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Police Officer on the Frontline or a Soldier? The Effect of Police Militarization on Crime*

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Abstract

Sparked by high-profile confrontations between police and citizens in Ferguson, Missouri, and elsewhere, many commentators have criticized the excessive militarization of law enforcement. We investigate whether surplus military-grade equipment acquired by local police departments from the Pentagon has an effect on crime rates. We use temporal variations in US military expenditure and between-counties variations in the odds of receiving a positive amount of military aid to identify the causal effect of militarized policing on crime. We find that (i) military aid reduces street-level crime; (ii) the program is cost-effective; and (iii) there is evidence in favor of a deterrence mechanism.

JEL classification: K42, H49, H76
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I Introduction

In recent years, the Law Enforcement Support Program (LESO) in the US has been the subject of considerable political controversy. This program, known as the "1033 Program", is a federal initiative that, since 1997, has transferred more than $4.3 billion worth of surplus military equipment from the Department of Defense to domestic police agencies across the US. The program came under scrutiny in the summer of 2014, following the fatal shooting of an unarmed 18-year-old African-American by a police officer; and an ensuing series of protests in the city of Ferguson, Missouri. In the aftermath of the protests, Ferguson's police force used military-grade weapons and armored tactical vehicles - believed to be acquired through the "1033 Program" - to quell the riots. The perceived disproportionality of the reaction of law enforcement has sparked a contentious debate about the consequences of giving military capabilities to local police forces.

We investigate the causal effect of an increase in the militarization of US local police forces on their effectiveness in preventing and solving crime and, with Harris *et al.* (2016), we provide the first empirical analysis of the consequences of the 1033 Program. To what extent has the proliferation of military weapons within US local police forces affected their effectiveness in countering crime? Has the acquisition of military-style equipment contributed "to the protection of the public" and provided "effective and efficient contributions to public safety" (White House, 2014, p.6)? Although these questions have crucial policy implications for security policies, they have so far remained unanswered.

We use newly released data by the US Department of Defence on more than 176,000 transfers of equipment held by 8,000 local police agencies over the period 2006-2012. We explore whether this military grade equipment has a tangible effect on the production function for law enforcement, measured by crime and arrest rates. These two variables allow us to disentangle the deterrence effect produced by the display of military equipment from the efficiency effect when the police use military equipment to solve more crime and arrest perpetrators. Our data allow us to explore the black box of policing by observing whether inputs to the physical stock of capital have an effect on the quantity and efficiency of police personnel. Additionally,

we investigate whether there is a discernibly different effect between lethal vs. non-lethal equipment transfers by exploring variation in the type of military hardware redistributed.

To identify the causal effect of militarized policing on crime, we interact exogenous time variation induced by military spending and local variation between counties in the likelihood of being an aid recipient. High military spending, driven by international factors such as the war in Afghanistan, caused the Department of Defence to accumulate excess reserves during high spending years. The "1033 Program" allows the reallocation of this excess property to law enforcement agencies across the country. We interact this variable, which varies over time, with a county's time-invariant propensity to acquire military aid, measured as the fraction of years that a county receives a positive quantity of equipment. Our identification rests on a comparison between frequent and infrequent recipients, in years following high military spending to years following low spending, similar to a difference-in-difference approach.

We find that military aid reduces crime rates. In particular, more military aid leads to a decline in robberies, assaults, larcenies and motor vehicles thefts, which are all part of the so-called "susceptible crimes" (*a la* Draca *et al.* , 2011), i.e., crimes that are more likely to be prevented by police visibility. By the most conservative estimate, a ten percent increase in aid reduces total crime by 5.9 crimes per 100,000 population.[1] Although the magnitude of this effect is relatively small, 0.24% of the average crime rates in treated areas (2470 crimes), the annual average value of aid acquired by a county is around $58,000, suggesting that this is a very inexpensive crime-reducing tool. Our results survive a variety of robustness checks such as population weighting, differential county crime trends, and alternative instruments such as US military fatalities.

When we focus on police activities, we find essentially no effect of aid on arrest rates. We further find that the observed effects on crime are not explained by an observable increase in police manpower. Yet, we do find that an increase in the military aid might lead to release of employees of law-enforcement agencies, suggesting that labor and capital could be

[1]Note that throughout the article we always refer to units of crime per 100,000 population, even when this is not explicitly stated.

considered substitutes for some of the activities of the police. Furthermore, we do not find evidence of an effect of military aid on injuries and assaults on police officers. Crucially for the *causa* of recent public debate, we find no effect on the number of offenders killed.

Taken together, our results suggest that employing military equipment improves the capabilities of law enforcement to deter crime, potentially through an unobservable police effort channel. Our cost-benefit analysis shows that, for a ten percent spending increase, around $5,800 per county per year, the crimes deterred amount to a social benefit of roughly $112,000. Our results partially mirror those of Harris *et al.* (2016), who similarly find that receiving tactical items leads to a reduction in property crime rates. Interestingly, this study also finds that military aid brings a reduction of the assaults on police officers, the number of complaints against them, and an increase of arrest rates for drug and weapons charges.

Our study is closely related to the empirical literature on the causal effect on crime of an increase in the funds provided to the police force (see Chalfin & McCrary, 2014, for a recent review). Machin & Marie (2011) find a decrease in robbery rates following the increase in targeted funds to police forces in England and Wales. Evans & Owens (2007) find a decline in auto theft, burglary, robbery, and aggravated assault following receipt of grants to hire police officers. Similarly, in order to identify the effect of an increase in the labor component of the production function of the police, several studies have exploited temporary redeployment policies arising from terror-related events (Di Tella & Schargrodsky, 2004; Klick & Tabarrok, 2005; Draca *et al.* , 2011). Yet, none of these studies tackles the effect of an increase in police equipment on crime.[2] Our study therefore contributes to the literature more broadly by specifically focusing on the effect on crime rates of more "capital", rather than more "labor".

