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Abstract

I examine the effect of local police militarisation on violent crime using evidence from the 1033-programme in the US. Exogenous cost shifters due to the particular logistics of the programme are exploited to instrument for the amount of equipment received by local law enforcement. The results do not support previous county-level studies, who find strong and consistent negative effects on crime. I show that those findings are likely based on a combination of (i) inconsistencies in the underlying data and (ii) limited comparability of different subsamples. Accounting for these factors, I find only weak evidence of a negative impact on violent crime – notably for more rural areas, which form a majority of US counties. For this subsample, the results do not support the notion that military equipment enhances the effectiveness of enforcement agencies: if anything, arrests fall while any resulting crime reduction is of negligible economic significance.

Keywords

Police militarization, 1033-program, violent crime, police violence

JEL Classification

H56, H76, K42, Z18

I. Introduction

"This is my rifle. There are many like it, but this one is mine. My rifle is my best friend. It is my life. I must master it as I must master my life. My rifle, without me, is useless. Without my rifle, I am useless."[1]

These words from the 'Rifleman's Creed' of the US Marine Corps doctrine appear out of place in the context of local law enforcement. But while it is true that they are not part of a police officer's training, the associated military culture and hardware has gradually made its way into the reality of everyday policing in US cities and towns.

Even though the phenomenon dates back much further, the large scale militarisation of some local police forces was brought into the national spotlight by the events following a fatal encounter between police officers and an unarmed black teenager in Ferguson, Missouri, in 2014, when pictures of scores of heavily armed and armoured policemen clashing with protesters prominently featured in media outlets.[2] More recently, the death of George Floyd in May 2020 led to a resurgence of widespread protests against police practices.

These high-profile events aside, aggregate crime statistics show a more positive picture: there has been a marked decline in most crimes over the last 15 years. However, this development has neither been shared by all the regions within the country, nor are the trends unanimously positive. According to the Economist, clearance rates for homicides have declined from roughly 90% in 1965 to 64% in 2012, which many officials blame on a lack of resources.[3]

Until recently, the question of whether the provision of military grade equipment through the so-called *1033-programme* is able to reduce crime by bridging material shortages of police forces has not been subject to empirical scrutiny. Using newly released equipment disposition data, two papers – Bove and Gavrilova (2017, henceforth BG) and Harris et al. (2017, HPBM) – provide the first large scale systematic evaluation of the programme and find sizeable negative effects on street level crime rates without indications of increased police brutality due to heavier equipment.

In contrast to their contribution, I focus exclusively on local law enforcement, rather than

[1] Source: http://www.usmcpress.com/heritage/marine_corps_rifleman%27s_creed.htm, as of 15 July 2019.
[2] See e.g. Li et al. (2014).
[3] The Economist (2015).

local, state and federal agencies combined. Similar to the instrumental variable approach of HPBM, identification is based on exogenous cost shifters caused by the physical distance between equipment distribution sites and law enforcement agencies.

My contribution to the existing literature is twofold: First, I illustrate the large caveats associated with the data on military equipment disposition in the 1033-programme and the caution necessary in interpreting the results of earlier publications. Only a subset of material categories is traced with sufficient reliability, such that any attempts of disentangling the effects of different equipment types is unlikely to succeed based on existing approaches.

Second, I provide an alternative view of the impact of large scale equipment inflows on small-town law enforcement agencies, which are arguably less likely to encounter situations that would require the use of military grade items. For such agencies in particular, my results do not show a strong effect of the presence of military equipment on the types of violent crime considered. There is some evidence of a decline in robberies, but the size of the measured short-run effect translates into an upper-bound estimate of roughly -0.041 for the elasticity of the corresponding crime rate with respect to the value of material receipts. This is only a fraction of what is typically measured for other interventions.[4] For aggravated assault and murder, no robust effects were found. However, despite the lack of clear impacts on crime rates, there is evidence that violent crime arrest rates fall in response to the material inflows. Judging by these outcomes, it appears that the 1033-programme failed to enhance the effectiveness of small town agencies and may even have increased their potential exposure to officer involved shootings.

The remainder of this paper is organised as follows: Section II provides some practical and empirical context. Section III summarises the dataset. Section IV describes the empirical strategy used to get to the results in Section V. Section VI discusses these findings. Section VII concludes.

[4] See e.g. Chalfin and McCrary (2017).

II. Policing, Crime and the 1033-Programme – Background

A. A Brief Introduction to the 1033-Programme

Section 1033 of the National Defense Authorisation Act (NDAA) of 1997 created a permanent legal basis for the transfer of military grade equipment to local law enforcement and gave rise to what is now frequently called the '*1033-programme*'.

According to the website of the Defense Logistics Agency's Law Enforcement Support Office (DLA-LESO), which is in charge of equipment distribution, surplus valued at more than $6.8 billion has been transferred up until 2017, with more than 7,000 participating agencies.[5]

The initial purpose of the programme was to equip federal and state law enforcement agencies with 'suitable' military surplus material to support anti-drug and counter-terrorism efforts. Over the years, the programme has been extended from the initial distribution of some firearms to a relatively small number of agencies to the provision of equipment worth several hundred million dollars every year (see Section III for more details). By the same token, the definition of 'suitable' equipment for local law enforcement has evolved to include grenade launchers and mine resistant ambush protected vehicles (MRAPs). However, the range of items distributed extends beyond weapons and vehicles, and also includes gear/clothing items, office equipment and various tools.[6]

Following the public outcry after the events in Ferguson (MO), the Obama administration introduced stricter rules on equipment distribution in May 2015, banning the distribution of tracked vehicles and high-calibre rifles, among other measures,[7] although the practical significance of these restrictions has been questioned.[8] In August 2017, president Trump lifted the restrictions on the 1033-programme imposed by his predecessor.[9]

I refer to HPBM for a more in-depth description of the operational side. The most

[5]The valuation is based on acquisition value – see http://www.dla.mil/DispositionServices/Offers/-Reutilization/LawEnforcement.aspx, retrieved on 08 March 2018.

[6]Due to the properties of the data, it is not possible to give a clear indication of the proportion of non-combat related items. See Section III for more details.

[7]See Nakamura and Lowery (2015).

[8]Only an insignificant proportion of items distributed until 2014 falls into any of the banned categories, see National Public Radio (2015).

[9]Executive order 13809, "Restoring State, Tribal, and Local Law Enforcement's Access to Lifesaving Equipment and Resources", available at https://www.gpo.gov/fdsys/pkg/DCPD-201700590/content-detail.html, as of 11 June 2019.

important programme features are that (i) any equipment is distributed free of charge, but recipients have to cover the cost of transportation, and (ii) agencies have to make use of the equipment within one year. In addition, (iii) once a certain item is declared military surplus, the time frame for local law enforcement agencies to act is just 14 days.[10]

The implication of these programme features is that agencies closer to one or more DLA distribution centres face lower costs of acquiring military equipment both in terms of shipping and time needed for on-site inspection in case of larger items such as vehicles.

B. Empirical Evidence

Due to the history of police militarisation in the United States, spanning a period of several decades at the very least (see e.g. Balko, 2013), the criminology literature has produced a considerable body of (mainly qualitative) research on the phenomenon. An overview is given by Kraska (2007), who distinguishes between four interconnected dimensions of police militarisation: (i) material, (ii) cultural, (iii) organisational, and (iv) operational. He observes a trend towards stronger militarisation far beyond the material dimension, which is the main focus of this paper. For example, he notes that the role of SWAT teams has largely moved from a purely reactive force designed for extreme events to a more proactive mode of operation, where these units are increasingly deployed for routine operations such as drug raids or patrol duties.

Turning to quantitative studies, BG and HPBM provide the first thorough analyses of the impacts of the 1033-programme. Their results point to a negative effect of police militarisation on street-level crime, but they find no evidence of increased police brutality – although the authors emphasise that the available data on police violence is much more limited than the available statistics on crime. A few other empirical studies have also examined the 1033-programme, most notably Masera (2019a,b). He uses more granular agency level data and finds substantial effects on crime (large reductions), but notes that there are spillovers to nearby areas where police forces are less militarised. In addition, he claims that over 400 additional police killings might have occurred due to the presence of

[10] See http://www.dla.mil/Portals/104/Documents/DispositionServices/LESO/DISP_QuickStartGuide_-20170306.pdf, accessed on 10 October 2017.

military grade equipment supplied by the 1033-programme.[11]

Both HPBM and Masera (2019a,b) use an instrumental variable approach based on the distance to equipment distribution centres, while BG rely on interactions of military expenditures and an estimated take-up probability to identify the parameter of interest. The effect sizes vary quite substantially across studies, but they are in agreement regarding the signs of the parameters.

Using SWAT presence and deployment data in a generalised difference-in-differences setting, Mummolo (2018) disputes the negative effect on crime and also finds no effect on officer safety. Further examples are Ajilore (2017), who looks at documented use of force incidents in an instrumental variable probit framework, but finds no convincing evidence of the 1033-programme increasing the frequency of such events, and Lawson (2019), who provides correlational evidence of an association between officer-involved shootings and the degree of militarisation. A more unorthodox approach is taken by Delehanty et al. (2017), who use a collaborative database on dog killings by law enforcement to explore another dimension of potentially elevated police violence in response to the programme. However, their dataset and reported results are too thin to draw firm conclusions.[12]

There have been other studies focusing on deterrence effects of policing more generally, most of which are summarised in Chalfin and McCrary (2017). They see mixed evidence regarding deterrence effects of different measures. While economic opportunities and improved policing can be effective in reducing crime, the severity of punishment appears to be of much lower relevance.

However, perceptions about consequences of criminal behaviour do not necessarily match the real parameters. Surveying a sample of adults of 54 urban counties in the US, Kleck and Barnes (2013) find wildly inaccurate perceptions of both the risk of getting caught and the severity of the resulting punishment. On the other hand, there is some evidence from data on young males that those more directly involved in criminal activities have

[11] However, it is not entirely clear what kinds of controls he employs in the estimations (none on display), and the absence of such variables would place a lot of weight on his instrument. Also, he seems to be unaware of the consistency problems in the equipment disposition data.

[12] Although they report a positive effect of militarisation on both civilian deaths and dog killings, these conclusions are based on just four states and only a few hundred county-level observations. In addition, identification is not based on instrumentation or any other standard econometric technique and mainly rests on informal arguments.

more accurate beliefs (Lochner, 2007). Nonetheless, they are still placing more emphasis on personal experiences to determine their likelihood of getting caught than a purely rational framework would suggest. A more comprehensive review of this issue can be found in Apel (2013).

While the positive effects of a short term deployment of additional police officers have been documented in several studies using exogenous variation linked to terrorism and elections (Di Tella and Schargrodsky, 2004; Draca et al., 2011; Klick and Tabarrok, 2005; Levitt, 1997), the impact of a general infusion of capital/equipment or funds not reserved for additional hiring of police personnel is more scarcely covered by the existing literature. Looking at a quasi-experimental increase in police funding directed at combating street level crime in England and Wales, Machin and Marie (2011) find sizeable reductions in robberies in treated regions. However, the additional funds were primarily invested into more overtime and staff – only a minor portion went into capital investment (IT infrastructure).

In sum, much of the existing literature concludes that the 1033-programme led to reduced crime rates for street-level offences. The negative consequences remain more obscure, which is mainly due to the scarcity of data in this area. Some researchers have tried to differentiate the effects by equipment type. But as shown in the next section, the validity of such attempts is doubtful given the range of inconsistencies in the equipment data.

III. The Data

A. Data Sources

The present dataset is built from original sources, which are all public. The information on the supply of military equipment and location of distribution centres is published by the issuing office (the LESO) itself. The equipment data used consists of the 30 September 2017 release.

All the data on crime and law enforcement agencies is from the FBI Uniform Crime Reporting programme (UCR), but was accessed through the Inter-university Consortium for Political and Social Research (ICSPR) online platform.[13] The crime data used is part of the "*County-Level Detailed Arrest and Offense Data*" - releases, which have already been aggregated to the county level. On the agency level (number of employees, officers assaulted or killed in action), the "*Police Employee (LEOKA)*"-releases by the ICSPR were used.[14] Figure 4 plots equipment stocks and the evolution of crime rates over time.

Demographic and geographic data are sourced form the US Census Bureau. For the economic variables (poverty and income), the Census Bureau's "*Small Area Income and Poverty Estimates (SAIPE)*" - data was used because it is more accurate than American Community Survey (ACS) data. Unemployment was sourced directly form the US Bureau of Labor Statistics. Data on the veteran population is published by the US Department of Veteran Affairs, but was accessed through a proprietary data service (SAGE stats).

Data on fatal encounters between police and civilians are based on *fatalencounters.org*, an independent project not linked to any official body. This dataset captures more than 23,000 police killings from 2000 onwards and was assembled by journalists and other contributors. It is the most extensive dataset available on this issue. Table 56 in the appendix contains web links to all the data sources.

[13] The ICSPR adjusts the UCR data for changes in response by reporting agencies and imputes crime data where there was only partial reporting.

[14] The ICSPR also provides a crosswalk to link agency level data to census information.

B. Sample

In my dataset, 751 out of 3,007 counties have no equipment recorded.[15] Since I do not model selection into the programme, only agencies with a positive stock of military equipment will be considered. Therefore, any results measure effects on the intensive margin, i.e. the impact of getting more military equipment *conditional* on having already self-selected to participate. This contrasts with BG and HPBM. Both papers include all counties in what is similar to a difference-in-differences setting, but with a continuous treatment variable.[16] In the present case, distance to a distribution centre alone is unlikely to account for the major drivers of the decision to participate in the programme.[17]

Regarding the entities included in the sample, I focus exclusively on local law enforcement, which is largely equivalent to local police forces and the county sheriff's department.[18] Equipment supplied to state-wide agencies and local branches of federal institutions is difficult to allocate to specific counties and likely to be an uninformative measure of the militarisation of such entities. Removing such bodies translates into dropping 731 out of 7,474 entities from the DLA equipment data. Appendix L provides more information on the types of entities excluded.

[15]This number refers to the subset of equipment that was more reliably traced, it is therefore slightly higher than what e.g. BG report.

[16]BG rely on an ex-post evaluation of the frequency of material take-up to estimate the county-inherent suitability for treatment, akin to Nunn and Qian (2014). HPBM follow an approach based on Nunn and Qian (2011), which relies on the assumption that selection into treatment is well approximated by considering distance to the nearest centres, county land area and high-intensity drug trafficking (HIDTA) designation, although it is not clear why the last one should be exogenous to local crime rates.

[17]For example, Nebraska has large number of counties where local police forces did not participate, but the population density in the state is less than 10 people per square kilometre. Indeed, comparing Table 1 with Table 8 in the appendix reveals that non-participating counties have much lower crime rates (e.g. median crime rate for robberies is about 75% lower over the sample period) and typically only about a third of the population density, also judged by the median. Therefore, such counties are unlikely to constitute a suitable control group in terms of criminal behaviour.

[18]Both BG and HPBM used equipment data that was released on the county level, so they were unable to filter out these agencies.

Variable	N	Mean	Std. Dev.	Min	Max	Median
Crime rates (crimes/100,000 pop)						
Serious (part 1) offences, total	22,964	2,490.68	1,443.98	0	11,380.97	2,284.48
Violent crime, total	22,964	264.62	222.55	0	2,584.46	207.2
Property crime, total	22,964	2,226.06	1,281.43	0	9,925.06	2,054.83
Murder	22,964	3.25	5.32	0	97.62	1.32
Robbery	22,964	41.9	64.25	0	785.24	18.75
Aggravated assault	22,964	193.12	173.41	0	2,086.25	148.2
Arrest rates (arr./100,000 pop.)						
Total arrests, serious offences	22,964	529.57	357.39	0	3,400.18	489.03
Total arrests, violent crime	22,964	122.88	106.34	0	1,342.1	99.36
Total arrests, property crime	22,964	406.63	286.43	0	2,838.39	369.59
Arrests, murder	22,964	2.87	5.69	0	161.81	0
Arrests, robbery	22,964	15.57	21.32	0	639.34	8.55
Arrests, aggravated assault	22,964	97.68	90.54	0	1,178.32	76.16
Economic/demographic controls						
Unemployment rate, percent	22,954	7.11	2.93	1.1	25.5	6.6
Population, in 100,000	22,964	1.21	3.54	.01	100.67	.35
Pop. density, pop./sq. mile	22,964	248.49	848.09	.25	18,144.31	57.84
Veteran population, in %	22,963	8.82	2.2	1.29	24.46	8.6
Black population, in %	22,964	9	13.49	0	96.07	3.41
Median household income, $	22,964	44,232.69	11,590.74	17,843	125,635	42,076.5
Poverty rate, in %	22,964	16.07	6.1	2.4	62	15.3
Land area, sq. miles	22,964	977.6	1,397.24	2	20,056.94	608.86
Military equipment stocks (t-1)						
Total value, $	22,964	73,871	327,454	0	13,800,000	4,045
Total value/100,000 pop., $	22,964	121,992	4,617,532	0	14,100,000	11,831
Value of guns, $	22,964	7,390	22,413	0	711,531	2,111
Value of vehicles, $	22,964	49,972	238,257	0	8,614,417	0
Officer related variables						
Officers killed (share, in %)	18,245	.05	1.1	0	65.29	0
Officers assaults/100 officers	18,245	19.95	40.97	0	1,200	10.04
Officers/1,000 pop.	18,245	1.77	.65	.1	10.01	1.68
Police killings	22,964	.55	2.3	0	83	0
Year	22,964	2009.55	2.87	2005	2014	2010

Table 1: *Summary Statistics*

C. Peculiarities and Inconsistencies in the Equipment Data

The equipment data as released by the DLA does not contain standard agency names, nor any geographical indication other than the state. The roughly 6,700 local agencies included in my sample were matched to their respective counties using the Google Maps API for agency coordinates, and the Federal Communication Commission's Census Block Conversions API to match coordinates to standardised county codes.[19]

Moreover, the officially stated total equipment supply figure of over $6.8 bn[20] does not match the total value of equipment in the data, which is just over $2.4 bn over a similar time horizon. This highlights a more fundamental issue: not all the distributed items are listed.

