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Police safety, killings by the police, and the militarization of US law enforcement

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A B S T R A C T

The debate over police use of military equipment often revolves around the supposed tradeoff between increasing police safety and reducing killings by the police. In this paper, I rely on institutional features that exogenously determine the distribution of military equipment to US police departments to show that, contrary to previous evidence, there is no such tradeoff: police militarization increases killings by the police and *reduces* police safety. Each year police militarization results in 64 additional killings by the police, 12,440 police officer assaults, and 2653 police officer injuries.

1. Introduction

Over 1000 people in the US are killed by law enforcement agents each year, with per capita rates that are over 15 times higher than those found in most Western countries.[1] These killings are responsible for approximately 8% of all homicides with adult male victims and are especially concentrated in central urban areas (Edwards et al., 2018). In 2014, after a series of high-profile killings by the police, the Obama Administration and Congress started reviewing the equipment and practices used by police departments.[2] As a result, President Obama signed an executive order reducing the ability of police departments to acquire and use military equipment. In July 2016, the death of 8 police officers in Dallas and Baton Rouge reignited the debate on police militarization, with critics of President Obama's executive order claiming that militarization is needed for police safety. In line with this argument, President Trump revoked President Obama's executive order in August 2017.[3] In 2020, nationwide protests against the use of force by the police have sparked a reexamination of police militarization.[4]

The extensive policy debate surrounding the supposed tradeoff between police safety and killings by the police when making police militarization decisions has recently sparked substantial academic interest. One strand of the literature intends to estimate the effects of militarization by exploiting the panel structure of the data to control for time-invariant unobservables. Carriere and Encinosa (2017) shows that US states that received more military equipment experienced a decrease in assaults on the police and that this relationship goes in the opposite direction when studying operational militarized items (e.g., sonar or radar). Delehanty et al. (2017) at the county level and Lawson (2018) at the police department level both find a positive relationship between killings by the police and military equipment transferred by federal programs. Mummolo (2018) compares law enforcement agencies before and after receiving a special weapons and tactics team and finds no relationship with officer safety. While controlling for time-invariant characteristics, these studies may still produce biased estimates if militarization decisions are not random. For example, this may be due to police departments getting militarized in response to changes in crime (Ramey and Steidley, 2018).

To solve this issue, another strand of the literature uses an instrumental variable approach that attempts to eliminate biases caused by unobservables that vary over time within a county. Among other results, Bove and Gavrilova (2017) show that counties that tend to receive more military aid experienced no changes in either police safety or the use of deadly force by the police. Harris et al. (2017) show instead show that militarization has no effect on killings by the police and that it increases

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[1] In most years, the rate of killings by the police in the US is over 3 per million. In comparison, this same rate in other Western countries is: Australia, 0.17 (Australian Institute of Criminology, 2013); Denmark, 0.19 (Politi, 2016); England and Wales, 0.05 (Home Office, 2015); France, 0.15 (A Toutes les Victimes, 2016); Germany, 0.10 (DH Pol, 2018); and the Netherlands, 0.20 (NOS, 2016).

[2] The Senate Committee on Homeland Security and Governmental Affairs held a hearing called "Oversight of Federal Programs for Equipping State and Local Law Enforcement." The House of Representatives Armed Services Committee held another hearing called "The Department of Defense Excess Property Program in Support of US Law Enforcement Agencies." As a consequence, President Obama initiated the "President's Task Force on 21st Century Policing" Presidential Report (2015).

[3] The title of President Trump's executive order left no doubt about its motive: "Restoring State, Tribal, and Local Law Enforcement's Access to Life-Saving Equipment and Resources."

[4] In June 2020, a bill named "Stop Militarizing Law Enforcement Act" was introduced in the Senate to establish new limitations on the distribution of military equipment to law enforcement agencies. The Senate rejected the bill by two votes. During the presidential campaign, President Biden has stated that law enforcement does not need surplus military equipment and that he will reinstate Obama-era regulations via executive order.

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police safety. Gunderson et al. (2020) using new data at the police department level did not manage to replicate some of the results found in the aforementioned studies. These differences may be due to an ecological fallacy caused by the difference between the treated unit (i.e., police department) and the level at which the analysis was conducted (i.e., county). Steidley and Ramey (2019) express similar concerns regarding Bove and Gavrilova (2017) and Harris et al. (2017), stating that "the effect of military equipment is weak and causality remains questionable."

I contribute to this debate by improving on previous instrumental variable estimations in three major dimensions. First, I show that previous estimates may be contaminated by factors that simultaneously determine killings by the police, police safety, and militarization decisions. I then propose a new identification strategy to overcome this problem. Second, in order to avoid the ecological fallacy highlighted by Gunderson et al. (2020), I employ data at the police department level. Third, I build a new dataset on killings by the police based on non-governmental information to bypass some well-known problems with official data. Each of these innovations has important implications for the estimated signs and magnitudes of the effects of militarization on police safety and killings by the police. Upon incorporating all of these innovations, I show that police militarization increases killings by the police and reduces police safety.

To estimate the effects of militarization on police safety and police killings I rely on exogenous differences in police militarization determined by institutional features of the 1033 Program; the main federal program that redistributes military equipment to police departments across the US.[5] Although this program was created in 1996, it was practically inactive until 2009. Withdrawal from the wars in Afghanistan and Iraq in 2010 led to the arrival of vast quantities of military equipment in the US. Much of this equipment, now unused by the military, has been redistributed to police departments via the 1033 Program.[6]

Similar to Harris et al. (2017), I notice that military equipment redistributed by the 1033 Program is made available at storage facilities located at certain military bases. While police departments do not have to pay for the items themselves, because of various institutional features, the effective cost of an item increases with distance to the storage facility housing it. Due to these costs, I expect that, when items became available after withdrawal from the aforementioned wars, police departments closer to these storage facilities became more militarized.

I use this differential change in militarization, combined with the fact that only some military bases were selected as storage facilities, to exogenously predict militarization. I use this as a first stage in an instrumental variable setting to then estimate the effects of militarization. With this identification strategy I compare changes in the outcomes of police departments equally distant to a military base, but that differ in terms of their distance from a 1033 Program storage facility. This identification strategy has been previously used in Masera (2021) to study the effects of the militarization of the police through the 1033 Program on violent crime. The identifying assumption is that changes in the outcomes of interest between police departments close to a military base with or without a 1033 Program storage facility would have been the same had it not been for their differential militarization. In this regard, I show that for the purposes of the instrumental variable estimation, the selection of military bases can be considered as good as random. More specifically, I demonstrate that police departments close to 1033 Program storage facilities and those close to other military bases were similar in terms of levels and trends of many observables before the withdrawal from the wars.

