The Effect of Firearm Seizure on Criminal Activity

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Abstract

This study investigates whether the seizure of firearms during police interventions affects subsequent criminal activity. Although firearm confiscation in response to criminal incidents is a routine law enforcement practice rather than a targeted policy intervention, it offers a valuable opportunity to examine the effects of removing guns from circulation. Measuring this effect is empirically challenging because firearm seizure is, by definition, positively correlated with criminal incidents (i.e., without a violation or incident reported to the police there is no confiscation). To address this endogeneity, we exploit temporal and spatial variation and implement an instrumental variable strategy that leverages exogenous variation in police response times caused by nearby traffic accidents. We find that one additional firearm seized from a block group leads to approximately 0.65 fewer crimes in the same area in the following month, which is equivalent to a 6% reduction on average. Our results offer support to gun control measures, and could guide law enforcement strategies.

Key words: gun violence, crime, traffic accident, instrumental variable
JEL Codes:

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1 Introduction

The United States has the highest gun ownership rate globally, with approximately one firearm per person, nearly double the rate of the second-ranked country.[1] This prevalence coincides with disproportionately high levels of gun violence compared to other wealthy nations. In 2021, the firearm-related death rate in the U.S. stood at 4.3 deaths per 100,000 people, 20 times higher than the average in Western Europe.[2] These statistics raise critical questions about how firearm prevalence influences gun violence. A large body of work has sought to measure this relationship empirically (Section 2 reviews part of this literature).

In this paper, we answer a related question but from a slightly different perspective on firearm prevalence. Specifically, we estimate the effect of removing "crime guns" – firearms confiscated by law enforcement that were illegally possessed, used in a crime, or suspected of being involved in criminal activities – on various categories of crime. To achieve this, we construct a unique longitudinal dataset from Louisville, KY, and employ a novel instrumental variable approach.[3] Our method leverages high-frequency, fine-grained spatial data to isolate exogenous variation in firearm seizure, enabling a credible estimate of its potential effect on future criminal activity.

There are two main channels through which firearms could increase criminal activity. First, potential offenders who possess a gun may be more confident in the success of their actions, leading them to attempt more crimes. Second, firearms may intensify criminal acts. The mere presence of a firearm has the potential to escalate or aggravate any incident. For example, a disagreement at a bar can escalate to assault with a deadly weapon when individuals feel prompted to fire their weapon. Because we observe the withdrawal of firearms from a specific context – those potentially related to a criminal offense or a weapon violation

[1] Sources: Small Arms Survey and Migiro, Geoffrey. "Countries With the Highest Rate of Gun Ownership." WorldAtlas, Nov. 5, 2018, worldatlas.com/articles/countries-with-the-highest-rate-of-gun-ownership.html..

[2] Sources: CDC and OECD.

[3] We plan to expand this study by including data from Chicago, IL; Pittsburgh, PA; and Washington, DC.

– we expect, via the logic of Becker's model (Becker, 1968), a reducing effect on crime. Specifically, the idea is that with their weapons confiscated, individuals would be less likely to engage in criminal activities, or certain events would be less likely to escalate to a criminal act. On the other hand, if the firearm market is highly dynamic and replacement is easy, the effect of firearm seizure could be negligible.

Firearm seizures are inherently endogenous and positively correlated with local crimes and policing intensity. Gun prevalence tends to be higher in high-crime areas, where both criminals and the population at large buy more guns (Depew and Swensen, 2019). Consequently, the number of guns seized typically rises and falls with overall crime levels, complicating causal inference even with rich longitudinal data. Then, besides controlling for a comprehensive set of fixed effects, we apply a novel empirical strategy of using exogenous variation from car accidents to instrument firearm seizure to account for endogeneity from time-varying confounders.

The core idea is that car accidents, conditional on time and location fixed effects, are plausibly exogenous shocks that reposition police units across the city. A traffic accident just minutes before and near an incident reported to law enforcement places police officers closer to the scene, potentially reducing their response time.[4] Faster response increases the likelihood of firearm discovery and seizure. For instance, if a crime occurs and police were already nearby responding to an earlier traffic accident, they can reach the scene more quickly. The identifying assumption is that, conditional on time and location fixed effects, car accidents are exogenous to both criminal activity and police behavior related to firearm seizures.

Using detailed longitudinal data from Louisville (2010-2017), we first show that, after conditioning on hour of the day, crime type, location, and daily conditions at the police division, a car accident 15 to 30 minutes before and within 200 meters to 2 kilometers of

[4]We may refer to an incident reported to the police as a "criminal incident" in order to distinguish it from a traffic accident, which is also reported to the police. However, note that this is informal language (not in a strictly legal sense), as an incident reported to the police may not necessarily be a criminal event.

an incident reported to the police increases the likelihood that a firearm is seized from the incident by 12%. Leveraging this exogenous variation, our estimates imply that an additional firearm seized in a block group reduces total crime in the same block group by 0.65 incidents (a 6% decline) in the following month. Effects are particularly pronounced on property crimes (-5%), theft (-8%), and weapons violations (-92%).

