
The Thin Blue Line: Police Militarization and Violent Crime¹

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ABSTRACT

Recent events such as the riots in Ferguson, Missouri, have highlighted the increased militarization of police forces in the United States. This paper utilizes a new dataset that covers all military equipment transfers between the Defense Logistics Agency and local police forces from 1990 to 2014 to consider the effect of increased militarization on crime. These transactions were conducted under the Department of Defense's 1033 Program and constitute a major transfer of capital resources to local police departments with nearly two billion dollars transferred in the form of surplus military equipment. To deal with concerns of identification, we instrument for participation in the 1033 program using state-level exposure to the military through federal military spending set by Congress. We find that increased capital transfers to states embodied in military equipment reduces total violent crime and violent crime subcategories. The effect is large for overtly militaristic equipment such as assault rifles, but also for less militaristic transfers such as communication equipment, implying that both enhanced capabilities as well as power projection are important drivers of violent crime reduction. In addition, we find no evidence of a labor input response through additional hiring of sworn police officers, indicating that the program resulted in a more capital-intensive police force. Further, we find that increased police militarization results in lower incarceration rates even after controlling for reduced crime rates, suggesting a broader law and order impact beyond just enhanced capabilities. The results make clear that increased police militarization in the United States has played a meaningful role in the reduction in violent crime observed over the last twenty-five years.

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1 INTRODUCTION

On the night of November 24th, 2014, men patrolled the streets armed with M-4s adorned in body armor and camouflage fatigues, while shielding their faces with gas masks. This scene seemed reminiscent of a patrol in Iraq, Afghanistan, or some other foreign war-zone. However, these heavily armed men were not soldiers, but police officers in Ferguson, Missouri. The police presence in Ferguson put a spotlight on the growing trend of police forces across the United States employing military grade equipment.

Much, if not all, of this equipment is loaned to police departments across the country through the Department of Defense (DoD) Excess Property Program 1033. 1033 has transferred nearly \$2 billion worth of military equipment to law enforcement agencies since its roots were formed in the National Defense Authorization Act for Fiscal Years 1990 and 1991. This militarization of United States police departments has raised serious questions about the practical need and implications for police forces to have access to military equipment such as Mine Resistant Ambush Protected (MRAP) vehicles and military style uniforms. Supporters state that police may operate more effectively with access to military grade equipment and tactics, while detractors argue policing is fundamentally distinct from, and antithetical to, military operations. While many Americans found the militaristic police images in Ferguson startling and uncomfortable, only limited empirical work has been done to understand the implications of this massive militarization of local police forces.

In this paper, we utilize data for all 50 U.S. States since the introduction of the 1033 program in 1997 to study the impact on crime rates through 2013. The amount of military equipment transferred to local police departments varies wildly across states and over time. We exploit this information to identify the impact of increased militarization on violent and property crime rates, as well as additional aspects of law and order.

To preview our results, we find that after accounting for state heterogeneity, national secular crime trends, and selection into the 1033 program, increased police militarization does meaningfully reduce violent crime rates. In addition, we find no effect of the program on sworn police officer rates, which suggests the program works primarily through enhancing the capital-intensity of police officers. We find evidence for both a capabilities enhancement effect and a "projection of power" effect on reducing violent crime rates. Finally, we find that incarceration rates are lower in areas with more militaristic police forces, even after controlling for declines in crime rates, suggesting a broader impact on law and order. Based on these results, we conclude that increased police militarization in the United States has played a meaningful role in the reduction in violent crime observed over the last twenty-five years.

The primary identification issue is that military transfers are not randomly distributed across states, but may respond to current crime or expectations about future crime. For example, if those areas with the most crime or those with appropriate expectations about future crime are the localities requesting more militarization, the estimated impact of militarization will capture both the true effect of increased militarization as well as the selection bias. Since both the sign of the effect and the selection bias are unknown, this could lead to a variety of inferential mistakes about the true impact of police force militarization.

To address this concern, we deploy an instrumental variables approach based on state exposure to militarization through federal military spending. Federal military spending differs across states and over time, and is driven by historical and international military needs that are unlikely to be directly related to current state crime levels. Federal military spending is controlled by Congress and mostly set in advance, making it unresponsive to local crime.

However, the presence of federal military spending provides state exposure to military culture, language, personnel, and equipment. While federal military spending is not directly correlated with local crime, exposure through increased interaction with military customs and personnel influences awareness and interest in military equipment. Federal military spending is therefore a viable instrument for militarization of the local police force through the 1033 program.

Using a naive bivariate approach, we find no effect of increased police militarization on violent crime rates when only police militarization is included in the empirical specification. The estimated effect is essentially zero. This observation, however, conceals important features in the data. First, the inclusion of time trends is significant as the US as a whole has experienced a secular decline in crime rates. Second, states differ dramatically in their exposure to crime, and failing to account for these differences results in misleading inferences about the impact of increased militarization. When both state fixed effects and control variables identified in the previous literature as significant determinants of crime are included, the estimated effect of militarization is negative, but small. One might be tempted to conclude that the massive resources transferred via the 1033 program were thus wasted.

However, even after controlling for observables, there is a lingering concern that selection into the 1033 program has not been adequately accounted for in the analysis. In particular, it seems likely that high crime areas might be more interested and willing to seek out additional resources, including military equipment transfers through the 1033 program. This would create a positive correlation between militarization and crime rates, and result in a biased estimated coefficient compared to the true causal effect.

These concerns are born out when militarization is instrumented for using federal military spending. The IV specification results in estimates vastly more negative, suggesting that selection into the 1033 program is a significant factor that needed to be accounted for in the analysis. As a result, the true estimated effect of militarization is quite large and statistically significant for total violent crime as well as disaggregated violent crime categories such as murder, forcible rape, aggravated assault, and robbery. We find no robust, statistically significant effects on total property crime or property crime categories. Our results are robust to alternative specifications, sample periods, manpower instruments, and assumptions on error terms.

The plan for the rest of the paper is as follows. Section 2 provides a review of the relevant literature, while Section 3 provides a brief description of the history and evolution of the 1033 program. Section 4 introduces the data and descriptive statistics. Section 5 presents the empirical approach and results. Section 6 concludes.

2 Related Literature

Despite the controversial nature of the 1033 program, there has been only limited empirical evaluations of the program, or the more general impact of police militarization on crime. The historical evolution of police militarization has been documented in Balko (2006), Paul and Birzer (2008) and Hall and Coyne (2012), with a more extensive treatment found in Balko (2013). Starting with Becker (1968), economists have long considered the interactions between policing, punishment, and crime. While theory has helped to develop a better understanding of the possible channels through which policing and punishment might impact crime, the economic literature has predominantly focused on empirical analysis (see Dilulio (1996) and Cameron (1988)).

The best empirical work has shown that the 1033 program has had a significant and important impact on crime. The two foundational papers in the literature are Harris et al. (2017) and Bove and Gavrilova

(2017). Harris et al. (2017) look at the impact of the 1033 program on local interactions between police officers and citizens, and find that the program reduced the number of citizen complaints, assaults on police officers, and increased drug arrests. Bove and Gavrilova (2017) find that overall crime rates are reduced, although there is no effect on arrest rates. Further exploration of mechanisms leads them to conclude that military equipment works by improving the capabilities of law enforcement to deter crime. Our approach here is similar to Bove and Gavrilova (2017), although we consider a longer period of analysis and use a related, but distinct instrumental approach.

As we treat 1033 as a transfer of capital, parallels arise between it and other programs designed to provide additional resources to police forces, such as the Community Oriented Policing Services (COPS) program that was established with the Violent Crime Control and Law Enforcement Act of 1994. Two of the main components of the COPS program were the Universal Hiring Program (UHP) and the Making Officer Redeployment Effective (MORE) grants. COPS began at a similar time as 1033 (at the time, the program was known as Program 1208 as discussed in the next section) due to high levels of crime at the time. 1033 and COPS are similar in that they both work via transfers of resources to Law Enforcement Agencies (LEAs). While 1033 is a direct equipment transfer, COPS provides grants for hiring more officers through UHP and grants for purchasing equipment through MORE. Both resource transfer programs were designed in order to improve the functioning of police forces and ultimately reduce crime. COPS focused on manpower and common capital equipment, while 1033 introduces restricted military grade equipment.

The COPS program has been analyzed in depth in Evans and Owens (2007). Utilizing the COPS program as an instrument to test whether shocks in the number of police officers has any effect on crime rates, they find that the COPS program increased the size of police forces and that there is a negative relationship with COPS UHP grants and four crime categories: auto theft, robbery, burglary, and aggravated assault. The authors also found that MORE grants had a negative relationship with the previous four crime rates as well as larceny rates. This finding suggests that 1033 may also have a negative relationship with crime rates since both 1033 and the MORE grant portion of the COPS program were designed to enhance police capabilities via equipment.

There is a long tradition of trying to estimate the effect of more police on crime. Cornwell and Trumbull (1994) were an early attempt to use a panel data approach to control for unobserved heterogeneity when they estimated a model of crime using counties in North Carolina. However, even after accounting for time-invariant heterogeneity and common trends, dealing with selection bias has been the key challenge in this literature (see Marvell and Moody (1996)). Solving the selection bias inherent in understanding the effect of police resources on crime has proven challenging, spurring innovative solutions. Levitt (1997) uses election cycles to instrument for police hiring with the identifying assumption being that politicians up for an election have an incentive "to be tough on crime" by hiring more police officers independent of the actual crime dynamics, although questions have been raised about the approach (see McCrary (2002), Levitt (2002), and Worrall and Kovandzic (2010)). Fisher and Nagin (1978) argues there are more fundamental endogeneity concerns when studying police force size and crime determination.