I then construct a new panel dataset on killings by the police at the police department level using the Fatal Encounters database (www.fatalencounters.org). This information allows me to solve the non-random reporting and systematic underrepresentation of this phenomenon observed in official datasets. I use this information to show that militarization increases killings by the police with a firearm, while it does not affect other types of killings. The estimates indicate that after 2010, 8.4% of the killings by firearm were due to the militarization of the police, which is, equivalent to an additional 64 killings by the police each year. The effects of militarization on killings by the police estimated here are in line with the arguments proposed by Lawson (2018) and Delehanty et al. (2017), who claim that militarization may increase killings by the police by making errors committed by the police more likely to be fatal and by generating a more aggressive culture within police departments.

I then use the FBI's "Law Enforcement Officers Killed and Assaulted" (LEOKA) dataset to show that the militarization of the police has reduced the safety of police officers. The estimates imply that, since the start of the withdrawal from the wars, militarization has caused 15.9% of the assaults and 12.8% of the injuries of police officers. In this period, militarization resulted in an extra 12,440 assaults and 2653 injuries each year. Additionally, I show that police militarization has increased the probability of an officer being killed by 5.3%. In line with the evidence that military equipment is often being used in disputes with armed and barricaded suspects (Kraska, 2007) and in drug raids (American Civil Liberties Union, 2014), I find that militarization only has a negative effect on the safety of police officers when they are answering a disturbance call, stopping a robbery or burglary, or engaging in a drug raid.

These results contribute to the literature on the effects of police militarization that go beyond police safety and killings by the police. Another focus of the literature has been the study of the crime-reducing effect of police militarization. For example, Bove and Gavrilova (2017) and Harris et al. (2017) show that militarization reduces many types of crime. Masera (2021) confirms these results and finds that while violent crime decreases in areas that become militarized, part of this reduction is due to the relocation of crime to neighboring areas. With this paper, I highlight that the gains from crime reduction can come at the cost of reduced police safety and an increase in killings by the police. Thus, policy makers' decisions regarding the use of military equipment by the police should be informed by this tradeoff.

2. Institutional background and data

2.1. The 1033 Program and police safety

On September 23, 1996, President Clinton signed into law the creation of the 1033 Program. This program makes military equipment deemed no longer useful for military operations by the Department of Defense available to police departments. The types of items distributed by the program include military vehicles, weapons, and equipment. If a police department is interested in acquiring an item, an official must visit the storage facility where the item is located and, after inspecting it, place a request. If the request is approved, the police department is required to cover all transportation and potential repair costs, but does not pay for the item itself.[7] A total of 69 storage facilities in the contiguous US participate in the 1033 Program. The Defense and Logistics Agency provides information about the type of item transferred, its market value, the date of the transfer, and the name of the receiving police department for all military equipment transferred through the 1033 Program. During the period of analysis, a total of 8000 police departments

[5] The 1033 Program is the primary federal program that allows local enforcement agencies to acquire military equipment in the US. Alternatively, local law enforcement agencies may buy military weapons using their finances or acquire them through the Department of Homeland Security grants.

[6] The analysis is confined to the lowest level of law enforcement in the US. There are more than 16,000 agencies at this level. More than 80% of these agencies are police departments. Other types of agencies include sheriff's departments, tribal agencies, and special district agencies. For simplicity, throughout the paper, I refer to all of these agencies as police departments.

[7] After the withdrawal from the wars, requests for military equipment were rarely denied given their high availability during this period (US House of Representatives hearing held on November 13, 2014).

received more than US$1 billion worth of military items through the 1033 Program. Using the name of the enforcement agency, I match this data with the Uniform Crime Reporting dataset produced by the FBI, which contains information on all police departments in the US, including data on violent and property crimes, population, and the number of officers.[8] I then establish the value of the stock of military equipment available to a police department at any point in time by summing the value (in US dollars) of the military equipment received by that police department to date.[9] The baseline measure of militarization used is the value of the stock of military equipment divided by the population under the jurisdiction of the police department.[10] The 1033 Program was practically inactive for the first 10 years, when only 130 million dollars worth of military equipment were distributed between 1997 and 2009. After withdrawal from the Iraqi and Afghan wars in late 2009, the 1033 Program began redistributing large amounts of equipment. Between 2010 and the end of the analysis in 2014, the 1033 Program distributed an average of 226 million dollars of military equipment each year.[11] This is a considerably large inflow of resources for police departments. For example, the scale of this program is similar to the amount of funding delivered by the American Recovery and Reinvestment Act for hiring new police officers, which was by far the largest police-hiring scheme in the last 20 years (James, 2011).[12]

Finally, to measure police safety, I link this information with the FBI's LEOKA dataset. This dataset provides different measures of police safety for a subsample of police departments that serves approximately 60% of the US population.

2.2. Data on killings by the police

Although the use of deadly force by the police has ignited public interest, official data sources are incomplete and unreliable. These official statistics are provided by the FBI supplementary homicides report and the Department of Justice (DoJ)' s arrest-related-death dataset. Unfortunately, agencies are not required to communicate to the FBI or the DoJ about killings by the police. Therefore, any aggregate information is severely underreported. One important factor to consider when using these datasets is that this underreporting could be non-random because agencies can strategically decide not to submit a report in years when these types of homicides are high. Notably, the extent of these issues have been described by various studies (Fyfe, 2002; Michael et al., 2015). For example, a recent report commissioned by the DoJ (Banks et al., 2015) shows that at best, these official sources have captured approximately half of all killings by the police, with considerable variability in quality between states. These issues have also been highlighted by the President's Task Force on 21st Century Policing (Presidential Report, 2015) which recommends that future policies "should also require agencies to collect, maintain and report data to the Federal Government on all officer-involved shootings." Additionally, the Task Force noted that future policies should "also mandate external and independent criminal investigations in cases of police use of force resulting in death." When taken together, these two recommendations would generate comprehensive and reliable data.

Given the lack of data available from law enforcement agencies, researchers have attempted to infer killings by the police using other official datasets. One commonly used dataset comes from the National Vital Statistics System of the CDC. In particular, researchers have used the fact that one of the possible causes of death provided in this dataset is "homicide caused by law enforcement officer." As originally highlighted by Sherman and Langworthy (1979) and later by Barber et al. (2016), this data severely underestimates these types of death given they are only coded as caused by a law enforcement officer when it is explicitly mentioned in the death certificate. However, this type of information is not provided in most death certificates. Additionally, by construction, the death counts provided by the National Vital Statistics System are based on the place of residence, not the place where the death occurred. For the present study, I would need to be able to identify the agency responsible for the death; however this information is not available in the National Vital Statistics System.