This paper is structured as follows: Section II provides some insights into the 1033 Program; Section III presents our data and Section IV explains our identification strategy. Section V describes our results and Section VI offers concluding remarks.

[2]The only exception is Mastrobuoni (2014), who investigates whether differences in clearance rates across two police forces in Milan can be attributed to the availability of advanced Information Technology strategies. He finds that this is indeed the case, and that adopting IT innovation doubles the productivity of policing.

II The "1033 Program"

In 1990, following several years of increasing crime levels, the US Congress, through the National Defense Authorization Act, authorized the transfer of excess property from the Department of Defense to federal and state agencies, mainly for counter-drug related activities. The Congress made the program permanent in 1997, expanding its scope by allowing law enforcement agencies to acquire military property to assist in arrest and apprehension tasks, whilst retaining a focus on counter-drug and counter-terrorism requests. The program was renamed the "1033 Program" in 1996, following the replacement of Section 1208 with Section 1033. The program is under the jurisdiction of the Defense Logistics Agency (DLA) and is overseen by the Law Enforcement Support Office (LESO), located at DLA Disposition Services Headquarter.

Law enforcement agencies follow a three-step procedure to acquire military hardware: (1) they obtain the approval of the State Coordinator and LESO to participate in the program; (2) they place requests and provide justification for specific items. Requests are screened by the State Coordinator and the LESO Staff; (3), law enforcement agencies then receive a decision and if their request is approved, they must cover all transportation and/or shipping costs in connection with the receipt of the equipment.[3] Since the inception of the "1033 Program", over 8,000 federal and state law enforcement agencies have requested a variety of equipment, from assault rifles and grenades to Mine Resistant Ambush Protected (MRAP) vehicles, helicopters and drones, to non-lethal equipment, such as high-tech cameras, camouflage/deception equipment and office supplies.

It was not until media coverage of the Ferguson unrests, however, that the program drew media and public attention. Since the Ferguson incident, there has been much debate on whether local authorities' response to crime is often disproportionate and why e.g., the police force in a city of 20,000 residents looked like an invading army engaged in urban warfare against street protesters.[4]

[3]We refer the readers to Harris *et al.* (2016) for a more detailed discussion of the allocation process. See also `http://www.dla.mil/DispositionServices/Offers/Reutilization/LawEnforcement/ProgramFAQs.aspx`

[4]See Amanda Taub, Why America's police forces look like invading

In 2014 US President Barack Obama ordered a review of the distribution of military hardware to police agencies (Reuters, 23/08/14). Following this request, the White House released a report stating that while military equipment has "contributed to the protection of the public and to reduced operational risk to peace officers [..] when police lack adequate training, make poor operational choices, or improperly use equipment, these programs can facilitate excessive uses of force and serve as a highly visible barrier between police and the communities they secure" (White House, 2014, p. 6). Consequently, by an executive order federal transfers of certain types of military-style gear to local police departments were banned in 2015.[5]

III Data

To address the question of the effect of military equipment on the activity of the police force we use Uniform Crime Reports (UCR) data at the county level and Law Enforcement Officers Killed and Assaulted (LEOKA) data at the agency level. Crime in the United States is reported by law-enforcement agencies to the FBI, which creates summaries of these reports published as annual statistics. The Interuniversity Consortium for Political and Social Research (ICPRS) aggregates the separate agencies into counties, taking into account issues such as agencies spanning several counties, agencies not reporting for certain periods and agency closures and openings.

The UCR data allows us to distinguish between several major crime categories such as homicide, assault, robbery, burglaries, larceny and motor vehicle theft. The LEOKA data allows us to look for an impact of military aid on law-enforcement characteristics such as the numbers of officers and civilian employees at the agency, the ratio between them, the number of calls received by the police, injuries, assaults suffered by the police and the number of offenders killed. Recently, Chalfin & McCrary (2013) raised

armies, Vox, Aug. 19, 2014, `http://www.vox.com/2014/8/14/6003239/police-militarization-in-ferguson`

[5]President Barack Obama ordered a ban on grenade launchers, tracked armored vehicles, armed aircraft, guns and ammunition of .50 caliber or higher, and restrictions to the transfer of wheeled armored vehicles, drones, helicopters, firearms and riot gear, to ensure that officers are trained in their use (Washington Post, 18/05/2015).

concerns about the measurement error in UCR police records. We have no reason to assume that this measurement error is associated with the amount of military aid received. In addition, note that we drop eight percent of the crime data due to missing control variables for some counties.

Data for the amount of military aid awarded to each county have been recently released by the US Department of Defense and are now available in the public domain.[6] We use the DLA's federal supply category and class name to identify the type of equipment. We then aggregate several categories into four groups: weapons (e.g., explosives, guided missiles, guns), vehicles (e.g., aircrafts, combat, assault, and tactical vehicles, including their components), gears (e.g., communication devices, special clothing, night vision equipment) and a residual category (e.g., office supplies, furniture, plumbing items). Table A.1 shows our classification categories, and the relative frequency of each category as well as the average value of each acquisition. We use information about the original acquisition value that was paid by the military services for the equipment.[7]

We also use information on the poverty rate, median income, unemployment rate, the size of the population, and the shares of males, blacks, and people between 15-19, 20-24, 25-29 and 30-34 years old. These covariates capture both individual criminal decision-making analysis and heterogeneous trends across counties. The data are taken from the US Census Bureau and the US Department of Labor. Table 1 offers summary statistics for the main variables of interest as well as for the control variables. The table shows that, although each county in our sample experiences approximately 2500 crimes per 100,000 population every year, most of these crimes are burglaries and larcenies, whereas homicide is a much rarer event. All military aid is recorded as quantities and acquisition value per unit; according to our data, a county on average receives equipment worth $58,000 per year. As one would expect, the most expensive items obtained through

[6]In Figure A.1 we show the monetary value of the given aid for the years in the sample period. In the period between 2006 and 2012, the value of military donations to law enforcement agencies went from slightly less than $30 million to almost $500 million per year, thus expanding significantly. It then reached $750 million in 2014.