More recently, researchers have used features of terrorism (and institutional responses to terrorism) as instruments for police resources. Klick and Tabarrok (2005) use terror alert levels to instrument for police levels in DC to tease out the effect of police on crime. They find that higher levels of alerts, during which DC police are more highly mobilized, are associated with lower crime levels. Di Tella and Schargrodsky (2004) utilize the allocation of police forces after a terrorist attack in Buenos Aires to study the effect on motor vehicle theft under the assumption that police force allocation in response to a terrorist attack is unrelated to crime dynamics. They find that the deterrence effect of police is large, but highly local. Draca et al. (2011) follow a similar approach in the aftermath of the 2005 terrorist attacks in London, and find similar results.

There is further debate about determinants of crime beyond police resources. Levitt (2004) and Shoemith (2010) provide an overview of the literature on the determinants of crime, and in particular, the decline starting in the 1990s. There is some consensus on appropriate demographic and employment factors. Ethnic and racial disparities in crime and the criminal justice system are discussed in Sampson and Lauritsen (1997), Howson and Jarrell (1987), and Resignato (2000). Macroeconomic cycles have been identified as important by Raphael and Winter-Ebmer (2001) while measures of income have been studied by Doyle et al. (1999). The role of the age distribution and its impact on violent crime has been documented in Perkins (1997), while Levitt (1998) focused on juvenile crime and punishment. Howsen and Jarrell (1987) discusses determinants of property crime, and shows that violent and property crime are influenced by different factors.

Prison populations are thought to have a negative relationship with crime as suggested by Marvell and Moody Jr (1994) among others. Incarceration is thought to work through two specific channels, an incapacitation effect ("criminals off the street") and a deterrence effect through the increased threat of punishment. Levitt (1996) uses prison overcrowding and institutional features as a natural experiment to identify the causal effects of prison population growth on crime. Kuziemko and Levitt (2004) study the impact of imprisoning drug offenders specifically on the determination of crime.

Our identification strategy is reminiscent of Nakamura and Steinsson (2014) (see also Hooker and Knetter (1997)), who use the differential impact of military procurement at the state level to identify fiscal multipliers, and Barro and Redlick (2011) who use military spending changes related to wars to identify the effects of government spending and taxes on output fluctuations. In a cross-country context, Creasey et al. (2012) use variation in military foreign aid to study national building and growth. The political economy of military spending is highly political, as documented by Mintz (2002). This is particularly useful for our identification strategy for two reasons. First, national military spending is driven by geopolitical events rather than local crime dynamics. Second, there is significant variation in state exposure to military spending, and this is related to historical circumstances that are unlikely to respond to local crime changes (see Braddon (1995)). These two observations suggest that the exclusion restriction is valid, and as we show in the analysis below, federal military spending is highly correlated with intensity of participation in the 1033 program, making this a valid instrument for police force militarization.

Having surveyed the related literature, we turn next to the specific characteristics of the 1033 program.

3 Background: 1033 Program

With the National Defense Authorization Act for Fiscal Years 1990 and 1991, Congress set the foundation for what would later become the 1033 program. This act allowed the DoD to transfer surplus equipment to state and federal agencies to help fight the *War on Drugs* through the 1208 Program. The 1208 Program was operated directly from the Pentagon through the Regional Law Enforcement Support Offices. These offices facilitated the transfer of surplus military equipment from the DoD to state and federal law enforcement. In October of 1995 the Defense Logistics Agency (DLA), the primary supply agency of the Department of Defense, took control of the program. The National Defense Authorization Act for Fiscal Year 1997 expanded 1208 and re-branded it as the 1033 Program.

The transition from 1208 to 1033 greatly increased the size of the DoD transfer program. This expansion allowed for all law enforcement agencies to acquire property for bona fide law enforcement purposes that assist in their arrest and apprehension mission. Under 1033, the requirement for equipment to be used to support the War on Drugs was dropped, yet requests for materials to support counter drug operations still received priority. Following the transition to 1033, between 1997 and 1999 a National Program Office was created at DLA in order to oversee the entire program. In 2009 oversight of the program was transitioned to the newly founded DLA Disposition Services Law Enforcement Support Office (LESO) headquartered at Battle Creek, Michigan. This transition created an office dedicated to the execution and support of the 1033 program.

To become a beneficiary of the 1033 program a state must create a Memorandum of Agreement (MOA) with the DLA. Once an MOA is formed, the governor of the state must appoint a DLA State Coordinator who is charged with the oversight of the program within the state to include accountability and proper use of transferred equipment. Once a State Coordinator is assigned Law Enforcement Agencies (LEAs) may apply to participate in 1033. Currently, DLA has an MOA with all 50 states, D.C., and the territories of Puerto Rico, Guam, and the Virgin Islands. Once approved by the State Coordinator and DLA LESO, a representative from an LEA may visit a DLA Disposition Services Site or visit the DLA's online webpage to see what equipment is available for transfer. After visiting a DLA Site, a LEA may prepare a request for equipment accompanied by a justification for why the LEA needs said equipment. Equipment requests are then sent to the State Coordinator and then DLA LESO, who has the final say, for approval. If approved, a LEA may take charge of the property and is required to cover all costs associated with the transportation of the property.

Since the creation of the 1208-1033 program, nearly \$2 billion worth of equipment has been transferred from the DoD to state and federal law enforcement. Figure 1 displays the yearly evolution of program transfers. These transfers take the form of a regulated loan of capital with the potential for later ownership. Once 1033 property has been transferred, LEAs are required to utilize the equipment within one year, and

must use the equipment for one year. After this initial year, LEAs are required to submit proof of possession in the form of pictures, descriptions, and serial numbers of all equipment transferred.

LEAs are only required to submit proof of possession annually for equipment valued over \$20,000 or equipment that requires special demilitarization when it is no longer of use. Equipment that does not meet this criteria is assigned a DLA demilitarization (DEMIL) code of A. DEMIL Code A equipment does not need to be reported following the initial year of ownership, and LEAs are free to dispose of this equipment as they see fit after the initial year.⁴ LEAs never receive de jure ownership of equipment valued over \$20,000, or equipment that requires special demilitarization. LEAs that have been loaned these types of equipment must send them back to the DLA when they are deemed no longer useful by the LEA or by the State Coordinator. For items that are of low value and do not require demilitarization, LEAs take full ownership of the equipment after one year and are free to use and dispose of it as they see fit.

Accountability of property is a key feature of the 1033 program. Due to the nature of the items transferred, it is essential for states and the DLA to keep records of all DoD equipment in LEAs hands. To facilitate accountability and transparency, the DLA provides a roster of all equipment transferred from the DoD to LEAs from 1990 up to the present. The dataset is organized by state (or territory), and includes information on which specific agencies received items, what equipment was transferred, quantity received, value of transferred equipment (when purchased by DoD), date of transfer, and whether or not it requires special demilitarization or return to DLA.

For example, the Baltimore County Police Department received 275 M16A1 assault rifles (national stock number 1005-00-073-9421) with a total value of \$137,225 on July 31st, 2007, with the requirement that they be returned to DLA for demilitarization. The rosters are not entirely military equipment though, as 1033 includes a large amount of Code A items. For example, the Washington County Police Department in Washington, GA, received three guitar amplifiers (national stock number 7720-00-415-1343) with a value of \$583.50 each on February 26, 2014. This information forms the basis on the following analysis to study the impact of police militarization on crime.

Of the \$2 billion of equipment transferred to LEAs through 1033, only 17.3% of it is equipment that has received a DEMIL code of A. Examples of these code A items include shredders, sleeping bags, cameras, tools, and various other construction or office items. The vast majority of the equipment, 82.7%, must be returned to the DLA for demilitarization. Examples of items with these codes include firearms, firearm components, firearm optics, tactical vehicles, aircraft, boats, and night vision or infrared equipment. One of the most expensive items in the code B-Q items is the "Mine Resistant Vehicle". This nomenclature is assigned to a variety of vehicles commonly referred to by the military as Mine-Resistant Ambush Protected vehicles, or MRAPs. These vehicles were designed for the DoD to better protect service members in Iraq and Afghanistan from the threat of IEDs, small arms fire, and land-mines. Since the beginning of 1208-

⁴ DEMIL codes are standardized and applied to all DLA equipment. Equipment that requires demilitarization is assigned a DEMIL code of B, C, D, E, F, G, or Q.

1033, 559 of these vehicles have been transferred to LEAs, ranging in value from \$412,000 per unit to \$1,309,299. Total MRAP transfers are valued at nearly \$380 million, accounting for 22% of all code B-Q transfers and 18.3% of the entire program - more than all code A items combined.

Each locality participating in the 1033 program is in control of how much equipment they request from the DLA. Figure 2 displays the total value of transfers from 1990-2013 for each state via the 1033 program. Every state has received some form of transfer, although participation in the program varied widely. Florida is a clear outlier, with almost \$300 million worth of transfers from 1990 to 2014. On the other end of the spectrum are the Virgin Islands (not depicted) and Alaska with \$164,000 and \$990,000 respectively. Summary statistics of total value of military transfers are provided in Table 1 below. Geographically, Southern states (including Washington D.C.) occupy seven of the top 10 states in terms of military transfers, with four in the top five: Florida, Texas, Tennessee, and Virginia.⁵ Summary statistics for crime and military exposure are also reported in Table 1.