In order to infer something about the effect of firearm seizure on domestic violence or incidents where the offender and victims have some type of connection, we use the data at the census block level. We document that one firearm seized in a given block decreases reported incidents at residences in the same block in the following month by 0.075 incidents, a 27% decline on average. Even more substantial is the effect on homicides: one additional firearm seized lowers homicides by nearly three times the block average in the month after the seizure. The strong localized reduction in crimes following firearm seizures suggests that targeted removal of firearms can meaningfully enhance public safety. The results at the block level could also support the rationale behind red flag laws, which allow law enforcement to temporarily remove firearms from individuals deemed to pose a danger to themselves or others.

To improve the credibility of our identification strategy, we examine additional relationships to help support the identifying assumption that, after conditioning on time and location, car accidents occurring within a certain radius and time interval are exogenous to criminal events. First, we show that our instrument – having a nearby car accident – does not affect the time it takes for the incident to be reported to law enforcement. We also take advantage of the fact that, for some crimes, firearms are confiscated at a later stage of the investigation, often days after the offense. Specifically, we show that a car accident occurring before and near a criminal incident is a statistically significant and sizable predictor of a firearm being seized on the day of the incident, but not afterward. These two results support the interpretation that the effect of car accidents on firearm seizure operates through faster police response at the time of the incident, rather than through other channels.

Through the argument that the instrument is relevant as it increases police response time, one can argue that the instrument could have a direct effect on future crimes by also increasing the probability that an arrest is made. Because arrest information linked to each incident reported to law enforcement is available, we are able to measure the impact of our instrument on the probability of arresting and do not find statistically significant or sizable estimates. Moreover, in all the estimations, the number of arrests is included as a control, which allows us to estimate the effect of firearm seizure net of the arrest effect.

Our study is closest to Sherman and Rogan (1995), who use a difference-in-differences design and find that intensive patrol near gun crime hot spots produced a 65% increase in firearms seized by police. Gun crimes declined in the target area by 49%, with no significant displacement to untreated areas. Our work also relates to a study by Khalil (2017), who uses the number of firearms reported stolen as a proxy for illegal firearm flows and shows that more stolen firearms result in more future violent crimes like aggravated assaults and robberies.

Methodologically, this study relates to prior work in the crime literature that exploits variation in police presence across locations as a source of identification.[5] Our identification strategy is more closely aligned with recent studies that exploit routine or quotidian factors affecting police presence. Weisburd (2016) exploits police responses to calls outside of their allocated coverage beat as a plausible shift in police presence within the abandoned beat. DeAngelo et al. (2020) use the number of vehicles within a 2.5 mile radius of the call at the time it is received by the call center to instrument for police response time. However, to the best of our knowledge, we are the first to use a recurrent, high-frequency, and commonplace event – traffic accidents – as an exogenous shock to the geographic distribution of law enforcement.

A major contribution of this study is to quantify the short-term effect of firearm seizures on future crimes. Another innovation is to use detailed and rich data on the number of

[5]Some studies rely on a single point in time or deliberate police responses to extreme events (Di Tella and Schargrodsky 2004, Draca et al. 2011, and Klick and Tabarrok 2005).

firearms seized and criminal incidents in an urban area, which allows us to measure dynamic effects on local crimes and to avoid the use of gun prevalence proxies or over-aggregated geographic units. Finally, our identification strategy uses an everyday city event, car accidents, as a plausibly exogenous instrument, making the approach adaptable to other urban settings.

By showing that the seizure of even a single firearm can lead to significant short-run reductions in crime, our findings underscore the potential effectiveness of proactive and well-targeted gun enforcement strategies as components of broader firearm regulation frameworks. These insights are especially relevant in contexts where legislative reforms face political constraints, but more focused, enforcement-driven policies remain feasible.

The paper is organized as follows. Section 2 provides a brief literature review on empirical studies about gun prevalence and crimes. Section 3 offers detailed information about the data sources and discusses background information on gun laws and firearm seizures in the context of our data. Section 4 outlines the empirical strategy by discussing identification and the estimation approaches. Section 5 discusses and interprets the results. Finally, Section 6 provides some concluding discussion.

2 Related Literature

The empirical literature examining the relationship between gun prevalence and crime is voluminous. However, there is no clear consensus on the effect that gun prevalence has on crime. Moreover, the vast majority of the literature approaches the problem from the perspective of firearm availability among the general population. For instance, Duggan (2001) uses gun magazine subscription levels across U.S. counties and shows that changes in this proxy for gun ownership are positively related to changes in the homicide rate. Cook and Ludwig (2006) use the percentage of suicides committed with a gun as a measure of gun prevalence. Matching this with county-level longitudinal data, they find a positive elasticity

of homicide with respect to gun prevalence. While influential, these papers rely on imprecise measures of gun ownership, or lack exogenous variation in gun prevalence.