[7]We prefer not to use quantities (e.g., 10 guns over 30 mm up to 75 mm), as in each category, different classes of aid have very different values (e.g., helicopters vs tractors). The same applies to the other categories, but as a robustness check, we do replace values with quantities.

the 1033 Program are vehicles, such as aircraft, watercraft and armored vehicles, and the most commonly requested items are gear, for example clothing.

——————— [Table 1 in here] ———————

Figure 1 presents the scatter plot of military aid per capita and crime rate. Although counties with higher crime rates should be more likely to request support by the federal government, there is almost no association in the aggregate between the crime rate and the total value of military aid per capita acquired by counties. We therefore now turn to the presentation of the empirical strategy that will allow us to find the effect of equipment on crime.

——————— [Figure 1 in here] ———————

IV Empirical strategy

We are interested in the coefficient β from the following model:

$$Y_{c,t} = \beta Equipment_{c,t-1} + \gamma` X_{c,t} + \alpha_c + \eta_{s,t} + \epsilon_{c,t} \tag{1}$$

The outcome variable $Y_{c,t}$ is the crime rate for county c in year t whereas $Equipment_{c,t-1}$ is the monetary size of the military equipment that has been acquired by the county. We use a linear-log model i.e., we take the log values of $Equipment_{c,t-1}$ and we keep $Y_{c,t}$ in its original scale.[8] We lag the values of aid by one year to allow time for the equipment to be transferred and placed into use.[9] $X_{c,t}$ is a vector of control variables described in section III. County fixed effects α_c absorb county-specific constant features such as geographical location. It is reasonable to assume that particular counties, such as those belonging to border states, could display a positive correlation

[8]Note that 21 percent of the counties did not receive any aid in the period under consideration. We therefore use the transformation $log\,(1 + Equipment)$. It is easy to check that $log\,(1 + Equipment) \approx log\,(Equipment)$ for numbers around the minimum value of most equipment. This specification is useful in the presence of diminishing marginal returns and it is easy to interpret.

[9]We do not use more than one lag as the federal government requires agencies that receive 1033 equipment to use it "within one year of receipt" (White House, 2014, p.7).

between crime rate and the amount of aid received because of state-specific effects. For example, states at the Mexican border require more resources to control drug-smuggling, have higher crime rates than other states, and are therefore more likely to request aid from the DoD. This state-specific effect would give a positive bias to β, yet changing political policies make this effect time-variant. We thus interact state fixed effects with time fixed effects $\eta_{s,t}$, which allows us to control for state-specific policies and the common factor in aid delivered to counties within a given state at a given point in time.

Theoretically, an effect of aid on crime could be channeled through police manpower and through responses of criminals. A significant effect of equipment on police and crime would constitute evidence for the former. To verify whether an effect of aid on the crime rate might be driven by increased police efficiency, we attempt to capture clearance rates by using arrest rates as dependent variable and comparing the resulting coefficients with those on crime rates. We also look at additional police outcomes through the LEOKA data.[10] If there is an effect of aid on police outcomes, note that equation 1 with crime rates as dependent variables will resemble the reduced form in a two stage model estimating the elasticity of crime with respect to police characteristics, which would be comparable to previous literature (see Chalfin & McCrary, 2014). If the effect of aid on crime is driven by responses of criminals, and not by observable police responses, we would find an effect only on crime rates.

There could be an ex-ante positive correlation between the crime rates a county experiences and the amount of military aid it requests. To alleviate this concern, we employ an IV estimation method by using US military spending in the previous year. By law, the "1033 Program" allows local police forces to acquire *excess* property from the Department of Defense. When US military spending is high (e.g., due to an increase in the intensity of the war in Afghanistan), this generates an accumulation of military hardware. This, in turn, increases the amount of military aid that can be delivered to local law enforcement agencies.

[10]Note that the dependent variable from the LEOKA data is at the level of the reporting agency, whereas X and *Equipment* remain defined at the (geographically larger) county level.

Given that US military spending exhibits only time variation we follow the same procedure as Nunn & Qian (2014). We create an instrument by interacting US military spending with a county's tendency to receive military aid from the federal government.[11]

The first stage is then:

$$Equipment_{c,t} = \alpha + \theta \left[USMilex_{t-1} \times \left(\frac{1}{7} \sum_{t=2006}^{2012} P_{ct} \right) \right] + \delta` X_{c,t} + \alpha_c^2 + \eta_{s,t}^2 + \upsilon_{c,t} \tag{2}$$

where $USMilex_{t-1}$ is US military spending in constant US$ and P_{ct} is a dummy for whether c received any military aid in year t. Conceptually, we have two sources of variation. We have identification along the extensive margin of whether aid is received and along the intensive margin of how many items are received. The former variation is captured by the probability of aid receipt, allowing us to compare crime responses between counties that have received aid for the same number of years. Naturally, this gives the IV a mechanical positive correlation to the dependent variable in the first stage, which is, however, alleviated by the inclusion of county fixed effects, absorbing the probability factor in the instrument. This leaves the variation in military expenditure to aid identification through its effect on the intensive margin. As Nunn & Qian (2014) make clear, this strategy resembles a difference-in-differences estimation strategy, where in the first-stage (and in the reduced-form) we compare counties that frequently receive aid to counties that rarely receive aid, in years following high US military spending relative to years following lower military spending. The main difference from a difference-in-differences strategy is that our treatment variable is continuous.