4 Data

The first step in the analysis is to define a measure of militarization of police forces. The source for this information comes from a DLA roster of all items transferred from the DoD to law enforcement agencies in all 50 states, DC, and three territories from 1990 to 2014. Our unit of militarization is the monetary value of military equipment provided by the DLA to each state per year in US Dollars. This metric is not representative of the total value of equipment provided to each state by the DLA, but only of equipment that receives a DEMIL code of B, C, D, E, F, G, or Q. This equipment was chosen as the metric of militarization due to the special demilitarization requirement that it be returned to the DLA.

Code A items are loaned to LEAs with the ability for them to have de jure ownership after one year, and most items are readily available on the civilian market and thus not truly “military”. Code A items were almost exclusively transferred from 2012-2014. The relatively short period for analyzing Code A items makes it inappropriate for inclusion in the primary analysis, although results are robust to the inclusion of these codes.

The remaining data is drawn from a variety of sources. The crime statistics for violent and property crime rates come from the FBI's Uniform Crime Reporting program (UCR). This data includes rates of crimes per 100,000 population for 50 states and Washington D.C. from the years 1990-2013. The data includes disaggregated measures of crime such as murder and non-negligent manslaughter, forcible rape, robbery, aggravated assault, burglary, larceny, and motor vehicle theft. The first four of these measures are aggregated up into a measure of Violent Crime (VCR), while the last three are grouped together as Property Crime (PCR). Of particular note is the great difference between the mean violent crime rate and property crime rate, with property crime being nearly eight times as prevalent as violent crime. Both rates

⁵ Results are robust to the exclusion of these states, and therefore not driven by the states receiving the most military transfers.

vary widely across localities, which influences the decision to use a fixed effects model in order to account for locality specific heterogeneity. Furthermore, during the time period under consideration, crime rates have followed a secular decline nationally (see Figure 3), which motivates the inclusion of time trends in the analysis so as not to wrongly attribute secular forces to militarization.

Demographic data including total population, percentage of the population between 18-24, and percentage that is black comes from the U.S. Census. This data covers all 50 states and D.C. from 1990-2013 for population, and all states and D.C. from 2003-2013 for young and black percentages. Unemployment rates and median income data come from the Federal Reserve Economic Data (FRED) and covers all 50 states and D.C. from 1990-2013. The portion of a state's GDP devoted to the federal military comes from the Bureau of Economic Analysis (BEA) and covers all 50 states and D.C. from 1997-2013. Prison population statistics come from the Bureau of Justice Statistics for the years 1990-2013. Manpower data comes from Department of Defense personnel historical publications for 1997-2009, and is used for alternative instrument robustness confirmation.

Due to the limited or lack of available data for Guam, Puerto Rico, and Virgin Islands, these localities are not included in the analysis, while multiple sample periods are used due to limited availability of control variables. Where possible, we show that sample period selection has no qualitative effects on the results. Our primary model focuses on 1997-2013 and 2003-2013.

5 Empirical Analysis

The 1033 program allows for a unique analysis of the interaction between police and crime as it is focused on the capital of policing as opposed to the more common focus on the labor of policing. In what follows, we will first focus on model selection using the Violent Crime Rate, before turning to the Property Crime Rate and the seven disaggregated measures of crime rates. Along the way, we will highlight key aspects of the data that help to causally identify the effect of increased police militarization on crime.

We begin the analysis by conducting a simple bivariate regression between violent crime rates and our measure of police militarization, the value of restricted military equipment transferred to the state in a given year. Limitations in data force us to consider three different samples. First, we focus on all years between 1990 and 2013, which includes periods prior to the implementation of the 1033 program when procurement took place under the smaller and more drug-specific 1208 program. Compared to later years, there were far fewer transfers in terms of quantity and value. Our second sample focuses on the 1033 program only, from 1997 until 2013. Finally, because of limitations with available controls, we also study a more recent period between 2003 and 2013 when all relevant controls are available. One result of the paper is that sample selection does not change the fundamental results, although the magnitudes of the point estimates do differ.

Table 2 reports estimates for the three different samples. In column (1), we estimate a naive pooled bivariate regression without exploiting the panel structure of the data. For all three samples, the point estimate is positive, although the all year sample is not statistically significant. If we took this estimate

seriously, one would conclude that the increase in police militarization lead to an increase in the violent crime rate in the US during the 1033 program. For every 1 million dollars of military equipment transferred to local police departments in the US, violent crime increased by 1.3 to 1.8 crimes per 100,000 residents.

One concern is that at the same time that police militarization was on the rise, there were national secular downward trends in all crimes, both violent and property, that were unrelated to militarization. Figure 3 shows both violent and property crime rates decreased dramatically from 1990 through 2013, and follow very similar national trends. Crime rates in 2013 were nearly half of what they were in 1990. This national decline in crime is a first order feature of the data, and important to account for if a true causal impact of militarization is to be estimated. If the true effect of police militarization on crime is positive, national trends towards lower crime might bias the estimates downward. In column (2) of Table 2 yearly time trends are included. The estimated coefficients on police militarization increase, and all samples imply that for each additional million dollars spent on military equipment for local police forces, violent crime increased by 3 offenses per 100,000 residents.

An additional concern, however, is that police militarization is not randomly assigned to police departments, but rather requested based on characteristics of the police departments. That is, there is likely a selection bias in the transfer of police departments. While the direction of the selection bias could go in either direction, one plausible view is that areas with high crime or expected high crime are likely to seek out more resources, including both labor and capital. This would create a positive correlation between militarization and (expected) violent crime, which would bias the estimate and understate the true effect. One simple approach to attempt to account for selection bias is to pull out state-specific factors that do not vary over time using state fixed effects. This is done in Column (3) and Column (4), where the latter includes both year and state fixed effects.

The estimated coefficient changes dramatically once differences across states are accounted for. The implied effect is now negative rather than positive, suggesting that increased police militarization actually reduces violent crime rather than exacerbates it. The switch in sign of the estimated effect is consistent with a selection bias story whereby the areas with higher (expected) crime are also the areas which utilize more resources including capital (military) equipment. Column (4) adds time fixed effects to the state fixed effects, pushing the point estimates closer to 0, once again confirming that national trends in crime need to be accounted for as well as state differences. The point estimate in Column (4) is highly significant at the 1% and 0.1% level depending on the sample period.

Table 2 encapsulates the challenges facing an empirical analysis of police militarization and crime rates. Local police departments face very different situations across the country and over time, and participation in the 1033 program is an active choice. To correctly identify the true causal effect running from police militarization to crime rates requires a plausible strategy to eliminate the selection bias inherent in the problem. Our next attempt to deal with the selection bias is through the use of control variables that have been previously identified in the literature as relevant for understanding the dynamics of crime in the US.

One limitation of the results thus far is the possibility of state and time-varying omitted variables. Without the inclusion of control variables, it is impossible to determine the robustness and validity of the previous estimates. The problem is that identifying correct control variables is not without debate. In an attempt to establish robust estimates, control variables are added to the model accounting for economic indicators as well as demographic indicators. These results are detailed in columns (1) through (6) of Table 3.

To get a sense of the possible omitted variables problem, control variables are included one by one in Columns (1) through (5), while Column (6) includes all control variables. In Column (1), the state unemployment rate is included, which is negative and statistically significant. The point estimate on police militarization is slightly smaller at -0.7 (compared to -0.9 without any controls) and is statistically significant at the 1% level. Column (2) includes the median income of the state with no resulting change in the original point estimate, or its statistical significance. Column (3) includes the prison population percentage, which has been identified as an important determinant of crime in previous literature. The point estimate on police militarization becomes more negative and is statistically significant at 0.1%. Column (4) includes the percentage of the population that is black, while Column (5) includes the percentage of the population that is between 18 and 24. In both cases the point estimate is similar to the original estimate and highly significant at the 0.1% level. Finally, in Column (6), we include all controls variables at once. The net result is that the estimated coefficient on police militarization is highly significant, and only slightly smaller than the estimate that only include time and state fixed effects. The results imply that for each one million dollars of military equipment transferred to local police departments, violent crime decreased by 0.72 offenses. Police militarization, time and state effects, and control variables explain about 50% of the observed variation in violent crime rates.

As discussed above, however, the addition of controls is ultimately an unsatisfactory exercise, as the underlying selection issue is not directly addressed. Instead, our preferred approach looks to address the selection issue head on through the use of instrumental variables. The key identifying assumption here is that federal spending on military is set by Congress, often years in advance, and that spending differs across states for historical reasons. That is, federal military spending in a state varies over time and across states, but for reasons that have nothing to do with local crime in a given year. Instead, we hypothesize that federal military spending influences access and use of the 1033 program through exposure to military customs, personnel, and equipment, but does not directly influence local crime rates. Our preferred specification is therefore:

$$Crime_{st} = \alpha + \beta_{militarization} * Militarization_{st} + \beta_z * Controls_{st} + State_s + Year_t + \epsilon_{st}$$

where the dependent variable is a crime rate variable, militarization is measured as the value of transferred military equipment to a state in a given year, and controls are drawn from previous literature. Given

concerns over selection bias, we instrument for militarization using federal military spending, and focus on $\beta_{militarization}$.