More recent work find less of a connection. Tannenbaum (2020) exploits public information on the location of handgun permit holders and finds that the disclosure of a higher density of permit-holding neighbors did not affect crime. While Johnson and Robinson (2024) use local gun dealer density as a more granular measure of gun prevalence. They find that an increase in gun dealer density is positively associated with more homicides. However, they show that the effect of gun dealer density is limited to counties with a high percentage of Black residents and metropolitan areas.

There is also a large literature that exploits differences across states regarding the right to carry a concealed gun (see, e.g., Durlauf et al. 2016). Using county-level data, Lott and Mustard (1997) find that allowing citizens to carry concealed weapons deters violent crimes. Manski and Pepper (2018) report more mixed results, concluding that right-to-carry laws increase assaults but decrease murder and larceny rates. Also using county-level data, but contrary to Lott and Mustard (1997), Donohue et al. (2019) document that right-to-carry laws are associated with a 15% increase in aggregate violent crime rates ten years after adoption. Other studies take advantage of changes in national gun laws. Examining the repeal of gun control in Australia, Taylor and Li (2015) find a significant decrease in armed robbery and attempted murder relative to sexual assault after the law was enacted. Schneider (2021) documents a reduction in homicides following a nationwide law that banned the carrying of concealed weapons in Brazil. In summary, the balance of these studies suggests that constraining the overall prevalence of firearms among the population decreases the incidence of violent crime.

3 Background and Data

3.1 Gun Law in the State of Kentucky

The U.S. does not have a nationalized gun control law, and the rules governing gun ownership and carrying vary by state. Kentucky, in particular, has very low barriers to acquiring and carrying firearms, even by U.S. standards. The only requirement is that individuals be legal U.S. residents over the age of 18. State law prohibits individuals convicted of a felony from owning firearms. Federal restrictions also apply to additional groups, including those convicted of domestic violence misdemeanors or subject to domestic violence restraining orders, illegal drug users, individuals who have been committed to a psychiatric institution, former military members dishonorably discharged, and those in the country illegally or on a non-immigrant visa. Firearms may be purchased from licensed dealers, manufacturers, importers, or collectors, as well as from unlicensed individuals. Gun owners are not required to register their firearms and do not need a license to own a rifle, shotgun, or handgun.

Both open carry and concealed carry of handguns are permitted in Kentucky. Open carry refers to a firearm being visibly carried on a person or within a vehicle. It is legal without a permit for anyone at least 18 years old. If an openly carried weapon is brought to their attention, law enforcement officers have the right to question the carrier. Concealed carry refers to a weapon not easily visible but readily accessible for use. During the study period of this paper, Kentucky residents were required to obtain a concealed carry deadly weapons license. However, in 2019, the state passed a law eliminating that requirement. Since then, concealed carry has been legal without a license for anyone aged 21 or older who is legally permitted to possess a firearm.

Over the study period, more than 10,000 firearms were confiscated by the Louisville Metro Police Department (LMPD). These weapons included stolen property as well as guns legally purchased but used in illegal activities. Firearms were seized during criminal investigations, arrests, or drug searches, and some were found in the possession of felons, which is prohibited

by law. From the data, we learn that most firearms are seized from drug-related offenses, assaults, and weapon violations. These three categories account for 80% of incidents from which firearms are confiscated. However, these types of crime correspond to only 24% of the total number of crimes.

Kentucky is one of approximately eleven U.S. states that do not destroy seized firearms. Under a law passed in 1998, weapons that cannot be returned to their legal owners must be auctioned and sold to federally licensed firearms dealers. In 2015, 4,379 firearms were sold at auction in Kentucky. Guns from LMPD accounted for nearly one-third of those weapons sold in state auctions. In 2016, the state police sold about $575,000 worth of firearms.[6] Funds from the sale of confiscated weapons by the state are transferred to the Kentucky Office of Homeland Security, which provides grants to city, county, charter county, unified local government, urban-county government, and consolidated local government police departments, as well as university safety and security departments.

3.2 Data Sources

We consider the period from January 2010 through March 2017, period when information on firearm seizure is available. Our sample consists of 574 block groups, each comprising about 160 census blocks and are areas with 1,300 inhabitants on average. We also consider the census block as a unit of analysis in order to study residential crimes and violence hot spots.

The region of study is the Jefferson County, Kentucky which encompasses the city of Louisville and other smaller jurisdictions. The Jefferson County's population in 2010 was 741,075 in according to U.S. Census Bureau. The area is divided into eight police divisions plus a general classification called metro area. Crime rate is around 42 crimes per thousand residents which is comparable to cities like Chicago and Philadelphia.[7]

[6]Information retrieved from https://www.wdrb.com/news/seize-and-sell-new-scrutiny-for-kentucky-law-requiring-sales-of-confiscated-guns/article_719f19f3-79f8-5f4b-ac4e-dfaa8b2b9ac0.html, accessed February 2021.

[7]According to the website https://www.neighborhoodscout.com/.