Due to the unreliability of official data, many non-governmental organizations have created alternative data sources on killings by the police. Even the head of the FBI in 2015, James Comey, admitted that unofficial data are the best source of information on this topic.[13] The first of these efforts was conducted by the "Washington Post" that focused on the 51 largest law enforcement agencies and collected data on the universe of police killings between 1990 to 2000. Following increased attention regarding killings by the police in 2014, many other projects were created. Media outlets such as "The Guardian", "The Wall Street Journal", and "FiveThirtyEight" generated datasets for the years 2015 and 2016 based on media mentions of these events. Additionally, a still ongoing dataset is maintained by "Killed by the Police" (killedbythepolice.net). Other crowd-sourced projects such as "Fatal Encounters" (www.fatalencounters.org) and another by the media outlet "Deadspin" have also attempted to generate the same type of dataset for the years prior to 2015.

The creation of these non-governmental databases raised questions regarding the credibility of these types of data. Finch et al. (2019) tries to evaluate the accuracy and reliability of the "Fatal Encounters" database and conclude that "the best option to date may be the collection of all police-related-death data, as is done in Fatal Encounters". To assess the completeness of the "Fatal Encounters" database, Finch et al. (2019) compared it to a random sample of full records from 328 law enforcement agencies in 11 states. They "found that Fatal Encounters data are fully complete for 9 of the states sampled. Data are missing for 1 incident in CT (92% complete) and 8 in FL (95%)". Similarly, Ozkan et al. (2018) compares "Deadspin" and "Fatal Encounters" datasets to the official data from the Dallas police department, which claims to contain all killings by the police. The "Fatal Encounters" dataset contained 71 of the 74 killings by the police between 2003 and 2016, while the "Deadspin" dataset was more unreliable and contained only 13 of the 24 killings by the police between 2012 and 2014.

Therefore, in this paper, I use the "Fatal Encounters" database which appears to be the most reliable database for killings by the police with data available prior to 2015. This data has also been used by Edwards et al. (2018) to measure the impact of police killings on mortality risk. One other advantage of this dataset is that it allows me to distinguish between the different ways people may die when interacting with the police. In fact, not all deaths that occur during interactions with the police are considered killings by the police. For example, 8%

[8] Matched police departments cover 98% of the contiguous US population.

[9] Nearly 8000 types of items are delivered to police departments by the 1033 Program. By 2014, military vehicles represented 55% of the total value transferred by the 1033 Program, while 40% were military weapons and equipment such as assault rifles, night-vision goggles, and body armor. The remaining equipment transferred were non-tactical items such as computers, office items, and gym equipment. More details on the type of items and how I categorize them can be found in Appendix 10.

[10] It is plausible that the value of an item received by a police department may decrease over time. I take into account the devaluation of the items in the Appendix 5, where I further show that the results are robust to measuring militarization per officer instead of per capita.

[11] Given that in 2015, Executive Order 13688 introduced many legislative changes to the 1033 Program that may influence the identification strategy, my analysis considers only data up to 2014.

[12] Appendix 2 reports the full time series of military equipment acquired by police departments and the timing of US military withdrawals. While the militarization process started after 2009, the last two years in the sample (2013, 2014) are the one with more intense militarization. Additionally, Appendix 1 includes summary statistics of the main variables of interest.

[13] Declaration by James Comey, October 7, 2015 at the Summit on Violent Crime Reduction in Washington, D.C.

of deaths during interactions with the police are suicides. Such deaths most commonly occur in domestic disputes or when the suspect has no way to escape. Another 8% of deaths are due to accidents caused by the deceased. These include deaths from drowning or falling from a height when attempting to flee the police. Other types of accidents include medical emergencies, which are often caused by drug overdose. Once suicides and accidents have been excluded, I define the remaining deaths that occurred when interacting with the police as killings by the police.

The data shows that killings by the police have increased rapidly in recent years, rising from 551 in the year 2000, to 809 in 2010, and finally reaching a peak of 1197 in 2013. This increase is even more surprising when compared to the general decrease in violent crimes. In the year 2000, there were approximately 15.7 violent crimes per 1000 inhabitants. By 2014, the incidence rate had substantially decreased to 12 violent crimes per 1000 inhabitants. The numbers reported by Fatal Encounters align with the efforts of other non-governmental organizations. For instance, "The Guardian" reported that the police killed 1146 people in the US in 2015 and 1093 in 2016. Similar numbers have been estimated by the database "Killed by Police" and "FiveThirtyEight".

3. Econometric framework

The effect of the militarization of the police on an outcome of interest $y_{i,t}$ can be described by following equation:

$$y_{i,t} = \alpha_i + \alpha_t + \beta_1 mil_{i,t} + \beta X_{i,t} + \epsilon_{i,t} \tag{1}$$

where i identifies the police department, t the year, and $y_{i,t}$ is either a measure of killings by the police or police safety. $mil_{i,t}$ is the dollar value of the stock of military equipment divided by the jurisdiction population. Many unobservables that simultaneously determine militarization and the outcomes of interest render the identification of β_1 problematic. As shown in Appendix 4.1, the distribution of militarization is not random across the US. In particular, police departments in rural counties, in Republican states with high percentages of white citizens and high levels of unemployment are more militarized.

To overcome these problems and uncover the causal effect of militarization, I exploit two features of the 1033 Program. First, I use the timing and speed of the withdrawal from the Iraqi and Afghan wars. Starting in 2010, the US significantly decreased its military presence in Iraq with the same occurring in Afghanistan just a few years later. At the peak of military engagement, the US had almost 200,000 soldiers on the ground in these two countries. By 2014, fewer than 50,000 soldiers were deployed. This withdrawal led to the return of massive amounts of military equipment to the US. Some of this equipment, now unused, is made available to police departments through the 1033 Program.

Second, I use the fact that the effective cost of an item acquired through the 1033 Program is increasing with the distance to its storage facility. This happens because, while a police department does not have to pay for the item itself, they are responsible for the cost of the special transportation required to deliver the item from the storage facility. Moreover, items transferred via the 1033 Program may require repairs, the charges for which are also incurred by the police department. Screening is therefore essential for police departments to avoid or limit these expenses. The screening process is also rapid, lasting by law a maximum of 21 days. Thus, being able to rapidly inspect any military equipment of interest is of fundamental importance for a police department. Therefore, I expect that when items become available through the 1033 Program, police departments close to these storage facilities should become more militarized.

The main threat to using distance to storage facilities to exogenously predict the militarization of a police department is that these police departments, by definition, are also close to military bases. Because of this, even before the massive influx of military equipment to the US, police departments close to these storage facilities could have been different from other departments. Indeed, these police departments were less likely to be headed by a sheriff, the jurisdiction they cover is more populated, and they have a higher prevalence of violence, crime, and arrests.[14] Additionally, police departments close to military bases may differ from those situated further away due to shocks directly caused by the withdrawal from the wars in Afghanistan and Iraq. For example, numerous studies have shown that conscription into the military and exposure to combat situations can have subsequent effects on violent and criminal behavior (Anderson and Rees, 2015; Galiani et al., 2011; Rohlfs, 2010; Sreenivasan et al., 2013). This fact is particularly relevant for the analysis given that it is common for police departments to hire veterans.