To understand the impact of the IV approach, we begin implementation of the IV using federal military spending in a model with no control variables. Due to data limitations, the addition of the controls lowers the number years of data that may be used. Starting first with no controls and the longer time series, Column (1) of Table 5 replicates the regression from Table 2. In Column (2), using the IV, the estimated coefficient on police militarization becomes much more negative and continues to be highly statistically significant. Whereas the simple OLS specification estimates the effect to be a reduction of -1.7 violent offenses per million dollars of military equipment, the IV estimates is nearly 15 times larger at -22.45. The IV estimates tells a very different story than the OLS estimate. The IV estimates suggests that there is a very large and meaningful reduction in violent crime as a result of the 1033 program and the resulting increased militarization of local police forces.

To evaluate the viability of the instrument, we need to consider the first stage effect of federal military spending in a state, and its impact on the 1033 program. The first stage (Table 4) finds a statistically significant relationship between federal military spending at the state level and participation in the 1033 program. Furthermore, the first stage F statistics is 15, suggesting we are unlikely to have a weak instruments problem. The first stage significance is capturing the fact that exposure to military personnel, language and customs promotes awareness of the 1033 Program and the equipment available. Furthermore, the changes in military spending at the state level are correlated with new investments in military equipment. The product cycle of military equipment means that old equipment is now surplus to requirements, and therefore available for procurement through the 1033 program.

Based on the assumed validity of the exclusion restriction and the strength of the first stage, this suggests the instrument is appropriate and useful, and that the resulting IV estimates strongly suggest that the 1033 program of increased police militarization has had an economically and statistically significant effect on reducing violent crime in the US.

Without properly accounting for national trends in crime, state heterogeneity, or selection bias associated with participation in the 1033 program, one would be likely to conclude that the 1033 program has a positive or negligible negative effect on violent crime. The truth of the matter is much different. Once properly considered, the 1033 program of transferring military equipment to local police departments has a meaningful impact on the reduction of violent crime.

To confirm that this finding is robust, we next consider the effect on violent crime with controls and instruments over the shorter sample. Columns (3) and (4) replicates results from Tables 2 and 3. Columns (5) and (6) report results when police militarization is instrumented for using federal military spending by state. While the magnitudes are smaller, the general result is confirmed. Failure to properly account for selection severely understates the true effect of militarization on violent crime. The impact of increased police militarization through transfers of military equipment lead to a 12.6 reduction in violent crime offenses

per 100,000 residents for each million dollars transferred. The effect is statistically significant at the 0.1% level, with the model explaining 85% of observable variation in violent crime across US states over time.

To check the robustness of the results to alternative instruments, we next consider multiple instruments based on the same underlying logic. We augment the federal military spending data with information on military personnel and total Department of Defense personnel. The idea is that exposure to DoD personnel, both civilian and military, has an effect on access to the 1033 program, but has no direct effect on local crime.

Table 6 reports results using personnel data from 2003 to 2009. Column (1) reports a simple multivariate regression over this shorter time horizon. The estimated effects are more negative, but also more variable, and no longer statistically significant, likely due to the lack of data. However, when we instrument using federal spending in Column (2), the coefficient is nearly 15 times as large as in Column (1), and statistically significant at the 5% level. The difference between this estimate and that found in column (6) of Table 5 is driven in part by differences in sample size due to data limitation on personnel data, although given the standard errors we cannot statistically distinguish between the two estimates. Column (3) of Table 6 extends the analysis by including military personnel along with federal military spending, with no discernible effect on the estimation. Column (4) includes a third instrument of total DoD personnel (both military and civilian), resulting in a slightly smaller point estimate of -24.47 and with greater statistical significance.

Next, we consider the robustness of inference to alternative assumptions on the error term. Table 7 re-estimates the model of Column (6) in Table 5 under four different error term assumptions. Column (1) uses a simple homoskedastic assumption, and concludes that the effect is statistically distinct from zero at 5% significance. Column (2) uses a heteroskedasticity-consistent standard error estimator, resulting in smaller standard errors and significance at 0.1%. Column (3) clusters at the state level under the assumption of correlated shocks within a state, resulting in slightly larger standard errors when compared to the robust estimator, but still significant at 1%. Lastly, in Column (4), we allow for both spatial and temporal dependence using a Driscoll-Kraay estimator. Geographic regions in the U.S. tend to be similar in their approach to 1033. The concern is that geographic interdependence is not properly accounted for using alternative error assumptions. The standard errors are smallest under this assumption, resulting in statistical significance at the 0.1% level. Regardless of the assumption on error terms, the inferential results are unchanged. Police militarization has a statistically significant reduction in violent crime.

The lessons from analyzing aggregate violent crime and police militarization are clear and robust. There is significant heterogeneity across states, and a national secular decline in violent crime, but increased police militarization via equipment transfers through the 1033 program are associated with significant (both statistically and economically) declines in violent crimes at the state level. While the estimated effect appears to be small if only control variables are included, the effect is noticeably larger once the selection issue is directly addressed using a valid IV approach. We find that increases in police militarization lead to lower violent crime rates.

Having established this result, we next consider the impact of militarization on property crime. There are good reasons to believe that while property crime and violent crime tend to move together over time, the specific effect of militarization could deter violent crimes while at the same time, push criminals into less confrontational crimes such as property crime. That is, militarization can alter the relative cost of committing a violent crime compared to property crime, not just the overall cost of committing crime. On the other hand, if a more militarized police force works primarily through a power projection effect, the impact on less aggressive property crime may be limited.

The results of Table 8 suggest that the impact on property crime is less robust than for violent crime. Columns (1) through (3) report simple bivariate regressions using the three different sample periods (as in Table 2 for violent crime) as well as state and year effects. In all three cases, the estimated coefficient is negative, although it decreases in magnitude as the sample size declines. In Column (3), which focuses on the period 2003-2013, the estimated effect is not significant. When controls are added in Column (4), the effect is reduced by 33% and is statistically insignificant. As discussed above, controls do not directly address concerns over selection. Columns (5) and (6) re-estimate the models in columns (3) and (4) using an IV approach. The results are mixed. In both cases, the estimated coefficient is significantly more negative after accounting for possible selection in the 1033 program, as was the case with violent crime. However, statistical significance is far from robust. In column (5), with no additional covariate controls, the estimated impact of police militarization is significant at conventional levels. Adding in socio-economic controls in column (6) reduces the estimated coefficient, while increasing standard errors, resulting in a loss of statistical significance. While the point estimates are always negative, fragile inference prevents drawing strong conclusions about the impact of police militarization on aggregate property crime. However, the evidence does not support the view that police militarization reduces certain types of crime at the expense of other types of crime.

While VCR and PCR are useful aggregates, the process of simple aggregation may be hiding important information about the determinants of crime, and how they respond to increased police militarization. To consider these potential differences, we separate VCR and PCR into their seven component crime rates to determine what specific offenses militarization may be providing deterrence for, or uncover any positive effects that may be hidden by focusing on the aggregate rate.

The results are displayed in Table 9. The violent crime rate is a simple aggregation of murder, forcible rape, robbery, and aggravated assault crime rates. Aggregate property crime includes burglary, larceny theft, and motor-vehicle theft. General patterns found at the aggregate level are confirmed when looking at the disaggregated components. First, there are no hidden reversals at lower levels of aggregation once selection is accounted for in the estimation. Second, the general estimation patterns uncovered for aggregate crime rates hold at disaggregated levels as well. When time and state fixed effects are included as well as controls, the estimated effects on police militarization are negligible even when they are statistically significant. However, using an instrumental variable approach results in noticeably larger and

more negative estimated effects. The consistency of the patterns at both aggregate and disaggregate levels is comforting.

All four violent crime categories are statistically significant, while all three property crime categories are insignificant. While the point estimates for all seven categories are negative, the disaggregated category results underscore that property crime effects are fragile while the police militarization impact on violent crimes is robust. By considering the disaggregated categories, we both confirm the robustness of the results (and the patterns of the results), and also discover the channels through which increased police militarization operates.

While the 1033 program is effective in reducing violent crime, to more fully evaluate the effectiveness of the program, we should consider whether it is the militarization that matters, or whether it is the additional resources that matter. One interpretation of the program is that it transferred significant capital resources to police departments in need of resources, and as such, one should expect a reduction in crime. An alternative interpretation is that specific military resources were transferred, and these military resources have specific effects on crime deterrence. Or put differently, was it the capital resources in general or the military resources in particular that affected crime.

We can try to provide a preliminary answer to this question by disaggregating transfers based on the National Stock Number (NSN). A NSN is a 13-digit code attached to each piece of equipment, broken up into a 4-digit Federal Supply Classification (FSC) followed by a 9-digit item code. The FSC groups together similar supply products. The first FSC of interest are codes in the 1000s, which include weapons and aircraft and account for about 40% of the value of equipment transferred. The second FSC of interest are codes in the 2000s, which includes marine and ground vehicles and accounts for another 40% of transfers. The third FSC of interest are codes in the 5000s which include tools, hardware, scaffolding and prefabricated structures, construction and building materials, and communication equipment. This third category accounts for 10% of transfers.

