708***	0.0513***
	(21.29)	(6.01)	(4.92)	(4.96)	(6.07)	(4.02)
MVD PC	-0.00597	0.0228*	0.0130	0.00459	0.0176*	0.00736
	(-0.75)	(2.57)	(1.63)	(0.63)	(2.23)	(1.05)
OBS	71601	71601	71601	71601	71601	71601
Counties	2971	2971	2971	2971	2971	2971
R2	0.827	0.828	0.829	0.830	0.829	0.832

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

Table D7: 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. County clustered standard errors are in parentheses; * p<0.05, ** p<0.01, *** p<0.001.

Table D8: 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. County clustered standard errors are in parentheses; * p<0.05, ** p<0.01, *** p<0.001.

Table D9: Estimated Relationship with Preferred Specification (Property Crime Clearance Rate)

	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)
% Clear (Property)	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 × % Clear (Property)	-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

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. County clustered standard errors are in parentheses; * p<0.05, ** p<0.01, *** p<0.001. Clearance data is from LEAs that reported more than 90 percent of the time in the years 1993-2019.

Table D10: Estimated Relationship with Preferred Specification (DUI Arrest Rate)

	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 per 100K	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 per 100K	-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

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. County clustered standard errors are in parentheses; * p<0.05, ** p<0.01, *** p<0.001. Clearance data is from LEAs that reported more than 90 percent of the time in the years 1993-2019.

Table D11: Estimated Relationship with Preferred Specification (Clearance Rate)

	Homicides	Gun Homicides	Non-Gun Homicides	Suicides	Gun Suicides	Non-Gun Suicides
L3.Local Guns Sales	0.159***	0.209***	0.00261	0.0914***	0.151***	0.0176
	(7.61)	(7.59)	(0.11)	(10.31)	(13.32)	(1.38)
% Cleared	2.126**	2.994**	0.548	0.0270	0.0701	0.0857
	(3.22)	(3.19)	(0.72)	(0.10)	(0.21)	(0.21)
L3.Local Guns Sales × % Cleared	-0.238***	-0.337***	-0.0604	0.00232	-0.00251	-0.00614
	(-3.35)	(-3.35)	(-0.76)	(0.09)	(-0.07)	(-0.14)
OBS	62559	60787	60180	64534	64405	63927
Counties	2751	2661	2634	2880	2868	2827
R2	0.880	0.869	0.733	0.866	0.771	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. County clustered standard errors are in parentheses; * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table D12: Estimated Relationship with Preferred Specification (Violent Crime Clearance Rate)

	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)
% Cleared (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 × % Cleared (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. County clustered standard errors are in parentheses; * p<0.05, ** p<0.01, *** p<0.001.

Table D13: Interaction Effects (Local Gun Sales and Various Assaults)

	Gun Assaults			Knife Assaults		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
L3.Local Guns Sales	-0.0487	0.242***	0.480***	-0.119**	-0.00733	0.0539
	(-0.50)	(5.08)	(3.58)	(-2.89)	(-0.13)	(0.45)
MVD PC	-0.738*			-0.219		
	(-2.40)			(-1.72)		
L3.Local Guns Sales × MVD PC	0.0844**			0.0257		
	(2.59)			(1.92)		
% Clear (Property)		3.851*			2.712	
		(2.41)			(1.72)	
L3.Local Guns Sales × % Clear (Property)		-0.443**			-0.274	
		(-2.61)			(-1.61)	
DUI Arrests per 100K			0.519*			0.175
			(2.31)			(1.07)
L3.Local Guns Sales × DUI Arrests per 100K			-0.0482*			-0.00981
			(-2.05)			(-0.57)
OBS	59653	63677	59462	59742	63878	59707
Counties	2789	2822	2735	2797	2828	2747
R2	0.965	0.966	0.966	0.960	0.961	0.961

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. County clustered standard errors are in parentheses; * p<0.05, ** p<0.01, *** p<0.001.

E Online Appendix: State-Level Analysis

Here we discuss the state-level analysis. The benefit here is that we can compare our county level estimates to state-level counterparts and other measures that are only available at the state level. Additionally, county-level data aggregated to the state level opens the door to more conventional linear approaches via a log-log models.

We begin by estimating the relationship between the natural log of per capita homicides, gun homicides, non-gun homicides, suicides, gun suicides, and non-gun suicides for each state i in year t using a set of linear fixed effects regressions. Our main variable of interest is the natural log of the gun prevalence measures: estimated gun sales (Equation 1) and household firearm ownership (HFR). Both of these measures are lagged 3 years.