Our identification strategy is based on the premise that, conditional on other contextual variables, our instrument has an impact on the crime rate only through the provision of military equipment. Note that the exclusion restriction is not violated if higher US military spending affects crime rates through its national or regional influence on e.g., voluntary military re-

[11] Nunn & Qian (2014) investigate the effect of US food aid on conflict by using exogenous variations in US wheat production and in recipients' tendency to receive a positive amount of US food aid.

cruitment, as the inclusion of state-year fixed effects and control variables flexibly account for any national or state-specific changes over time. Note also that neither our instrument nor the crime rate in US counties displays monotonic trends, thus ruling out the possibility of a spurious correlation.[12]

Figure 2 presents a comparison between counties that have received aid only once or two times, denoted as low recipients, and counties that have received aid at least three times throughout the sample period, denoted as high recipients. Each group makes up about 40% of the sample. We observe a decrease in crime for both groups of counties, yet this decrease is more pronounced for the high recipients (Figure 2a); this is consistent with the fact that high recipients display a more marked increase in the amount of military aid they acquire in relation to total surges in available military hardware (Figure 2b). Figure 3 presents a taste of our results in the first and second stages of our IV approach. We observe a remarkable positive correlation between total military aid and our instrument. We also observe a substantial negative relation between the fitted values of total aid and crime rate.

——————— [Figure 2 in here] ———————

——————— [Figure 3 in here] ———————

V Results

In this section we first examine the impact of military aid on crime and arrest rates; we also discuss the degree to which our estimates can be interpreted as providing evidence of deterrence; additionally, we try to establish the extent to which distinct crimes such as robbery and assault respond differently to increases in each category of military aid such as

[12]On one hand, although the US has experienced a general decline in crime rates in recent years, there is a lot of heterogeneity across counties and some categories of crime such as burglaries and larcenies have hardly seen significant changes in the aggregate over time. At the same time, both military spending and the number of US military casualties per year display an inversely U-shaped pattern. Moreover, the interaction between US military spending (or casualties) and P_{ct} gives lower weights to counties with arguably less crime i.e., those that have requested/received aid less frequently. Thus, assuming that the relation is spurious, if the resulting coefficient is negative, it would have an upward bias toward 0.

weapons and vehicles; we then present results from a variety of robustness tests; and we conclude with a basic cost-benefit analysis.

A Does aid affect crime rates?

Our first question revolves around the existence of a causal effect of military aid on crime. Table 2 incorporates the baseline model. Column 1 presents the reduced form relationship between aid and crime. We find that a naive OLS estimation would not reveal any influence of aid on crime, which is consistent with Figure 1.[13] Column 2 shows the first stage estimates. As expected, we find that an increase in the military expenditure of the previous year - holding aid receipt probability constant - leads to a higher amount of aid received by the county. The Kleinbergen-Paap F-Statistic is similar to the conventional F-statistic, but takes into account the clustering of the standard errors, with a value of 49, above conventional levels that characterize weak instruments.

Column 3 of Table 2 shows that military aid reduces the total number of crimes: a ten percent increase in the total value of military aid leads to a decrease of 5.9 crimes per 100,000 population. The negative coefficient reveals that our prediction that the positive correlation between aid and crime could bias the naive OLS estimates upward, as in column 1, was correct.

Further, reading across the first row of results in Table 2, we find that this reduction can be attributed to a decrease in robberies, assaults, burglaries, larcenies and motor vehicle thefts. The effect is very pronounced for street-level crime types, like larceny and vehicle theft, whereas it is insignificant for homicide. This suggests that military aid could have a deterrent effect based on greater visibility. This "display" mechanism could deter crime by increasing the subjective probability of arrest.

On the last line of Table 2, we present estimates of the elasticity of crime with respect to the value of equipment. The biggest elasticity we observe is -.15 for robbery, followed by motor vehicle theft with -.09, assault -

[13] We report the results for the control variables in Table A.2 in the Appendix. Moreover, in Table A.3, we show the OLS estimates of the effect of military aid on all categories of crime, which are all consistent with the absence of a statistically significant effect.

.236 and -.023 for the total crime rate.[14] These elasticities are well within the boundaries of -.01 to -2 presented in previous literature (Chalfin & McCrary, 2014). They are smaller than Evans & Owens's (2007) elasticities of crime with respect to the size of the police force, between - .26 and - .99, also based on US data.

——————— [Table 2 in here] ———————

B Is the effect of aid on crime driven by police efforts?

In this subsection we explore several potential channels through which aid could influence crime reduction. We first establish the effect of aid on arrest rates. If military aid increased police efficiency and ability to solve crimes, then we should find that the number of arrests increases relative to the crime rate. Moving across the columns of Table 3, we do not find strong support for an effect of military equipment on the number of arrests. On the one hand, the substantive effect is negligible, and, e.g., a ten percent increase in total aid leads to a 0.16 decrease in the number of arrests for robbery per 100, 000 population. On the other hand, the decline in the number of arrests could be fully attributed to the decline in the underlying crime rate.[15] Therefore, it seems that military aid does not improve the arresting performance of local police units, thus revealing that it most likely helps to deter individuals from participating in illegal activities in the first place.

——————— [Table 3 in here] ———————

Table 4 captures other ways in which law-enforcement can alter the arrest perceptions of criminals. The dependent variables are the number of police officers, the number of civilian employees, the ratio between officers and civilian employees, the number of received calls, the number of injuries

[14] Running a log-log model yields virtually the same elasticity.

[15] In fact, when we control for crime rate, the effect of military aid on the number of arrests becomes insignificant at the conventional level. We do not explicitly include this additional model though, as crime rate is endogenous to military aid, and can constitute an improper control.

or assaults in the line of duty, and the number of offenders killed by the police. As we can see, military aid might influence hiring decisions by inducing law enforcement agencies to devote more resources to hiring of new police officers and civilian employees. If labor and capital are substitutes in the police production function, then the increase in available capital might lead to a decrease in the labor employed. Whereas the number of officers declines by a very small magnitude, the number of civilian employees declines by 0.2 for a ten percent increase in the size of the military aid. Moreover, military aid neither affects police activities measured by the number of calls they receive nor has a significant impact on the number of police officers assaulted or injured in the line of duty; it also does not have an effect on the number of offenders killed by the police.