Data on detailed location and time specific incident-level for crime, seized firearms, police arrests are assembled from the LMPD. Each crime incident carries a unique identifier that can be used to link with firearm seizure and arrests. An incident may contain duplicates such that all type of offenses involved in the event are listed. For instance, for a drug related incident where the offender also committed a gun violation there will be two entries in the data under the same identifier but listed with different crime category. In order avoid multiple counts of crime we keep incidents by their degree of severity as classified by the FBI in part I and part II.

Although the crime dataset does not carry the latitude and longitude of each incident, the block address where the incident occurred is disclosed. We use several tools to retrieve the geolocation associated with each incident so that we can merge the crime data with car accidents and census block boundaries. In particular, we use the Census Geocoder tool available at https://geocoding.geo.census.gov/geocoder/. We were not able to geolocate about 6% of crimes, and these incidents are dropped from our sample. We drop incidents that occurred on express highways or that did not have a proper police beat associated with them. There are also arrests that are not linked to a criminal incident. However, the addresses available in these datasets are not clear enough for us to retrieve the geolocation as done in the crime data. Thus, for arrest incidents not linked to a crime, we use the police beat to associate arrests with a certain geographic area.

Data on car traffic accidents in Jefferson County are from the Kentucky State Police. The data provide information on the exact time and geolocation of the accident. This allows us to match them with criminal incidents.

3.3 Summary Statistics

Table 1: Summary statistics.

	Mean	Median	SD	Min	Max
Panel A: all incidents reported to law enforcement					
Violent crime	0.039	0	0.193	0	1
Property Crime	0.402	0	0.490	0	1
Weapon Viol.	0.004	0	0.063	0	1
# Firearms seized	0.019	0	0.211	0	35
Firearm seized = 1	0.015	0	0.120	0	1
Firearm seized same day = 1	0.010	0	0.099	0	1
# Arrests	0.266	0	0.541	0	25
Arrest = 1	0.230	0	0.421	0	1
Arrest same day = 1	0.189	0	0.392	0	1
Arrest immediate = 1	0.099	0	0.299	0	1
# Car accidents	0.034	0	0.190	0	5
Observations			529,172		
Panel B: Sample - block group and month-year					
All crimes	11.339	8	13.265	0	389
Violent crime	0.442	0	0.864	0	17
Property Crime	4.594	3	5.988	0	100
Weapon Viol.	0.045	0	0.232	0	5
# Firearms seized	0.218	0	0.771	0	35
# Arrests	3.054	1	5.182	0	96
# Car accidents	0.392	0	1.173	0	87
Observations			45,429		

Table 1 provides basic summary statistics for the main variables in our study. Panel 1 shows our sample of incidents reported to law enforcement, which is large in size including more than 500,000 records. About 1.5% of these crimes had a firearm seized and 3.4% had at least one car accident happen within 200 to 2000 kilometers and 15 to 30 minutes before its occurrence. Around 23% of crimes had an arrest linked to it.

The second panel shows our longitudinal sample by block group and month-year. The average number of total crimes per block group-month is 11.34, but the standard deviation is high (13.27), suggesting a right-skewed distribution with some high-crime areas. Violent crimes (e.g., assault, robbery, rape and homicides) occur much less frequently, with a mean of 0.44. Property crimes (e.g., theft, burglary, arson and motor vehicle theft) are more

common, averaging 4.59 incidents. Weapon violations are rare, with a mean of 0.045, which is in line with the conext of gun laws in the state. On average, about 0.22 firearms are seized per block group-month. Arrests are more common, averaging 3.05 per block group-month. There are on average 0.39 traffic accidents per block group-month.

4 Empirical Strategy

We are interested in measuring the effect of firearm seizures on the occurrence of future crimes. These firearms are confiscated during arrests or drug searches, from crime scenes, during criminal investigations, or due to weapons violations. Identifying this effect is difficult because firearm seizure is by definition a result of incidents where law enforcement is involved. Crime path dependence, i.e. current crime is a strong predictor of future crimes (Jacob et al. 2007; Caetano and Maheshri 2018) and the fact that firearms are seized when the police responds to crimes or practices law enforcement measures introduces a positive association between the number of firearms seized and future crimes. Additionally, firearm seizure depends on local policing strategies, which are not fully observed by the researcher, but obviously are reaction to and have an impact on criminal activities. Thus, law enforcement activity is an omitted factor that could also cause bias.

With these issues in mind, consider the following equation that models the amount of crime in census block group j and month-year t as a function of firearms seized by the police force in the previous month. The equation also states that number of crimes depend on the past number of crimes, past number of arrests, police division-month-year fixed effects (δ_{Jt}), block group-year fixed effects (δ_{jT}), and other unobserved factors and random shocks denoted by u_{jt}.

$$crime_{jt} = \beta firearm_{jt-1} + \gamma_1 crime_{jt-1} + \gamma_2 arrest_{jt-1} + \delta_{Jt} + \delta_{jT} + u_{jt} \tag{1}$$

This equation takes into consideration the mechanic and positive relationship between firearm

seizure and crime by explicitly controlling for lagged crimes. Division-month-year fixed effects account for monthly fluctuations on crime at the police administrative level due to seasonality, time trends and law enforcement campaigns. Block group-year fixed effects take care of time invariant local level criminogenic factors that could be correlated with both firearm prevalence and crime levels.