We consider five general specifications (fifth not shown). The first three equations below are standard panel methodologies with differing trend and fixed effect modeling assumptions, the fourth equation is a first-difference dynamic panel approach:

$$Y_{i,t} = \beta_0 + \beta_1 X_{i,t} + \psi + e \tag{1}$$

$$Y_{i,t} = \beta_0 + \beta_1 X_{i,t} + \psi + \pi + e \tag{2}$$

$$Y_{i,t} = \beta_0 + \beta_1 X_{i,t} + \psi + \psi * year + e \tag{3}$$

$$\Delta Y_{i,t} = \beta_0 + \beta_1 \Delta X_{i,t} + \beta_2 \Delta Y_{i,t-1} + e \tag{4}$$

The first is the standard fixed effects model (the fixed effect is the state or ψ), the second includes both state, ψ, and year fixed effects, π, and the third includes state fixed effects and state-specific trends, $\psi * year$. The fourth and fifth (not shown) models are dynamic panel estimators. The first dynamic panel estimator is a standard first difference model, and the second uses the Anderson-Hsiao approach which instruments the lagged difference with the second lagged difference. All models include the same control variables as those discussed in the main text. Linear models are selected with the histograms of the state-level data in mind. In particular, these data are more well-behaved (i.e., normally distributed) than the county-level data which has an over-dispersion quality to it.

We include the following control variables: the state population, the employment-to-population ratio, the percentage of the population that identifies as Black or African American, and the number of motor vehicle deaths per capita. We use the natural log of each of these variables so that the coefficients can be interpreted as an elasticity. Standard errors are clustered at the state. Coefficient estimates for estimated gun sales and motor vehicle deaths for all specifications are found in Tables A1 (homicides) and A2 (suicides).

Table A1: Estimated Relationship Between Gun Measures and Homicide (State Level Panel)

	Homicides									
	Gun Sales					HFR				
	State FE+ Trend	State FE + Year FE	State FE + State - Trend	First Difference	Anderson Hsiao	State FE+ Trend	State FE + Year FE	State FE + State - Trend	First Difference	Anderson Hsiao
Gun Measure	0.228***	0.194*	0.238***			1.238***	0.906*	1.105***		
	(9.31)	(2.67)	(8.45)			(7.04)	(2.35)	(5.14)		
MVD PC	0.326**	0.269**	0.171**			0.438***	0.292**	0.286***		
	(3.33)	(2.71)	(2.75)			(4.67)	(3.03)	(3.76)		
D.Gun Measure				0.160***	0.182***				0.665***	0.648***
				(5.05)	(4.92)				(4.12)	(4.27)
D.MVD PC				0.0870	0.119				0.118*	0.0974
				(1.68)	(1.88)				(2.39)	(1.72)
OBS	1200	1200	1200	1152	1152	1200	1200	1200	1200	1152
States	48	48	48	48	48	48	48	48	48	48
R2	0.921	0.926	0.934	0.189	-0.125	0.919	0.926	0.931	0.215	0.124
	Gun Homicides									
	Gun Sales					HFR				
	State FE+ Trend	State FE + Year FE	State FE + State - Trend	First Difference	Anderson Hsiao	State FE+ Trend	State FE + Year FE	State FE + State - Trend	First Difference	Anderson Hsiao
Gun Measure	0.323***	0.179	0.332***			1.651***	1.029	1.395***		
	(8.47)	(1.64)	(9.21)			(6.25)	(1.72)	(4.88)		
MVD PC	0.386**	0.271	0.202			0.542***	0.293	0.359**		
	(2.78)	(1.68)	(1.92)			(4.18)	(1.85)	(3.07)		
D.Gun Measure				0.278***	0.321***				0.894**	0.932**
				(7.46)	(6.39)				(2.75)	(2.95)
D.MVD PC				0.0748	0.245				0.108	0.0196
				(0.55)	(1.64)				(0.86)	(0.14)
OBS	1199	1199	1199	1149	1149	1199	1199	1199	1197	1148
States	48	48	48	48	48	48	48	48	48	48
R2	0.899	0.906	0.917	0.250	-0.00787	0.896	0.907	0.912	0.247	0.161
	Non - Gun Homicides									
	Gun Sales					HFR				
	State FE+ Trend	State FE + Year FE	State FE + State - Trend	First Difference	Anderson Hsiao	State FE+ Trend	State FE + Year FE	State FE + State - Trend	First Difference	Anderson Hsiao
Gun Measure	0.0537*	0.129	0.0407			0.414**	0.355	0.303*		
	(2.32)	(1.93)	(1.38)			(3.42)	(1.62)	(2.12)		
MVD PC	0.142	0.187*	0.110			0.171*	0.202*	0.133		
	(1.97)	(2.15)	(1.46)			(2.45)	(2.36)	(1.71)		
D.Gun Measure				-0.0108	-0.0197				0.477*	0.421
				(-0.18)	(-0.28)				(2.32)	(1.65)
D.MVD PC				0.155	0.0884				0.175	0.224
				(1.84)	(0.88)				(1.81)	(1.96)
OBS	1199	1199	1199	1149	1149	1199	1199	1199	1197	1148
States	48	48	48	48	48	48	48	48	48	48
R2	0.776	0.781	0.790	0.222	-0.0556	0.777	0.781	0.790	0.243	-0.0248