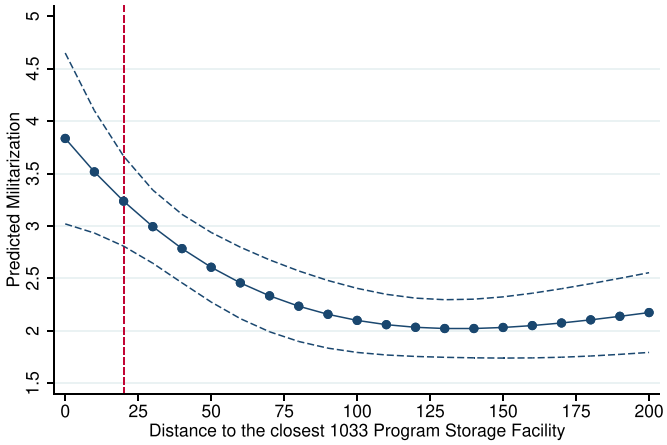

Fig. 1. The effect of distance to a 1033 Program storage facility on militarization. The vertical line indicates 20 km.

To solve this issue, as a control group, I use police departments that are close to a military base without a 1033 Program storage facility. Given that police departments in both the control and the treated groups are close to a military base, any difference in unobservables between police departments close and far away from a military base should not bias the estimates. The identifying assumption of this new empirical strategy is that police departments close to storage facilities are similar to the control group, except for the fact that they enjoy cheaper access to military equipment once withdrawal from the wars began.

In Section 5, I validate this identification strategy by showing that before 2010, police departments close to 1033 Program storage facilities and those close to other military bases (without such storage facilities) were similar in terms of the trends and levels of many observables. This identification strategy has been used in Masera (2021) to study the effects of police militarization on violent crime. This paper has already shown that when the outcome of interest is violent crime, the identification assumptions are likely to hold. Due to the different outcomes of interest in this paper, it is also important to show that the level of police safety and the number of killings by the police did not differ before the massive arrival of military equipment when comparing "Treated" and "Control" police departments.

Formally, I estimate the following two stages in an instrumental variable setting:

$$\underline{FirstStage:}$$

$$mil_{i,t} = \delta_i + \delta_t + \gamma_1 eqp_t * closeness(storage)_i + \gamma_{2,t} 1(year = t) * closeness(mil.base)_i + \epsilon_{i,t} \tag{2a}$$

$$\underline{SecondStage:}$$

$$y_{i,t} = \alpha_i + \alpha_t + \beta_1 mil_{i,t} + \beta_{2,t} 1(year = t) * closeness(mil.base)_i + \gamma_{i,t} \tag{2b}$$

[14] A comparison between police departments close to and far away from the storage facilities of the 1033 Program is reported in Appendix 4.2.

The function $closeness(.)$ can be calculated in many ways by using information on the locations of police departments, 1033 Program storage facilities, and military bases. As a baseline, I use a dummy variable indicating whether the police department is located within 20 km of a storage facility (or military base). This distance was selected because it generates the dummy variable with the highest predictive power in the first stage. I demonstrate that the results are robust to defining a police department as close if it is located within a 25 or 30 km radius from a storage facility (or military base). All results are also robust to continuous measures of closeness as the natural logarithm and a fourth-degree polynomial of distance. The variable eqp_t measures the availability of excess military equipment as proxied by the difference between the boots on the ground in Afghanistan and Iraq at time t and the peak reached in 2009. The measure eqp_t is then normalized to 1 in 2014. eqp_t is formally defined below, where $boots_t$ is the number of boots on the ground that the US military deployed to Iraq and Afghanistan:[15]

$$eqp_t = \begin{cases} 0 & if\ t < 2010 \\ \frac{boot_{2009} - boots_t}{boots_{2009} - boots_{2014}} & if\ t \geq 2010 \end{cases}$$

4. Results

4.1. First stage

In this section, I present the relevance of the proposed instrumental variable. To determine this, I first estimate the following equation:

$$mil_{i,t} = \alpha_i + eqp_t * f_1(distance\ storage_i) + eqp_t * f_2(distance\ mil.base_i) + \epsilon_{i,t} \quad (3)$$

where $f_1(.)$ and $f_2(.)$ are fourth-order polynomials of the distance to the closest storage facility and military base, respectively.[16] Fig. 1 plots the predicted militarization resulting from the estimation of Eq. (3) as a function of distance to the closest storage facility. The predicted value is calculated at the mean of all other regressors. Since Eq. (3) includes a flexible function of the distance to a military base ($f_2(.)$), the function displayed in Fig. 1 should be interpreted as the relationship between the distance to a 1033 Program storage facility and militarization after controlling for the distance to a military base.

The results presented in Fig. 1 show that independent of the distance to a storage facility, militarization increased after 2009. We also observe that distance to a 1033 Program storage facility has a negative effect on militarization. While police departments extremely close to a storage facility increased their militarization by nearly US$4 per capita, those located far away from a storage facility increased by only half of that amount. This is in line with the fact that the effective cost of an item increases with distance to a storage facility. Finally, the relationship between distance to a storage facility and militarization is non-linear. After 100 km, the distance to a 1033 Program storage facility appears to have no effect on the decision to militarize.

Furthermore, Fig. 1 shows that police departments to the left of the vertical line (i.e., those less than 20 km from a 1033 Program storage facility) increased their militarization by on average US$3.5 per capita. Conversely, police departments to the right of the vertical line (i.e., more than 20 Km from a 1033 Program storage facility) show a less striking increase. The baseline estimation of the first stage parameter γ_1 in Eq. (2a) uses the difference in the increase in militarization between these two groups of police departments. This estimate is reported in Column (1) of Table 1. The militarization of police departments less than 20 Km from a military base with a 1033 Program storage facility increased by US$1.76 per capita more than for police departments located at less than 20 Km from a military base without a storage facility. The F-statistic of the excluded instrument $eqp_t * 1(storage < 20\,\text{Km})_i$ is 54. Table 1 shows that the first stage is robust to different definitions of closeness. In Columns (2) and (3), police departments at less than 25 Km and 30 Km from a military base with a 1033 Program storage facility are considered close. In Column (4), closeness is measured with the logarithmic distance. This function mimics the non-linearities displayed in Fig. 1. This estimate implies that every doubling of the distance to a storage facility reduces militarization by US$0.53 cents per capita.[17]

4.2. Killings by the police

Critics of police militarization have claimed that the latter increases the use of deadly force by the police by affecting the types of operations that police officers are deployed in, making errors committed by the police more likely to be fatal, and generating a more aggressive culture within police departments. In line with these arguments, the American Civil Liberties Union (2014) examined police operations that used military equipment and reported that "these tactics and equipment often increased the risk of property damage and bodily harm." Building on the work of Kraska (2007), Delehanty et al. (2017) argues that military equipment generates more violent policing "because the equipment leads to a culture of militarization". Similarly, Lawson (2018) poses that militarization leads to a "psychological transformation within police departments — and officers — that shifts behavior toward lethal force as a more acceptable and earlier response".