After accounting for location fixed effects, the idea is to compare two areas in the same police division that during a given month had the same number of crimes and arrests, but, for some reason, one of them had more firearms seized than the other. If we believe that the difference in the amount of firearms confiscated between these two locations was random, then we would be able to identify β, the effect of firearm seizure on crime in the next period.

4.1 Instrumental Variable

Even though Equation 1 addresses some of the endogeneity problems encountered when measuring the effect of firearm seizures on crime, the specification is probably not able to account for all time-variant omitted factors at the local level. It is very likely that shocks in criminal activity are autoregressive. For instance, suppose an offender that is actively committing crimes in a certain area is captured and arrested in month $t-2$. The potential effect on crimes due to this arrest will likely last for several periods. This implies that the arrest made in the past is likely to affect crimes and policing behavior in $t-1$. Since policing behavior at the local level is not directly observed or easy to measure, it will belong to u_{jt}, which consequently makes the seizure of firearms in $t-1$ endogenous as it is correlated with policing.

Moreover, if, across months, police force is not consistently distributed within an police district, the area with more policing would likely see higher numbers of seized firearms. The bias sign is not obvious once it depends on underlying dynamics between policing and local crimes. That is, the bias will depend on whether policing allocation is a response to an increase in criminal activity or a more proactive action of increasing law enforcement

presence to prevent future crime.

Finally, local underlying trends by categories of crime could be related to the use of firearms and have dynamic effects on future crimes. Meaning the current profile of crimes at the local level matters but it is hard to be observed. Depew and Swensen (2019) document a good example, homicides increase applications for concealed-carry permits in areas relatively near to the incident.

In summary, firearm seizure is by definition linked to both crime and policing dynamics, which in turn are correlated with future movements in crimes. Therefore in order to measure the causal effect of firearm seizure on future crimes and not only associations, we would need an exogenous factor that affects firearm removal, but has no implication on criminal activity.

To deal with endogeneity from time varying confounders we apply a novel empirical strategy of using exogenous variation from car accidents to instrument firearm seizure. We argue that a traffic accident that occurs near and a few minutes before an incident happens and is reported to the police brings policemen closer to the incident's scene. This results in an increase in the probability that a firearm is seized. The argument that, by arriving more quickly, police officers would be able to arrest any suspect and to prevent the destruction or concealment of physical evidence seems self-evident. In fact Blanes i Vidal and Kirchmaier (2018) demonstrate that 10% increase in response time leads to a 4.7 percentage points decrease in the likelihood of clearing a crime.

The equation below models the probability that a firearm is seized from incident i as a function of car accident occurrences minutes before and in a certain distance from the incident site. It is worth noticing that the unit of analysis is a criminal incident.

$$firearm_i = \alpha car_accident_i + \pi arrest_i + \delta_{i(h)} + \delta_{i(J\tau)} + \delta_{i(j)} + \delta_{i(k)} + v_i \tag{2}$$

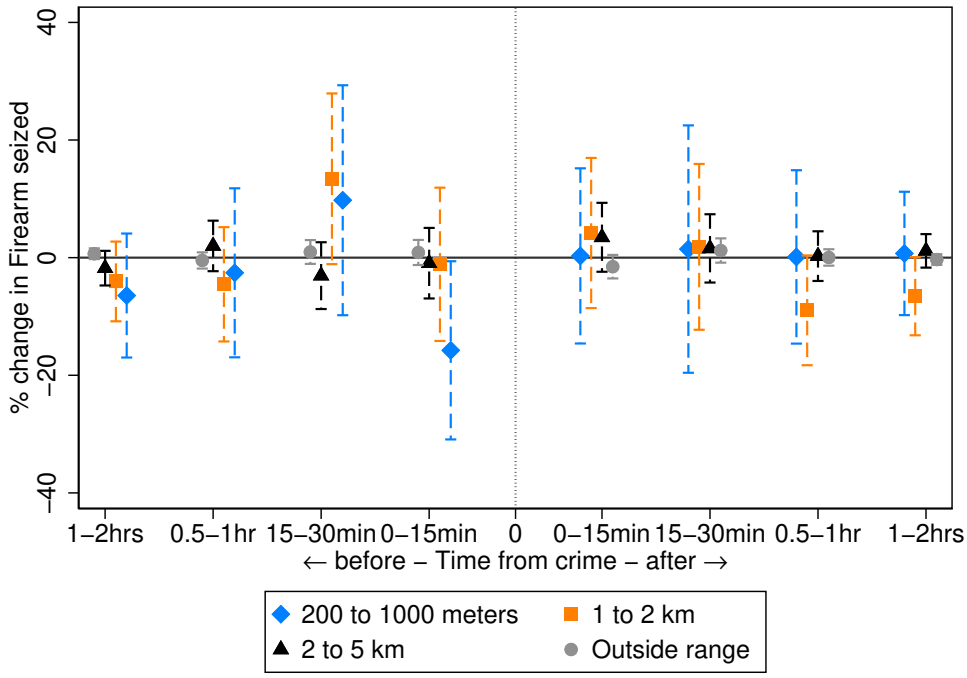
Conditional on hour of the day, δ_h, daily condition at the police division, $\delta_{J\tau}$, the fixed effect of the block where incident i occurs, δ_j, and crime category fixed effects, δ_k, the exclusion restriction requires that car accidents only relates to future crime through its effect on the