According to the chief of public affairs of the DLA Disposition Services, Kenneth Macnevin,[21] the reason for this discrepancy is that the DLA distinguishes between different categories of equipment, namely '*controlled*' and '*non-controlled*' items. *Controlled* items, such as firearms, aircraft or armoured vehicles, remain property of the DLA and as such only constitute a loan to law enforcement authorities. Therefore, their location must always be accounted for. On the other hand, non-controlled items such as clothing or office equipment are typically included in the DLA-LESO data for one year before being dropped out of their releases.[22]

The principal motivation behind the released data is to track the controlled equipment currently in possession of law enforcement entities, which also implies that items that have been returned to the DLA are no longer shown in the data releases.[23]

[19] I used several verification steps to ensure accuracy of the matching, including allowing for non-uniqueness of the Google result, ensuring state matching, and verifying that the building type logged in the Google server answer was consistent with designated public service/police buildings. Manual corrections were made where the abbreviated agency name in the DLA data was too cryptic for Google's algorithms.

[20] http://www.dla.mil/DispositionServices/Offers/Reutilization/LawEnforcement.aspx (retrieved on 5 January 2018).

[21] He is the main source of the detailed data information provided here. The following *GitHub* page also provides a good overview of the equipment data and known issues: *https://github.com/SCPR/kpcc-data-team/blob/master/guides/primer-on-defense-logistics-agencys-1033-program-data.md*, last accessed on 13 August 2019.

[22] I write 'typically' because the data shows a rather erratic picture with some non-controlled items remaining in the dataset for years. There is by no means a clean cut-off date for when these items are dropped.

[23] I couldn't find any information on the extent of the returns, but there is no indication that this is a major proportion. The main cause of returns would likely have been the tougher guidelines imposed under Obama, but even there an article by the National Public Radio (2015) notes that for agencies already in possession of equipment banned from distribution, "*[...] it's still being determined whether those law enforcement agencies should give up the equipment.*", hinting at a general reluctance to recall items already issued. Looking at the number of guns recorded until the end of 2013 in several different releases, the number dropped from about 90,000 in the November 2014 release to roughly 80,000 in the September 2017 release.

A further issue is that different DLA releases show different shipment dates for the same transaction. The number of these instances is not trivial - the distribution pattern changes completely in the case of guns (see Figure 1). The reason for this appears to be a change in database, where the date of migration has been confused with an actual shipping date, which was mostly fixed in newer releases.[24]

In conclusion, the track record of the DLA's efforts to account for the whereabouts of their material is mixed, although in recent years it appears that the remaining problems concern oversight rather than proper handling of transaction data.[25]

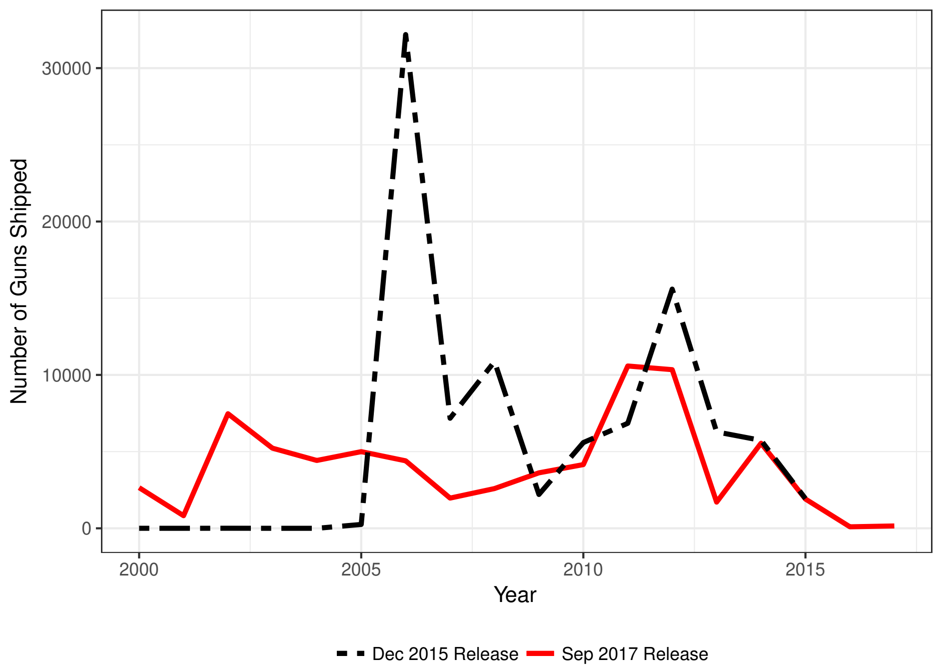

Figure 1: *Comparison of Gun Shippings Across Data Releases*
The discrepancies relative to older versions of the data (as used by HPBM and BG) are clearly visible, especially from 2006-2008.

In the present analysis, I will account for the above issues in three ways. First, I do not use any shipping data released prior to September 2017 to avoid wrong shipping dates as

[24]To quote form my communication with the DLA: "*The data folk say we updated almost all of the weapons with their original ship[ing] dates.*"

[25]According to Wagner (2012), an internal audit by the Dep. of Defense's inspector general in 2003 ruled the transaction data unreliable due to missing or wrong information in a majority of transactions - although I was not able verify this from official sources. A more recent review by the United States Government Accountability Office (2017) was satisfied with the general quality of the transaction data, but found a rather serious lack of oversight: The office was able to register as a non-existent law enforcement agency and successfully requested military equipment in excess of $ 1 million.

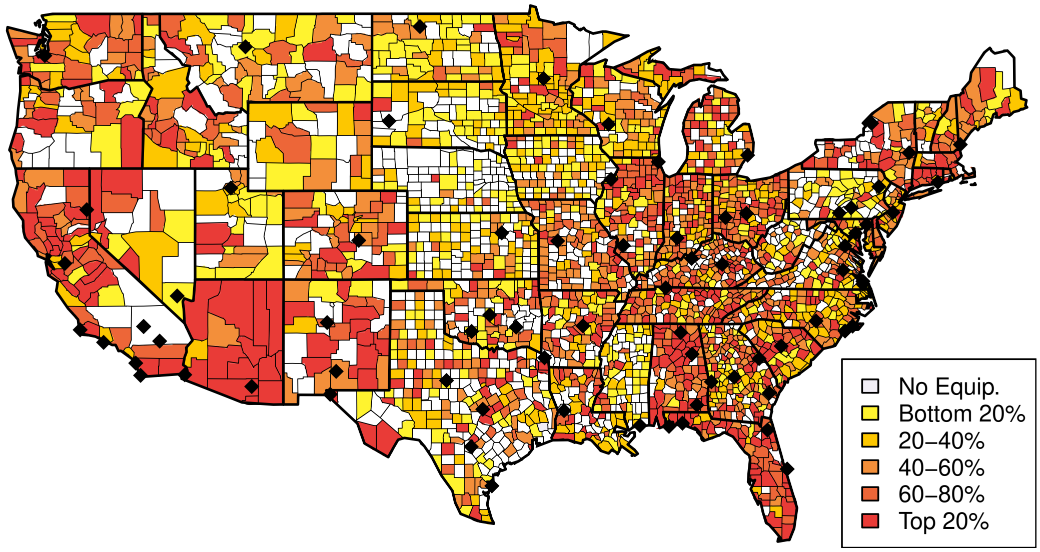

Figure 2: *Geographical Distribution of Equipment Stocks in 2014*
This map is based on the more consistently recorded subset of equipment used in my analysis. The shaded areas refer to the quintiles of the value of equipment for all agencies that have participated in the programme. The black dots mark DLA equipment distribution sites.

far as possible. I also limit my main analysis to the period after 2004 to minimise the impact of the years where the equipment tracking was least reliable.

Second, I remove all the non-controlled equipment contained in the data. This is equivalent to dropping about 38,000 out of 180,000 shipping entries in the dataset. For equipment recorded in 2017, this restriction will keep only 8% of shipped items, but about 55% of the total value recorded. However, the amount of controlled equipment is a good predictor of the amount of non-controlled equipment an agency receives.[26] This suggests that the amount of controlled equipment will still be an informative measure of treatment intensity under the programme.

Third, I create further subsets of the data containing only items where the incentive to reliably track them is highest, namely guns and heavy vehicles. More details on the categories can be found in Appendix L.

[26] For agencies who received both controlled and non-controlled equipment in 2017, the correlation between the two categories is 0.85 for the number of items and 0.29 for their value (this value is lower due to a number of very expensive non-law enforcement related items such as meteorological stations in the non-controlled equipment). The corresponding values for 2016 are 0.96 and 0.76. For 2015, there are hardly any non-controlled items left. Agencies who exclusively received non-controlled equipment obtained only about a fifth of the total non-controlled material even though their number is larger than the number of agencies who received both, which implies a much lower equipment take-up per agency.

IV. Empirical Strategy

Unless all the equipment requests were akin to the small town of Keene (N.H.) acquiring a fully armoured personnel truck (BearCat) to protect their annual pumpkin festival from terrorist threats,[27] an agency's demand for equipment is likely to be positively correlated with local crime rates.

To account for this endogeneity problem, two aspects of the 1033-programme can be exploited: How much material the programme releases across time and how the material is distributed to local law enforcement. In particular, these sources of exogenous variation are (i) the decision by the DLA on how much material is declared military surplus in any given year, (ii) the distance to the nearest distribution site as a time-invariant cost shifter due to shipping and transport costs.

These can be combined into a set of instruments for the amount of surplus in any given county over time. Figure 3 illustrates the relationship between the distance to the nearest distribution site (measured in miles of road distance from a county's population centre) and equipment stocks. The corresponding exclusion restriction requires the total volume of equipment released by the DLA and the distance to the distribution sites to be uncorrelated with local crime rates as well as any other outcome of interest.

The amount of material declared surplus by the DLA depends on the needs of the US military – the agency does not acquire any material for the explicit purpose of providing it to local authorities.[28] Therefore, it appears unlikely that there is any correlation with local crime rates.

The location of the distribution centres is also driven by military considerations because their primary purpose is to fulfil the logistic requirements of the troops. However, these sites' locations (indicated in Figure 2) are determined by other practical aspects as well – for obvious reasons it is not possible to have an army base in the middle of a large city for example (distribution centres are always at or near a military base).

[27]As pointed out in an August 2014 episode of *Last Week Tonight*, a late-night TV show. Ironically, 2 months later there were 30 injuries after riots broke out at said pumpkin festival (though the BearCat was apparently not used), see https://www.vox.com/2014/10/20/7009803/pumpkin-riot-keene-new-hampshire-festival-police-white-john-oliver-ferguson, retrieved on 02 March 2018.

[28]As stated on the program website, *http://www.dla.mil/DispositionServices/Offers/Reutilization/Law-Enforcement.aspx*, last accessed on 02 March 2018.

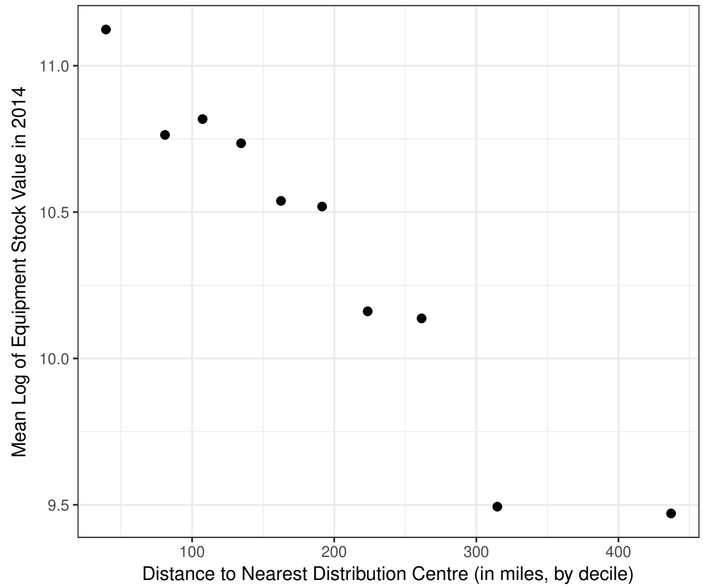

Figure 3: *2014 Equipment Stock by Distance to Closest Distribution Centre*

One particular aspect that could pose problems to the identification strategy is that towns close to distribution centres might differ in terms of crime rates or criminal behaviour more generally. For example, if soldiers return from combat at the same time as large portions of the equipment, then any effect these soldiers have on the local crime rates would bias my estimates. Indeed, there is a sizeable body of literature pointing to higher propensity to commit violent crimes among returning soldiers and near military bases in general (e.g. Anderson and Rees, 2015; Galiani et al., 2011; MacManus et al., 2012; Rohlfs, 2010: Sreenivasan et al., 2013; White et al., 2012). Even though the literature does not suggest that these effects are of actual significance for total crime rates, I control for the veteran population in each county in addition to the fixed effects specification.[29]

The estimated models are of the form

$$Y_{c,t} = \eta_0 + \gamma ln(Eq\hat{ui}p_{c,t-1}) + X'_{c,t}\beta + \eta_c + \eta_t + \epsilon_{c,t}, \quad (1)$$

where $ln(Eq\hat{ui}p_{c,t-1})$ is the instrumented lagged stock of military grade equipment of county c, measured by the log of its total value, η_c and η_t are county/time fixed effects.

[29] In the Appendix, I rerun the main estimations without counties with a population centre closer than 60 miles to a distribution site to further test for biases due to potential local effects around these sites. This does not change the qualitative and quantitative nature of the effects (see Appendix Tables 53, 54, and 55).

Y is the outcome of interest, usually a crime rate, $X_{c,t}$ is a vector of county economic and demographic controls, $\epsilon_{c,t}$ is the error term.

The corresponding first stage is

$$ln(Equip_{c,t-1}) = \kappa_0 + Instruments'_{c,t-1}\delta + X'_{c,t}\xi + \kappa_c + \kappa_t + u_{c,t}. \tag{2}$$

For the main specifications, the instrument 'vector' only contains one element, the first principal component of the street distances, and is generated out of the distance instruments for the 10 closest locations $j = 1, ..., 10$, taken from (ii) below:

(i) $\sum_{j=1}^{10} \omega_{j,PC1} * ln(TotEquipValue_t) * Dist_{c,j}$.

Other specifications use a larger set of instruments of the form

(ii) $ln(TotEquipValue_t) * Dist_{c,j}$,

which allows to perform tests of over-identifying restrictions.

$ln(TotEquipValue_t)$ denotes the log of the aggregate dollar value[30] of the controlled equipment shipped to the counties in the sample up until time t, i.e. the total equipment stock at t. $Dist_{c,j}$ refers to the street distance (in miles) from the population centre of county c to the j-th closest DLA distribution site.[31] $\omega_{j,PC1}$ represents the weight given by the first principal component to the distance to the j-th closest distribution site.[32]

Table 2 shows that the first stage performance of this instrument is stronger than alternative specifications based on subsets of distance instruments, although in both cases there is a clear negative relationship between the equipment stock and the distance to the closest distribution sites. The pattern holds for both aggregate and per capita militarisation

[30]No attempt has been made to adjust the value for inflation, because the market value of most of the goods would differ from the listed prices, which are based on the initial acquisition cost for the military. But as shown in Appendices C and F, the results are robust to using the number of items instead of their value.

[31]The street distances were generated using *OpenStreetMap* data and the *OSRM API* provided in the *R* package *osrm*.