To test whether this is the case, in the first three columns of Table 2 I estimate the causal effect of militarization on different types of killings by the police. These estimates show that militarization has a large positive effect on killings by the police involving a firearm. For every extra dollar in militarization per capita, the rate of killings by the police increases by 0.76 per million inhabitants. This is a substantial effect given that the rate of killings by the police involving a firearm rate was 2.7 per million in 2014.

To appreciate the size of this result, I use this estimate to create a counterfactual where no military equipment was ever distributed by the 1033 Program. The counterfactual implies that, since 2010, killings by the police involving a firearm increased by 8.4% due to the militarization of the police. This estimate implies that each year an additional 64 killings by the police are caused by militarization. Since its inception in 1997, the 1033 Program has resulted in a total of 416 additional killings by the police involving firearms. Using conservative estimates of the statistical value of life in the US, the total cost of lives lost amounts

[15] In Appendix 5.5, I show that results are robust to use the total amount of military equipment distributed in the US by the 1033 program that year instead of eqp_t. Additionally, different types of military equipment may have substantially different effects on police safety and killings by the police. The identification strategy used in this paper is not appropriate to study this heterogeneity as it uses an exogenous shifter in the cost and availability of an average military item. Therefore focusing only on one type of military equipment may invalidate the exclusion restriction. With that caveat in mind, in Appendix 10, I show that all results are robust to focusing only on tactical military equipment. I also show that non-tactical items (such as furniture, office supplies, etc...) do not drive the results as the first stage on this type of items is very weak.

[16] It is important to note that Eq. (3) does not include time fixed effects. This is because I want to estimate the functions $f_1(.)$ and $f_2(.)$, which are fourth-order polynomials and thus include a constant. This constant multiplied by $eqpt_t$ would be collinear to the time fixed-effects. For comparability, I show that all of the baseline results are robust to not including year fixed effects but only including $eqpt_t$ (See Appendix 5.4).

[17] In Appendix 10, I estimate a first stage where the outcome of interest is not all military equipment but only certain specific categories. The F-statistics are large when focusing only on military weapons and other tactical equipment. However, the F-statistics are approximately 10 when focusing on military vehicles and lower than 5 when focusing on non-tactical equipment. Notably, it is not appropriate to use this identification strategy in an instrumental variable setting where the endogenous variable in the second stage is only one type of military equipment since this would likely violate the exclusion restriction. In the Appendix I replicate Table 1 for the sample of police departments with data on police safety.

Table 1
Robustness definition of closeness.

	(1)	(2)	(3)	(4)	(5)
	<20Km	<25Km	<30Km	ln(Dist.)	4th Order
eqp× Closeness(Storage)	1.759***	1.292***	1.197***	-0.532***	Figure 1
	(0.455)	(0.408)	(0.382)	(0.116)	
Observations	115,536	115,536	115,536	115,536	115,536
Year FE	Yes	Yes	Yes	Yes	Yes
Police Dep. FE	Yes	Yes	Yes	Yes	Yes
Year FE× Closeness(Milbase)	Yes	Yes	Yes	Yes	Yes
F-stat	53.94	35.74	35.84	45.13	19.19

The table reports the first stage estimates for various definition of closeness. In all columns clustered standard errors at the Commuting Zone * Year level are reported in brackets. Regressions are weighted by population. The sample includes all police departments yearly data from 2007 to 2014. The dependent variable is militarization in dollars per capita. Column (1), (2) and (3) measure closeness with a dummy variable that is equal to 1 if the distance in Km is less than 20, 25 and 30 respectively. Column (4) measures closeness with natural logarithm of the distance. Column (5) uses a 4th degree polynomial. * *p-value<0.10,* ** *p-value<0.05,* *** *p-value<0.01*

Table 2
Killings by the Police.

	(1) Killings Firearm	(2) Killings Vehicle	(3) Killings Other	(4) Accidents	(5) Suicides
Militarization per Capita	0.761**	-0.251	0.00833	-0.0409	0.0590
	(0.340)	(0.183)	(0.0336)	(0.0822)	(0.0935)
Observations	115,558	115,558	115,558	115,558	115,558
Year FE	Yes	Yes	Yes	Yes	Yes
Police Dep. FE	Yes	Yes	Yes	Yes	Yes
Year FE×1(Milbase<20Km)	Yes	Yes	Yes	Yes	Yes
F-stat	53.95	53.95	53.95	53.95	53.95

The table reports instrumental variable estimates. In all columns clustered standard errors at the Commuting Zone * Year level are reported in brackets. Regressions are weighted by population. The sample includes all police departments yearly data from 2007 to 2014. From column (1) to (3) the dependent variables are different types of killings by the police per million citizens. Accidental deaths and suicides rate when interacting with the police are correspondingly the dependent variable in columns (4) and (5). * *p-value<0.10,* ** *p-value<0.05,* *** *p-value<0.01.*

to US$3.3 billion.[18] Notably, deaths caused by a vehicle or other uses of deadly force were not affected by militarization.

These results are in line with Kraska (2007) and the American Civil Liberties Union (2014), which report that military equipment is usually used in hostage, barricade, and active shooter scenarios, as well as in drug raids. These types of operations may end up creating situations in which officers use deadly force with firearms. Additionally, it is important to note that even if some of the military equipment received by police departments are vehicles, we should not expect militarization to have an effect on vehicle-related killings by the police. This is because most vehicle-related killings by the police occur during a police chase and rarely involve military vehicles. Vehicles transferred through the 1033 Program are mine-resistant armored vehicles that are heavy, slow, and difficult to maneuver. Therefore, these vehicles are primarily used to transport officers to and from hostile situations. Additionally, such vehicles are employed in natural disaster relief given their ability to drive over almost any type of terrain.

Finally, in Columns (4) and (5) of Table 2, I estimate the effects of police militarization on two other types of deaths that are not considered killings by the police since they do not involve the use of deadly force by a law enforcement agent: 1) accidents caused by the deceased; 2) suicides. Notably, militarization has no effects on these outcomes. Furthermore, these results tentatively imply that there was no increase in the number of operations by the police. In this case, we would expect an increase in the number of accidents and suicides.