speed of response by the police, which potentially translates to more firearm seized. There are two major threats to the exclusion restriction, such that car accidents could correlate with future crime through other channels. We detail them below and explain what is done to overcome each of these threats.

First, besides confirming that shorter police response time has a positive effect on crime clearance, Blanes i Vidal and Kirchmaier (2018) also find that the likelihood of making an arrest at the crime scene decreases with response time. Along the same lines, car accidents near to and minutes before a crime occurs could affect the speed of police response, and consequently increase the probability that a suspect is arrested. Since the source of variation we use to form our instrument could also affect arrests, for our approach to be valid in capturing exclusively the firearm seizure effect, it is crucial that we also condition on the number of arrests.

Another channel that could invalidate the exclusion restriction comes from the fact that police presence due to an accident could intimidate criminals from acting or change the type of criminal act. In fact several papers have shown that police presence decreases crime counts (See Nagin et al., 2013 and Chalfin and McCrary 2017 for literature review). Because this channel should only holds during a certain time interval from the time of the accident and at a certain radius around the accident, to ensure the instrumental variable is valid we do not consider car accidents right before (within 15 minutes) and too close to the criminal incident location.

Besides not violating the exclusion restriction, we also need car accidents to truly affect police speed in responding to an incident in adjacent areas, which would in turn increase the number of firearms seized. This relationship can be tested in the data. The points plotted in Figure 1 are estimated from Equation 2 where the variable *car_accident* is expanded to be indicator variables about car accidents within certain distances from the incident and before and after the incident time. Having a car accident happening at a 200 meters to two kilometer radius from the crime site and 15 to 30 minutes before the crime increases



Note: Figure shows the percent change in the probability of having a firearm seized from an incident when a car accident happens at some distance (different group colors), and at some time before (left side) and after (right side) a criminal incident..

Figure 1: Firearm recovery and car accidents.

the probability that a firearm is seized from that criminal event. On the contrary, a car accident happening very close to the crime site, less than 200 meters, and right before the incident (within 15 minutes) decreases the probability of recovering a firearm. As discussed above that is unsurprising since the police presence due to the accident likely inhibits certain criminal acts in surrounding areas, especially more risky ones using weapons. Thus we base our instrument, *car_accident*, on an indicator variable equal to one if a car accident occurred 15 to 30 minutes before and from 200 to 2000 kilometers radius of the crime location.[8] Finally, the fact that car accidents that happened a half hour or more before or after the crime do not have sizable effects on the probability of that a firearm is confiscated reassures that the exclusion restriction is likely satisfied.

[8] After estimating the first stage equation stated as in Equation 2, we aggregated the predicted probability by block group and month-year and use the sum of predicted probability as the instrument for seized firearms in Equation 1.

Our instrumental variable estimates identify a local average treatment effect (LATE). It is difficult to determine whether this effect is larger or smaller than the average treatment effect (ATE). The instrument affects firearm seizures that occur contemporaneously with the incident. As a result, we are unable to capture the impact of firearms seized in the days following an incident, such as those recovered during ongoing criminal investigations. Additionally, traffic accidents may not be uniformly distributed across geographic areas or times of day. If the distribution of accidents does not align with the distribution of firearm seizures by law enforcement, then our estimates could deviate further from the average effect.

5 Results

5.1 First Stage Results

Table 2 reports the first-stage results in order to verify the instrument's relevance. Across all specifications from Columns (1) to (3), we find that a car accident shortly before and near a criminal incident increases the probability that a firearm is seized by 0.2 percentage points. Given that the baseline probability of firearm seizure across all incidents is 1.5%, this implies an increase of approximately 12% (0.2/1.5) in the probability of firearm seizure, confirming the the instrument is relevant in predicting firearm seizures.

Table 2 also displays the estimates of other pertinent relationships. In Column (4) shows that the instrument is not a good predictor for firearm seized days after the incident happened. This result supports the interpretation that the effect of car accidents on firearm seizure operates through faster police response at the time of the incident, rather than through other channels. In columns (5) and (6) we see that the instrument's effects on the probability that an arrest is made at the incident scene or later on is close to null and not statistically significant. In Column (7) we see the that car accidents nearby and just before an incident does not alter the probability that the incidence is reported to the police right the way. So the effect on firearm is not due to changes in reporting but likely due to quick

response time by the police.