The three categories we focus on are important because they make up the vast bulk of transfers and are regularly transferred throughout the period under consideration, but also because they represent different degrees of militarization. The FSC codes in the 1000s reflect high militarization resource transfers in the sense that these are capital resources that have very specific military capabilities. The FSC codes in the 2000s include equipment that has high military capabilities (such as MRAPs), but also less military capabilities such as utility trucks and cars. The FSC codes in the 5000s include specialized and non-specialized equipment that provides capital resources without the heavy military emphasis. By disaggregating the data long measures of high and low militarization, we can try to estimate the relative impact of specific military equipment compared to general capital equipment transfers.

Results are reported in Table 10. In columns (1)-(3), the baseline regression with state and year fixed effects are reported with each of the three militarization variables included in the specification using the different sub-samples under consideration. Regardless of the sample period, medium military transfers have the largest (negative) impact on violent crime. Low military transfers have the next largest negative

effect, but the coefficient is not statistically significant. High military transfers have a consistently small negative and statistically significant effect on crime. The addition of controls in Column (4) doesn't alter the story noticeably. The impact on high and medium militarization is negligible, while the estimated coefficient on low military turns positive (although still statistically insignificant). These results suggest that it is the militarization of the equipment transfers that matters for violent crime reduction

The story changes, however, once selection concerns are addressed directly with instrumental variables. We focus on high and low militarization categories, which have the sharpest contrast in military capabilities. In columns (5)-(8), we instrument for the highlighted category only, both with and without control variables. Comparing column (5) to column (7), we see that the estimated impact for low military transfers is nearly 3 times as large as high military transfers, although both are large and significant. Comparing columns (6) and (8), which include additional controls, the point estimates are very similar and tell the same story. Low militarization equipment transfers have a noticeably larger effect on violent crime than high military transfers, although both are economically meaningful and statistically significant.

The result suggest that militarization in and of itself does matter for violent crime deterrence, but that local police departments would also benefit from additional capital transfers. In terms of a cost-benefit analysis, this suggests that a brand new dollar of spending on capital resources would provide more bang when transferred as low militarization equipment. However, given the nature of the 1033 program, which transfers depreciated surplus military equipment, such a comparison is not completely appropriate. The empirical evidence supports the view that surplus depreciated military capital equipment of all kinds does reduce crime rates in the US, making the 1033 program quite successful in achieving the stated mission of increased police effectiveness in deterring and reducing crime.

To further explore the channels through which the 1033 program impacts crime, we next consider the impact of the 1033 program on the other major input to police production besides capital: labor. It is possible that the additional resources transferred via the 1033 program are reducing crime because the program frees up additional resources that can be used to hire more sworn police officers, or alternatively the value of an additional police officer increases because of the increased capital stock. Although identification is challenging, the general consensus is that increased police officers do reduce the amount of crime in an area. It may be that the observed effects from the 1033 program are operating through a labor channel rather than a specific militarization channel.

To address this possibility, we explore the impact of capital transfers on the sworn police officer rate in the state (officers per 100,000 residents). In column (1) of Table 11 we find a positive correlation between the value of military transfers and police officer rate in a state. In column (2), we include year dummies to account for any national changes in police officer rates, increasing the estimated coefficient close to 1, which literally implies that for each additional million dollars in transferred equipment, the state hired 1 additional police officer per 100,000 citizens. In column (3), state fixed effects are included and the coefficient switches signs and loses statistical significance. This suggests that within states over time, states

that receive more military equipment through the 1033 program have fewer police officers per 100,000 citizens, but the effect is not statistically distinguishable from zero.

The inclusion of controls in column (4) confirms the lack of a relationship. To deal with the selection concerns highlighted above, we again instrument for the intensity of participation in the 1033 program using federal military spending, and while the estimated effect becomes larger in magnitude, it continues to be negative and statistically insignificant. Column (6) includes controls along with the IV, and while the estimated coefficient switches signs again, it is statistically insignificant. Taken together, these results suggest that increased capital equipment through the 1033 program had no effect on labor inputs.

The lack of a labor input response supports the view that militarization itself is important for the estimated reduction in crime. Police departments in the US are becoming more capital-intensive, and in particular, more military-capital-intensive as a result of the 1033 program, resulting in reduced crime. To confirm the lack of an effect from labor, column (7) includes the police officer rate as an additional determinant of violent crime, with no statistically distinguishable effect. The coefficient on police militarization is slightly more negative compared to the estimate when police officer rate is excluded (-15 vs. -16.3), although the two estimates are not statistically distinct.

Finally, we consider the interaction between police militarization and institutions of order via the incarceration rate. Incarceration rates are thought to have a negative effect on crime rates through two different channels. One is a deterrence effect through increased cost of punishment when caught, and the other is an incapacitation effect by taking criminals off the street.

In Table 12, we explore the relationship between increased police militarization and incarceration rates. In column (1), a simple bivariate relationship suggests that areas with higher police militarization also have higher incarceration rates, which is weakly significant at the 10% level. The inclusion of year effects in column (2) has a negligible effect, while the inclusion of state fixed effects resulted in a much smaller point estimate that is no longer significant, but still positive. In columns (4) and columns (5), the total crime rate and the lagged total crime rate are included to capture the relationship between crime and incarceration. The estimates imply that crime in the previous year is associated with higher incarceration rates in the current year, consistent with a lag in the judicial process. After controlling for the relationship between crime and incarceration in a state, there is no significant different effect for areas with greater police militarization.

Once we account for selection using our instrumental variables approach, we find that police militarization has a negative effect on incarceration rates. This effect is significant at the 5% level. Even after controlling for the relationship between crime rates and incarceration rates, police militarization results in lower incarceration rates. Taken together with the earlier results, this suggests that areas with increased police militarization have lower violent crime rates, and furthermore incarceration rates are lower even after accounting for the fact that crime rates are also lower. This suggests that police militarization is working by making police forces more productive, which lowers crime directly, but also changes the calculus of crime, resulting in an additional reduction in incarceration rates. Given recent concerns about the social costs and

externalities of incarceration, it is particularly notable that a more military capital-intensive police force reduces both violent crime and incarceration rates.

6 Conclusions

The 1033 program has come under greater scrutiny due to concerns over the impact of recent trends of police militarization. Until recently, most of the rhetoric was driven without regard to empirical evidence. In this paper, we find robust support for the view that increased police militarization does reduce violent crime rates significantly, consistent with the emerging literature.

Furthermore, we find that both high military equipment (assault rifles, MRAPs, aircraft) and low military equipment (communications equipment and specialized tools) reduce violent crime rates, with the estimated effect somewhat larger for capital transfers that enhance capabilities independent of any power projection and deterrence. In addition, increases in capital stock had no effect on sworn police offer rates, suggesting that the program is working through increased (military-) capital-intensive police forces. That is, police departments used the 1033 program to enhance the capital stock per officer rather than hire more officers to utilize the additional equipment. This more capable police force has reduced violent crime, and even after controlling for these reductions in crime, states with a more militarized police force also have lower incarceration rates, suggesting a broader impact on law and order institutions beyond police productivity.

Unlike previous results found in Bove and Gavrilova (2017), we do not find robust support for the view that police militarization reduces property crimes. Although our point estimates are similar, we do not have sufficient statistical support to conclude these effects are different from zero. Our approaches differ in a number of dimensions, including of level of aggregation, length of time series, and identification strategy. However, all of these differences are also true for our analysis of violent crime, for which we find much more robust support for the view that police militarization reduces these types of crimes. Caution is warranted in overstating the impact of police militarization on less violent types of crimes such as motor vehicle theft and larceny, and deserves further investigation.

A potential issue with the present approach concerns heterogeneity within states. There are two separate problems to consider. First, as discussed in Aneja et al. (2012), there are serious concerns about the reliability of crime data at sub-state levels. Maltz and Targonski (2002) argue that because of concerns over reporting, county-level crime statistics should not be used to evaluate the effects of policy changes. Maltz and Targonski (2003) argue that the state-level crime data are less problematic than county-level data due to the FBI's cleaning and imputation process.

The second issue concerns the spatial nature of crime and crime deterrence. Crime is highly local, often affecting neighboring street blocks differently. The process of aggregation to the state level may be missing important differences at lower levels. Pushing back on this concern is the fact that police equipment can be deployed easily across local jurisdiction. Much of the equipment transferred was to county and state

LEAs that operate across multiple local jurisdictions. Furthermore, inter-agency cooperation would generate additional spillovers of resources across LEAs.

The question ultimately becomes one of appropriate aggregation. Our view is that the state is the appropriate level of analysis because of the nature of the 1033 program, which runs through a state coordinator. Spillovers across jurisdictions within a state are likely to be large, while spillovers across states relatively small. Concerns over measurement error at sub-state levels further support the decision to focus on state outcomes. Compared to Bove and Gavrilova (2017), choice of aggregation does not seem to matter for violent crime, but may matter for property crime, possibly because property crime is more substitutable across space and more susceptible to measurement error.

While the stated objective of the 1033 Program is to provide capital resources to facilitate the mission of crime deterrence and reduction of law enforcement agencies, there may be additional effects of the program beyond crime reduction. In this paper, we focus solely on the effect on crime. However, additional dimensions of social well-being beyond crime may be impacted by increased police militarization. If increased police militarization increases social fracture, for example, the costs may exceed the benefits of reduced violent and property crime estimated here. Insler et al. (2018) find that police militarization reduces civic engagement (measured by charitable giving and volunteering) for black households, but has no impact on white households.