[32]I include more distribution centre locations than HPBM, because I could find no evidence to substantiate their claim that most of the items get routed through so-called 'field activity centres', a subset of DLA distribution locations. My instruments perform much worse when I limit the analysis to the distances to those centres. In response to my queries, the DLA Disposition Service's chief of public relations wrote that they would not gather material at specific sites for the purpose of distribution to law enforcement and that the size of each site (or rather the number of troops it serves) would likely be the best indicator of how much material is available at any given DLA Disposition Services location. Unfortunately, this information is hard to come by because the amount of military personnel at a given site is partially confidential. Therefore, any attempt to classify the importance of specific distribution sites is beyond the scope of this draft.

measures. Somewhat counter-intuitively, the distance to the closest distribution centre is not among the top predictors of equipment uptake. However, it is beneficial to have several sites close-by because a particular item might not be available at the nearest location. And the distance to the third closest site also contains information on how far away the first two are, lending further plausibility to these findings.

Dependent Variable:	ln(Equipment)		ln(Equip. / 100,000 pop.)	
	(1)	(2)	(1)	(2)
ln(value) * $distancePC1$	-1.653***		-1.691***	
	(0.224)		(0.255)	
ln(value) * $\frac{distance\_2}{1000}$		-0.960**		-1.025**
		(0.377)		(0.429)
ln(value) * $\frac{distance\_9}{1000}$		-0.369*		-0.364
		(0.213)		(0.245)
Observations	22953	22953	22953	22953
KP F-statistic (excluded Instr.)	54.352	29.855	43.841	24.832

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Controls include county/year fixed effects, demographic (black and veteran population, pop. density) and economic (poverty and median income) variables.

Table 2: *First Stage Estimates*

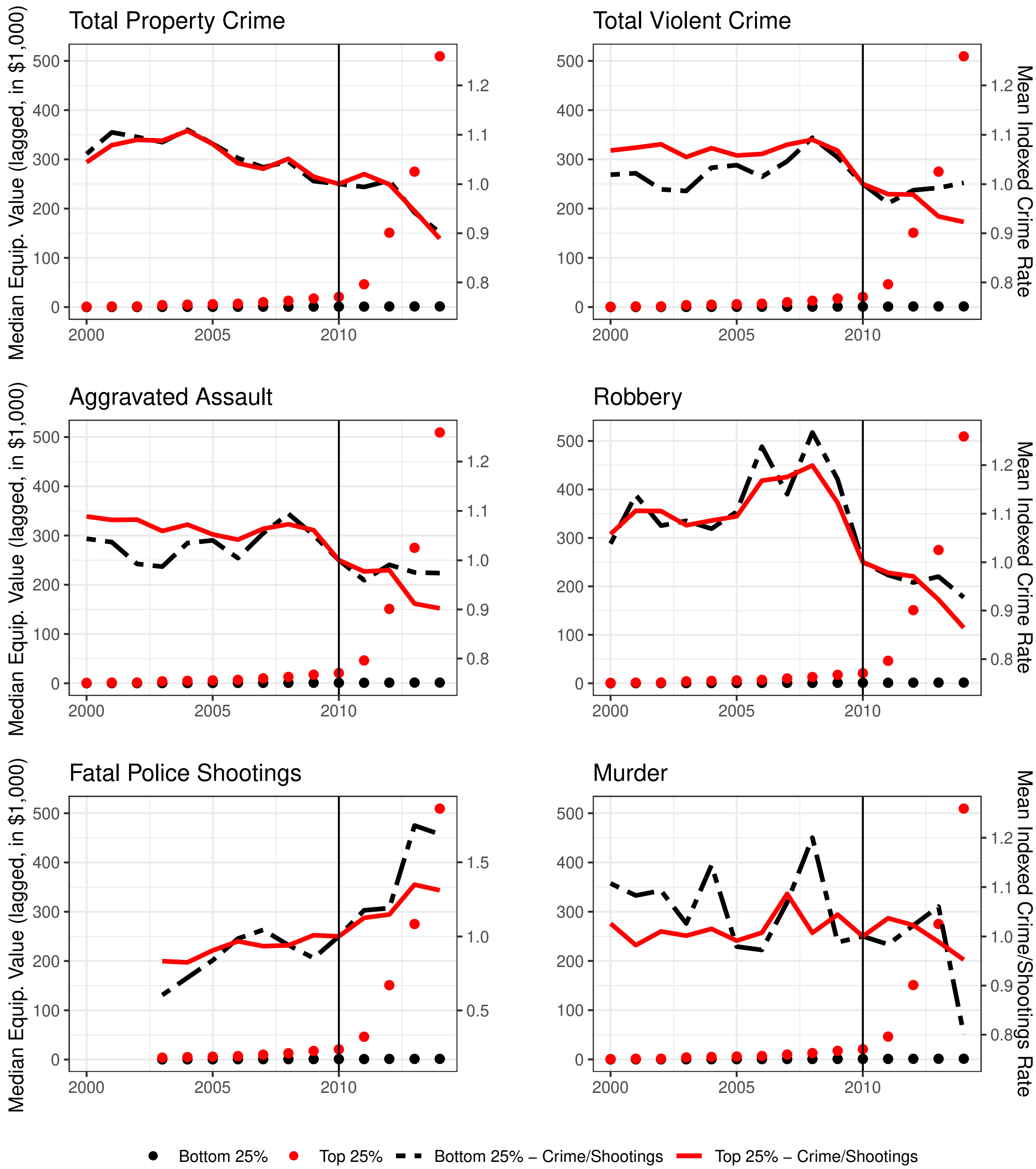

Figure 4: *Crime Rates and Equipment by Treatment Intensity*
The dots indicate median equipment stocks in the top (red) and bottom (black) quarter of the counties by total take-up, measured by the material stock at the end of 2014. All the stocks are lagged by one period. Police killings are measured relative to county population. The solid/dashed lines display the corresponding crime/police shooting rates for the two groups (indexed, 2010 = 1). The vertical line marks the year after the peak of American soldiers in Afghanistan and Iraq, triggering the resulting material inflow.

V. Results

As indicated, the current approach bears some similarity to a difference-in-differences analysis. In this spirit, Figure 4 contains descriptive evidence on various outcomes that were indexed to the year when the large material influx from the partial US retreat from Afghanistan/Iraq started. Compared to the most militarised quarter of the counties in the sample, the bottom quarter experiences very little change in median equipment stock value.[33]

For property crime, trends between heavily and barely militarised counties are very similar and the 1033-programme appears to leave this aggregate unaffected. For violent crime, there is sizeable heterogeneity across different types of offences. Murders exhibit no clear pattern and aggravated assault declines faster in more militarised counties, but trends do not look identical for the two groups. For robberies on the other hand, the movement across time is very similar up to 2012, where the decline becomes stronger as treatment intensity increases. For the rate of fatal police encounters, there is a strong upward trend for both groups, but in this case the increase relative to 2010 is largest for the participating counties with the least amount of equipment.

Overall, Figure 4 suggests that there are possible 1033-effects, but the differences in trends indicate that counties who militarise to a high degree are indeed distinct from areas who do not.

A. Pooled OLS and Simple Fixed Effects

As an initial reference, Table 3 shows the estimates without instruments. Property crime includes burglary, larceny, motor vehicle theft, and arson. Violent crime covers murder (incl. non-negligent manslaughter), rape, robbery, and aggravated assault.

[33] The equipment distribution in levels is highly skewed, which is why I resort to the median for ease of interpretation (relative to a log-scale). For the main analysis, only logs were used.

Type of crime:	Violent	Property	Murder	Aggr. Ass.	Robbery
Pooled OLS					
ln(value of equip. stock)	7.960***	77.488***	0.079***	4.168***	3.139***
	(0.394)	(2.231)	(0.009)	(0.318)	(0.103)
Pooled OLS - with lag dep. var.					
ln(value of equip. stock)	0.911***	6.398***	0.060***	0.560***	0.284***
	(0.199)	(0.952)	(0.009)	(0.173)	(0.049)
lag(dep. var)	0.856***	0.890***	0.257***	0.830***	0.890***
	(0.007)	(0.004)	(0.019)	(0.008)	(0.010)
Fixed effects - static					
ln(value of equip. stock)	-0.047	10.164***	-0.002	-0.241	0.211**
	(0.524)	(2.432)	(0.017)	(0.464)	(0.090)
Fixed effects - dynamic					
ln(value of equip. stock)	-0.195	5.093***	-0.001	-0.271	0.127*
	(0.366)	(1.604)	(0.018)	(0.329)	(0.065)
lag(dep. var)	0.422***	0.464***	-0.055***	0.408***	0.343***
	(0.022)	(0.014)	(0.017)	(0.023)	(0.038)
Observations	22952	22952	22952	22952	22952

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. Controls include year fixed effects, demographic (black and veteran population, pop. density) and economic (poverty and median income) variables. Dynamic fixed effects specifications include the first lag of the dependent variable.

Table 3: *Pooled OLS and Fixed Effects (uninstrumented)*

The pooled OLS results indicate that counties with higher crime rates tend to have more military equipment. The sizeable estimate for property crime is due to the large number of such offences. As shown in Table 1, the number of property crimes per 100,000 people is about ten times the number of violent crimes.

The fixed effects specifications are in line with the contention that much of the OLS effects are due to county specific factors that were not accounted for. Including a lagged dependent variable enhances the precision of the estimates, but will also introduce a bias by construction (see Nickell, 1981).[34]

[34] The appendix contains dynamic specifications for all the main outcomes, accounting for any Nickell-bias through either sensitivity analysis or Blundell and Bond (1998) dynamic panel models.

B. Instrumented Panel Specifications

Most types of serious property crime are not street-level offences and data on smaller crimes is not reported with the same consistency, therefore the main focus of this analysis are violent crimes. I omit rape because the FBI changed the definition underlying its data collection in 2013, impairing the comparability across time.[35]

Accounting for endogeneity, the estimates in Table 4 show a decrease in robberies consistent with earlier findings of a decline in street-level crime, and no effect on most other types of offences.[36]

The coefficients are in line with the notion that law enforcement agencies in high-crime areas choose to acquire more military equipment, thereby causing an endogeneity issue in the absence of an appropriate identification strategy. Yet, with the exception of robberies, the results do not suggest a clear reversal of effects in the sense that the equipment causes a drop in violent crimes. And as shown below, even the impact on robberies can be questioned once heterogeneity of patterns in different subsamples is considered.

[35] See *https://ucr.fbi.gov/crime-in-the-u.s/2013/crime-in-the-u.s.-2013/violent-crime/rape*, last accessed on 20 April 2019.

[36] Table 12 in the appendix contains very similar estimates using the two distance instruments displayed in models (2) of the first stage results (Table 2) in a dynamic setting. All the violent crime specifications pass a Sargan-Hansen test of over-identifying restrictions (i.e. no rejection of instrument validity at the 10% significance level).

Type of crime	Violent	Agg. Ass.	Murder	Robbery
Parameter of interest				
ln(value of equip. stock)	-8.229**	-3.750	-0.224	-3.979***
	(4.041)	(3.535)	(0.138)	(0.863)
Controls				
Population (1,000)	-0.729***	-0.404***	-0.008***	-0.275***
	(0.141)	(0.087)	(0.002)	(0.055)
Population density	-0.135***	-0.068***	-0.003**	-0.061***
	(0.034)	(0.023)	(0.002)	(0.016)
Median HH income	0.002***	0.002***	0.000	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.475***	0.220	0.006	0.257***
	(0.174)	(0.145)	(0.004)	(0.045)
Prop. veterans	0.270	-0.645	0.013	0.663
	(2.119)	(1.847)	(0.073)	(0.454)
Poverty rate	-2.558***	-1.592**	-0.003	-0.749***
	(0.839)	(0.746)	(0.028)	(0.152)
Unemployment	-1.182	-0.744	-0.039	0.139
	(1.091)	(0.972)	(0.036)	(0.192)
County fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	22952	22952	22952	22952
Kleibergen-Paap F-Statistic	61.80	61.80	61.80	61.80

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 4: *The Effect of Equipment on Crime Rates*

C. Separating Rural and Urban Counties

It would appear natural to assume that police agencies in predominantly rural counties differ from more urban law enforcement entities in important aspects (e.g. size, or type of crime most frequently encountered). In consequence, the impact of receiving military equipment might also differ between the two types. Indeed, splitting the sample into two parts based on population density (pop./sq. mile) reveals an entirely different pattern – see Table 5. Judging by the Kleibergen-Paap F-Statistic, the instrument performs poorly in terms of relevance for the medium to high density subsample, while the opposite is the case for more rural counties. Looking at the corresponding point estimates, we can see that they are much larger and very imprecise in the higher density sample. A similar pattern arises when the sample is split according to population, but the difference is less pronounced (see Tables 28 to 30 in the appendix).

In light of this heterogeneity in subsample results and instrument performance, it is uncertain to what extent a joint estimation can yield meaningful coefficients for the crime outcomes. There are at least two potential reasons for the limited explanatory power of the distance-based instrument for urban counties. First, urban centres tend to be further away from equipment distribution sites because military bases (with naval bases being the main exception) require the kind of space scarcely abundant near sprawling cities. Second, larger city police departments might not encounter the same types of resource constraints for the acquisition of military equipment – they tend to have well funded tactical units and likely easier access to military equipment through homeland security grants. All this might not only hamper instrument performance, but also introduce endogeneity concerns with respect to my distance-based instrument. Therefore, the present analysis will focus on rural counties, where the instrument is a substantially stronger predictor of equipment take-up.

The population density threshold used here is arbitrary of course, but the underlying pattern is largely insensitive to the particular cut-off. In an effort to approximate the point which separates the sample where the instrument performs well from the part with poor performance, Table 5 also contains estimates for a cut-off which maximises the Kleibergen-Paap F-Statistic of the lower density subset, which happens at around 100 inhabitants per square mile.[37] In this case, the point estimates for aggravated assault and robberies are only a fraction of those estimated for the whole sample. And with this separation, the inadequacy of the instruments for the omitted part of the sample is even more apparent: the Kleibergen-Paap F-Statistic amounts to only 1.29 (with N=7,773) and the model parameters are not only weakly identified, but under-identified. For robberies, the effect in Table 5 remains robust to a wide range of specifications including per capita militarisation, the use of different instruments and other subsets/measures of military equipment stocks (see

[37]This is obviously an ad-hoc approach. The underlying rationale is that increasing the sample size using only observations which respond to the instrument should increase the likelihood of rejection and thus the F-statistic, while adding random noise should have the opposite effect. However, this rule-of-thumb method does not take the change in degrees of freedom into account.

Appendix F).[38] However, as argued in the next section, the respective coefficient points ot a low economic significance.

Model	Agg. Ass.	Murder	Robbery
Whole sample			
ln(value of equip. stock)	-3.750	-0.223	-3.979***
	(3.535)	(0.138)	(0.863)
Observations	22952	22951	22952
Kleibergen-Paap F-Statistic	61.800	61.783	61.800
Mid./high dens. subsample (pop. dens. $>$ 50)			
ln(value of equip. stock)	8.273	-0.166	-6.992
	(12.241)	(0.309)	(5.321)
Observations	12527	12526	12527
Kleibergen-Paap F-Statistic	3.694	3.683	3.694
Low density subsample (pop. dens. $\leq$ 50)			
ln(value of equip. stock)	1.280	-0.217*	-0.347
	(3.184)	(0.127)	(0.374)
Observations	10415	10415	10415
Kleibergen-Paap F-Statistic	86.076	86.076	86.076
Low./mid density subsample (pop. dens. $\leq$ 100)			
ln(value of equip. stock)	0.275	-0.183	-0.842**
	(2.887)	(0.117)	(0.386)
Observations	15208	15208	15208
Kleibergen-Paap F-Statistic	93.893	93.893	93.893
Econ. & demogr. controls	Yes	Yes	Yes
County & Year fixed effects	Yes	Yes	Yes

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 5: *Separate Estimations - Urban vs. Rural Counties*

[38]In appendices D, E and F, I include both Blundell and Bond (1998) dynamic panel specifications and sensitivity tests to show that any autoregressive behaviour of the dependent variable does not affect the conclusions presented in the main part. The sensitivity tests are based on the fact that autoregressive parameters in OLS/2SLS without fixed effects will be biased upward, while the fixed effects Nickell-bias is a downward distortion of the true parameter. Therefore, any sensible value for the autoregressive parameter should be contained in the interval $(\alpha_{FE}, \alpha_{OLS})$. I exploit these properties to run sensitivity tests for the parameter of interest using the whole range of plausible autoregressive coefficients, with the most likely values located in column (3) of the respective tables.

VI. Discussion

Overall, the evidence presented in the previous sections does not lend support to the hypothesis that the 1033-programme has had a clear negative effect on violent crime. There is some indication that robberies have declined due to the infusion of military equipment in more rural areas of the country, but even this assertion is less robust than previous studies suggest. These findings contrast with existing county-level studies. Some of the differences are doubtlessly owed to the fact that I exclusively focus on local law enforcement entities.

But especially for HPBM, whose identification strategy provides the basis of the present analysis, the data inconsistencies (which were likely not discernable yet at the time of their analysis) and subsample heterogeneity suggest more fundamental reasons for the differences. Looking at small town police departments and large city agencies jointly appears to mask vital dimensions of heterogeneity.

Because BG use a different identification approach, I am not able to directly compare the present estimates to their findings due to likely differences in the identified local average treatment effect (LATE). However, when I re-estimate some of the key specifications based on their main instrument, I get similar results - particularly in terms of crime outcomes in the rural subsample (see Table 48 in Appendix I).