To further check the existence of alternative channels linking the "1033 Program" to police performance, we look at the effect of aid on citizens' complaints. One of the most popular arguments against the militarization of the police forces is the probability of a disproportionate use of aggressive weapons and tactics on undeserving targets which can undermine a productive and trustworthy interaction between the police and the local population. If subscribing to this claim, we would observe a positive impact of aid on citizens' complaints. Yet, as Harris *et al.* (2016) point out, military hardware may also reduce complaints if citizens are too intimidated to express dissatisfaction with the behavior of the police. We therefore consider the impact of the 1033 Program on the number of citizen complaints, compiled from published annual reports of police departments, and made available by Harris *et al.* (2016). The last column of Table 4 shows that acquiring military items has a negative effect on complaints, yet not significant.

Therefore, it seems that the deterrent effect observed in Table 2 comes either through a response of the supply side of the market for crimes, that is, criminals themselves refrain from crime or there is an unobservable police effort component not captured by our indicators. Military aid therefore seems to lead to a reduction in crime rates mainly through a deterrence mechanism.

Taken together, our results are similar to those of Harris *et al.* (2016). Both studies find a decrease in robberies, assaults and vehicle thefts, albeit

Harris *et al.* (2016) find bigger effects. Both studies also find a negative effect of tactical equipment on citizen complaints. Finally, Harris *et al.* (2016) find a negative effect on the clearance of motor vehicle theft crime; whereas we also find a negative effect of military aid on overall arrest rates, the magnitude of the coefficients in our models is very small. The differences between our findings and those of Harris *et al.* (2016) should be considered in light of the peculiar operationalization of military aid (Harris *et al.* (2016) use the quantity rather than the value), the different sample size, the choice of the control variables and, perhaps more importantly, the heterogeneity of treatment effects. We use two conceptually distinct instruments and it is likely that in our framework the subset of "compliers" is different from that in Harris *et al.* (2016). While Harris *et al.* (2016) filter the effect for counties which are more sensitive to transportation costs, we give more weight to recipients in high spending years.

——————— [Table 4 in here] ———————

C Which type of military aid is the most constructive in combating crime?

Our third question relates to the existence of distinctive effects of different categories of military aid on crime rates. This is a crucial question in light of the recent heated debate on the questionable use of military weapons and tactics by the police forces. We restrict our attention to aggregate crime as well as to the crime indices which were found to be significantly affected by aid (Table 2). Results are reported in the Appendix, Table A.4, which shows a sort of hierarchy in the marginal impact of aid on crime: the residual category, labeled "others", which includes only non-lethal equipment, without military attributes, has the biggest marginal effect on the reduction of crime. This is most evident in the fifth panel of Table A.4, where a ten percent increase in this category reduces the total number of crimes by almost seven units per year. The second class of items is vehicles, and the final one is gears, where the effects found do not seem to be significantly different than for the effect for Total Aid.[16] Two basic implications emerge:

[16] In Table A.4, we also present the p-values for a Wald test for difference in the coefficient of the row variable with the coefficient on "Lagged Total Aid".

firstly, two highly visible tools, gears and vehicles, have strong and sizable effects on all the types of crime. Although vehicles are easily detectable, note that gears include sophisticated electronic equipment, training aid and, in almost half of the instances, clothing. This is consistent with early studies by e.g., Bell (1982), which explore how police wearing military-style uniforms influences citizens' perception of the police's authority and legitimacy, and reinforces the notion that a main causal channel could be based on perceptual deterrence. Secondly, even though the residual category is still too aggregate and too large to make reasonable claims about which of its subcomponents are driving the effect, the inclusion of diverse office equipment could entail that law enforcement agencies are able to improve the efficiency of their organizational practices and, therefore the allocation of their time resources, resulting in more patrols or other unobserved crime-deterring activities. Additionally, the inclusion of IT hardware in the residual category might ultimately lend support to previous findings by Mastrobuoni (2014) on how IT adoption (or innovation) affects crime.[17]

Although weapons do not appear to work as a deterrence tool, note that our instrument seems to be weak at predicting the allocation of the weapon category. This applies to all the four sub-categories of crime in which we are interested as well as to the overall crime level. It seems that our instrument does not capture well the allocation decisions related to weapons in high spending years, most likely pointing to a caution from the policymaker associated with the controversy on the value-added of using battlefield weapons to police urban areas.

D Robustness Checks

We verify our findings with a round of additional checks. We omit tables due to space limitations, although all additional models can be found in the online Appendix. Table A.5 limits the underlying sample to counties in which the mean population size is lower than 250,000 inhabitants, and where the unemployment rate, the poverty rate, the median income within a county and the share of blacks are higher than the national median.

[17] As aid without clear military attributes is not the focus of this article, however, we leave this facet to future research.

Exploring various cuts, we can see that the control variables are not substantially driving our results and neither can we report a heterogeneity in the effect of aid on crime.

In Table A.6, panel A, we replace the value of military aid by its quantity. Our results remain significant, thus lending additional support to previous results. Note that we cannot explicitly comment on the substantive effects as each category contains highly heterogeneous items. Panels B and C of Table A.6 exploit alternative, yet related, instruments, such as the interaction between the probability of receipt and the amount of military spending in percentage of GDP or the total number of US fatalities in Afghanistan and Iraq (instead of the level of US military spending). The rationale behind the inclusion of the share of output devoted to the military, also called the military burden, is that it measures the priority given to defense rather than to military power or the absolute level of military expenditure (Smith, 2009). As we can see, the coefficients are significant and in the same order of magnitude as in Table 2. Using US military casualties as an alternative instrument allows us to effectively capture the intensity of military deployments and the severity of war, which, in turn, influence the procurement of military equipment. Again, previous findings about aid and crime are strongly borne out by this new set of empirical results. The coefficients are similar to our baseline models and the F-statistics are above conventional levels.