Nonetheless, the findings of this paper are important for understanding the impact increased police militarization has on crime rates. Without properly accounting for geographic heterogeneity, secular national declines in crime, and selection into the 1033 program, one would conclude that increase militarization had no effect or even increased crime. Once all of these factors are accounted for, however, it becomes clear that the 1033 has played a meaningful role in the reduction of violent crime in the United States.

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Figures and Tables

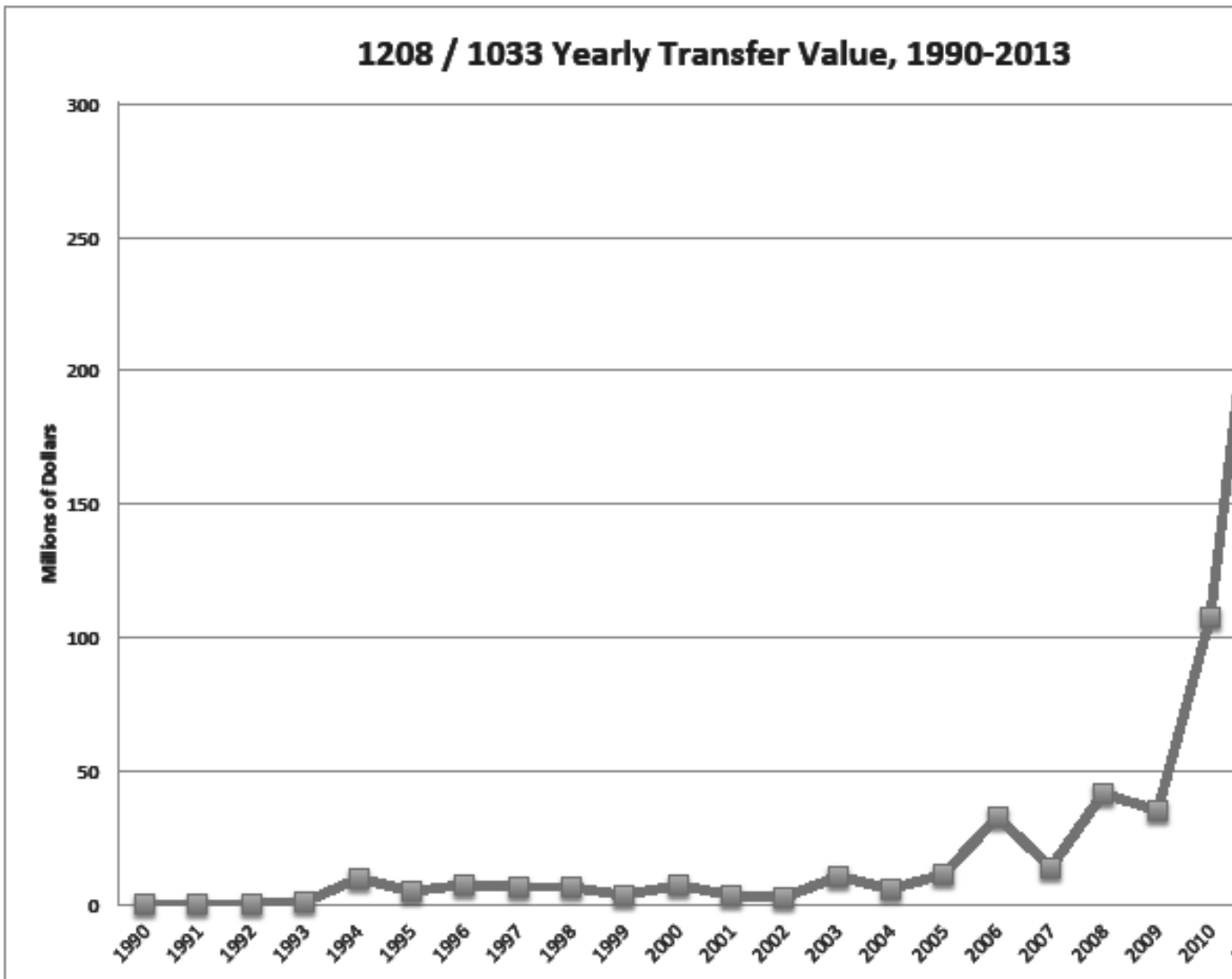


Figure 1: Program 1208 / 1033 Annual Transfer Values, 1990-2013

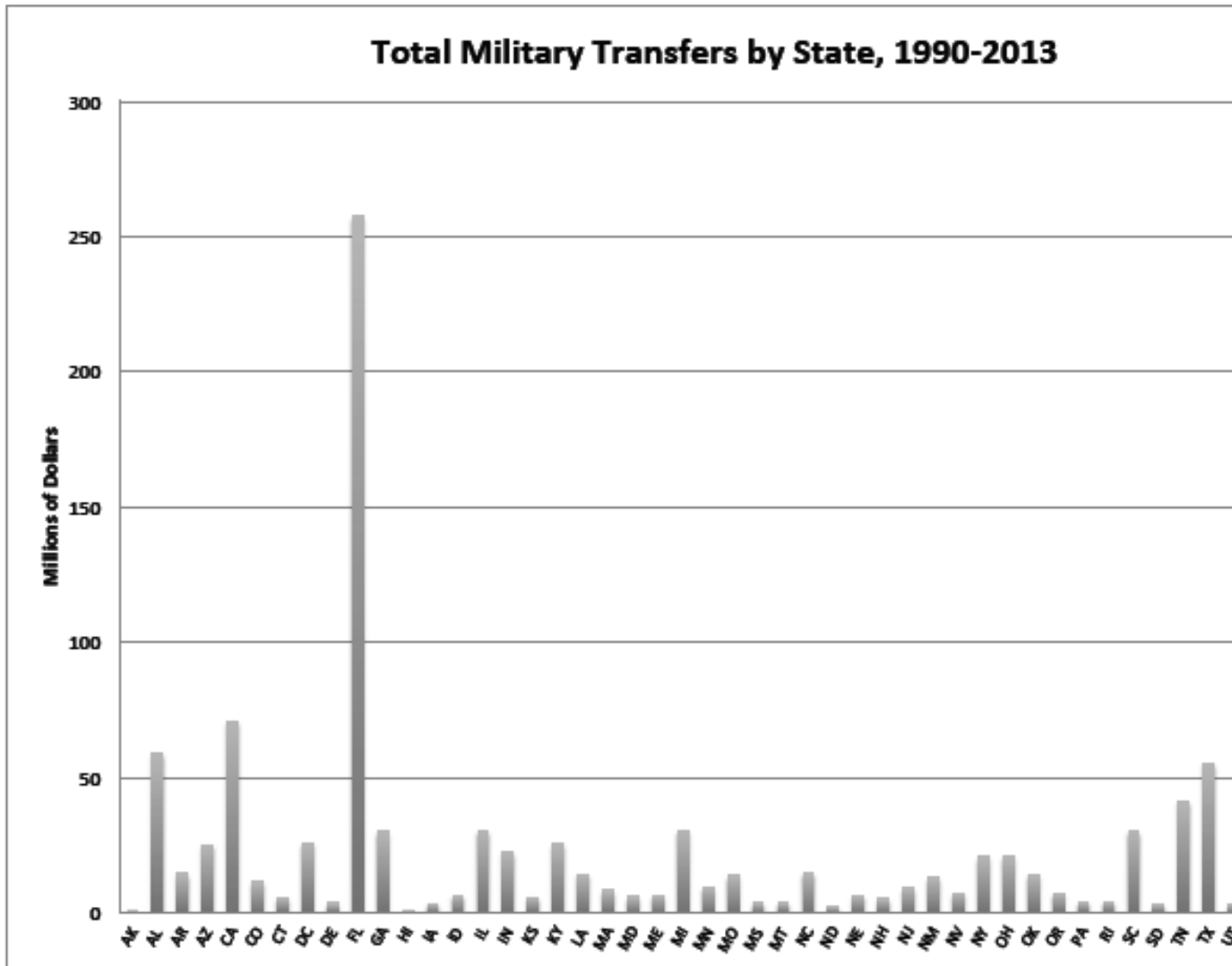


Figure 2: Total Transfers by State, 1990-2013

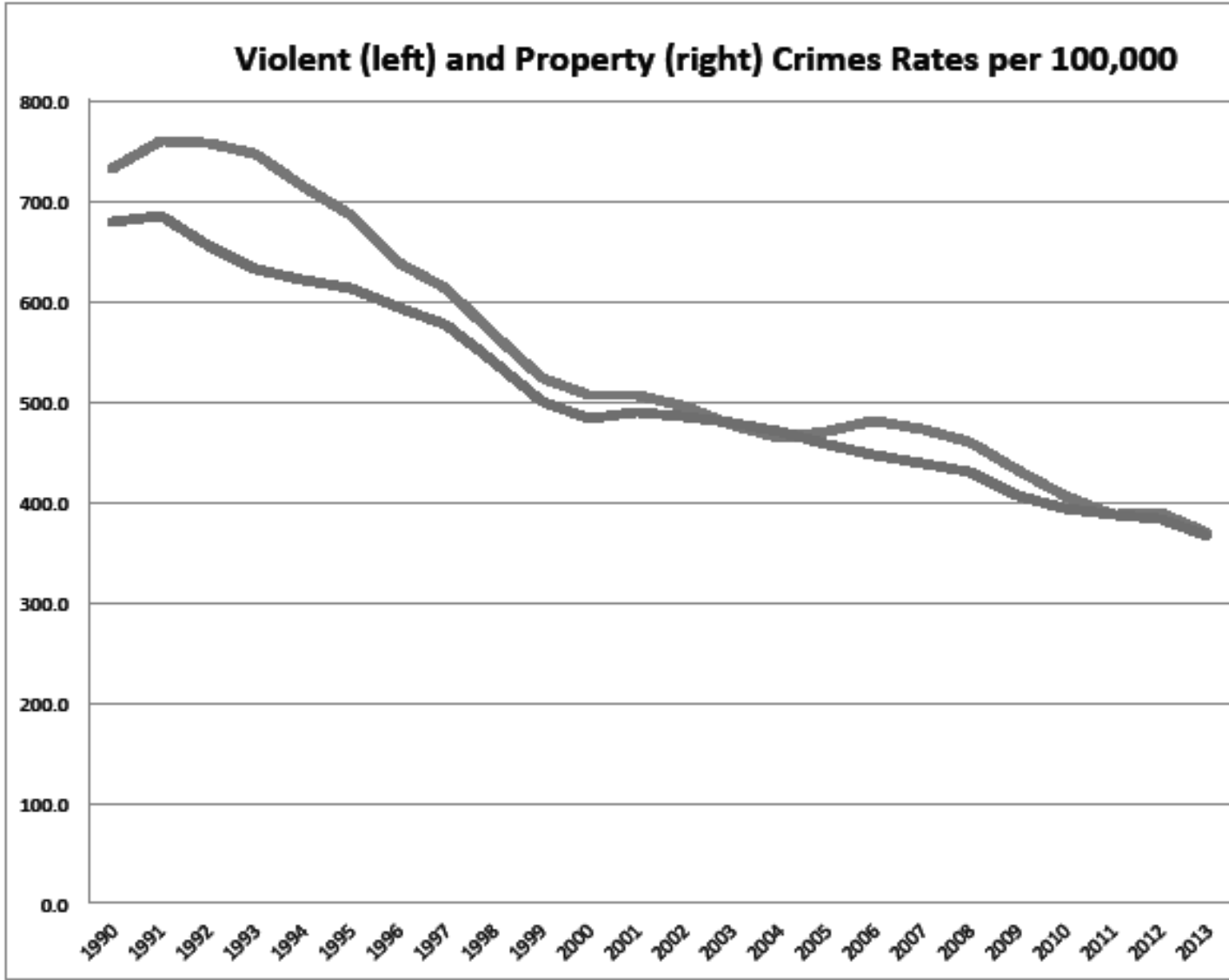


Figure 3: Violent and Property Crime Rates, 1990-2013

	Obs	Mean	Standard Deviation
Violent Crime Rate	1224	473.9	302.2
Murder Rate	1224	6.2	7.1
Forcible Rape Rate	1224	35.4	12.8
Robbery Rate	1224	134.1	133.7
Agg Assault Rate	1224	298.2	177.7
Property Crime Rate	1224	3725.9	1153.5
Larceny Rate	1224	2554.1	731.1
Burglary Rate	1224	798.4	307.3
Motor Vehicle Theft Rate	1224	373.4	242.7
All Transfers	1224	0.85	5.24
Demil Code B-Q (restricted)	1224	0.80	5.14
FSC 1000 (Weapons and Aircraft)	1224	0.34	4.77
FSC 2000 (Marine and Group Vehicles)	1224	0.36	1.28
FSC 5000 (Tools, Construction Materials, Comm. Equip.)	1224	0.09	0.53
State GDP Federal Military (millions)	868	3337	4453
Total DoD Personnel	816	33804.38	42972.6
Military DoD Personnel	816	20646.58	29149.1

Notes: Crime rates are measured as rates per 100,000. Violent Crime is an unweighted aggregate of five subcategories. Property Crime is an unweighted aggregate of three subcategories. All militarization variables are reported in millions of dollars. DoD personnel variables are number of personnel by state on September 30 of each year. Total DoD personnel is the sum of military and civilian DoD personnel.

Table 1: Summary Statistics: Crime and Militarization Variables

Violent Crime Rate	(1)	(2)	(3)	(4)
All Years 1990-2013	0.113 (0.706)	3.043 (1.585)*	-5.793 (2.515)**	-3.117 (1.010)***
1033 Program 1997-2013	1.316 (0.710)*	2.924 (1.494)*	-3.180 (1.232)**	-1.729 (0.432)***
Sample Restriction: Available Controls 2003-2013	1.847 (0.917)**	2.839 (1.453)*	-1.856 (0.720)**	-0.899 (0.203)***
Year FE	No	Yes	No	Yes
State FE	No	No	Yes	Yes

Notes: Each cell reports the coefficient and standard error for police militarization in a regression with the crime rate as the outcome variable. Each row reports the estimated coefficient for a different sample selection. Column 1 estimates a simple bivariate regression with no year or state fixed effects. Column 2 reports a simple bivariate regression with year but not state fixed effects. Column 3 estimates a simple bivariate regression with state but not year fixed effects. Column 4 estimates a simple bivariate regression with both state and year fixed effects. Standard errors are clustered at the county level. Significance: * 10 percent; ** 5 percent; *** 1 percent, **** 0.1 percent.