Overall, the parameter estimates presented in the previous section tend to be of limited economic significance even if we are willing to believe in the statistical validity of all the specifications. Taking the bottom robbery coefficient in Table 5 as an example, the short term impact of increasing equipment by 10% reduces the annual robbery rate by about 0.085 robberies per 100,000 inhabitants. With an average robbery rate of 20.651 for the corresponding subsample, this translates into an elasticity of robberies with respect to military equipment of -0.041.[39] To put this in some context: The recent literature review of Chalfin and McCrary (2017) finds a range of about -0.1 to -2 in the elasticities reported in several IV-studies, most of which focus on an increase in police labour input.

In consequence, military equipment does not appear to be a very effective way of reducing the number of violent crimes in rural areas. However, there are plenty of other outcomes by

[39] Estimates using the number of items tend to be up to several times larger, but also much less precise in terms of standard errors (see e.g. Table 34 and 35). Those estimates would still put the effect close to the lower end of the elasticity range found for other measures mentioned above.

which the impacts can be assessed.

Table 6 contains estimates for the effect on the number of arrests in a static fixed effects model. There is no evidence of an increase in the number of arrests. On the contrary, results indicate a negative impact.[40] The negative effect for arrests under a constant crime rate is counter-intuitive if we think of a Becker (1968) context (where this would translate into a negative marginal product of capital/equipment in the police production function). One possible explanation for such an observation would be that the acquisition, maintenance and use (incl. necessary training) of military equipment absorb police resources/manpower to the extent that other activities are negatively affected.

Arrest rate for	Agg. Ass.	Murder	Robbery
ln(value of equip. stock)	-3.600**	-0.485***	-1.578***
	(1.643)	(0.115)	(0.309)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	15208	15208	15208
Kleibergen-Paap F-Statistic	93.893	93.893	93.893

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as the number of arrests per 100,000 inhabitants. Controls include demographic (black and veteran population, total population, and pop. density) and economic (poverty and median HH income) variables. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 6: *The Effect of Equipment on Arrests - Subsample With Population Density Below 100 People per Square Mile*

Turning to further outcomes, Table 7 shows estimates for employment effects, officer safety and killings of civilians by police officers.[41] Only observations where a county's population weighted reporting coverage indicator was at least 50% are used. This leads to a removal of nearly half the observations for agency data (see Appendix H for robustness

[40] I also report Blundell and Bond (1998) system-GMM specifications in Appendix G which are similar to the estimates presented here, although due possible instrument endogeneity in some robbery/agg. assault models, no firm conclusions can be drawn. This appendix further contains estimates for the number of arrests relative to the reported offences (i.e. a type of police productivity measure). In this case, only the robbery-effect is marginally significant, but missing data/zeros substantially reduce the number of observations. Estimates based on a BG-type instrument produce yield no significant effects of arrest for the rural subsample, but a slight reduction in robbery arrests for higher density counties, see Tables 51 and 52 in the Appendix.

[41] The agency level data has been aggregated to the county level as a population-weighted sum. Only data of local agencies (no state or federal) was used. Data for agencies reporting less than the full 12 months has been linearly extrapolated to arrive at yearly totals.

checks).[42] Taken at face value, the estimates in Table 7 suggest a reduction in police officers being assaulted as well as an (albeit very modest) positive effect on the number of local law enforcement employees.[43]

Looking at fatal police encounters, I find no robust effect for the regular sample period (2005-2014) using the distance-based instruments.[44] But given the importance of this particular outcome, the fact that the coefficient changes sign when restricting the analysis to smaller counties - and is of a similar order of magnitude using a Bove and Gavrilova (2017)-type instrument (see Table 50 in the Appendix) - deserves further attention by future work.

In contrast to many other patterns observed in the data, there is a more compelling narrative linking military equipment to fatal encounters – as opposed to trying to relate a decline in robberies to the presence of mine resistant vehicles for instance. Counties with a population below 30,000 (of which there are more than 1,000 in the US) are unlikely to have the sort of incidents that could justify the widespread use of military grade equipment and heavily armed SWAT teams. And yet, Balko (2013) notes that the militarisation trend did not stop in larger cities, but that for smaller towns, SWAT-duty was something regular patrol officers started doing 'on the side'.[45]

[42]The results are robust to a simplified instrument set, per capita measures of militarisation, and using the numbers of guns or vehicles (in which case point estimates are again larger).

[43]The average employee rate is about 2.6 in the relevant subsample.

[44]When the sample period is extended back to 2000, the effect persists however across a range of specifications, see Tables 42 to 47 in the Appendix.

[45]Balko (2013):210 points out that "*[b]y the mid-2000s, SWAT had come to Middleburg, Pennsylvania (population: 1,363); Leesburg, Florida (17,000); Mt. Orab, Ohio (2,701); Neenah, Wisconsin (24,507); Harwich, Massachusetts (11,000); and Butler; Missouri (4,201), among others.*"

Dep. Var.	Off. Killed	Off. Ass.	Empl. Rate	Fatal Police Encounters		
ln(Equip.)	-0.010	-2.625***	0.071***	-0.018	0.011	0.014*
	(0.008)	(0.722)	(0.027)	(0.012)	(0.008)	(0.008)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	P. Dens. <100	P. Dens. <100	P. Dens. <100	P. Dens. <100	Pop. <30,000	Pop. <30,000
Extended Period	No	No	No	No	No	Yes
Observations	7559	7559	7559	15208	10447	12466
KP F-Stat.	30.795	30.795	30.795	93.893	44.063	42.923

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Employee and officer rates are measured as number of employees/officers per 1000 population. Officers killed or assaulted are measured relative to the total number of officers, scaled to lie between 0 and 100. Fatal police encounters are measured in the absolute number of incidents in a county. Controls include year fixed effects, demographic (black and veteran population, total population, and pop. density) and economic (poverty and median income) variables. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$. The extended period sample covers the years 2000-2014, while the rest is restricted to 2005-2014. All estimates are based on a static fixed effects specification.

Table 7: *The Effect of Equipment on Officer Assault, Employment and Fatal Encounters*

When officers are issued with military grade material without the proper training or experience in its use, there are indeed grounds to suspect that these instances bear a higher risk of a fatal encounter between officers and civilians. But at this stage, it would be incorrect to interpret the results in Table 7 in any other form than as a reason for further inquiry.

Judging by the entirety of my results, the 1033-programme does not appear to have much of an effect in rural counties, which according to the population density threshold of 100/sq. mile used here contains nearly 1,600 counties in my sample, with a total population of about 45 million in 2014. Indeed, it is not straightforward to come up with a convincing narrative that would suggest otherwise. Higher visibility of heavy duty equipment on the street can certainly help prevent some criminal activity, but bearing in mind that the average county population in the rural subsample is less than 30,000, regular street patrols of heavily equipped police officers cannot be expected to have the same crime dampening effect as, say, in parts of suburban Chicago. The most plausible case for a 1033-effect on crime for these counties might be through non-combat related equipment that could substitute regular department purchases and free up some budget for an increase in man-hours, which, as previously mentioned, has been linked to lower crime by earlier research. But while there are indications that receipt of equipment led so a slight increase in employees, the absence of a clear effect on violent crime paired with a possible reduction in the number of arrests points in a different direction as far as agency effectiveness is concerned.

In light of the downside risks of a large scale militarisation of local police forces, the absence of clear positive effects for a substantial part of the country (with the possible exception of a reduction in assaults of police officers) provides a strong case for further scrutiny of the 1033-programme.

VII. Conclusion

The present work examines the effect of local police militarisation on violent crime rates using evidence from the 1033-programme in the US. Exogenous cost shifters due to the particular logistics of the programme are exploited to instrument for the amount of equipment received by local law enforcement. The results do not support previous county-level studies (Bove and Gavrilova, 2017; Harris et al., 2017), who find considerable negative effects on street-level offences.

I show that these findings are based on data containing a significant number of inconsistencies and that pooling small town/rural agencies with more urban police departments likely masks crucial dimensions of heterogeneity in effects. In addition, instruments based on the distance to the closest distribution centres exhibit poor first stage performance for urban counties.

Taking all these factors into account, I find only weak evidence of a negative impact of military grade equipment on violent crime in rural counties. Furthermore, even in cases with statistical significance, the corresponding economic significance is low. If anything, estimates suggest a negative impact on the number of arrests for violent crimes. The evidence on police killings is inconclusive, but hint at the possibility that counties with a very low population density – and therefore law enforcement entities lacking the resources for a professional full-time SWAT team – might be particularly vulnerable to potential negative consequences.

Given the severe downside risks associated with widespread proliferation of military grade equipment, the dynamics in small town police departments provide a natural starting point for future research. Further emphasis should also be put on the identification of the programme's effects in urban areas. After all, '*Even if it doesn't do much good, it probably won't hurt (too much)*' is unlikely to be the type of conclusion which can do justice to a reallocation of resources worth billions of US-Dollars.

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Appendix

A. Non-participating Counties – Summary Statistics

Variable	N	Mean	Std. Dev.	Min	Max	Median
Crime rates (crimes/100,000 pop)						
Serious (part 1) offences, total	7,510	1,741.02	1,472.39	0	37,037.04	1,525.98
Violent crime, total	7,510	191.91	200.41	0	2,912.62	141.85
Property crime, total	7,510	1,549.11	1,339.87	0	35,185.19	1,354.25
Murder	7,510	2.6	6.73	0	139.47	0
Robbery	7,510	20.35	47.24	0	889.39	4.67
Aggravated assault	7,510	148.52	161.38	0	2,912.62	103.57
Arrest rates (arr./100,000 pop.)						
Total arrests, serious offences	7,510	438.41	414.1	0	8,402.91	360.73
Total arrests, violent crime	7,510	109.42	114.04	0	1,859.55	83.39
Total arrests, property crime	7,510	328.95	348.86	0	8,241.31	262.03
Arrests, murder	7,510	2.7	8.59	0	240.67	0
Arrests, robbery	7,510	10.62	21.8	0	373.83	0
Arrests, aggravated assault	7,510	89.03	98.32	0	1,761.16	65.04
Economic/demographic controls						
Unemployment rate, percent	7,506	6.56	3.12	1.3	28.9	5.8
Population, in 100,000	7,510	.34	1.51	0	26.13	.12
Pop. density, pop./sq. mile	7,510	319.32	3,263.39	.06	71,592.99	21.25
Veteran population, in %	7,510	9.14	2.68	0	52.5	9.03
Black population, in %	7,510	8.86	15.26	0	94.18	2.32
Median household income, $	7,510	40,911.74	10,012.22	16,868	95,927	39,700.5
Poverty rate, in %	7,510	17	7.3	4	53	15.4
Land area, sq. miles	7,510	880.71	1,004.02	2.5	10,180.88	624.68
Military equipment stocks (t-1)						
Total value, $	7,510	0	0	0	0	0
Total value/100,000 pop., $	7,510	0	0	0	0	0
Value of guns, $	7,510	0	0	0	0	0
Value of vehicles, $	7,510	0	0	0	0	0
Officer related variables						
Officers killed (share, in %)	4,930	.05	1.38	0	62.85	0
Officers assaults/100 officers	4,930	26.99	64.57	0	1,200	4.81
Officers/1,000 pop.	4,930	1.81	1.1	.1	28	1.62
Police killings	7,510	.13	.61	0	14	0

Table 8: *Summary Statistics – Counties Excluded from Sample Because of Non-participation*

B. Population Density Split Subsamples – Summary Statistics

Variable	N	Mean	Std. Dev.	Min	Max	Median
Crime rates (crimes/100,000 pop)						
Serious (part 1) offences, total	15,219	2,115.03	1,293.71	0	11,380.97	1,940.54
Violent crime, total	15,219	226.52	202.02	0	2,584.46	175.53
Property crime, total	15,219	1,888.51	1,156.51	0	9,467.99	1,738.17
Murder	15,219	2.88	5.59	0	97.62	0
Robbery	15,219	20.65	31.24	0	653.63	10.3
Aggravated assault	15,219	178.03	173.68	0	2,086.25	131.36
Arrest rates (arr./100,000 pop.)						
Total arrests, serious offences	15,219	465.07	326.68	0	3,117.09	417.97
Total arrests, violent crime	15,219	113.33	100.82	0	1,165.85	89.91
Total arrests, property crime	15,219	351.65	262.23	0	2,838.39	307.53
Arrests, murder	15,219	2.69	6.2	0	161.81	0
Arrests, robbery	15,219	10.03	16.54	0	639.34	4.02
Arrests, aggravated assault	15,219	93.53	88.57	0	786.71	72.24
Economic/demographic controls						
Unemployment rate, percent	15,217	7.17	3.07	1.1	25.5	6.6
Population, in 100,000	15,219	.3	.48	.01	20.05	.21
Pop. density, pop./sq. mile	15,219	37.95	27.08	.25	100	33.98
Veteran population, in %	15,219	9.03	2.14	1.29	19.6	8.82
Black population, in %	15,219	8.31	14.26	0	96.07	2.37
Median household income, $	15,219	40,551.03	8,348.27	17,843	92,560	39,596
Poverty rate, in %	15,219	17.16	6.19	3.7	62	16.4
Land area, sq. miles	15,219	1,142.75	1,560.33	101.23	20,056.94	687.03
Military equipment stocks (t-1)						
Total value, $	15,219	36,660.57	137,199.4	0	3,016,791	2,134
Total value/100,000 pop., $	15,219	149,715.8	550,502.8	0	14,100,000	12,838.71
Value of guns, $	15,219	3,291.54	7,642.86	0	405,701	1,278
Value of vehicles, $	15,219	24,930.54	103,887.3	0	2,596,015	0
Officer related variables						
Officers killed (share, in %)	11,446	.06	1.33	0	65.29	0
Officers assaults/100 officers	11,446	21.85	45.8	0	1,200	7.69
Officers/1,000 pop.	11,446	1.72	.66	.1	10.01	1.63
Police killings	15,219	.13	.55	0	19	0

Table 9: *Summary Statistics – Subsample With Population Density <100*

Variable	N	Mean	Std. Dev.	Min	Max	Median
Crime rates (crimes/100,000 pop)						
Serious (part 1) offences, total	10,427	1,928.03	1,220.57	0	11,380.97	1,780.49
Violent crime, total	10,427	214.15	194.52	0	2,584.46	170.25
Property crime, total	10,427	1,713.88	1,093.65	0	9,467.99	1,582.72
Murder	10,427	2.77	6.06	0	97.62	0
Robbery	10,427	15.51	25.35	0	356.07	6.67
Aggravated assault	10,427	171.71	170.2	0	2,086.25	130.34
Arrest rates (arr./100,000 pop.)						
Total arrests, serious offences	10,427	421.3	303.5	0	3,117.09	372.68
Total arrests, violent crime	10,427	108.11	97.85	0	1,165.85	86.92
Total arrests, property crime	10,427	313.09	243.9	0	2,838.39	267.31
Arrests, murder	10,427	2.51	6.63	0	161.81	0
Arrests, robbery	10,427	7.93	15.37	0	639.34	0
Arrests, aggravated assault	10,427	90.53	87.25	0	786.71	70.57
Economic/demographic controls						
Unemployment rate, percent	10,425	6.95	3.15	1.1	25.5	6.3
Population, in 100,000	10,427	.21	.22	.01	2.37	.15
Pop. density, pop./sq. mile	10,427	22.49	14.77	.25	49.99	21.98
Veteran population, in %	10,427	9.16	2.22	1.75	19.6	8.96
Black population, in %	10,427	8.47	14.98	0	95.67	2.12
Median household income, $	10,427	40,025.66	8,417.57	17,843	92,560	39,018
Poverty rate, in %	10,427	17.25	6.37	4	62	16.4
Land area, sq. miles	10,427	1,349	1,750.88	129.22	18,618.89	792.21
Military equipment stocks (t-1)						
Total value, $	10,427	29,552.82	111,019.8	0	2,785,556	1,635
Total value/100,000 pop., $	10,427	164,701.8	605,690.3	0	14,100,000	13,097.93
Value of guns, $	10,427	2,690.27	5,538.9	0	102,228	1,051
Value of vehicles, $	10,427	19,145.25	78,324.29	0	2,596,015	0
Officer related variables						
Officers killed (share, in %)	7,862	.07	1.45	0	65.29	0
Officers assaults/100 officers	7,862	22.94	52.32	0	1,200	4.84
Officers/1,000 pop.	7,862	1.74	.65	.13	9.25	1.64
Police killings	10,427	.09	.36	0	7	0