Notes: Linear panel regression results. The outcome variable is indicated by the panel heading. Gun measure indicated by sub-heading. Time controls are indicated by column labels. All models control for percent of the residents who identify as Black or African American, the employment to population ratio, and the state population. State clustered standard errors are in parentheses; * p<0.05, ** p<0.01, *** p<0.001.

We begin with homicides. Overall, we find a 1 % increase in the estimated gun sales increases homicides by approximately 0.2 %. This is the case regardless of the chosen specification. The effect of gun sales on gun homicides is larger than what is found when the outcome variable is homicides but the confidence intervals overlap, suggesting the effect is largely from gun sales increasing gun homicides. Gun sales have no significant relationship with non-gun homicides. This is expected, but it's still a helpful check on spurious relationships. The lagged HFR has a positive relationship with homicides as our preferred dealer-based measure, but the effect seems to be much larger when using the HFR. The lagged HFR also has a positive and significant relationship with non-gun homicides, a failed placebo test which indicates this may be a poor measure and picking up spurious correlations. The leads of the HFR gun prevalence measures are also statistically different from zero when the outcome variable is homicide - suggesting serial correlation or an omitted variable. Motor vehicle deaths have a positive relationship with homicides and gun homicides but are generally only significant in specifications 1, 2, and 3 when the gun prevalence measure is estimated gun sales and in 4 and 5 when the gun prevalence measure is the HFR.

We now discuss suicides. We find a positive relationship between lagged gun sales and overall suicides - though we would characterize the relationship as less well-behaved relative to homicides (i.e., modeling choices result in noticeably different point estimates). We find a 1 % increase in lagged gun sales increases suicides by 0.1 % but the effect drops by about half when using a first difference estimator. The effect seems to be driven by gun suicides as the relationship between gun sales and suicides is smaller than the relationship between gun sales and gun suicides. As with homicides, the HFR tends to have a larger (in magnitude) relationship with suicides but these differences decline when switching to a first-difference approach.

Full results using our preferred specification (Equation 3) are found in Table A3. In the top panel, we use estimated gun sales as the measure of gun prevalence and the bottom uses the alternative, HFR. Here we find that few of the variables are statistically significant; namely, motor vehicle deaths per capita. Estimates suggest that a 1 % increase in motor vehicle deaths per capita is associated with a 0.2 % increase in homicides and gun homicides (again, more motor vehicle deaths imply less policing at the margin). Motor vehicle deaths per capita have no significant relationship with non-gun homicides, suicides, gun suicides, and non-gun suicides when using estimated gun sales as the measure of gun prevalence. This estimate is consistent with the hypothesis that active policing reduces homicides, but private decisions like suicide are not affected by policing.

E.1 Interaction of Gun Prevalence and Policing

We now consider the potential interaction between gun presence and active policing. In Table A4 we present estimates of models similar to those shown in Table A3 but now include an interaction between motor vehicle deaths per capita and estimated gun sales (top panel)/HFR (bottom panel). The interaction between gun sales and motor vehicle deaths per capita is positive and highly significant when the outcome variable is homicides or gun homicides. The interaction is not significant with any of the other outcome variables. The interaction between the HFR and motor vehicle deaths per capita is also not significantly correlated with any of the outcomes. On the one hand, this may mean that greater po-

Table A2: Estimated Relationship Between Gun Measures and Suicides (State Level Panel)

Suicides

	Gun Sales					HFR				
	State FE+ Trend	State FE + Year FE	State FE + State - Trend	First Difference	Anderson Hsiao	State FE+ Trend	State FE + Year FE	State FE + State - Trend	First Difference	Anderson Hsiao
Gun Measure	0.138***	0.0928**	0.124***			0.709***	0.387*	0.629***		
	(14.08)	(2.90)	(8.38)			(10.26)	(2.61)	(6.95)		
MVD PC	-0.0115	0.00162	0.0303			0.0552	0.0127	0.0915**		
	(-0.37)	(0.04)	(1.00)			(1.57)	(0.37)	(2.75)		
D.Gun Measure				0.0183	0.0384*				0.257**	0.0810
				(1.19)	(2.14)				(3.03)	(0.95)
D.MVD PC				0.0356	-0.0218				0.0364	0.0785*
				(1.13)	(-0.53)				(1.11)	(2.09)
OBS	1200	1200	1200	1152	1152	1200	1200	1200	1200	1152
States	48	48	48	48	48	48	48	48	48	48
R2	0.942	0.949	0.952	0.211	-0.0987	0.938	0.949	0.950	0.213	0.0465