In Table A.7, panel A, all results are weighted by the size of the mean population to reflect crime as a population mean, and, by and large, the results carry over. In fact, the coefficients are now distinctly higher, and still statistically significant. In panel B we replace total aid by its per capita counterpart, and the coefficients retain the same magnitude and are statistically significant. In panel C we keep a subsample of counties that contain law-enforcement that have fully complied with Uniform Crime Reports reporting, that is, with 100 percent coverage of reported crime. We, however, drop almost half of the observations and find that the results on robbery, assault and vehicle-theft survive this robustness check.

In the last table (Table A.8), we account in different ways for preexisting trends in crime. In panel A we include year fixed effects instead of the interaction between year and state fixed effects, whereas in panel B we

include county-specific linear trends to the baseline specification and we exclude state-time dummies. In this way we account for differential crime trends across counties. On both exercises our baseline results remain unaffected and, if anything, accounting for differential crime trends leads to higher effect sizes. Finally, in panel C, we only focus on counties which have received a positive amount of aid for at least one year and for less than seven years. The purpose of this exercise is purely mechanical as we want to show that our effects are not driven by the most frequent recipients of aid. We find the same effects, with a first stage coefficient that is higher than the one estimated in the baseline model. Intuitively, the variation in such counties is more likely to be absorbed by the fixed effects, which explains why the absence of these counties has such a small impact on the estimated coefficient in the first stage and leads to an increase of the same coefficient. To further demonstrate this, consider Figure A.2, a binned scatterplot for the first stage by different probabilities of receiving aid. The Figure shows that the relationship between the instrument and Total Aid is more positive for counties that received aid in two, three or four years out of seven.[18] Hence, exploring various estimation techniques and specifications, we feel confident to conclude that military aid reduces crime, and that the effect may be driven by a deterrence mechanism.

E Cost and Benefit Analysis

We perform a basic cost-benefit analysis by comparing estimates from our baseline models (Table 2) with estimates of the social cost of particular crimes. Heaton (2010) provides one of most recent reviews of academic research on the cost of crime in the US, including accounting-based methods (all the individual costs borne by individuals and society) and contingent valuations (what individuals are willing to pay for crime reduction). He summarizes the cost estimates from three studies of the cost of crime, two using accounting-based methods (Cohen & Piquero, 2009; McCollister *et al.* , 2010) and one using contingent valuation (Cohen *et al.* , 2004). He calculates that the average cost of a robbery is $67,277 (in 2007 US dollars),

[18]Figure A.2 also suggests a lack of mechanical positive bias between the instrument and the instrumented variable, otherwise the fit lines would be arranged with seven years as the highest positive slope and one year of aid as the lowest.

of a serious assault $87,238, of a burglary $13,096, of a larceny $2,139 and of a motor-vehicle theft $9,079. By our baseline and most conservative models (Table 2) a ten percent increase (around $5,800) in the value of aid reduces robberies by almost 0.6 units, assaults by 0.5 units, burglaries by 0.9 units, larceny by 2.7 units and vehicle thefts by 1.2 units. This means that the benefit of a ten percent increase in aid is roughly $112,000, compared to a cost of $5,800, making the donation of military equipment a good investment.

VI Conclusions

In 1990 the US Congress enacted the National Defense Authorization Act, later called the "1033 Program", allowing local law enforcement agencies to acquire excess property from the Department of Defense, including drones, military weapons and armored vehicles. The program came under severe scrutiny in 2014, following a wave of public protests against the disproportionate use of military tools by local police forces. By most reports, providing military equipment free of charge encourages hyper-aggressive forms of domestic policing, which can increase tension, mistrust and uncooperative behaviors between local police departments and local communities. Yet, so far there have been no attempts to examine the tangible outcomes of issuing military equipment to law enforcement agencies, not even its effect on crime rates.

Using panel data for US counties over the 2006-2012 period, we provide quantitative evidence on the effect of the "1033 Program" on police performances. En route, we complement the economic literature on the determinants of policing. Our identification strategy relies on exogenous variation in timing and size of military spending to test whether the militarization of local police forces improved their performances. The results reported in this article provide evidence of a positive effect of military hardware on crime rates, most likely via a deterrence mechanism. We run a number of additional models to isolate this mechanism from other competing channels such as various measures of observable police effort. Interestingly, although all the non-lethal categories of aid are effective in preventing crime, this effect is not reflected in a shift in observable police characteristics such as

arrest rates, manpower or others, hinting at a possible alternative effect channel of unobservable police effort.

The empirical literature on police resources and crime and most of the public debate on this issue, assume that additional resources are allocated to increase the size of the police force. Therefore, implicitly police costs are labor costs. Yet, there is an important capital component in the production function for law enforcement that is usually neglected, regardless of whether new equipment is bought, provided for free or acquired at a greatly reduced price. We estimate that a ten percent increase in the value of military aid reduces the total number of crimes by 5.9 units. Despite a small total effect, the program is quite cost-effective, and adding an extra $5,800 in overall aid leads to a drop of roughly $112,000 in the social costs of robberies, assaults and vehicle thefts combined. Our results seem to suggest that the returns per dollar spent on the margin to capital might be even higher than for labor, and this is an issue that certainly deserves further empirical research. That said, taken together, our results do not directly provide evidence in favor of or against the possibility that military equipment contributes to overly aggressive approaches by police units, which can in turn escalate to a standoff between urban communities and the officers that police them. This is a social cost that our analysis cannot duly capture and it is an important point for future research.