4.3. Police safety

One of the arguments in favor of the militarization of the police is the positive effect it may have on the safety of police officers. This has become a heavily debated issue over the last decade, because criminals have started arming themselves with increasingly powerful weapons (Police Executive Research Forum, 2010). Proponents of militarization argue that military protective equipment may help to counteract this danger and shield officers from serious injury, while military detection tools may enable police to identify potential threats. Furthermore, following what is known as the Powell Doctrine, a show of force through the use of highly visible military equipment may encourage suspects to surrender without the use of violence (Leach, 2001). However, militarization may also mean that police officers are involved in more aggressive and dangerous operations that ultimately lead to higher, not lower, risks for law enforcement agents. This argument is in line with evidence provided by the American Civil Liberties Union (2014), showing that after a police department militarizes, it increases the number of "Special Weapons and Tactics" (SWAT) operations. These operations are deployed in circumstances deemed as high risk and exceeding the capabilities of traditional law enforcement. Additionally, police officers may take more dangerous action if they feel safer while using military equipment. Finally, criminals may react to police militarization by similarly being more aggressive or by acquiring high-powered weapons. I explore the effect of militarization on different measures of police safety in Table 3.

Ultimately, militarization substantially decreases police safety. For a better sense of the magnitudes of these effects, notice that after the start of the withdrawal from the wars, each year an average of 78,398 officers were assaulted and 12,655 were injured. Estimates imply that, since 2010, militarization has increased the assault rate by 15.9% and the injury rate by 12.8%. Therefore, on average each year, 12,440 assaults and 2653 injuries are due to the militarization of the police. The substantial size of these effects is not surprising considering that at least 200 operations are deployed each day by the police using military equipment. This number is particularly striking when compared to the historical frequency of this type of operations. In the80ı́s there were only eight operations of this type a day and by the early2000ı́s this number reached 120 (Coyne and Hall-Blanco, 2016). I also study the effect of po-

[18] To assess the value of a life, I use the $7.9 million estimated by the Food and Drug Administration.

Table 3
Police safety.

	(1) Police assaulted	(2) Police assaulted injured	(3)Police killed feloniously	(4) Police killed accidentally
Militarization per Capita	39.41**	8.968**	10.72*	-2.633
	(16.83)	(4.290)	(6.358)	(2.972)
Observations	47,966	47,966	47,966	47,966
Year FE	Yes	Yes	Yes	Yes
Police Dep. FE	Yes	Yes	Yes	Yes
Year FE×1(Milbase<20Km)	Yes	Yes	Yes	Yes
F-stat	12.10	12.10	12.10	12.10

The table reports instrumental variable estimates. In all columns clustered standard errors at the Commuting Zone * Year level are reported in brackets. Regressions are weighted by the number of officers. The sample includes all police departments yearly data from 2007 to 2014. The outcome in columns (1) and (2) is police assault rate per 1000 officers. Column (1) includes all assaults, column (2) includes only assaults that injured an officer. Columns (3) and (4) report the rate of police deaths per 100,000 officers. Column (3) includes only killings that were done with a felonious intent while column (4) includes only accidental deaths. * *p-value<0.10,* ** *p-value<0.05,* *** *p-value<0.01.*

Table 4
Type of police assaults.

	(1) Distubance Call	(2) Burglary	(3) Robbery	(4) Other Arrest	(5) Civil Disorder	(6) Custody Prisoner	(7) Traffic Stop
Militarization per Capita	10.16*	0.961*	0.754**	9.330**	0.290	0.252	2.743
	(5.304)	(0.552)	(0.351)	(3.795)	(0.728)	(1.352)	(1.835)
Observations	47,966	47,966	47,966	47,966	47,966	47,966	47,966
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Police Dep. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE×1(Milbase<20Km)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-stat	12.10	12.10	12.10	12.10	12.10	12.10	12.10

The table reports instrumental variable estimates. In all columns clustered standard errors at the Commuting Zone * Year level are reported in brackets. Regressions are weighted by the number of officers. The sample includes all police departments yearly data from 2007 to 2014. The dependent variables is in all columns the police officers assault rate for every 1000 officers. Columns differ by the circumstances under which the assault happened. * *p-value<0.10,* ** *p-value<0.05,* *** *p-value<0.01.*

lice militarization on the most extreme form of work hazard: the death of a police officer. Given the rare occurrence of this event, it is difficult to precisely estimate the effects of militarization. However, even if the effect is imprecisely estimated, its size is substantial. Militarization has a positive effect on the probability of a police officer being feloniously killed. Each year, an average of 35 police officers are killed feloniously. Estimates imply that the officer death rate has increased by 5.3% since 2010 due to militarization. Overall, 2 officer deaths each year are due to police militarization. I also show that militarization has no effect on accidental deaths. This result is consistent with the fact that accidental deaths of police officers are most likely to occur when they are struck by or driving a motor vehicle.

Finally, I use the classification provided by the LEOKA dataset, which categorizes the circumstances under which assaults on police officers occur. The most common circumstances are those involving a response to a disturbance call, handling a prisoner, a traffic stop, and the category "other arrests" (most likely drug-related). I then use this categorization, in Table 4, to study the effects of militarization on different types of assaults on police officers.

Most notably, militarization has a positive and significant effect on assaults that occur when answering a disturbance call and when officers are involved in "other arrests." These results are in line with evidence that describes how military equipment is often used in domestic disputes with armed and barricaded suspects (Kraska, 2007) and drug raids (American Civil Liberties Union, 2014). Militarization has no effects on assaults that occur during the handling of prisoners or traffic stops. These results are expected given that militarized police officers and SWAT teams are unlikely to perform these types of operations. Militarization also has no effect on assaults during civil disorder even though the police have famously used military equipment during protests and civil disobedience. The lack of effect in this type of situation may be due to the difference between acts of civil disorder and other instances where police use military equipment. In most situations, military equipment is used in raids by the police to apprehend suspects. These types of operations are substantially different from episodes of civil disorder. Furthermore, since assaults on police officers during civil disorder are rare, it may be difficult to identify any effect.

I then study if police militarization changes the number of officers or employees in a police department. On the one side, as a police department becomes more militarized, it may need to hire more officers in order to use this specialized equipment. On the other side, military equipment may be used to substitute some of the employees. In Appendix 9, I show that when a police department becomes militarized, it does not change the number of police officers or employees. This makes interpreting the effect of militarization on killings by the police and police safety more straightforward since the estimates compare the outcome of a police department with different levels of militarization but the same number of police officers and employees.

5. Comparison with the literature

In this section, I explore how my estimates compare to those found in the literature. I focus on the results found by Bove and Gavrilova (2017) and Harris et al. (2017), which also use an instrumental variable setting to estimate the effects of police militarization. It is important to highlight that these papers' main focus is not only on the effects of militarization on police safety and police killings and perform their analysis on many other outcomes. Here, I first describe what I believe are the potential issues with previous studies. I then propose some innovations to their analyses to address these issues.

The first potential issue stems from the fact that militarization decisions are made at the police department level but Bove and Gavrilova (2017) and Harris et al. (2017) use county-level data. As highlighted by Gunderson et al. (2020), this mismatch between observation and treatment levels may generate ecological fallacy bias. The second potential issue is that the cross-sectional variation used in constructing the instrumental variable by Bove and Gavrilova (2017) and Harris et al. (2017) may be problematic. Table 2 in the Appendix shows that many observables are correlated with the share of years that a positive amount of military equipment is received (the cross-sectional

Table 5
Comparing results with the literature - police safety.