Gun Suicides

	Gun Sales					HFR				
	State FE+ Trend	State FE + Year FE	State FE + State - Trend	First Difference	Anderson Hsiao	State FE+ Trend	State FE + Year FE	State FE + State - Trend	First Difference	Anderson Hsiao
Gun Measure	0.234***	0.256***	0.193***			1.281***	1.232***	0.994***		
	(15.96)	(5.51)	(8.38)			(13.15)	(4.94)	(6.99)		
MVD PC	-0.0230	0.00267	-0.0280			0.0920*	0.0338	0.0679		
	(-0.52)	(0.05)	(-0.62)			(2.14)	(0.79)	(1.44)		
D.Gun Measure				0.0498*	0.0641*				0.414***	0.291**
				(2.28)	(2.40)				(4.14)	(2.99)
D.MVD PC				0.00716	-0.0352				0.0134	0.0396
				(0.18)	(-0.89)				(0.33)	(0.78)
OBS	1200	1200	1200	1152	1152	1200	1200	1200	1200	1152
States	48	48	48	48	48	48	48	48	48	48
R2	0.958	0.964	0.965	0.236	-0.0353	0.956	0.965	0.963	0.240	0.140

Non - Gun Suicides

	Gun Sales					HFR				
	State FE+ Trend	State FE + Year FE	State FE + State - Trend	First Difference	Anderson Hsiao	State FE+ Trend	State FE + Year FE	State FE + State - Trend	First Difference	Anderson Hsiao
Gun Measure	0.0409**	0.137***	0.0326*			0.224*	0.316	0.199*		
	(3.03)	(4.18)	(2.42)			(2.49)	(1.70)	(2.54)		
MVD PC	0.0591	0.0327	0.101*			0.0793	0.0481	0.118*		
	(1.42)	(0.67)	(2.16)			(1.83)	(0.99)	(2.47)		
D.Gun Measure				-0.0199	0.000559				0.100	-0.216
				(-1.02)	(0.02)				(0.99)	(-1.61)
D.MVD PC				0.0774	0.00487				0.0702	0.145*
				(1.29)	(0.07)				(1.22)	(2.34)
OBS	1200	1200	1200	1152	1152	1200	1200	1200	1200	1152
States	48	48	48	48	48	48	48	48	48	48
R2	0.883	0.890	0.897	0.202	-0.110	0.882	0.889	0.897	0.206	-0.125

Notes: Linear panel regression results. The outcome variable is indicated by the panel heading. Gun measure indicated by sub-heading. All models control for percent of the residents who identify as Black or African American, the employment to population ratio, and the state population. Time controls are indicated by column labels. State clustered standard errors are in parentheses; * p<0.05, ** p<0.01, *** p<0.001. .

Table A3: Full Results Using Preferred Specification

	Homicides	Gun Homicides	Non-Gun Homicides	Suicides	Gun Suicides	Non-Gun Suicides
Gun Sales (t-3)	0.238***	0.332***	0.0407	0.124***	0.193***	0.0326*
	(8.45)	(9.21)	(1.38)	(8.38)	(8.38)	(2.42)
MVD PC	0.171**	0.202	0.110	0.0303	-0.0280	0.101*
	(2.75)	(1.92)	(1.46)	(1.00)	(-0.62)	(2.16)
Oil Per Capita	0.223	0.345*	-0.0541	-0.151*	-0.219*	-0.0656
	(1.65)	(2.05)	(-0.36)	(-2.06)	(-2.09)	(-0.85)
% Black	-0.176	-0.677	-0.0499	-0.229**	-0.357***	-0.132
	(-0.80)	(-1.89)	(-0.29)	(-2.93)	(-3.55)	(-1.22)
Emp/Pop	0.243	0.151	0.129	-0.287***	-0.0681	-0.483***
	(1.01)	(0.44)	(0.57)	(-3.62)	(-0.54)	(-3.79)
Population	-0.00500	0.262	-0.0383	-0.464	-0.931	0.231
	(-0.01)	(0.30)	(-0.07)	(-1.84)	(-1.92)	(1.07)
Constant	-2.015	-7.389	0.773	8.966*	15.53	-2.289
	(-0.17)	(-0.54)	(0.09)	(2.12)	(1.97)	(-0.69)
OBS	1200	1199	1199	1200	1200	1200
States	48	48	48	48	48	48
R2	0.934	0.917	0.790	0.952	0.965	0.897