Table 2: Violent Crime and Militarization - Simple Bivariate Regression

Violent Crime Rate 2003 - 2013	(1)	(2)	(3)	(4)	(5)	(6)
Police Militarization	-0.702 (0.208)***	-0.877 (0.192)****	-1.008 (0.184)****	-0.869 (0.190)****	-0.893 (0.198)****	-0.893 (0.198)****
Unemployment Rate	-10.92 (4.444)**					-8.4 (4.1)
Median Income		18.77 (14.81)				10.1 (10.1)
% Prison			353.2 (145.2)**			30.1 (143.2)
% Black				17.77 (4.083)****		13.1 (8.5)
% Young					-3.999 (2.920)	-0.1 (2.1)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
N	561	561	550	561	561	561
within R ²	0.425	0.435	0.420	0.447	0.397	0.425

Notes: State clustered standard errors in parentheses. Significance: * 10 percent; ** 5 percent; *** 1 percent; **** 0.1 percent. Prison population measurement not consistent in DC over sample period. As a result, DC is dropped from the analysis when Prison Population controls are utilized.

Table 3: Violent Crime and Militarization - with Control Variables

Police Militarization	(1) 1997-2013	(2) 1997-2013	(3) 1997-2013	(4) 2003-2013	(5) 2003-2013	(6) 2003-2009	(7) 2003-2009
Federal Military GDP (Millions)	0.000269 (0.000113)**	0.000228 (0.000101)**	0.000656 (0.000252)**	0.000904 (0.000293)***	0.000853 (0.000349)**	0.000341 (0.000165)**	0.000341 (0.000165)**
Military (DoD) Personnel							-0.000341 (0.000165)**
DoD Personnel							
Unemployment Rate					1.311 (1.174)	0.0749 (0.0425)*	0.0749 (0.0425)*
Median Income					0.314 (0.714)	-0.108 (0.125)	-0.108 (0.125)
% Prison					25.16 (19.53)	2.791 (1.751)	2.791 (1.751)
% Black					-0.913 (0.664)	-0.00657 (0.0810)	-0.00657 (0.0810)
% Young					-0.0504 (0.170)	0.0716 (0.0420)*	0.0716 (0.0420)*
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	Yes	Yes	Yes	Yes	Yes
N	867	867	867	561	550	350	350
R ²	0.04	0.10	0.21	0.26	0.28	0.63	0.63

Notes: State Clustered standard errors in parentheses. Significance: * 10 percent; ** 5 percent; *** 0.1 percent. First stage results are reported for IV regressions presented in Tables 5 and 6. Three data periods are presented based on data limitations as discussed in paper. Police militarization is defined as transfers through 1033 Program in millions of dollars.

Table 4: Instrumental Variables: First Stage Results

Violent Crime Rate	(1) 1997-2013	(2) 1997-2013	(3) 2003-2013	(4) 2003-2013	(5) 2003-2013	(6) 2003-2013
Police Militarization	-1.729 (0.432)****	-22.45 (6.965)****	-0.899 (0.203)****	-0.795 (0.193)****	-15.61 (5.282)***	-15.02 (5.26)***
Unemployment Rate				-8.20 (4.13)*		12.35 (14.74)
Median Income				16.2 (10.43)		22.43 (12.98)*
% Prison				302.7 (143.8)**		537.24 (255.78)**
% Black				13.46 (8.93)		-1.87 (13.11)
% Young				-0.96 (2.14)		-0.465 (2.466)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Instrumented?	No	Yes	No	No	Yes	Yes
N	867	867	561	550	561	550
First Stage F	.	15.02	.	.	15.94	11.59

Notes: State clustered standard errors in parentheses. Significance: * 10 percent; ** 5 percent; *** 1 percent, **** 0.1 percent. Prison population measurement not consistent in DC over sample period. As a result, DC is dropped from the analysis when Prison Population controls are utilized.