Table 10: *Summary Statistics – Subsample With Population Density <50*

Variable	N	Mean	Std. Dev.	Min	Max	Median
Crime rates (crimes/100,000 pop)						
Serious (part 1) offences, total	12,537	2,958.63	1,448.46	0	10,762.32	2,748.1
Violent crime, total	12,537	306.59	235.31	0	1,920.81	248.68
Property crime, total	12,537	2,652.04	1,270.22	0	9,925.06	2,476.08
Murder	12,537	3.65	4.57	0	78.16	2.44
Robbery	12,537	63.85	77.23	0	785.24	37.39
Aggravated assault	12,537	210.92	174.04	0	1,760.62	166.07
Arrest rates (arr./100,000 pop.)						
Total arrests, serious offences	12,537	619.62	373.49	0	3,400.18	584.95
Total arrests, violent crime	12,537	135.16	111.45	0	1,342.1	111.06
Total arrests, property crime	12,537	484.42	295.76	0	2,600.2	455.16
Arrests, murder	12,537	3.17	4.74	0	135.84	1.74
Arrests, robbery	12,537	21.93	23.4	0	256.41	15.24
Arrests, aggravated assault	12,537	103.63	92.76	0	1,178.32	81.08
Economic/demographic controls						
Unemployment rate, percent	12,529	7.24	2.72	1.9	21.2	6.8
Population, in 100,000	12,537	2.05	4.62	.06	100.67	.76
Pop. density, pop./sq. mile	12,537	436.45	1,113.33	50	18,144.31	134.62
Veteran population, in %	12,536	8.53	2.14	1.29	24.46	8.35
Black population, in %	12,537	9.44	12.1	.2	96.07	4.74
Median household income, $	12,537	47,731.66	12,656.78	22,368	125,635	45,213
Poverty rate, in %	12,537	15.09	5.68	2.4	45	14.6
Land area, sq. miles	12,537	668.71	903.52	2	20,056.94	526.64
Military equipment stocks (t-1)						
Total value, $	12,537	110,730.8	427,984	0	13,800,000	8,600
Total value/100,000 pop., $	12,537	86,469.91	287,522.3	0	10,800,000	10,710.37
Value of guns, $	12,537	11,298.04	29,342.97	0	711,531	4,239.05
Value of vehicles, $	12,537	75,609.65	312,142.4	0	8,614,417	0
Officer related variables						
Officers killed (share, in %)	10,383	.05	.73	0	32.85	0
Officers assaults/100 officers	10,383	17.69	29.4	0	1,014.47	12.18
Officers/1,000 pop.	10,383	1.79	.65	.1	10.01	1.71
Police killings	12,537	.94	3.05	0	83	0

Table 11: *Summary Statistics – Subsample With Population Density >50*

C. IV Estimation of Crime Effects – Robustness - Aggregate Sample

Type of crime	Violent	Property	Agg. Ass.	Murder	Robbery
Parameter of interest					
ln(value of equip. stock)	-5.928**	8.648	-2.656	-0.276*	-2.855***
	(2.697)	(17.011)	(2.364)	(0.143)	(0.630)
Controls					
lag(dep. var.)	0.419***	0.458***	0.406***	-0.055***	0.317***
	(0.022)	0.015)	(0.023)	(0.017)	(0.038)
2nd lag(dep. var.)					0.064***
					(0.017)
Demogr. & Econ. Controls	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	22952	22952	22952	22952	22951
Kleibergen-Paap F-Statistic	33.922	32.043	33.872	33.946	34.788
Hansen J-Statistic (p-value)	0.543	0.008	0.568	0.225	0.595

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. All specifications include county and year fixed effects. Dependent variable is measured as reported crimes per 100,000 population. Controls include year fixed effects, demographic (black and veteran population, pop. density) and economic (poverty and median income) variables. The log of the equipment stock value was instrumented by the distances to the 2nd and 9th closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 12: *The Effect of Equipment on Crime Rates Using Simple Distance Based Instruments Rather Than Their Principal Component*

Type of crime	Violent	Property	Agg. Ass.	Murder	Robbery
Parameter of interest					
ln(value of equip. stock)/Cap.	-5.818**	2.276	-2.578	-0.227	-2.808***
	(2.669)	(17.086)	(2.323)	(0.141)	(0.648)
Controls					
lag(dep. var.)	0.420***	0.459***	0.407***	-0.055***	0.318***
	(0.022)	(0.015)	(0.023)	(0.017)	(0.038)
2nd lag(dep. var.)					0.065***
					(0.017)
Demogr. & Econ. Controls	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	22952	22952	22952	22952	22951
Kleibergen-Paap F-Statistic	50.341	47.111	50.262	50.388	51.689

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. All specifications include county and year fixed effects. Dependent variable is measured as reported crimes per 100,000 population. Controls include year fixed effects, demographic (black and veteran population, pop. density) and economic (poverty and median income) variables. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 13: *The Effect of Equipment on Crime Rates Using Military Equipment Value per Capita*

Log(Crime-rate) of	Violent	Property	Agg. Ass.	Murder	Robbery
Parameter of interest					
ln(value of equip. stock)	-0.017	0.011	-0.011	-0.036	-0.057**
	(0.016)	(0.013)	(0.018)	(0.050)	(0.026)
Controls					
lag(dep. var.)	0.310***	0.382***	0.296***	-0.055***	0.096***
	(0.015)	(0.026)	(0.013)	(0.014)	(0.014)
2nd lag(dep. var.)					0.023*
					(0.012)
Demogr. & Econ. Controls	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	21977	22522	21745	9235	15744
Kleibergen-Paap F-Statistic	56.454	55.406	53.011	8.577	33.176

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. All specifications include county and year fixed effects. Dependent variable is measured as reported crimes per 100,000 population. Controls include year fixed effects, differenced demographic (black and veteran population, pop. density) and economic (poverty and median income) variables. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

***Table 14:** The Effect of Equipment on Crime Rates Using Logs of Crime Rates*

Type of crime	Violent	Property	Agg. Ass.	Murder	Robbery
Parameter of interest					
ln(No. of items in equip. stock)	-19.835**	7.673	-8.782	-0.774	-9.667***
	(9.157)	(57.600)	(7.930)	(0.480)	(2.277)
Controls					
lag(dep. var.)	0.419***	0.460***	0.406***	-0.055***	0.312***
	(0.022)	(0.015)	(0.023)	(0.017)	(0.038)
2nd lag(dep. var.)					0.061***
					(0.017)
Demogr. & Econ. Controls	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	22952	22952	22952	22952	22951
Kleibergen-Paap F-Statistic	46.103	44.243	46.101	46.215	46.619

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. All specifications include county and year fixed effects. Dependent variable is measured as reported crimes per 100,000 population. Controls include year fixed effects, demographic (black and veteran population, pop. density) and economic (poverty and median income) variables. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

***Table 15:** The Effect of Equipment on Crime Rates Using the Number of Items Received (instead of the value)*

D. System-GMM – Robustness

Type of crime	Murder	Agg. Ass.	Robbery	Murder	Agg. Ass.	Robbery
Parameter of interest						
ln(value of equip. stock)	-0.054	-1.961	0.557	-0.107	-2.939*	-0.078
	(0.147)	(2.460)	(0.587)	(0.099)	(1.663)	(0.390)
Controls						
lag(dep. var.)	0.023*	0.474***	0.315***	0.021*	0.511***	0.382***
	(0.013)	(0.060)	(0.094)	(0.012)	(0.050)	(0.090)
2nd lag(dep. var.)		0.104***	0.214***		0.116***	0.245***
		(0.024)	(0.054)		(0.022)	(0.046)
3rd lag(dep. var.)		0.014	0.091**		0.037*	0.099***
		(0.024)	(0.040)		(0.020)	(0.035)
Population (1,000)	-0.012	-3.331	-0.235	-0.009	-0.429	-0.197
	(0.093)	(2.280)	(0.686)	(0.017)	(0.710)	(0.160)
Population density	-0.002	-0.024	-0.015	-0.024	-1.421	-0.076
	(0.005)	(0.029)	(0.013)	(0.041)	(1.218)	(0.284)
Median HH income	0.000	0.001	0.000	0.000	0.001	0.000
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.006	0.190	0.068**	0.004	0.190*	0.053**
	(0.006)	(0.132)	(0.033)	(0.005)	(0.106)	(0.026)
Prop. veterans	-0.057	-1.883	0.077	-0.091	-2.567	-0.018
	(0.103)	(1.893)	(0.321)	(0.092)	(1.734)	(0.268)
Poverty rate	0.051	-0.040	-0.186	0.029	-0.157	-0.131
	(0.046)	(0.699)	(0.149)	(0.037)	(0.626)	(0.129)
Unemployment	-0.056	0.324	-0.149	-0.065	0.850	-0.151
	(0.052)	(0.945)	(0.179)	(0.044)	(0.833)	(0.171)
Constant	2.299	107.973**	6.133	4.776**	129.049***	12.066
	(2.465)	(50.809)	(10.107)	(2.035)	(44.306)	(7.370)
Pop. density < 75	No	No	No	Yes	Yes	Yes
Pop. < 30,000	Yes	Yes	Yes	No	No	No
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10454	10454	10454	13348	13348	13348
Number of Counties	1101	1101	1101	1406	1406	1406
Avg. Obs./County	9.495	9.495	9.495	9.494	9.494	9.494
AR(2)-Test (p-value)	0.724	0.962	0.762	0.442	0.358	0.225
Hansen Test (p-value)	0.540	0.403	0.185	0.401	0.764	0.163
Number of Instruments	55	55	37	55	55	37

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Two step GMM estimation using the forward orthogonal deviations transformation. Windmeijer (2005) adjusted cluster robust standard errors in parentheses. Dependent variable is measured as reported crimes per 100,000 population. GMM-type instruments consist of lagged levels for the f.o.d. equation and the lag difference of the dep. var. for the level equation in case of murder. For agg. assault and robbery, the f.o.d. equation is instrumented with lag differences and the level equation with lagged second differences of the dep. var. All controls are treated as strictly exogenous. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 16: *The Effect of Equipment on Crime Rates – System-GMM Estimates – Rural County Subsamples*

Type of crime	Murder	Agg. Ass.	Robbery	Murder	Agg. Ass.	Robbery
Parameter of interest						
ln(value of equip. stock)	-0.139	-3.109*	0.979*	-0.167*	-1.434	-0.050
	(0.109)	(1.859)	(0.593)	(0.101)	(1.729)	(0.559)
Controls						
lag(dep. var.)	0.030**	0.512***	0.504***	0.016	0.480***	0.170***
	(0.013)	(0.036)	(0.038)	(0.016)	(0.043)	(0.050)
2nd lag(dep. var.)		0.098***	0.187***		0.109***	0.136***
		(0.016)	(0.022)		(0.020)	(0.030)
3rd lag(dep. var.)		0.026*	0.056***		0.025	0.033
		(0.014)	(0.021)		(0.020)	(0.032)
Population (1,000)	-0.008***	-0.169***	-0.035*	0.022	-0.581	-0.062
	(0.002)	(0.047)	(0.019)	(0.050)	(1.093)	(0.162)
Population density	-0.001	-0.020	0.011	-0.020	-3.332*	-0.435
	(0.001)	(0.015)	(0.007)	(0.072)	(1.801)	(0.513)
Median HH income	0.000	0.000	0.000**	0.000	0.001*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
Prop. black	0.006*	0.256***	0.137***	0.010	0.125	0.052*
	(0.004)	(0.077)	(0.024)	(0.006)	(0.112)	(0.030)
Prop. veterans	0.015	-0.246	0.434	-0.122	-2.955	-0.156
	(0.062)	(1.101)	(0.267)	(0.103)	(1.916)	(0.357)
Poverty rate	0.002	-0.605	-0.385***	0.048	0.139	0.047
	(0.026)	(0.448)	(0.128)	(0.043)	(0.669)	(0.145)
Unemployment	-0.002	-0.324	-0.543***	-0.026	0.495	-0.080
	(0.031)	(0.606)	(0.145)	(0.047)	(0.966)	(0.168)
Constant	4.454***	112.157***	3.379	3.308	142.189***	20.883
	(1.294)	(26.107)	(6.316)	(2.612)	(45.330)	(12.725)
Low density subsample	No	No	No	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22953	22951	22951	10425	10425	10425
Number of Counties	2349	2349	2349	1106	1106	1106
Avg. Obs./County	9.771	9.771	9.771	9.426	9.426	9.426
AR(2)-Test (p-value)	0.783	0.294	0.525	0.712	0.597	0.473
Hansen Test (p-value)	0.028	0.462	0.000	0.145	0.442	0.108
Number of Instruments	109	100	100	109	100	100

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Two step GMM estimation using the forward orthogonal deviations transformation. Windmeijer (2005) adjusted cluster robust standard errors in parentheses. Dependent variable is measured as reported crimes per 100,000 population. GMM-type instruments consist of lagged levels for the f.o.d. equation and the lag difference of the dep. var. for the level equation in case of murder. For agg. assault and robbery, the f.o.d. equation is instrumented with lag differences and the level equation with lagged second differences of the dep. var. All controls are treated as strictly exogenous. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$. The low density subsample consists of counties with a populations density of less than 50 people per square mile.

Table 17: *The Effect of Equipment on Crime Rates – System-GMM Estimates – Extended Instrument Set (Max. Number of Lags in f.o.d. Equation)*

Type of crime	Murder	Agg. Ass.	Robbery	Murder	Agg. Ass.	Robbery
Parameter of interest						
ln(value of equip. stock)	-0.042	-3.047	-0.638	-0.127	-2.154	-0.164
	(0.123)	(2.058)	(0.748)	(0.108)	(1.752)	(0.325)
Controls						
lag(dep. var.)	0.027**	0.574***	0.611***	0.009	0.465***	0.310***
	(0.012)	(0.081)	(0.063)	(0.014)	(0.133)	(0.116)
2nd lag(dep. var.)		0.115***	0.238***		0.114**	0.226***
		(0.032)	(0.031)		(0.055)	(0.061)
3rd lag(dep. var.)		0.040*	0.080***		0.040	0.085**
		(0.021)	(0.024)		(0.033)	(0.043)
Population (1,000)	-0.007***	-0.145**	-0.027	-0.027	-0.504	-0.074
	(0.002)	(0.061)	(0.017)	(0.038)	(1.014)	(0.139)
Population density	-0.001	-0.015	0.000	0.068	-3.305*	-0.315
	(0.001)	(0.018)	(0.005)	(0.074)	(1.781)	(0.439)
Median HH income	0.000	0.000	0.000	-0.000	0.001	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
Prop. black	0.004	0.289***	0.128***	0.007	0.107	0.050
	(0.004)	(0.084)	(0.024)	(0.006)	(0.145)	(0.030)
Prop. veterans	0.009	-0.158	0.464*	-0.092	-2.684	-0.006
	(0.065)	(1.192)	(0.263)	(0.108)	(2.152)	(0.301)
Poverty rate	0.012	-0.808	-0.436***	0.035	-0.495	-0.079
	(0.027)	(0.535)	(0.134)	(0.045)	(0.862)	(0.140)
Unemployment	-0.052	-0.196	-0.537***	-0.063	0.737	-0.136
	(0.033)	(0.631)	(0.154)	(0.055)	(1.045)	(0.174)
Constant	4.237***	98.752**	10.153	3.047	160.692**	14.681*
	(1.381)	(41.899)	(7.128)	(2.484)	(65.200)	(8.911)
Low density subsample	No	No	No	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22953	22951	22951	10425	10425	10425
Number of Counties	2349	2349	2349	1106	1106	1106
Avg. Obs./County	9.771	9.771	9.771	9.426	9.426	9.426
AR(2)-Test (p-value)	0.652	0.328	0.576	0.515	0.539	0.523
Hansen Test (p-value)	0.073	0.650	0.011	0.195	0.765	0.524
Number of Instruments	37	37	37	37	37	37

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Two step GMM estimation using the forward orthogonal deviations transformation. Windmeijer (2005) adjusted cluster robust standard errors in parentheses. Dependent variable is measured as reported crimes per 100,000 population. GMM-type instruments consist of lagged levels for the f.o.d. equation and the lag difference of the dep. var. for the level equation in case of murder. For agg. assault and robbery, the f.o.d. equation is instrumented with lag differences and the level equation with lagged second differences of the dep. var. All controls are treated as strictly exogenous. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$. The low density subsample consists of counties with a populations density of less than 50 people per square mile.