	Homicides	Gun Homicides	Non-Gun Homicides	Suicides	Gun Suicides	Non-Gun Suicides
HFR (t-3)	1.105***	1.395***	0.303*	0.629***	0.994***	0.199*
	(5.14)	(4.88)	(2.12)	(6.95)	(6.99)	(2.54)
MVD PC	0.286***	0.359**	0.133	0.0915**	0.0679	0.118*
	(3.76)	(3.07)	(1.71)	(2.75)	(1.44)	(2.47)
Oil Per Capita	0.0733	0.100	-0.0560	-0.218**	-0.320**	-0.0764
	(0.52)	(0.52)	(-0.45)	(-2.93)	(-2.99)	(-0.99)
% Black	-0.122	-0.628	-0.0235	-0.192*	-0.298*	-0.117
	(-0.53)	(-1.66)	(-0.14)	(-2.10)	(-2.52)	(-1.09)
Emp/Pop	-0.0877	-0.313	0.0802	-0.455***	-0.330*	-0.525***
	(-0.37)	(-0.96)	(0.32)	(-5.51)	(-2.52)	(-4.28)
Population	-0.470	-0.551	-0.00579	-0.652*	-1.215*	0.213
	(-0.62)	(-0.61)	(-0.01)	(-2.34)	(-2.33)	(0.95)
Constant	3.754	3.902	-0.489	10.88*	18.32*	-2.420
	(0.31)	(0.27)	(-0.07)	(2.29)	(2.10)	(-0.69)
OBS	1200	1199	1199	1200	1200	1200
States	48	48	48	48	48	48
R2	0.931	0.912	0.790	0.950	0.963	0.897

Notes: Estimated effects using a fixed effects linear regression. All models include county fixed effects and county specific time trends. Control variables are motor vehicle death rate, population, percent of the population that identifies as Black/African American, and the employment to population ratio. The outcome is indicated by the column label. County clustered standard errors are in parentheses; * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

lice presence does little to reduce fatal outcomes among those who accurately responded to the survey that the HFR measure is based on. On the other hand, this may be yet another indication that the HFR does a poor job of tracking general firearm prevalence in a community.

Table A4: Interaction Effects

	Homicides	Gun Homicides	Non-Gun Homicides	Suicides	Gun Suicides	Non-Gun Suicides
Gun Sales (t-3)	-0.227*	-0.319*	-0.0484	0.0956	0.185*	0.0929
	(-2.19)	(-2.21)	(-0.33)	(1.86)	(2.59)	(1.14)
MVD PC	-1.804***	-2.561***	-0.268	-0.0887	-0.0631	0.357
	(-4.12)	(-4.15)	(-0.40)	(-0.43)	(-0.22)	(1.05)
Gun Sales (t-3) × MVD PC	0.176***	0.246***	0.0338	0.0106	0.00314	-0.0228
	(4.66)	(4.73)	(0.57)	(0.59)	(0.13)	(-0.78)
OBS	1200	1199	1199	1200	1200	1200
States	48	48	48	48	48	48
R2	0.935	0.918	0.790	0.952	0.965	0.897

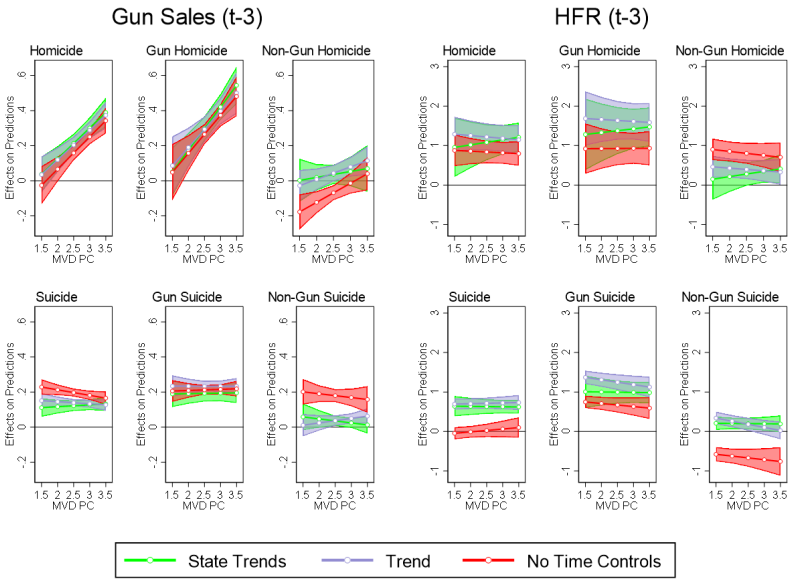
	Homicides	Gun Homicides	Non-Gun Homicides	Suicides	Gun Suicides	Non-Gun Suicides
HFR (t-3)	0.762	1.140	-0.0484	0.654**	1.023**	0.219
	(1.17)	(1.52)	(-0.10)	(3.21)	(3.10)	(1.38)
MVD PC	-0.172	0.0198	-0.335	0.124	0.107	0.146
	(-0.26)	(0.03)	(-0.55)	(0.63)	(0.32)	(0.69)
HFR (t-3) × MVD PC	0.128	0.0952	0.131	-0.00904	-0.0109	-0.00774
	(0.67)	(0.45)	(0.76)	(-0.16)	(-0.11)	(-0.12)
OBS	1200	1199	1199	1200	1200	1200
States	48	48	48	48	48	48
R2	0.931	0.912	0.790	0.950	0.963	0.897