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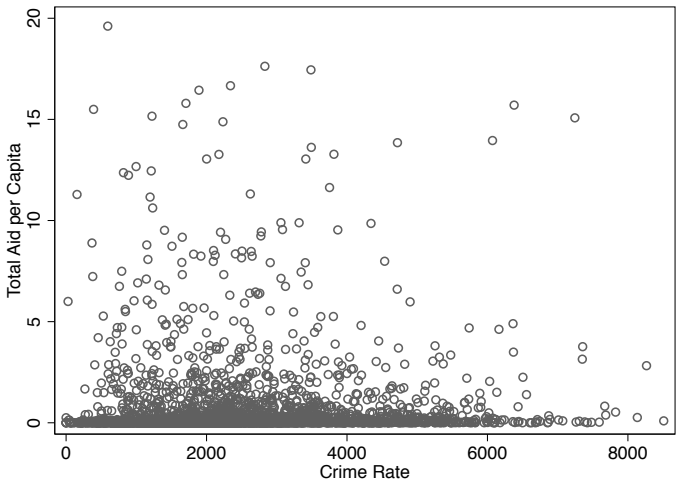
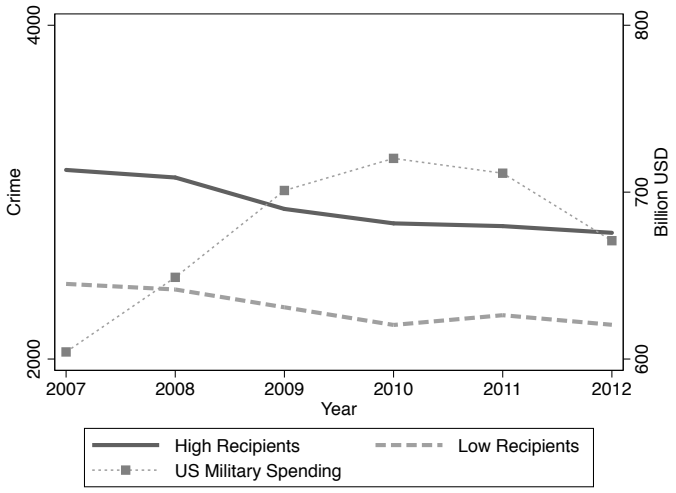
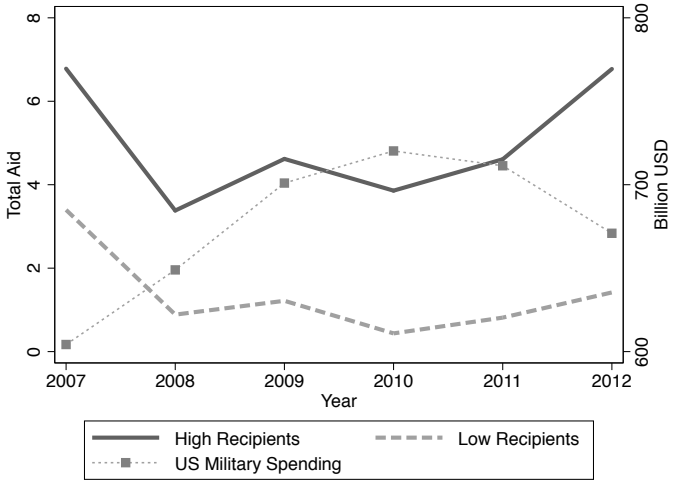


Figure 1: Crime Rate (per 100,000) and Average Aid per Capita



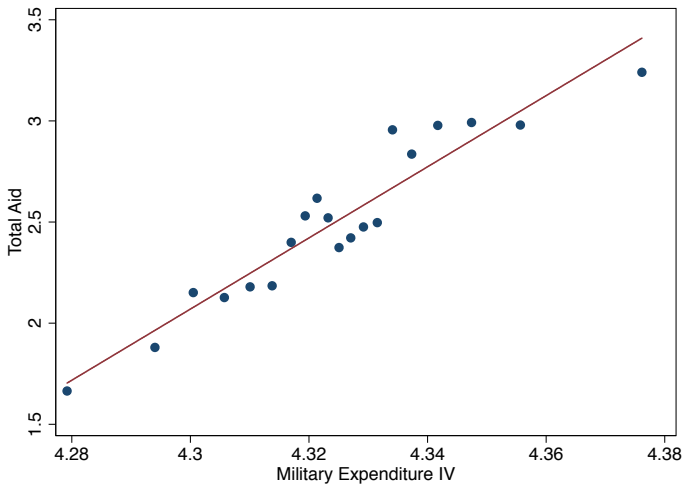
(a) Difference in Crime Rate (per 100,000) between Low and High Recipient Counties



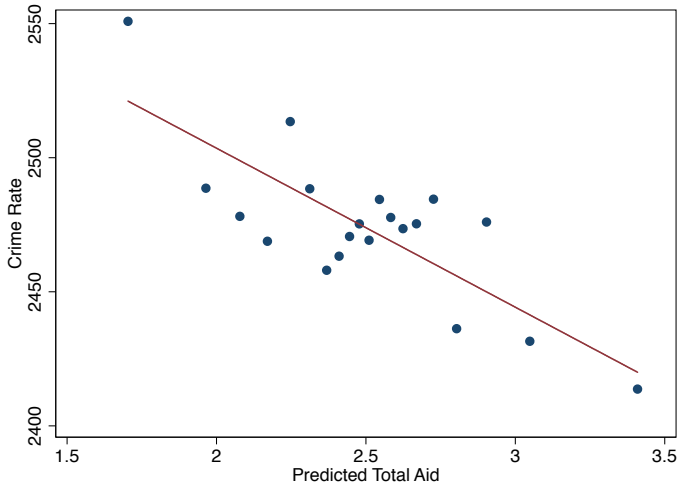
(b) Difference in Total Military Aid (in log) Received between Low and High Recipient Counties

Figure 2: Crime Rate and Military Raid

Notes: Both figures represent plots for raw data. Low Recipient Counties are defined as counties that have received military aid one or two times (40% of the sample). High Recipient Counties are those that have received military aid at least three times (40% of the sample).



(a) Total Military Aid (in log) and Military Expenditure IV: The First Stage



(b) Total Military Aid (in log) and Crime Rate (per 100,000): The Second Stage

Figure 3: Binned scatter plot representations of the first (second) stage of our analysis. Each point of the scatter represents the mean military expenditure (crime) and total aid over equally sized bin. The line presents a linear fit.