Table 18: *The Effect of Equipment on Crime Rates – System-GMM Estimates – Minimal Instrument Set (Min. Number of Lags in f.o.d. Equation)*

E. Sensitivity to AR Assumptions and Subsample Selection

Model	(1)	(2)	(3)	(4)	(5)
ln(value of equip. stock)	-2.846***	-2.800***	-2.753***	-2.461***	-1.745***
	(0.639)	(0.630)	(0.621)	(0.570)	(0.482)
Population (1,000)	-0.183***	-0.182***	-0.180***	-0.156***	-0.099***
	(0.038)	(0.038)	(0.037)	(0.033)	(0.023)
Population density	-0.042***	-0.041***	-0.040***	-0.036***	-0.024***
	(0.009)	(0.009)	(0.009)	(0.007)	(0.005)
Median HH income	0.000***	0.000***	0.000***	0.000**	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.199***	0.196***	0.192***	0.175***	0.137***
	(0.032)	(0.032)	(0.031)	(0.028)	(0.022)
Prop. veterans	0.531	0.512	0.494	0.463	0.381
	(0.337)	(0.332)	(0.327)	(0.302)	(0.262)
Poverty rate	-0.576***	-0.566***	-0.556***	-0.504***	-0.391***
	(0.127)	(0.126)	(0.126)	(0.123)	(0.123)
Unemployment	-0.124	-0.140	-0.156	-0.217	-0.379***
	(0.154)	(0.152)	(0.150)	(0.143)	(0.138)
County & Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	22950	22950	22950	22950	22950
Kleibergen-Paap F-Statistic	61.756	61.756	61.756	61.756	61.756
Imposed AR(1) coefficient	0.3	0.3	0.3	0.4	0.6
Imposed AR(2) coefficient	0.1	0.15	0.2	0.2	0.25
Imposed AR(3) coefficient	0	0.05	0.1	0.1	0.1

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 19: *Sensitivity of the Effect on Robberies to Different AR-Parameters – Entire Specification for Full Sample*

Model	(1)	(2)	(3)	(4)	(5)
ln(value of equip. stock)	-0.151	-0.151	-0.150	-0.107	0.013
	(0.291)	(0.287)	(0.283)	(0.268)	(0.256)
Population (1,000)	-0.156	-0.147	-0.138	-0.102	-0.021
	(0.216)	(0.213)	(0.210)	(0.200)	(0.190)
Population density	-0.414	-0.413	-0.412	-0.416	-0.432
	(0.457)	(0.450)	(0.444)	(0.429)	(0.418)
Median HH income	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.036	0.036	0.035	0.034	0.034
	(0.028)	(0.028)	(0.027)	(0.027)	(0.027)
Prop. veterans	0.085	0.080	0.076	0.097	0.131
	(0.299)	(0.295)	(0.292)	(0.280)	(0.272)
Poverty rate	-0.203	-0.207	-0.212	-0.205	-0.188
	(0.147)	(0.147)	(0.147)	(0.147)	(0.158)
Unemployment	-0.056	-0.060	-0.064	-0.080	-0.131
	(0.152)	(0.149)	(0.147)	(0.146)	(0.158)
County & Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	10415	10415	10415	10415	10415
Kleibergen-Paap F-Statistic	86.076	86.076	86.076	86.076	86.076
Imposed AR(1) coefficient	0.3	0.3	0.3	0.4	0.6
Imposed AR(2) coefficient	0.1	0.15	0.2	0.2	0.25
Imposed AR(3) coefficient	0	0.05	0.1	0.1	0.1

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 20: *Sensitivity of the Effect on Robberies to Different AR-Parameters – Entire Specification for Subsample Where Pop. Density < 50p./sq. mi.*

Model	(1)	(2)	(3)	(4)	(5)
ln(value of equip. stock)	-4.670	-4.503	-4.336	-3.777	-2.292
	(3.660)	(3.571)	(3.483)	(3.092)	(2.289)
Population (1,000)	-0.195***	-0.192***	-0.190***	-0.164***	-0.100***
	(0.048)	(0.047)	(0.046)	(0.040)	(0.027)
Population density	-0.039***	-0.039***	-0.038***	-0.033***	-0.021***
	(0.011)	(0.011)	(0.010)	(0.008)	(0.004)
Median HH income	0.000***	0.000***	0.000***	0.000**	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.432***	0.423***	0.415***	0.371***	0.272***
	(0.070)	(0.068)	(0.066)	(0.057)	(0.039)
Prop. veterans	1.518**	1.462**	1.406**	1.222**	0.761
	(0.712)	(0.695)	(0.678)	(0.613)	(0.500)
Poverty rate	-0.714***	-0.692***	-0.669***	-0.582***	-0.390*
	(0.228)	(0.226)	(0.225)	(0.214)	(0.200)
Unemployment	-0.404	-0.430	-0.455	-0.505*	-0.648***
	(0.316)	(0.309)	(0.304)	(0.282)	(0.247)
County & Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	12525	12525	12525	12525	12525
Kleibergen-Paap F-Statistic	3.672	3.672	3.672	3.672	3.672
Imposed AR(1) coefficient	0.3	0.3	0.3	0.4	0.6
Imposed AR(2) coefficient	0.1	0.15	0.2	0.2	0.25
Imposed AR(3) coefficient	0	0.05	0.1	0.1	0.1

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 21: *Sensitivity of the Effect on Robberies to Different AR-Parameters – Entire Specification for Subsample Where Pop. Density > 50p./sq. mi.*

Model	(1)	(2)	(3)	(4)	(5)
ln(value of equip. stock)	-2.717	-2.681	-2.646	-2.393	-2.140
	(2.392)	(2.202)	(2.052)	(1.892)	(1.761)
Population (1,000)	-0.246***	-0.212***	-0.177***	-0.158***	-0.138***
	(0.053)	(0.046)	(0.040)	(0.035)	(0.031)
Population density	-0.038**	-0.031**	-0.023*	-0.018	-0.013
	(0.015)	(0.014)	(0.013)	(0.011)	(0.010)
Median HH income	0.001***	0.001**	0.001*	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.211**	0.219**	0.228**	0.219**	0.211***
	(0.103)	(0.098)	(0.094)	(0.085)	(0.078)
Prop. veterans	-0.549	-0.530	-0.511	-0.466	-0.422
	(1.368)	(1.307)	(1.260)	(1.178)	(1.108)
Poverty rate	-0.992*	-0.869	-0.747	-0.630	-0.513
	(0.578)	(0.556)	(0.540)	(0.521)	(0.510)
Unemployment	-0.592	-0.567	-0.542	-0.506	-0.469
	(0.718)	(0.690)	(0.671)	(0.630)	(0.598)
County & Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	22950	22950	22950	22950	22950
Kleibergen-Paap F-Statistic	61.756	61.756	61.756	61.756	61.756
Imposed AR(1) coefficient	0.4	0.45	0.5	0.6	0.7
Imposed AR(2) coefficient	0	0.05	0.1	0.1	0.1
Imposed AR(3) coefficient	0	0	0	0.05	0.1

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 22: *Sensitivity of the Effect on Agg. Assault to Different AR-Parameters – Entire Specification for Full Sample*

Model	(1)	(2)	(3)	(4)	(5)
ln(value of equip. stock)	0.004	-0.550	-1.104	-1.109	-1.114
	(2.192)	(2.044)	(1.939)	(1.802)	(1.692)
Population (1,000)	-0.830	-0.979	-1.127	-1.103	-1.078
	(1.433)	(1.384)	(1.373)	(1.297)	(1.237)
Population density	-4.394*	-3.986*	-3.577	-3.384	-3.190
	(2.471)	(2.378)	(2.329)	(2.175)	(2.052)
Median HH income	0.002**	0.001**	0.001*	0.001*	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Prop. black	-0.022	0.022	0.066	0.091	0.115
	(0.155)	(0.150)	(0.146)	(0.133)	(0.121)
Prop. veterans	-3.734	-3.670	-3.607	-3.297	-2.986
	(2.492)	(2.398)	(2.327)	(2.185)	(2.060)
Poverty rate	-0.896	-0.719	-0.541	-0.437	-0.333
	(0.962)	(0.925)	(0.897)	(0.868)	(0.851)
Unemployment	0.361	0.355	0.349	0.370	0.391
	(1.171)	(1.126)	(1.094)	(1.027)	(0.976)
County & Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	10415	10415	10415	10415	10415
Kleibergen-Paap F-Statistic	86.076	86.076	86.076	86.076	86.076
Imposed AR(1) coefficient	0.4	0.45	0.5	0.6	0.7
Imposed AR(2) coefficient	0	0.05	0.1	0.1	0.1
Imposed AR(3) coefficient	0	0	0	0.05	0.1

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 23: *Sensitivity of the Effect on Agg. Assault to Different AR-Parameters – Entire Specification for Subsample Where Pop. Density < 50p./sq. mi.*

Model	(1)	(2)	(3)	(4)	(5)
ln(value of equip. stock)	5.250	4.897	4.544	3.257	1.969
	(7.977)	(7.241)	(6.621)	(5.865)	(5.234)
Population (1,000)	-0.197***	-0.164***	-0.131***	-0.119***	-0.106***
	(0.058)	(0.051)	(0.044)	(0.039)	(0.035)
Population density	-0.029*	-0.023	-0.017	-0.012	-0.008
	(0.016)	(0.014)	(0.013)	(0.011)	(0.010)
Median HH income	0.000	0.000	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.492***	0.462***	0.432***	0.391***	0.351***
	(0.129)	(0.117)	(0.107)	(0.095)	(0.084)
Prop. veterans	0.787	0.813	0.839	0.811	0.782
	(1.562)	(1.450)	(1.366)	(1.229)	(1.118)
Poverty rate	-0.299	-0.314	-0.329	-0.296	-0.263
	(0.658)	(0.634)	(0.619)	(0.587)	(0.567)
Unemployment	-1.863**	-1.764**	-1.664**	-1.570**	-1.476**
	(0.845)	(0.821)	(0.810)	(0.760)	(0.721)
County & Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	12525	12525	12525	12525	12525
Kleibergen-Paap F-Statistic	3.672	3.672	3.672	3.672	3.672
Imposed AR(1) coefficient	0.4	0.45	0.5	0.6	0.7
Imposed AR(2) coefficient	0	0.05	0.1	0.1	0.1
Imposed AR(3) coefficient	0	0	0	0.05	0.1

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 24: *Sensitivity of the Effect on Agg. Assault to Different AR-Parameters – Entire Specification for Subsample Where Pop. Density > 50p./sq. mi.*

Model	(1)	(2)	(3)	(4)	(5)
ln(value of equip. stock)	-0.223	-0.215	-0.202	-0.190	-0.164
	(0.138)	(0.132)	(0.124)	(0.118)	(0.109)
Population (1,000)	-0.009***	-0.008***	-0.008***	-0.007***	-0.005***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Population density	-0.003**	-0.003**	-0.003**	-0.003**	-0.002**
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Median HH income	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.006	0.005	0.005	0.005	0.004
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Prop. veterans	0.013	0.015	0.020	0.025	0.034
	(0.073)	(0.071)	(0.068)	(0.066)	(0.064)
Poverty rate	-0.003	-0.001	0.001	0.004	0.009
	(0.028)	(0.028)	(0.027)	(0.027)	(0.028)
Unemployment	-0.040	-0.038	-0.035	-0.032	-0.027
	(0.036)	(0.035)	(0.034)	(0.033)	(0.032)
County & Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	22951	22951	22951	22951	22951
Kleibergen-Paap F-Statistic	61.783	61.783	61.783	61.783	61.783
Imposed AR(1) coefficient	0	0.05	0.1	0.15	0.25
Imposed AR(2) coefficient	0	0	0.05	0.1	0.2

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 25: *Sensitivity of the Effect on Murder to Different AR-Parameters – Entire Specification for Full Sample*

Model	(1)	(2)	(3)	(4)	(5)
ln(value of equip. stock)	-0.217*	-0.211*	-0.196*	-0.180*	-0.150
	(0.127)	(0.122)	(0.115)	(0.109)	(0.100)
Population (1,000)	0.082	0.079	0.077	0.076	0.073
	(0.064)	(0.062)	(0.059)	(0.057)	(0.053)
Population density	-0.132	-0.130	-0.133	-0.137	-0.144
	(0.113)	(0.109)	(0.104)	(0.099)	(0.092)
Median HH income	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.008	0.008	0.007	0.007	0.006
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Prop. veterans	-0.104	-0.099	-0.095	-0.090	-0.081
	(0.126)	(0.123)	(0.119)	(0.116)	(0.111)
Poverty rate	0.021	0.023	0.027	0.031	0.039
	(0.045)	(0.044)	(0.044)	(0.044)	(0.045)
Unemployment	-0.069	-0.068	-0.070	-0.071	-0.074
	(0.058)	(0.057)	(0.055)	(0.054)	(0.052)
County & Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	10415	10415	10415	10415	10415
Kleibergen-Paap F-Statistic	86.076	86.076	86.076	86.076	86.076
Imposed AR(1) coefficient	0	0.05	0.1	0.15	0.25
Imposed AR(2) coefficient	0	0	0.05	0.1	0.2

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 26: *Sensitivity of the Effect on Murder to Different AR-Parameters – Entire Specification for Subsample Where Pop. Density < 50p./sq. mi.*

Model	(1)	(2)	(3)	(4)	(5)
ln(value of equip. stock)	-0.166	-0.161	-0.164	-0.166	-0.170
	(0.309)	(0.300)	(0.287)	(0.277)	(0.265)
Population (1,000)	-0.008***	-0.008***	-0.007***	-0.007***	-0.005***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Population density	-0.003**	-0.003**	-0.003**	-0.003**	-0.002**
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Median HH income	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.005	0.005	0.005	0.005	0.004
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)
Prop. veterans	0.115	0.115	0.119*	0.123*	0.131**
	(0.078)	(0.075)	(0.072)	(0.069)	(0.066)
Poverty rate	-0.034	-0.033	-0.033	-0.032	-0.031
	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)
Unemployment	0.000	0.004	0.013	0.021	0.039
	(0.037)	(0.035)	(0.035)	(0.034)	(0.033)
County & Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	12526	12526	12526	12526	12526
Kleibergen-Paap F-Statistic	3.683	3.683	3.683	3.683	3.683
Imposed AR(1) coefficient	0	0.05	0.1	0.15	0.25
Imposed AR(2) coefficient	0	0	0.05	0.1	0.2

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t - 1$.

Table 27: *Sensitivity of the Effect on Murder to Different AR-Parameters – Entire Specification for Subsample Where Pop. Density > 50p./sq. mi.*

Model	(1)	(2)	(3)	(4)	(5)
ln(value of equip. stock)	-0.364	-0.358	-0.351	-0.289	-0.149
	(0.448)	(0.443)	(0.439)	(0.428)	(0.430)
Population (1,000)	-1.057*	-1.041*	-1.026*	-0.967	-0.853
	(0.621)	(0.614)	(0.608)	(0.592)	(0.587)
Population density	-0.037***	-0.036***	-0.035***	-0.031***	-0.022***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)
Median HH income	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.063**	0.062**	0.061**	0.058**	0.054*
	(0.029)	(0.029)	(0.029)	(0.029)	(0.030)
Prop. veterans	-0.012	-0.026	-0.040	-0.020	0.006
	(0.300)	(0.296)	(0.293)	(0.280)	(0.270)
Poverty rate	-0.236	-0.243*	-0.250*	-0.238	-0.219
	(0.145)	(0.145)	(0.146)	(0.146)	(0.156)
Unemployment	-0.026	-0.034	-0.043	-0.071	-0.148
	(0.156)	(0.154)	(0.152)	(0.149)	(0.160)
County & Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	10447	10447	10447	10447	10447
Kleibergen-Paap F-Statistic	44.063	44.063	44.063	44.063	44.063
Imposed AR(1) coefficient	0.3	0.3	0.3	0.4	0.6
Imposed AR(2) coefficient	0.1	0.15	0.2	0.2	0.25
Imposed AR(3) coefficient	0	0.05	0.1	0.1	0.1

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 28: *Sensitivity of the Effect on Robberies to Different AR-Parameters – Subsample Where Population < 30,000*

Model	(1)	(2)	(3)	(4)	(5)
ln(value of equip. stock)	-4.768***	-4.672***	-4.576***	-4.079***	-2.815***
	(1.449)	(1.423)	(1.397)	(1.253)	(0.957)
Population (1,000)	-0.186***	-0.184***	-0.182***	-0.157***	-0.096***
	(0.041)	(0.041)	(0.040)	(0.035)	(0.024)
Population density	-0.036***	-0.036***	-0.035***	-0.031***	-0.020***
	(0.013)	(0.013)	(0.013)	(0.010)	(0.005)
Median HH income	0.000***	0.000***	0.000***	0.000***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.375***	0.368***	0.361***	0.324***	0.241***
	(0.057)	(0.056)	(0.054)	(0.047)	(0.032)
Prop. veterans	1.149*	1.117*	1.086*	0.931	0.542
	(0.671)	(0.659)	(0.647)	(0.588)	(0.482)
Poverty rate	-0.775***	-0.750***	-0.724***	-0.640***	-0.449**
	(0.206)	(0.205)	(0.204)	(0.196)	(0.189)
Unemployment	-0.629**	-0.647**	-0.665**	-0.701***	-0.813***
	(0.276)	(0.272)	(0.268)	(0.250)	(0.224)
County & Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	12490	12490	12490	12490	12490
Kleibergen-Paap F-Statistic	24.496	24.496	24.496	24.496	24.496
Imposed AR(1) coefficient	0.3	0.3	0.3	0.4	0.6
Imposed AR(2) coefficient	0.1	0.15	0.2	0.2	0.25
Imposed AR(3) coefficient	0	0.05	0.1	0.1	0.1

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 29: *Sensitivity of the Effect on Robberies to Different AR-Parameters – Subsample Where Population > 30,000*