Notes: Estimated interaction effects using a fixed effects linear regression. All models include county fixed effects and county specific time trends. Control variables are motor vehicle death rate, population, percent of the population that identifies as Black/African American, and the employment to population ratio. The outcome is indicated by the panel label. State clustered standard errors are in parentheses; * p<0.05, ** p<0.01, *** p<0.001.

The marginal effect of gun sales for different log levels of motor vehicle deaths is found in Figure A1. Similar plots, using a first difference approach, are found in Figure A2. We find that the relationship between lagged gun sales and homicide/gun homicide increases with increases in motor vehicle deaths – this is also observed when using the first difference approach (Figure A2). This relationship is not present in non-gun homicides which has no significant relationship with gun sales at any level of motor vehicle deaths – this is also observed when using the first difference approach. The effect of gun sales on suicide and gun suicide is constant with different levels of motor vehicle deaths – again, also observed when using the first difference approach. The leads of gun sales interacted with current motor vehicle deaths results in statistically insignificant effects on almost all of the outcome variables (see Figure A3 in the Appendix). The HFR has a relationship with the outcome variables that is independent of motor vehicle deaths and the leads are often positive and significant (see Figure A3 in the Appendix). The first difference approach, when using the HFR, produces completely different results from those presented in Figure A1 and the standard errors are very large.

The notable differences between HFR-based estimates and gun sales estimates are likely related to the construction of the HFR rather than a large problem with survey data that is used to generate the HFR. To support this, and as a robustness check, we estimate similar models to those presented but instead of using the HFR or gun sales estimates, we use the GSS responses directly (i.e., the proportion of households in state i in year t that reported

Figure A1: Estimated Joint Relationship of Motor Vehicle Deaths and Gun Sales and HFR



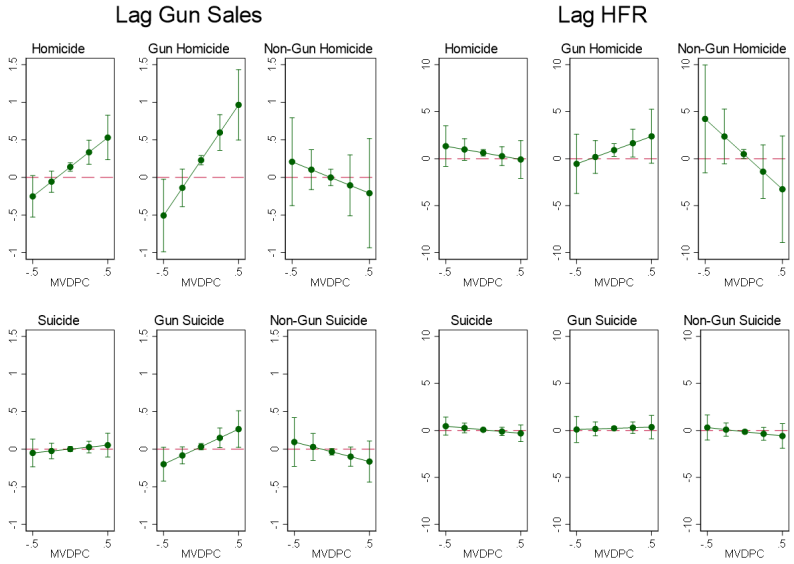
Notes: Estimated effect of guns by motor vehicle death rate. Outcome variable is indicated by the sub-figure heading label. Main heading indicates the gun measure. Control variables are motor vehicle death rate, population, percent of the population that identifies as Black/African American, and the employment to population ratio. Error bars indicate the 95% confidence interval using state-clustered standard errors.

having a firearm).[30] These estimates are presented graphically in Figures A4, A5, and A6 and are found in the Appendix.[31] Overall, we find that the point estimates using gun sales estimates – which are included in Figures A4 and Figures A5 to make comparisons easier – are similar to those generated using the GSS ownership rate. Additionally, we find a positive interaction effect between GSS-based gun ownership and motor vehicle deaths (Figure A6) which is not present when using the HFR. Of course, there are slight differences but this is expected considering differences in the two samples – namely estimates using the GSS data have about half the number of observations.