	(1) BG (2017)	(2) H et. al (2017)	(3) BG (2017) PD Level	(4) H et. al (2017) PD Level	(5) Ident.	(6) Ident. PD Level
			Effect of Militarization on Police Assaults			
Coefficient	0.27	−0.43**	−0.81	−3.82	14.55**	39.41**
	(0.27)	(0.20)	(4.00)	(2.42)	(6.73)	(16.83)
			Effect of Mililitarization on Police Injuries			
Coefficient	−0.08	1.01	−1.25	−2.30*	3.28*	8.97**
	(0.05)	(0.66)	(1.76)	(1.27)	(1.92)	(4.29)

Note: This table reports the estimates of the effect of militarization on police officers assault rate (per 1000 officers) in Panel A and injury rate (per 1000 officers) in Panel B. The first two columns estimates the results using the data and identifiation strategy in Bove, Gavrilova (2017) and Harris et. al (2017), respectively. Column (3) and (4) are the same estimation at the police department level instead of at the county level. Column (5) uses my proposed identification strategy using data at the county level. Column (6) are instead my baseline estimates that use both my proposed identifiaction startegy and data at the police department level.

variation used in Bove and Gavrilova, 2017), even before the rise of militarization in 2010. Table 3 in the Appendix shows that police departments close to and far away from a 1033 Program storage facility were already different in term of the levels and trends of many observables (the cross-sectional variation used in Harris et al., 2017). These results call into question the direct comparison between these two types of police departments to achieve exogenous variation. The last potential issue comes from the data regarding killings by the police used in the literature. Previous studies have used statistics provided by the FBI supplementary homicides report and the DoJ's arrest-related-death dataset. As discussed in Section 2.2, the quality of this data has been shown to be very poor, mainly due to the non-random measurement error they contain (Michael et al., 2015; Banks et al., 2015). Additionally, previously used data do not allow researchers to identify the circumstances under which the killing by the police occurred.

In this paper, I propose three innovations to tackle each of these potential issues. First, I use newly available data that reports militarization levels at the police department level. Second, I propose a new identification strategy that controls for the fact that, by construction, police departments close to a storage facility for the 1033 Program are also close to a military base. In Section 6, I show that this modified identification strategy is able to ensure that there are no pre-existing differences in observables between treated and control police departments before 2010. The other estimation strategies in the literature do not pass this diagnostic. The final innovation comes from the use of non-governmental data to measure killings by the police. As discussed in Section 2.2, this type of dataset is the best available at the moment to overcome many of the issues inherent in the official data that was previously used. Additionally, with this new data I can differentiate between different types of killings by the police. As shown in Section 4.2, this is an important distinction to make in order to observe the effect that militarization has on killings by the police and understand the underlying mechanisms that generate these effects.

In the remainder of this section, I first reproduce the results in Bove and Gavrilova (2017) and Harris et al. (2017) and then individually add each of my innovations to understand their impact. In Table 5 I focus on police safety and in Table 6 on killings by the police.[19] In both tables, the coefficient of interest is the one related to militarization per capita. In Columns (1) and (2) of both tables, I use the replication files of Bove and Gavrilova (2017) and Harris et al. (2017) to produce the estimates. As described in their respective papers, Bove and Gavrilova (2017) find no effect of militarization on officers' safety or killings by the police. Harris et al. (2017) instead find a negative effect of militarization on the assault rate but no effect on injuries and killings by the police. In the remaining columns I include one by one the innovations.

Table 5 shows that only when applying my proposed innovations, it is possible to estimate a positive and significant effect of militarization on injuries to police officers. When comparing my estimates with a zero effect, 35% of the gap is due to the new identification strategy. The remaining difference stems from the use of police department data. The new identification strategy is necessary to obtain positive and statistically significant results.

Table 6 shows that only when applying my proposed innovations, it is possible to estimate a positive and significant effect of militarization on killings by the police. Approximately 50% of the gap between a zero effect and the ones estimated in this paper are due to the use of the new identification strategy. The remaining half comes from the use of the new non-governmental dataset. The use of data at the police department level seems to have no substantial effect on the estimated parameters.

6. Plausibility of the identification strategy and robustness

The identification strategy is based on a comparison between police departments that are located less than 20 km from the closest 1033 Program storage facility ("Treated") and those that are less than 20 km from a military base without such a storage facility ("Control"). Given these definitions, the instrumental variable estimation hinges on the following identifying assumption: Changes in the outcomes of interest between years of high and low availability military equipment are not different between "Treated" and "Control" police departments, other than through the military equipment received. Alternatively, the identification assumption can be understood as follows: When the 1033 Program was created at the end of 1996, the selection of military bases as storage facilities was not biased toward bases located in areas that would have naturally experienced greater increases in killings by the police and larger decreases in police safety after 2009.

It seems reasonable to assume that the selection of storage facilities was not actively biased toward military bases located in areas that would experience a disproportionate change in killings by the police and police safety 14 years later. However, it is possible, that the choice of which military base would become a storage facility was determined by other factors that ended up being relevant in causing differential trends in the outcomes of interest after 2009.

To provide evidence that this was not, in fact, the case, in Table 7 I show that "Treated" and "Control" police departments, before the massive inflow of military equipment to the 1033 Program, had similar levels and trends for many observables potentially related to killings by the police and police safety.[20]

[19] Two of the innovations apply to the analysis of police safety, while all three apply to the analysis of killings by the police. Therefore, Table 5 has 6 columns while Table 6 has 12 columns. Given the results found in Section 4.2, when studying the effects on killings by the police and the data allows it, I focus on killings by the police that involved the use of a firearm. In Appendix 4.3, I show the same results including all killings by the police.

[20] In line with the fact that the decision to become militarized can be related to many unobservables correlated with the outcomes of interest in Appendix 7 I show that the OLS estimates are substantially different from the IV estimates. Additionally, notice that in Table 7 trends are calculated between 2007 and 2009 just before the huge inflow of military equipment to the US. In Appendix

Table 6
Comparing results with the literature - Killings by the Police (Firearm).