Model	(1)	(2)	(3)	(4)	(5)
ln(value of equip. stock)	-6.738**	-6.607**	-6.475**	-5.679**	-3.673**
	(2.637)	(2.591)	(2.545)	(2.249)	(1.596)
Population (1,000)	-0.183***	-0.181***	-0.179***	-0.154***	-0.092***
	(0.045)	(0.045)	(0.044)	(0.038)	(0.025)
Population density	-0.030*	-0.029*	-0.029*	-0.025*	-0.017**
	(0.017)	(0.017)	(0.016)	(0.013)	(0.007)
Median HH income	0.001***	0.001***	0.001***	0.001***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.455***	0.448***	0.442***	0.395***	0.287***
	(0.066)	(0.065)	(0.063)	(0.056)	(0.040)
Prop. veterans	2.320**	2.275**	2.229**	1.956**	1.292**
	(1.011)	(0.994)	(0.977)	(0.868)	(0.632)
Poverty rate	-0.725***	-0.694***	-0.664***	-0.578***	-0.388**
	(0.234)	(0.231)	(0.228)	(0.214)	(0.195)
Unemployment	-0.874***	-0.876***	-0.879***	-0.906***	-1.002***
	(0.322)	(0.316)	(0.310)	(0.285)	(0.251)
County & Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	8762	8762	8762	8762	8762
Kleibergen-Paap F-Statistic	10.794	10.794	10.794	10.794	10.794
Imposed AR(1) coefficient	0.3	0.3	0.3	0.4	0.6
Imposed AR(2) coefficient	0.1	0.15	0.2	0.2	0.25
Imposed AR(3) coefficient	0	0.05	0.1	0.1	0.1

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 30: *Sensitivity of the Effect on Robberies to Different AR-Parameters – Subsample Where Population > 50,000*

Model	(1)	(2)	(3)	(4)	(5)	Static
ln(value of equip. stock)	-0.183	-0.175	-0.160	-0.145	-0.114	-0.183
	(0.117)	(0.113)	(0.106)	(0.101)	(0.094)	(0.117)
Population (1,000)	-0.007	-0.007	-0.006	-0.005	-0.003	-0.007
	(0.013)	(0.012)	(0.012)	(0.011)	(0.010)	(0.013)
Population density	-0.058*	-0.055	-0.053	-0.050	-0.045	-0.058*
	(0.035)	(0.034)	(0.032)	(0.030)	(0.028)	(0.035)
Median HH income	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.003	0.003	0.003	0.003	0.003	0.003
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Prop. veterans	-0.053	-0.047	-0.039	-0.031	-0.015	-0.053
	(0.098)	(0.095)	(0.092)	(0.090)	(0.086)	(0.098)
Poverty rate	0.000	0.002	0.006	0.010	0.018	0.000
	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)
Unemployment	-0.055	-0.052	-0.050	-0.047	-0.042	-0.055
	(0.045)	(0.043)	(0.042)	(0.041)	(0.040)	(0.045)
County & Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15208	15208	15208	15208	15208	15208
Kleibergen-Paap F-Statistic	93.893	93.893	93.893	93.893	93.893	93.893
Imposed AR(1) coefficient	0	0.05	0.1	0.15	0.25	0
Imposed AR(2) coefficient	0	0	0.05	0.1	0.2	0

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 31: *Sensitivity of the Effect on Murder to Different AR-Parameters – Specification for Subsample Where Pop. Density < 100p./sq. mi.*

Model	(1)	(2)	(3)	(4)	(5)	Static
ln(value of equip. stock)	0.184	-0.022	-0.228	-0.182	-0.135	0.275
	(1.963)	(1.812)	(1.697)	(1.573)	(1.474)	(2.887)
Population (1,000)	-0.010	0.004	0.018	-0.019	-0.055	-0.048
	(0.296)	(0.294)	(0.301)	(0.283)	(0.268)	(0.399)
Population density	-2.765***	-2.553***	-2.341***	-2.063***	-1.785***	-3.921***
	(0.871)	(0.830)	(0.800)	(0.738)	(0.682)	(1.192)
Median HH income	0.001***	0.001***	0.001**	0.001**	0.001*	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Prop. black	0.094	0.124	0.153	0.165	0.177*	0.012
	(0.129)	(0.124)	(0.121)	(0.111)	(0.101)	(0.177)
Prop. veterans	-2.476	-2.327	-2.178	-1.900	-1.622	-3.374
	(1.887)	(1.807)	(1.747)	(1.637)	(1.542)	(2.508)
Poverty rate	-0.525	-0.389	-0.254	-0.169	-0.084	-1.093
	(0.720)	(0.693)	(0.673)	(0.652)	(0.640)	(0.917)
Unemployment	-0.107	-0.093	-0.078	-0.061	-0.043	-0.197
	(0.877)	(0.843)	(0.820)	(0.773)	(0.738)	(1.182)
County & Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15208	15208	15208	15208	15208	15208
Kleibergen-Paap F-Stat.	93.893	93.893	93.893	93.893	93.893	93.893
Imposed AR(1) coefficient	0.4	0.45	0.5	0.6	0.7	0
Imposed AR(2) coefficient	0	0.05	0.1	0.1	0.1	0
Imposed AR(3) coefficient	0	0	0	0.05	0.1	0

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 32: *Sensitivity of the Effect on Agg. Assault to Different AR-Parameters – Specification for Subsample Where Pop. Density < 100p./sq. mi.*

F. Rural Subsample – Robustness of Effect on Robberies

Model	(1)	(2)	(3)	(4)	(5)	Static
ln(value of equip. stock)	-0.465	-0.448	-0.432	-0.356	-0.158	-0.769**
	(0.311)	(0.306)	(0.302)	(0.290)	(0.285)	(0.384)
Population (1,000)	-0.181*	-0.174*	-0.167*	-0.141	-0.077	-0.282**
	(0.100)	(0.098)	(0.096)	(0.088)	(0.072)	(0.131)
Population density	-0.229	-0.230	-0.230	-0.214	-0.179	-0.282
	(0.184)	(0.181)	(0.178)	(0.166)	(0.147)	(0.235)
Median HH income	0.000	0.000	0.000	0.000	0.000	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.066***	0.066***	0.065***	0.062***	0.060***	0.069**
	(0.024)	(0.024)	(0.023)	(0.022)	(0.022)	(0.028)
Prop. veterans	0.060	0.051	0.041	0.059	0.087	0.017
	(0.263)	(0.259)	(0.256)	(0.247)	(0.245)	(0.318)
Poverty rate	-0.223*	-0.218*	-0.213*	-0.197	-0.162	-0.279**
	(0.126)	(0.126)	(0.127)	(0.129)	(0.139)	(0.134)
Unemployment	-0.043	-0.062	-0.081	-0.123	-0.236*	0.138
	(0.132)	(0.130)	(0.128)	(0.124)	(0.129)	(0.163)
County & Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15208	15208	15208	15208	15208	15208
Kleibergen-Paap F-Statistic	51.501	51.501	51.501	51.501	51.501	51.501
Hansen J-Statistic (p-value)	0.352	0.363	0.375	0.388	0.431	0.395
Imposed AR(1) coefficient	0.3	0.3	0.3	0.4	0.6	0
Imposed AR(2) coefficient	0.1	0.15	0.2	0.2	0.25	0
Imposed AR(3) coefficient	0	0.05	0.1	0.1	0.1	0

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the distances to the 2nd and 6th closest distribution site with the log of the sum of equipment issued until year $t-1$ (2 instruments, other distance combinations also work).

Table 33: *Sensitivity of the Effect on Robberies – Pop. Density < 100p./sq. mi. Subsample – Simple Distance Instruments Instead of Principal Component*

Model	(1)	(2)	(3)	(4)	(5)	Static
ln(No. of items)	-2.622*	-2.537*	-2.451	-2.093	-1.200	-4.053**
	(1.553)	(1.528)	(1.506)	(1.439)	(1.401)	(1.942)
Population (1,000)	-0.172*	-0.165*	-0.158*	-0.135	-0.077	-0.263**
	(0.099)	(0.097)	(0.095)	(0.087)	(0.071)	(0.129)
Population density	-0.206	-0.207	-0.208	-0.193	-0.164	-0.250
	(0.187)	(0.184)	(0.181)	(0.169)	(0.149)	(0.241)
Median HH income	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.062**	0.061**	0.061**	0.059**	0.060***	0.059**
	(0.025)	(0.024)	(0.024)	(0.023)	(0.023)	(0.029)
Prop. veterans	0.106	0.094	0.082	0.097	0.117	0.080
	(0.271)	(0.267)	(0.263)	(0.254)	(0.251)	(0.327)
Poverty rate	-0.223*	-0.218*	-0.212*	-0.194	-0.155	-0.285**
	(0.127)	(0.127)	(0.128)	(0.130)	(0.139)	(0.135)
Unemployment	-0.064	-0.082	-0.100	-0.139	-0.242*	0.103
	(0.133)	(0.131)	(0.130)	(0.126)	(0.129)	(0.165)
County & Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15208	15208	15208	15208	15208	15208
Kleibergen-Paap F-Statistic	36.692	36.692	36.692	36.692	36.692	36.692
Imposed AR(1) coefficient	0.3	0.3	0.3	0.4	0.6	0
Imposed AR(2) coefficient	0.1	0.15	0.2	0.2	0.25	0
Imposed AR(3) coefficient	0	0.05	0.1	0.1	0.1	0

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 34: *Sensitivity of the Effect on Robberies – Pop. Density < 100p./sq. mi. Subsample – Total (log) Number of Items*

Model	(1)	(2)	(3)	(4)	(5)	Static
ln(No. of items)	-3.293*	-3.185*	-3.077*	-2.629	-1.506	-5.089**
	(1.921)	(1.892)	(1.865)	(1.787)	(1.751)	(2.387)
Population (1,000)	-0.197*	-0.189*	-0.182*	-0.155*	-0.089	-0.302**
	(0.102)	(0.100)	(0.099)	(0.090)	(0.073)	(0.135)
Population density	-0.175	-0.177	-0.179	-0.168	-0.149	-0.202
	(0.190)	(0.187)	(0.184)	(0.171)	(0.151)	(0.246)
Median HH income	0.000	0.000	0.000	0.000	0.000	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.059**	0.058**	0.058**	0.057**	0.059**	0.055*
	(0.025)	(0.025)	(0.024)	(0.024)	(0.024)	(0.029)
Prop. veterans	0.084	0.073	0.062	0.079	0.107	0.046
	(0.269)	(0.265)	(0.261)	(0.252)	(0.249)	(0.324)
Poverty rate	-0.232*	-0.226*	-0.221*	-0.202	-0.159	-0.299**
	(0.127)	(0.127)	(0.128)	(0.129)	(0.139)	(0.137)
Unemployment	-0.085	-0.103	-0.121	-0.157	-0.252*	0.069
	(0.134)	(0.132)	(0.131)	(0.127)	(0.131)	(0.165)
County & Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15208	15208	15208	15208	15208	15208
Kleibergen-Paap F-Statistic	49.849	49.849	49.849	49.849	49.849	49.849
Imposed AR(1) coefficient	0.3	0.3	0.3	0.4	0.6	0
Imposed AR(2) coefficient	0.1	0.15	0.2	0.2	0.25	0
Imposed AR(3) coefficient	0	0.05	0.1	0.1	0.1	0

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 35: *Sensitivity of the Effect on Robberies – Pop. Density < 100p./sq. mi. Subsample – Total (log) Number of Guns*

Model	(1)	(2)	(3)	(4)	(5)	Static
ln(No. of items)	-6.732*	-6.511*	-6.291	-5.374	-3.080	-10.403**
	(3.956)	(3.895)	(3.839)	(3.670)	(3.581)	(4.950)
Population (1,000)	-0.069	-0.065	-0.062	-0.052	-0.030	-0.104
	(0.112)	(0.110)	(0.109)	(0.102)	(0.091)	(0.142)
Population density	-0.318*	-0.315*	-0.313*	-0.282	-0.215	-0.423*
	(0.191)	(0.188)	(0.185)	(0.173)	(0.154)	(0.244)
Median HH income	0.000	0.000	0.000	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.066***	0.065***	0.064***	0.062***	0.062***	0.065**
	(0.024)	(0.024)	(0.023)	(0.023)	(0.022)	(0.028)
Prop. veterans	0.130	0.117	0.105	0.116	0.128	0.117
	(0.273)	(0.268)	(0.265)	(0.255)	(0.253)	(0.330)
Poverty rate	-0.186	-0.182	-0.177	-0.165	-0.137	-0.228*
	(0.129)	(0.129)	(0.130)	(0.132)	(0.143)	(0.134)
Unemployment	-0.039	-0.058	-0.077	-0.119	-0.230*	0.141
	(0.133)	(0.131)	(0.130)	(0.125)	(0.129)	(0.165)
County & Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15208	15208	15208	15208	15208	15208
Kleibergen-Paap F-Statistic	36.995	36.995	36.995	36.995	36.995	36.995
Imposed AR(1) coefficient	0.3	0.3	0.3	0.4	0.6	0
Imposed AR(2) coefficient	0.1	0.15	0.2	0.2	0.25	0
Imposed AR(3) coefficient	0	0.05	0.1	0.1	0.1	0

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t - 1$.

Table 36: *Sensitivity of the Effect on Robberies – Pop. Density < 100p./sq. mi. Subsample – Total (log) Number of Vehicles*

Model	(1)	(2)	(3)	(4)	(5)	Static
ln(No. of items)	-0.482*	-0.466*	-0.451*	-0.385	-0.221	-0.745**
	(0.278)	(0.274)	(0.270)	(0.260)	(0.256)	(0.342)
Population (1,000)	-0.183*	-0.176*	-0.169*	-0.144	-0.083	-0.281**
	(0.102)	(0.101)	(0.099)	(0.091)	(0.073)	(0.134)
Population density	-0.247	-0.246	-0.246	-0.226	-0.182	-0.313
	(0.185)	(0.182)	(0.179)	(0.167)	(0.147)	(0.237)
Median HH income	0.000	0.000	0.000	0.000	0.000	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.067***	0.066***	0.065***	0.063***	0.063***	0.067**
	(0.024)	(0.024)	(0.023)	(0.022)	(0.022)	(0.028)
Prop. veterans	0.047	0.038	0.028	0.050	0.090	-0.010
	(0.263)	(0.259)	(0.256)	(0.247)	(0.244)	(0.319)
Poverty rate	-0.218*	-0.213*	-0.207	-0.190	-0.152	-0.278**
	(0.127)	(0.127)	(0.128)	(0.129)	(0.139)	(0.134)
Unemployment	-0.035	-0.054	-0.074	-0.116	-0.229*	0.147
	(0.132)	(0.130)	(0.129)	(0.125)	(0.129)	(0.164)
County & Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15208	15208	15208	15208	15208	15208
Kleibergen-Paap F-Statistic	87.355	87.355	87.355	87.355	87.355	87.355
Imposed AR(1) coefficient	0.3	0.3	0.3	0.4	0.6	0
Imposed AR(2) coefficient	0.1	0.15	0.2	0.2	0.25	0
Imposed AR(3) coefficient	0	0.05	0.1	0.1	0.1	0

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 37: *Sensitivity of the Effect on Robberies – Pop. Density < 100p./sq. mi. Subsample – Per Capita Equipment Value*

Model	(1)	(2)	(3)	(4)	(5)	Static
ln(No. of items)	-0.627**	-0.610**	-0.592**	-0.542**	-0.430*	-0.329
	(0.277)	(0.273)	(0.269)	(0.256)	(0.252)	(0.505)
Population (1,000)	-0.131**	-0.127**	-0.123**	-0.112**	-0.089**	-0.065
	(0.064)	(0.063)	(0.062)	(0.056)	(0.043)	(0.067)
Population density	-0.169	-0.171	-0.173	-0.152	-0.105	-0.261*
	(0.127)	(0.126)	(0.125)	(0.115)	(0.097)	(0.140)
Median HH income	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.051**	0.050**	0.050**	0.048**	0.047**	0.053**
	(0.020)	(0.020)	(0.020)	(0.019)	(0.018)	(0.025)
Prop. veterans	0.032	0.014	-0.004	0.005	0.011	0.313
	(0.229)	(0.226)	(0.224)	(0.215)	(0.212)	(0.193)
Poverty rate	-0.151	-0.146	-0.141	-0.118	-0.057	-0.367**
	(0.125)	(0.125)	(0.125)	(0.122)	(0.122)	(0.154)
Unemployment	0.074	0.061	0.048	-0.005	-0.129	0.241
	(0.122)	(0.120)	(0.119)	(0.114)	(0.115)	(0.157)
County & Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18165	18165	18165	18165	18165	18165
Kleibergen-Paap F-Statistic	98.970	98.970	98.970	98.970	98.970	98.970
Imposed AR(1) coefficient	0.3	0.3	0.3	0.4	0.6	0
Imposed AR(2) coefficient	0.1	0.15	0.2	0.2	0.25	0
Imposed AR(3) coefficient	0	0.05	0.1	0.1	0.1	0