[30] We thank Jeffrey Fagan for this suggestion. GSS data is found here: https://www.rand.org/pubs/tools/TL354.html

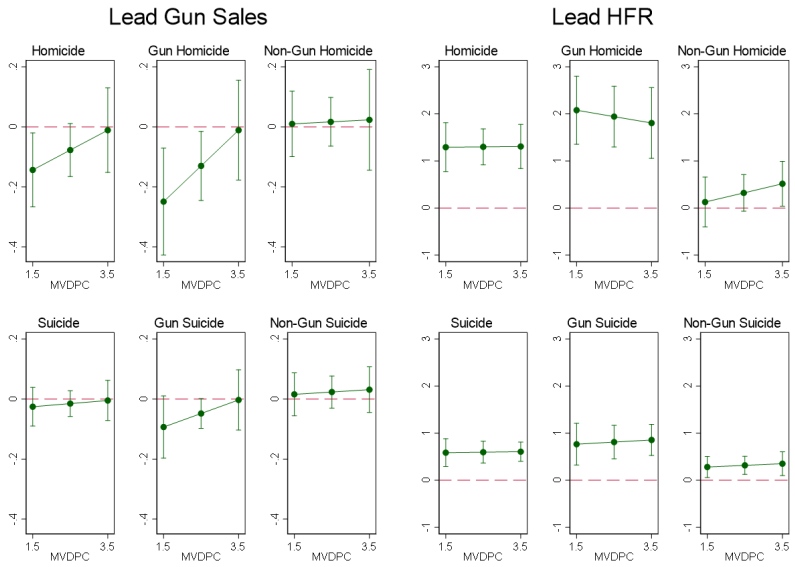
[31] Note that we do not estimate use first difference approaches when estimating models using the GSS ownership rate. This is because since 1994 the GSS has been conducted only every other year. Additionally, states are not always sampled when then the survey is conducted.

Figure A2: Estimated Joint Relationship of Motor Vehicle Deaths and Gun Sales and HFR (First Difference Regression)



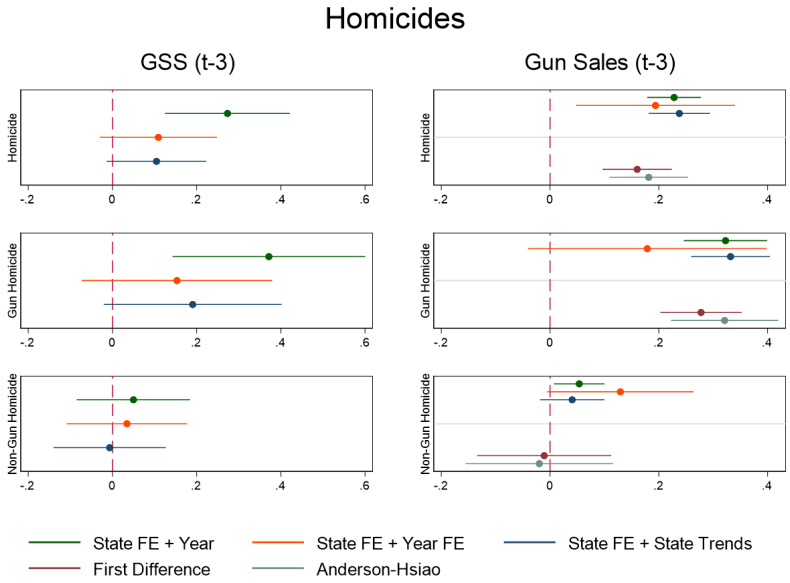
Notes: Estimated effect of guns by motor vehicle death rate. Outcome variable is indicated by the sub-figure heading label. Main heading indicates the gun measure. Control variables are motor vehicle death rate, population, percent of the population that identifies as Black/African American, and the employment to population ratio. Error bars indicate the 95% confidence interval using state-clustered standard errors.