Table 2: First stage results.

	(1) Firearm Seized	(2) Firearm Seized	(3) Firearm Seized	(4) Firearm days later	(5) Arrest at scene	(6) Arrest later	(7) Reported no delay
Car accidents	0.002**	0.002**	0.002**	0.000	-0.002	-0.003	-0.000
	(0.001)	(0.001)	(0.001)	(0.000)	(0.002)	(0.002)	(0.003)
Arrest			0.026***	0.008***			
			(0.001)	(0.000)			
Firearm seized					0.038***	0.177***	
					(0.006)	(0.006)	
Fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	529,172	529,172	529,172	529,172	529,172	529,172	529,172
R-squared	0.000	0.229	0.233	0.106	0.384	0.187	0.490
Mean dep. var.	0.015	0.015	0.015	0.005	0.099	0.131	0.325

Note: Observations are at the incident level. All estimations include on hour of the day fixed effects, day-month-year and police division fixed effects, the fixed effect of the block where incident occurred, and crime category fixed effects. Standard errors clustered by day-month-year and police division. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5.2 Main Results

Table 3 presents the main estimates about the effect of firearm seizures on subsequent criminal activity at the block group level. Column (1) presents a naive linear regression, which confirms the positive association between firearm seizures and future crime as discussed before. Column (2) introduces lagged crime and the firearm seizure estimate becomes negative. This dramatic change is likely due to the removal of positive bias from the fact that more firearm are seized whenever more criminal incidents happen. Column (3) includes fixed effects and the firearm seizure estimate barely changes, which indicates that most confounding effects are likely dynamic. Once we implement the instrumental variable strategy (Columns 4 to 6), the estimates become even more negative and statistically significant.

The instrumental variable (IV) specification in Column 4 indicates that the removal of one firearm leads to a reduction of 0.65 crimes in the same block group during the following month. Given the average monthly number of crimes per block group is 11.34, this

Table 3: Firearm seizure effect on overall crimes.

Estimation method:	(1) OLS	(2) OLS	(3) FE	(4) FE-2SLS	(5) FE-2SLS	(6) Dynamic panel GMM
Lagged firearm	3.76***	-0.14**	-0.10**	-0.65***	-0.64***	-0.45***
	(0.35)	(0.07)	(0.05)	(0.21)	(0.19)	(0.16)
Lagged crime		0.91***	0.13***	0.14***	0.14***	0.41
		(0.01)	(0.03)	(0.03)	(0.04)	(0.70)
Lagged arrest					-0.00	-0.31
					(0.04)	(0.61)
Kleibergen-Paap F				1981.5	1760.9	
Division x Month-Year	No	No	Yes	Yes	Yes	Yes
Block Group x Year	No	No	Yes	Yes	Yes	-
Observations	45,429	45,429	45,429	45,429	45,429	45,429

Note: Observations are by block group and month-year. Standard errors clustered by month-year and police division. OLS stands for Ordinary Least Squares, FE stands for the fixed effects model. FE-2SLS stands for Fixed Effects Two-Stage Least Squares. GMM stands for Generalized Method of Moments. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

corresponds to a 6% decrease in overall crimes. Moreover, the Kleibergen-Paap F-statistic indicate strong instrument relevance.

A potential concern is that the effects attributed to firearm seizures may instead reflect incapacitation due to arrests. To address this, in Column (5) lagged arrest is added as control. This does not materially alter the IV estimates, which remain large and significant, providing evidence that the effect operates through the removal of the firearm itself rather than through offenders' incapacitation.

Finally in Column (6) we present results using system generalized method of moments estimator (Roodman, 2009). This method is based on the seminal work by Arellano and Bond (1991) and uses past lags of the explanatory variables in Equation 1 as instruments. The advantage is that, besides firearm seizure, the other potentially endogenous regressors in Equation 1 – lagged crime and lagged arrests – are also instrumented. The point estimate for the effect of firearm seizure on crimes decreases but remains robust to the use of dynamic instruments, which reinforces the validity of our approach.

5.3 Heterogeneous Effects by Crime Category

We next examine whether the impact of firearm seizure differs across crime categories. Table 4 reports both fixed effects and IV estimates for a broad category of offenses. IV estimates show particularly strong negative effects on property crimes (-4.9%), theft (-8.1%), and weapons violations (-92.3%). Effects on violent crime are negative but not statistically significant. Some categories, such as drug offenses and motor vehicle theft, exhibit positive but not large or statistically significant estimates. These heterogeneous results suggest that firearm seizures may deter certain types of opportunistic or firearm-intensive crimes, but not others, possibly due to variation in substitution opportunities or weapon accessibility.

Table 4: Firearm seizure effect on local crimes – results by crime category.