	(1) BG(2017)	(2) H(2017)	(3) BG(2017) PD Level	(4) H(2017) PD Level	(5) BG(2017) FE Data	(6) H(2017) FE data	(7) BG(2017) PD Level FE Data	(8) H(2017) PD Level FE Data	(9) Ident.	(10) Ident. PD Level	(11) Ident. FE Data	(12) Ident. PD Level FE Data
Coefficient	0.01	0.29	-0.05	0.17	0.03	0.17	-0.05	0.18	0.34	0.43	0.82**	0.76**
	(0.02)	(0.38)	(0.10)	(0.12)	(0.19)	(0.12)	(0.15)	(0.16)	(0.27)	(0.34)	(0.37)	(0.34)

Note: This table reports the estimates of the effect of militarization on killing by the police (per 1,000,000 inhabitants). When using data coming from the "Fatal Encounters" dataset I restrict my attention to killing by the police with a firearm. The first two columns estimates the results using the data and identification strategy in Bove, Gavrilova (2017) and Harris et. al (2017) respectively. Column (3) and (4) are the same estimation at the police department level instead of at the county level. Column (5) and (6) show estimates as in column (1) and (2) where police killings are measured using data coming from the "Fatal Encounters" dataset. Column (7) and (8) show estimates as in column (3) and (4) where police killings are measured using data coming from the "Fatal Encounters" dataset. Column (9) uses my proposed identification using data at the county level and measuring police killings using the official data coming from the FBI. Column (10) reproduces the same estimates but at the police department level. Column (11) uses my proposed identification using data at the county level and measuring police killings using the "Fatal Encounters" dataset. Column (12) reports the same estimate when the regression is performed at the police department level.

Table 7
Comparison treated and control police departments.

Variable	Treated - Control	
	Levels	Trends
Sheriff Department	0.06	–
	(0.04)	
Municipal Level	0.02	–
	(0.10)	
Area (Square Miles)	82.19*	–
	(47.19)	
Population	5766	562
	(17629)	(507)
Officers per Crime	26.79	1.34
	(24.41)	(1.50)
Crime per Capita	10.54	1.08
	(7.30)	(1.31)
Arrests per Capita	1.43	0.67*
	(1.05)	(0.38)
Killing by the Police (Firearm)	0.93	0.67
	(0.60)	(0.56)
Killing by the Police (Vehicle)	0.47	0.53
	(0.35)	(0.38)
Killing by the Police (Other)	0.03	0.10
	(0.07)	(0.09)
Officers Assaulted	-43.66	-0.85
	(27.48)	(11.71)
Officers Injured	1.90	0.67
	(7.46)	(4.31)
Officers Killed (Feloniously)	2.72	9.30
	(11.06)	(17.89)
Officers Killed (Accidentally)	2.06	5.55
	(2.13)	(5.01)

Note: This table shows differences in levels and trends between police departments less than 20km from a storage facility that is part of the 1033 Program (Treated) and police departments less than 20km from a military base that is not storage facility that is part of the 1033 Program (Control). Levels are calculated in the year 2009. Trends are between 2007 and 2009. Clustered standard errors at the Commuting Zone * Year level are reported in parenthesis ⋆ 10% ⋆⋆ 5% ⋆⋆⋆ 1%.

Furthermore, Table 8 shows that the results are robust to different definitions of closeness to a storage facility or military base. Column (1) reports the baseline specification, while in Columns (2) and (3) expand the range to include all police departments located at less than 25 Km and 30 Km from a 1033 Program storage facility, respectively. In Column (4), I include the distance to the closest storage facility and military base logarithmically. This is a reasonable assumption given the non-linear relationship between distance and militarization. Finally, Column (5) uses the 4th order polynomial of distance as estimated in Fig. 1. Although, results related to the effects of militarization on killings by the police involving a firearm, police officers assaulted, and police officers injured tend to be smaller than the baseline, their signs and magnitudes are confirmed.

Furthermore, in the Appendix, I show that the results are robust to different definitions of militarization. I then perform a permutation exercise in which I randomly assign military bases as storage facilities for the 1033 Program. I show that the observed effects are only found for police departments close to storage facilities and not all military bases.

7. Conclusion

The use of military equipment by the police has become a heavily debated topic worldwide. In the US, social movements such as "Black Lives Matter" view militarization as a dangerous phenomenon that increases police killings of black people, while countermovements such as "Blue Lives Matter" highlight the dangers that police officers face while on duty and how military equipment can increase their safety. In this paper, I contribute to this debate by showing that the militarization of the police increases their use of deadly force and decreases their safety.

In addition to the direct costs of lives lost, many other collateral costs stem from killings by the police. For example, as illustrated by Mummolo (2018), policing as an institution may lose the trust of citizens. Gallup polls report that confidence in the police in 2015 was at its lowest since 1993, with only 52% of Americans trusting the police (Gallup Poll Social Series). Additionally, Bor et al. (2018) and Ang (2021) show that killings by the police may also have adverse effects on the mental health and educational achievement of the general population. A better understanding of the causes of killings by the police may also yield greater insights into the even more widespread phenomenon of people being injured by the police. As shown by Miller et al., 2016, 55,400 people were injured by the police in 2012. While no systematic dataset is available for police-related injuries, it is possible that, similar to killings by the police, injuries have increased due to militarization.

Finally, I show that contrary to what is often argued in policy debates, police militarization decreases police safety. Ultimately, the results indicate that militarization may have increased the number of aggressive and dangerous operations deployed by the police. At the same time, criminals may have reacted to police militarization by becoming more hostile or by acquiring high-powered weapons.

Credit Author Statement

Single author

8, I show similar results studying trends using a longer time frames (i.e., first between 2006 and 2009 and then between 2005 and 2009).

Table 8
Robustness closeness.

	(1)	(2)	(3)	(4)	(5)
	<20Km	<25Km	<30Km	ln(Dist.)	4th Order
			Effect of Militarization on Killing by the Police (Firearm)		
Militarization per Capita	0.761**	0.666*	0.677*	0.659**	0.619*
	(0.340)	(0.398)	(0.384)	(0.311)	(0.332)
Observations	115,536	115,536	115,536	115,536	115,536
F-stat	53.94	35.74	35.84	45.13	19.19
			Effect of Militarization on Police Assaults		
Militarization per Capita	39.412**	29.812**	26.221**	38.068*	29.524**
	(16.831)	(12.637)	(13.033)	(21.817)	(12.543)
			Effect of Milititarization on Police Injuries		
Militarization per Capita	8.968**	7.629**	7.335**	12.161*	6.063**
	(4.290)	(3.327)	(3.690)	(6.995)	(3.087)
Observations	47,966	47,966	47,966	47,966	47,966
F-stat	12.10	17.47	13.79	4.40	6.13

The table reports instrumental variable estimates. In all columns clustered standard errors at the Commuting Zone * Year level are reported in brackets. Regressions are weighted by population in the first panel and the number of officers in the remaining panels. The sample includes all police departments yearly data from 2007 to 2014. The dependent variable in the first panel is the killings by the police rate per million citizens. In the second and third panel the dependent variables are the police officers assault rate and injury rate for every 1000 officers. Columns differ in the way closness is calculated. Column (1), (2)and (3) measure closeness with a dummy variable that is equal to 1 if the distance in Km is less than 20, 25 and 30 respectively. Column (4) measures closeness with natural logarithm of the distance. Column (5) uses a 4th degree polynomial. * *p-value<0.10,* ** *p-value<0.05,* *** *p-value<0.01*

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Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jue.2021.103365.

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