Figure A3: Estimated Joint Relationship of Motor Vehicle Deaths and the Lead of Gun Sales and HFR



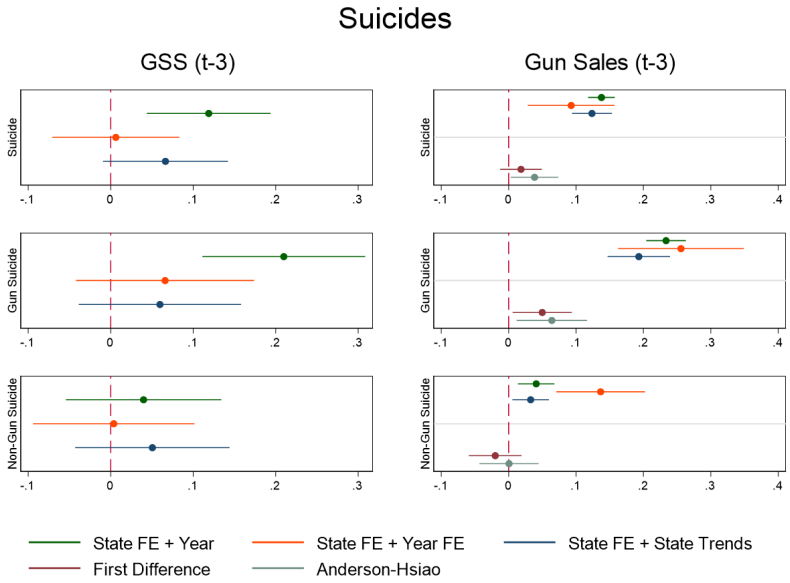
Notes: Estimated effect of guns by motor vehicle death rate using a linear panel regression. Outcome variable is indicated by the sub-figure heading label. Main heading indicates the gun measure. Control variables are motor vehicle deaths, population, percent of the population that identifies as Black/African American, and the employment to population ratio. The outcome is indicated by the panel label. Error bars indicate the 95% confidence interval using state-clustered standard errors.

Figure A4: Full Results Using Preferred Specification (Homicides)



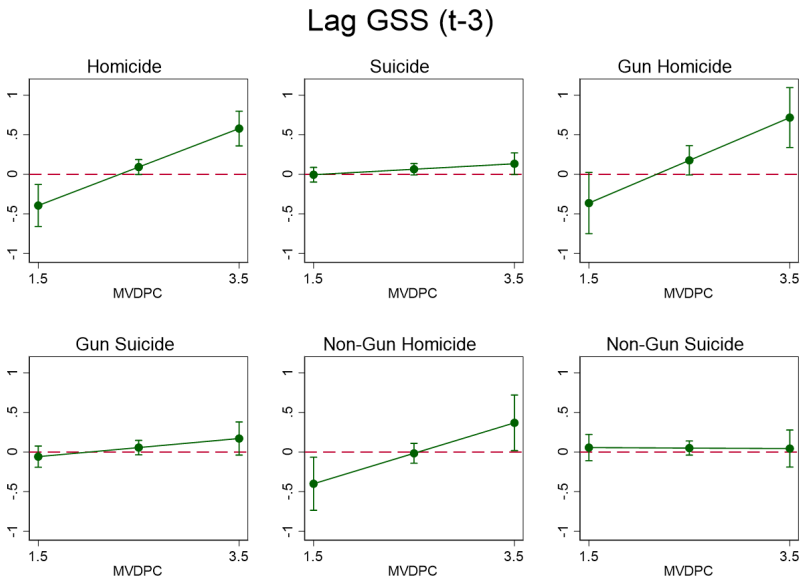
Notes: Coefficient estimates for GSS-based gun ownership and estimated gun sales. The outcome variable is indicated by y-axis label. Column heading indicates the timing and gun measure. Control variables are motor vehicle death rate, population, percent of the population that identifies as Black/African American, and the employment to population ratio. Model specification indicated in figure legend. Error bars indicate the 95% confidence interval using state-clustered standard errors.

Figure A5: Full Results Using Preferred Specification (Suicides)



Notes: Coefficient estimates for GSS-based gun ownership and estimated gun sales. The outcome variable is indicated by y-axis label. Column heading indicates the timing and gun measure. Control variables are motor vehicle death rate, population, percent of the population that identifies as Black/African American, and the employment to population ratio. Model specification indicated in figure legend. Error bars indicate the 95% confidence interval using state-clustered standard errors.

Figure A6: Estimated Joint Relationship of Motor Vehicle Deaths and GSS based Gun Ownership



Notes: Estimated effect of GSS-based gun owner ship by motor vehicle death rate. Outcome variable is indicated by the sub-figure heading label. Main heading indicates the gun measure. Control variables are motor vehicle death rate, population, percent of the population that identifies as Black/African American, and the employment to population ratio. Error bars indicate the 95% confidence interval using state-clustered standard errors.