Table 1: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Crime Rate	2473.9	1519.9	0	40594.9	17822
Murder Rate	3.4	6.8	0	182.8	17822
Robbery Rate	40.5	68.5	0	2432.4	17822
Assault Rate	199.1	195	0	5581	17822
Burglary Rate	585	422.9	0	12773.4	17822
Larceny Rate	1515.3	936.6	0	18897.6	17822
Vehicle Theft Rate	130.5	132.6	0	3839.1	17822
Murder Arrest Rate	2.8	5.9	0	134.2	17656
Robbery Arrest Rate	15	22.3	0	628.9	17656
Assault Arrest Rate	97.1	92.1	0	1747.9	17656
Burglary Arrest Rate	82.8	70	0	1097.8	17656
Larceny Arrest Rate	293.4	262.1	0	7844.2	17656
Vehicle Arrest Theft	19.1	23.8	0	608.5	17656
Military Exp. IV	4.3	3.7	0	13.5	17822
Total Aid (value)	57668.1	1235903.9	0	152824128	17822
Total Aid (quantity)	69.1	1076.5	0	97348	17822
Weapons Aid	1479.8	13795.9	0	675916.4	17822
Weapons Quantity	10.9	212.4	0	21586	17822
Vehicles Aid	34521.5	1196029.7	0	152279640.2	17822
Vehicles Quantity	1.4	15.3	0	715	17822
Gears Aid	11235.3	136707.7	0	8574024	17822
Gears Quantity	30.3	673.7	0	78634	17822
Others Aid	10431.5	130567.6	0	6847195.2	17822
Others Quantity	26.5	495.1	0	31032	17822
Percent Poverty	16.3	6.3	2.4	62	17822
Median Income	43689.5	11186.5	19829	121250	17822
Unemployment Rate	7.7	3.2	1.1	29.1	17822
Population	96694.5	261689	16	5151374	17822
Share Males	0.5	0	0.3	0.8	17822
Share Blacks	0.1	0.2	0	0.9	17822
Share Age 15-19	0.1	0	0	0.3	17822
Share Age 20-24	0.1	0	0	0.4	17822
Share Age 25-29	0.1	0	0	0.3	17822
Share Age 30-34	0.1	0	0	0.2	17822

Table 2: The Effect of Military Aid on Crime

	(1) OLS	(2) First Stage	(3) Crime	(4) Homicide	(5) Robbery	(6) Assault	(7) Burglary	(8) Larceny	(9) Vehicle Theft
Military Exp. IV		17.589***							
		(2.510)							
Lagged Total Aid	0.692		-59.293***	-0.063	-6.102***	-5.305**	-8.750*	-27.429***	-11.644***
	(1.185)		(14.040)	(0.094)	(1.088)	(2.348)	(5.307)	(7.218)	(2.807)
Constant	6800.270*	-55.150***							
	(3439.930)	(12.177)							
Observations	17822	17822	17807	17807	17807	17807	17807	17807	17807
Kleibergen-Paap F-Statistic			49.113	49.113	49.113	49.113	49.113	49.113	49.113
Elasticity			-0.023	-0.018	-.15	-0.026	-.014	-.018	-0.09

Notes: The dependent variable in columns 1 and 3 is total crime per 100,000 population, in column 2 it is military aid, from column 4 onwards it is the category of crime per 100,000 population. Control variables: median income, poverty rate, unemployment rate, population, share of males, blacks, and age 15-19, 20-24, 25-29, 30-34. All models include county and interacted state-year fixed effects. The instrument is lagged military expenditure times the probability of receiving military aid. Robust standard errors clustered at the state level reported in parenthesis. Asterisks denote: $***p < 0.01, **p < 0.05, *p < 0.1$.

Table 3: The Effect of Military Aid on Arrest Rates

	(1) Homicide	(2) Robbery	(3) Assault	(4) Burglary	(5) Larceny	(6) Vehicle Theft
Lagged Total Aid	-0.389**	-1.581**	-3.143	-4.002**	-7.317*	-1.484***
	(0.182)	(0.637)	(2.262)	(1.900)	(3.983)	(0.521)
Observations	17640	17640	17640	17640	17640	17640
Kleibergen-Paap F-Statistic	45.881	45.881	45.881	45.881	45.881	45.881

Notes: The dependent variable is arrest rate per category of crime. Control variables: median income, poverty rate, unemployment rate, population, share of males, blacks, and age 15-19, 20-24, 25-29, 30-34. All models include county and interacted state-year fixed effects. The instrument is lagged military expenditure times the probability of receiving military aid. Robust standard errors clustered at the state level reported in parenthesis. Asterisks denote: $***p < 0.01, **p < 0.05, *p < 0.1$.

Table 4: The Effect of Military Aid on Police Activities

	(1) Officers	(2) Civilian Employees	(3) Officers to Employees Ratio	(4) Calls	(5) Injuries	(6) Civil Disorder Assaults	(7) Offenders Killed	(8) Citizen Complaints
Lagged Total Aid	-0.405*	-2.368**	0.001	-0.185	-0.063	-0.003	-0.010	-0.085
	(0.227)	(0.966)	(0.001)	(0.195)	(0.048)	(0.005)	(0.032)	(0.081)
Observations	148691	148691	119069	148691	148691	148691	16605	516
KP F-Statistic	11.494	11.494	10.626	11.494	11.494	11.494	9.634	2.384

Notes: The dependent variable is the numbers of officers (column 1), of civilian employees (column 2), their ratio (column 3), the number of calls (column 4), injuries (column 5), assaults (column 6) on the police, the number of offenders killed (column 7) and the number of citizen complains (column 8). Control variables: median income, poverty rate, unemployment rate, population, share of males, blacks, and age 15-19, 20-24, 25-29, 30-34. All models include county and interacted state-year fixed effects. The instrument is lagged military expenditure times the probability of receiving military aid. Robust standard errors clustered at the state level reported in parenthesis. "KP F-Statistic" stands for Kleibergen-Paap F-Statistic. Asterisks denote: $***p < 0.01, **p < 0.05, *p < 0.1$.