Table 5: Instrumental Variables: Federal Military State Spending (Violent Crime Rate)

Violent Crime Rate	(1)	(2)	(3)	(4)
	2003-2009	2003-2009	2003-2009	2003-2009
Police Militarization	-5.53 (1.88)***	-28.31 (14.46)**	-28.67 (14.80)*	-24.88 (11.79)**
Unemployment Rate	-3.31 (3.79)	-0.92 (3.40)	-0.89 (3.41)	-1.28 (3.41)
Median Income	6.78 (8.91)	4.78 (8.72)	4.74 (8.74)	5.08 (8.59)
% Prison	243.1 (116.4)**	263.0 (117.5)**	263.3 (117.8)**	260.0 (115.9)**
% Black	14.1 (6.13)**	12.68 (5.43)**	12.7 (5.42)**	12.88 (5.44)**
% Young	-3.66 (2.56)	-1.11 (2.80)	-1.08 (2.812)	-1.50 (2.73)
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Instrumented?	No	Yes	Yes	Yes
N	350	350	350	350
First Stage F	.	7.08	3.63	3.55

Notes: State clustered standard errors in parentheses. Significance: * 10 percent; ** 5 percent; *** 1 percent, **** 0.1 percent. Prison population measurement not consistent in DC over sample period. As a result, DC is dropped from the analysis when Prison Population controls are utilized. Column (1) is standard regression. Column (2) instruments for police militarization using federal military component of state GDP. Column (3) instruments using first instrument plus number of DoD military personnel in state. Column (4) instruments using first two instruments plus total number of DoD personnel (military plus civilian).

Table 6: Alternative Instruments (Violent Crime Rate)

Violent Crime Rate	(1) Homoskedastic	(2) Robust SE	(3) Cluster SE	(4) Driscoll-Kraay SE
Police Militarization	-15.02 (7.39)**	-15.02 (4.381)****	-15.02 (5.26)***	-15.02 (1.15)****
Unemployment Rate	12.35 (12.25)	12.35 (10.28)	12.35 (14.74)	12.35 (1.77)****
Median Income	22.43 (21.42)	22.43 (10.88)**	22.43 (12.98)*	22.43 (6.97)***
% Prison	537.2 (235.1)**	537.2 (177.4)***	537.2 (255.8)**	537.2 (76.95)****
% Black	-1.872 (14.77)	-1.872 (9.05)	-1.872 (13.11)	-1.872 (4.10)
% Young	-0.465 (6.13)	-0.465 (2.77)	-0.465 (2.47)	-0.465 (1.70)
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Instrumented?	Yes	Yes	Yes	Yes
N	550	550	550	550

Notes: Significance: * 10 percent; ** 5 percent; *** 1 percent, **** 0.1 percent.

Table 7: Alternative Standard Error Assumptions (Violent Crime Rate)

Property Crime Rate	(1) 1990-2013	(2) 1997-2013	(3) '03-'13	(4) '03-'13	(5) '03-'13	(6) '03-'13
Police Militarization	-7.898 (1.438)****	-3.374 (0.954)****	-1.275 (0.863)	-0.897 (1.00)	-54.14 (26.2)**	-45.80 (31.26)
Unemployment Rate				-30.23 (32.42)		34.69 (42.78)
Median Income				41.92 (71.38)		61.56 (56.02)
% Prison				1021.0 (968.7)		1761.7 (701.1)**
% Black				-8.62 (51.81)		-57.05 (43.38)
% Young				20.87 (18.28)		22.42 (15.37)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
IV	No	No	No	No	Yes	Yes
N	1224	867	561	550	561	550
First Stage F	15.94	13.47

Notes: State clustered standard errors in parentheses. Significance: * 10 percent; ** 5 percent; *** 1 percent, **** 0.1 percent. Prison population measurement not consistent in DC over sample period. As a result, DC is dropped from the analysis when Prison Population controls are utilized.

Table 8: Property Crime Rate (PCR) and Police Militarization

LHS	(1)	(2)	(3)	(4)
Violent Crime:				
Murder	0.004 (0.006)	0.002 (0.002)	-0.006 (0.253)	-0.275 (0.119)**
Forcible Rape	-0.041 (0.016)**	-0.021 (0.011)*	-0.977 (0.390)**	-0.836 (0.391)**
Aggravated Assault	-0.651 (0.156)****	-0.582 (0.128)****	-9.990 (3.299)***	-9.376 (3.336)***
Robbery	-0.211 (0.0730)***	-0.194 (0.0851)**	-4.634 (1.886)**	-4.523 (2.098)**
Property Crime:				
Burglary	-0.332 (0.191)*	-0.364 (0.234)	-9.053 (6.103)	-10.45 (7.669)
Larceny	-0.510 (0.572)	-0.564 (0.557)	-36.32 (24.09)	-21.84 (18.35)
Motor Vehicle Theft	-0.433 (0.247)*	0.0304 (0.286)	-8.767 (12.07)	-13.50 (8.703)
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
IV	No	No	Yes	Yes
N	561	550	561	550

Notes: Each cell reports the coefficient and standard error for police militarization in a regression where the outcome variable is listed in each row. Column 1 reports a regression with no control variables and no instruments. Column 2 reports a regression with controls (not reported), but no instrument. Column 3 reports a regression with no controls, but instruments for police militarization. Column 4 reports a regression with controls (not reported) and uses an instrument for police militarization. Control Variables are the same set used in all previous specifications. Year and State fixed effects are included in each regression. State clustered standard errors in parentheses. Significance: * 10 percent; ** 5 percent; *** 1 percent, **** 0.1 percent.

Table 9: Disaggregated Violent and Property Crime Categories

Violent Crime Rate	(1) 1990-2013	(2) 1997-2013	(3) 2003-2013	(4) 2003-2013	(5) 1997-2013	(6) 1997-2013	(7) 1997-2013
High Military Weapons and Aircraft	-1.760 (0.303)****	-1.118 (0.107)****	-0.640 (0.0409)****	-0.520 (0.0815)****	-37.60 (15.83)**	-28.87 (10.13)**	
Medium Military Marine and Ground Vehicles	-13.92 (4.339)***	-6.890 (2.778)**	-4.612 (1.937)**	-4.42 (2.15)**			
Low Military Comm. Equipment, Tools, Building Materials	-10.57 (8.165)	-6.424 (6.404)	-2.066 (3.665)	0.820 (3.13)			-11.1 (26.87)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IV	No	No	No	No	Yes	Yes	Yes
N	1224	867	561	550	867	867	867
R ²	0.682	0.672	0.659	0.690	0.928	0.929	0.929
First Stage F	4.68	8.55	27.1

Notes: State clustered standard errors in parentheses. Significance: * 10 percent; ** 5 percent; *** 1 percent; **** 0.1 percent. Regressions on population. Column (4) includes all controls from earlier tables, while columns (6) and (8) only include controls for other militarization variables highlighted militarization variable only.

Table 10: Violent Crime and Militarization - High and Low Militarization

	(1) 2004-2013 Police Rate	(2) 2004-2013 Police Rate	(3) 2004-2013 Police Rate	(4) 2004-2013 Police Rate	(5) 2004-2013 Police Rate	(6) 2004-2013 Police Rate	(7) 2004-2013 VCR
Police Militarization	0.639 (0.242)**	0.965 (0.413)**	-0.173 (0.118)	-0.0971 (0.130)	-0.641 (1.758)	0.353 (2.788)	-16.33 (6.804)**
Unemployment Rate				-3.867 (2.079)*		-4.534 (5.085)	15.38 (19.69)
Median Income				1.974 (7.237)		1.768 (7.686)	26.03 (13.00)**
% Prison				-5.460 (84.59)		-13.69 (114.4)	508.1 (323.1)
% Black				12.82 (5.627)**		13.37 (6.830)*	-2.736 (17.26)
% Young				1.568 (1.576)		1.565 (1.542)	-1.534 (2.670)
Police Officer Rate							0.0363 (0.129)
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	Yes	Yes	Yes	Yes	Yes
IV	No	No	No	No	Yes	Yes	Yes
N	509	509	509	499	509	499	499
R ²	0.003	0.023	0.187	0.218	0.928	0.8380	0.965
First Stage F	9.76	9.11	8.54

Notes: Police Rate is number of police officers per 100,000 residents. State Clustered standard errors in parentheses. Significance: * 10 percent; ** 5 percent; *** 1 percent, **** 0.1 percent. Columns (1)-(6) focus on the determinants of the police officer rate, while column (7) considers the impact on Violent Crime Rate. Prison data is unavailable for District of Columbia, and therefore dropped from regressions in columns 4,6, and 7.

Table 11: Impact of 1033 Program on Sworn Police Officers

Incarceration Rate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Police Militarization	1.996 (1.021)*	1.948 (1.062)*	0.209 (0.196)	0.290 (0.185)	0.304 (0.186)	-12.88 (6.137)**	-13.43 (5.727)**	-13.30 (5.705)**
Total Crime Rate				0.0159 (0.0102)	-0.00318 (0.00887)		-0.00926 (0.0203)	-0.0194 (0.0102)*
Lagged Total Crime Rate					0.0217 (0.0101)**			0.0117 (0.0164)
Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
IV	No	No	No	No	No	Yes	Yes	Yes
N	850	850	850	850	850	850	850	850
R ²	0.006	0.016	0.965	0.966	0.966	0.968	0.968	0.969
First Stage F	12.60	8.68	10.18

Notes: Incarceration Rate is number of incarcerated per 100,000 residents. State Clustered standard errors in parentheses. Significance: * 10 percent; ** 5 percent; *** 1 percent, **** 0.1 percent. Total Crime Rate is the aggregate of the violent crime rate and property crime rate. The data covers 1997 until 2013 for all 50 states.

Table 12: Impact of 1033 Program on Incarceration Rates