Dep. Variable	FE	FE-2SLS	Dep. Variable	FE	FE-2SLS
Violent crimes	-0.002	-0.013	Homicide	-0.001	-0.001
	-0.5%	-3.0%		-10.7%	-16.1%
Property crimes	-0.050**	-0.226**	MV theft	-0.001	0.030
	-1.1%	-4.9%		-0.2%	6.9%
Crimes at residences	-0.014	-0.068	Others	-0.025	-0.252**
	-0.3%	-1.7%		-1.0%	-10.3%
Assault	0.005	-0.018	Suicide	-0.001	0.002
	0.3%	-1.2%		-6.1%	15.8%
Burglary	-0.006	-0.006	Robbery	0.001	-0.021
	-0.5%	-0.5%		0.7%	-9.8%
Drugs	-0.003	0.035	Sex crimes	0.001	-0.001
	-0.2%	3.1%		1.0%	-1.5%
DTP	-0.001	-0.016*	Theft	-0.044*	-0.249***
	-3.0%	-33.6%		-1.4%	-8.1%
DUI	-0.000	0.001	Vandalism	-0.005	-0.015
	-17.9%	88.8%		-0.7%	-1.9%
Fraud	-0.004	-0.085*	Weapon Viol.	-0.002	-0.042***
	-1.1%	-20.6%		-4.9%	-92.3%

Note: Observations are by block group and month-year. All estimation includes block group and year fixed effects and month-year and police division fixed effects. Standard errors clustered by month-year and police division. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5.4 The Localized Effect of Firearm Seizure

Table 5: Firearm seizure effect on crimes – data at census block level.

Dep. Variable:	Crime at Residences		Homicide		Suicide	
Estimation:	FE	FE-2SLS	FE	FE-2SLS	FE	FE-2SLS
Lagged firearm	-0.016**	-0.075***	-0.001**	-0.003**	-0.001**	-0.001
	(0.008)	(0.027)	(0.000)	(0.001)	(0.000)	(0.001)
Lagged arrest	-0.007	-0.003	-0.000	0.000	-0.000	-0.000
	(0.005)	(0.005)	(0.000)	(0.000)	(0.000)	(0.000)
Lagged crime	0.002	0.002	0.000	0.000	0.000***	0.000***
	(0.006)	(0.006)	(0.000)	(0.000)	(0.000)	(0.000)
Average	0.278		0.001		0.001	
Coeff./Average	-6%	-27%	-138%	-320%	-81%	-51%

Note: Observations are by block and month-year. Standard errors clustered by month-year and police division. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

In this section, we present results using data at the block level. The goal is to examine the highly localized effect of seizing a firearm in a specific block on future crimes occurring within the same block. By doing so, we aim to infer and proxy the potential impact of removing firearms from contexts where offender and victims have some type of connection.

Table 5 reports the results for selected crime categories. In particular, we find that the seizure of one firearm from a given block reduces crimes at residences in that block by 0.075 incidents in the following month, representing a 27% decline. The estimated effect on homicides is quite substantial, with firearm seizures reducing homicide incidents by nearly three (-0.003/0.001) times the block-level average.

The fact that the IV estimate for suicides is negative but not statistically significant lends credibility to our identification strategy. Suicides are less likely to involve firearms in the context to be seized by law enforcement, suggesting that our identification strategy is more precise in isolating effects related to criminal activity rather than broader firearm use.

These findings have important policy implications. The strong localized reduction in crime, particularly in homicides, following firearm seizures suggests that targeted removal of firearms from high-risk environments can meaningfully enhance public safety. This supports

the rationale behind red flag laws, which allow law enforcement to remove firearms from individuals deemed to pose a danger to themselves or others.

6 Discussion and Conclusion

This paper investigates the causal impact of firearm seizures on subsequent criminal activity using rich administrative data from Louisville, KY, between 2010 and 2017. Leveraging a novel instrumental variable strategy based on the quasi-random placement of police officers following nearby traffic accidents, we isolate exogenous variation in the likelihood of firearm seizures. Our results show that the removal of a single firearm leads to meaningful reductions in both total crime and specific categories such as property crimes, theft, and weapons violations. Notably, we also document strong localized effects, including a decline in residential crimes and a substantial reduction in homicides at the block level.

While national gun control legislation often encounters political resistance, our results highlight the potential of more targeted, enforcement-based strategies. The demonstrated crime-reducing effects of firearm removal provide empirical support for red flag laws and similar interventions that authorize law enforcement to temporarily confiscate weapons from individuals deemed high risk. These policies may be particularly valuable in urban settings where criminal activities are concentrated and where even small-scale enforcement actions can yield measurable reductions in violence.

Future research should aim to measure the medium- and long-run effects of firearm seizures. In settings where the gun market is highly dynamic and firearms are easily replaced, the initial crime-reducing impact of a seizure may diminish over time. Our estimates capture short-run effects and may not fully reflect these longer-term dynamics. Additionally, the external validity of our findings is context-dependent. Kentucky's relatively less constraining gun laws, may influence both the mechanisms and magnitude of the effects we observe. The relationship between firearm seizures and crime could differ substantially in

jurisdictions with stricter gun regulations or different patterns of law enforcement behavior. Understanding how these effects generalize across legal and institutional contexts remains an important avenue for future work.

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