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as reported crimes per 100,000 population. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 38: *Sensitivity of the Effect on Robberies – Pop. Density < 100p./sq. mi. Subsample – Extended Sample Period (back to 2000)*

G. Arrest Rate Impacts – Full Specification and Robustness

Arrest rate for	Agg. Ass.	Murder	Robbery
ln(value of equip. stock)	-3.600**	-0.485***	-1.578***
	(1.643)	(0.115)	(0.309)
Population (1,000)	-0.189	-0.053**	-0.051
	(0.304)	(0.026)	(0.070)
Population density	-1.440**	-0.029	-0.252*
	(0.584)	(0.043)	(0.132)
Median HH income	0.001***	0.000	0.000
	(0.000)	(0.000)	(0.000)
Prop. black	0.215***	0.028***	0.082***
	(0.074)	(0.007)	(0.019)
Prop. veterans	-1.548	-0.012	-0.304
	(1.302)	(0.104)	(0.278)
Poverty rate	-0.309	0.015	-0.291***
	(0.362)	(0.045)	(0.083)
Unemployment	-1.873***	-0.080	-0.002
	(0.521)	(0.057)	(0.123)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	15208	15208	15208
Kleibergen-Paap F-Statistic	93.893	93.893	93.893

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Dependent variable is measured as the number of arrests per 100,000 inhabitants. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 39: *The Effect of Equipment on Arrests – Full Table of Estimates in Discussion*

Arr. Rate for	Agg. Ass.	Murder	Robbery	Agg. Ass.	Murder	Robbery
Parameter of interest						
ln(value of equip. stock)	-1.585	-0.432***	-1.102***	-1.751	-0.227*	-0.786**
	(1.255)	(0.109)	(0.347)	(1.330)	(0.118)	(0.310)
Controls						
lag(dep. var.)	0.291***	0.026	0.066	0.259***	0.076	0.044
	(0.061)	(0.068)	(0.054)	(0.064)	(0.099)	(0.043)
2nd lag(dep. var.)	0.094***	0.002	0.017	0.085**	0.028	0.005
	(0.034)	(0.050)	(0.039)	(0.036)	(0.074)	(0.029)
3rd lag(dep. var.)		-0.001			0.024	
		(0.030)			(0.042)	
Population (1,000)	-0.203	-0.035*	-0.064	-0.636	-0.015	-0.056
	(0.192)	(0.020)	(0.115)	(0.678)	(0.029)	(0.081)
Population density	-0.801**	-0.024	-0.119	0.167	-0.011	0.025
	(0.352)	(0.034)	(0.125)	(0.963)	(0.091)	(0.230)
Median HH income	0.001***	0.000	0.000	0.001***	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Prop. black	0.147***	0.024***	0.062***	0.091	0.021**	0.054***
	(0.052)	(0.008)	(0.019)	(0.064)	(0.009)	(0.019)
Prop. veterans	-0.501	-0.012	-0.052	-0.703	-0.035	-0.052
	(0.940)	(0.098)	(0.233)	(1.191)	(0.117)	(0.260)
Poverty rate	0.145	0.004	-0.212**	-0.038	0.010	-0.198*
	(0.312)	(0.043)	(0.084)	(0.402)	(0.053)	(0.103)
Unemployment	-1.423***	-0.083	-0.030	-1.828***	-0.074	-0.188
	(0.450)	(0.054)	(0.111)	(0.591)	(0.067)	(0.131)
Constant	85.413***	7.302***	24.229***	59.550*	4.379*	19.140**
	(25.717)	(2.002)	(6.972)	(31.919)	(2.330)	(7.462)
Pop. dens. <100	Yes	Yes	Yes	No	No	No
Pop. dens. <50	No	No	No	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15217	15217	15217	10425	10425	10425
Number of Counties	1592	1592	1592	1106	1106	1106
AR(2)-Test (p-value)	0.853	0.642	0.284	0.566	0.721	0.322
Hansen Test (p-value)	0.054	0.319	0.002	0.414	0.455	0.045
Number of Instruments	37	37	37	37	37	37

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Two step GMM estimation using the forward orthogonal deviations transformation. Windmeijer (2005) adjusted cluster robust standard errors in parentheses. Dependent variable is measured as reported crimes per 100,000 population. GMM-type instruments consist of lagged levels for the f.o.d. equation and the lag difference of the dep. var. for the level equation in case of murder. In all cases, the f.o.d. equation is instrumented with lag differences and the level equation with lagged second differences of the dep. var. Only the second lag was used for all instruments. All controls are treated as strictly exogenous. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 40: *The Effect of Equipment on Arrest Rates – System-GMM Estimates*

Arr. Rate for	Agg. Ass.	Murder	Robbery
Parameter of interest			
ln(value of equip. stock)	0.024	-0.021	-0.039*
	(0.032)	(0.066)	(0.022)
Controls			
lag(dep. var.)	0.176**	-0.010	0.155***
	(0.078)	(0.077)	(0.046)
2nd lag(dep. var.)	0.060	-0.023	0.052*
	(0.042)	(0.043)	(0.028)
3rd lag(dep. var.)		-0.037	
		(0.028)	
Population (1,000)	-0.000	0.001	0.001
	(0.005)	(0.002)	(0.001)
Population density	0.008	-0.017	-0.002
	(0.008)	(0.011)	(0.006)
Median HH income	0.000	0.000*	0.000
	(0.000)	(0.000)	(0.000)
Prop. black	0.000	0.001	0.001
	(0.001)	(0.002)	(0.001)
Prop. veterans	-0.001	0.017	-0.005
	(0.014)	(0.045)	(0.016)
Poverty rate	0.004	-0.011	-0.006
	(0.005)	(0.016)	(0.005)
Unemployment	-0.018*	0.002	-0.009
	(0.009)	(0.021)	(0.007)
Constant	0.030	1.008	0.765*
	(0.504)	(1.117)	(0.405)
Pop. dens. <100	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	13702	1979	8299
Number of Counties	1536	455	1150
AR(2)-Test (p-value)	0.538	0.963	0.427
Hansen Test (p-value)	0.289	0.491	0.252
Number of Instruments	55	55	55

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Two step GMM estimation using the forward orthogonal deviations transformation. Windmeijer (2005) adjusted cluster robust standard errors in parentheses. Dependent variable is measured as reported crimes per 100,000 population. GMM-type instruments consist of lagged levels for the f.o.d. equation and the lag difference of the dep. var. for the level equation in case of murder. In all cases, the f.o.d. equation is instrumented with lag differences and the level equation with lagged second differences of the dep. var. For the f.o.d. equation, the second, third and fourth lags were used as instruments. All controls are treated as strictly exogenous. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 41: *The Effect of Equipment on Arrests per Reported Offence – System-GMM Estimates*

H. Officer Safety and Police Shooting Outcomes – Robustness

Dep. Var.	Off. Killed	Off. Ass.	Empl. Rate	Fatal Police Encounters		
ln(Equip.)	-0.010	-2.625***	0.071***	-0.018	0.011	0.014*
	(0.008)	(0.722)	(0.027)	(0.012)	(0.008)	(0.008)
Pop	0.000	0.092	0.003	0.009	-0.003	0.000
	(0.001)	(0.107)	(0.003)	(0.007)	(0.008)	(0.006)
PopDens	0.000	-0.268	-0.036***	-0.003	-0.000	-0.000
	(0.002)	(0.189)	(0.008)	(0.006)	(0.000)	(0.000)
Income	0.000	0.000	0.000***	-0.000*	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black	-0.000	-0.030	0.001	0.000	0.000	0.000
	(0.000)	(0.023)	(0.001)	(0.000)	(0.000)	(0.000)
VetPop	0.002	-0.506	-0.017	0.011	-0.005	-0.003
	(0.009)	(0.343)	(0.015)	(0.007)	(0.005)	(0.004)
Poverty	-0.000	-0.313***	-0.001	0.001	0.002	0.002
	(0.002)	(0.119)	(0.005)	(0.003)	(0.002)	(0.002)
Unemp.	0.001	-0.131	-0.003	0.001	0.001	0.001
	(0.006)	(0.134)	(0.005)	(0.003)	(0.003)	(0.002)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	P. Dens.	P. Dens.	P. Dens.	P. Dens.	Pop.	Pop.
	<100	<100	<100	<100	<30,000	<30,000
Extended Period	No	No	No	No	No	Yes
Observations	7559	7559	7559	15208	10447	12466
KP F-Stat.	30.795	30.795	30.795	93.893	44.063	45.947

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Employee and officer rates are measured as number of employees/officers per 1000 population. Officers killed or assaulted are measured relative to the total number of officers, scaled to lie between 0 and 100. Fatal police encounters are measured in the absolute number of incidents in a county. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$. The extended period sample covers the years 2000-2014, while the rest is restricted to 2005-2014. All estimates are based on a static fixed effects specification.

Table 42: *The Effect of Equipment on Officer Assault, Employment and Fatal Encounters – Full Table*

Dep. Var.	Off. Killed	Off. Ass.	Empl. Rate	Fatal Police Encounters		
ln(Equip.)	-0.047**	-4.944***	0.045***	0.001	0.047	0.045
	(0.020)	(1.198)	(0.016)	(0.047)	(0.077)	(0.073)
Pop	0.004	0.148	0.000	-0.002	-0.072	-0.047
	(0.003)	(0.114)	(0.002)	(0.004)	(0.048)	(0.036)
PopDens	-0.009	-0.181	-0.028***	-0.014	-0.000	-0.001
	(0.009)	(0.333)	(0.005)	(0.013)	(0.000)	(0.000)
Income	0.000	-0.000	0.000***	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black	-0.001	-0.057	-0.001*	0.001	0.002	0.001
	(0.002)	(0.060)	(0.001)	(0.002)	(0.003)	(0.002)
VetPop	0.042*	0.976	-0.023**	-0.065	-0.109**	-0.074*
	(0.022)	(0.863)	(0.011)	(0.042)	(0.052)	(0.043)
Poverty	0.001	0.141	-0.003	0.010	0.020	0.017
	(0.009)	(0.285)	(0.004)	(0.015)	(0.022)	(0.018)
Unemp.	0.005	-0.139	-0.006	-0.001	0.004	0.002
	(0.010)	(0.294)	(0.005)	(0.017)	(0.024)	(0.022)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	P. Dens.	P. Dens.	P. Dens.	P. Dens.	Pop.	Pop.
	<100	<100	<100	<100	<30,000	<30,000
Extended Period	No	No	No	No	No	Yes
Observations	11393	11393	11393	15208	10447	12466
KP F-Stat.	82.696	82.696	82.696	93.893	44.063	45.947

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. Standard errors (clustered at the county level) in parentheses. Employee and officer rates are measured as number of employees/officers per 1000 population. Officers killed or assaulted are measured relative to the total number of officers, scaled to lie between 0 and 100. Fatal police encounters are measured in the number of incidents per 100,000 inhabitants in a county. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$. The extended period sample covers the years 2000-2014, while the rest is restricted to 2005-2014. All estimates are based on a static fixed effects specification.

Table 43: *The Effect of Equipment on Officer Assault, Employment and Fatal Encounters - No Restriction of Reporting Coverage Indicator and Police Killings Measured Relative to Population (instead of absolute number of incidents)*

Dep. Var.	Off. Killed	Off. Ass.	Empl. Rate	Fatal Police Encounters		
ln(Equip.)	-0.011	-2.386***	0.074***	-0.018	0.013	0.016**
	(0.008)	(0.660)	(0.027)	(0.013)	(0.008)	(0.008)
Pop	0.000	0.093	0.003	0.009	-0.003	0.000
	(0.001)	(0.102)	(0.003)	(0.007)	(0.008)	(0.006)
PopDens	0.000	-0.257	-0.036***	-0.003	-0.000	-0.000
	(0.002)	(0.181)	(0.008)	(0.006)	(0.000)	(0.000)
Income	0.000	0.000	0.000***	-0.000*	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black	-0.000	-0.027	0.001	0.000	0.000	0.000
	(0.000)	(0.022)	(0.001)	(0.000)	(0.000)	(0.000)
VetPop	0.002	-0.522	-0.017	0.011	-0.005	-0.003
	(0.009)	(0.330)	(0.015)	(0.007)	(0.005)	(0.004)
Poverty	-0.000	-0.309***	-0.001	0.001	0.002	0.002
	(0.002)	(0.116)	(0.005)	(0.003)	(0.002)	(0.002)
Unemp.	0.001	-0.142	-0.003	0.001	0.001	0.001
	(0.006)	(0.131)	(0.006)	(0.003)	(0.003)	(0.002)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	P. Dens.	P. Dens.	P. Dens.	P. Dens.	Pop.	Pop.
	<100	<100	<100	<100	<30,000	<30,000
Extended Period	No	No	No	No	No	Yes
Observations	7559	7559	7559	15208	10447	12466
KP F-Stat.	19.048	19.048	19.048	44.264	20.062	20.523
Hansen J-Stat (p-val)	0.382	0.201	0.977	0.244	0.829	0.963

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Employee and officer rates are measured as number of employees/officers per 1000 population. Officers killed or assaulted are measured relative to the total number of officers, scaled to lie between 0 and 100. Fatal police encounters are measured in the absolute number of incidents in a county. The log of the equipment stock value was instrumented by interacting the distance to the 1st and 6th closest distribution sites with the log of the sum of equipment issued until year $t-1$ (for police killings, the second and seventh distance was used, but results are estimates are not responsive to particular choices - only the specification tests fare worse in some cases). The extended period sample covers the years 2000-2014, while the rest is restricted to 2005-2014. All estimates are based on a static fixed effects specification.

Table 44: *The Effect of Equipment on Officer Assault, Employment and Fatal Encounters – Using Simple Distance Instruments Instead of Principal Component*

Dep. Var.	Off. Killed	Off. Ass.	Empl. Rate	Fatal Police Encounters		
ln(Equip.)/Cap.	-0.009	-2.354***	0.064***	-0.016	0.010	0.012*
	(0.007)	(0.659)	(0.024)	(0.011)	(0.007)	(0.007)
Pop	0.000	0.074	0.004	0.009	-0.002	0.001
	(0.001)	(0.103)	(0.003)	(0.007)	(0.008)	(0.006)
PopDens	0.000	-0.347*	-0.034***	-0.004	-0.000	-0.000
	(0.002)	(0.196)	(0.008)	(0.006)	(0.000)	(0.000)
Income	0.000	0.000	0.000***	-0.000*	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black	-0.000	-0.031	0.001	0.000	0.000	0.000
	(0.000)	(0.023)	(0.001)	(0.000)	(0.000)	(0.000)
VetPop	0.001	-0.559	-0.015	0.010	-0.005	-0.003
	(0.009)	(0.350)	(0.015)	(0.007)	(0.005)	(0.004)
Poverty	-0.000	-0.316***	-0.001	0.001	0.002	0.002
	(0.002)	(0.120)	(0.005)	(0.003)	(0.002)	(0.002)
Unemp.	0.001	-0.135	-0.003	0.001	0.001	0.001
	(0.006)	(0.135)	(0.006)	(0.003)	(0.003)	(0.002)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	P. Dens.	P. Dens.	P. Dens.	P. Dens.	Pop.	Pop.
	<100	<100	<100	<100	<30,000	<30,000
Extended Period	No	No	No	No	No	Yes
Observations	7559	7559	7559	15208	10447	12466
KP F-Stat.	27.889	27.889	27.889	87.355	40.590	42.173

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Employee and officer rates are measured as number of employees/officers per 1000 population. Officers killed or assaulted are measured relative to the total number of officers, scaled to lie between 0 and 100. Fatal police encounters are measured in the absolute number of incidents in a county. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t - 1$. The extended period sample covers the years 2000-2014, while the rest is restricted to 2005-2014. All estimates are based on a static fixed effects specification.

Table 45: *The Effect of Equipment on Officer Assault, Employment and Fatal Encounters – Using Per Capita Militarisation*

Dep. Var.	Off. Killed	Off. Ass.	Empl. Rate	Fatal Police Encounters		
ln(No. guns)	-0.061	-15.579***	0.424**	-0.111	0.068	0.084*
	(0.048)	(4.951)	(0.167)	(0.075)	(0.049)	(0.044)
Pop	-0.000	-0.016	0.006	0.009	-0.002	0.001
	(0.001)	(0.091)	(0.004)	(0.007)	(0.008)	(0.006)
PopDens	0.001	-0.011	-0.043***	-0.001	-0.000	-0.000
	(0.002)	(0.236)	(0.010)	(0.007)	(0.000)	(0.000)
Income	0.000	0.000	0.000***	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black	-0.001	-0.087**	0.002	-0.000	0.000	0.000
	(0.000)	(0.038)	(0.001)	(0.000)	(0.000)	(0.000)
VetPop	0.001	-0.620	-0.013	0.011	-0.004	-0.002
	(0.009)	(0.400)	(0.016)	(0.007)	(0.005)	(0.004)
Poverty	0.000	-0.266**	-0.003	0.000	0.002	0.002
	(0.002)	(0.116)	(0.005)	(0.003)	(0.002)	(0.002)
Unemp.	0.001	-0.301**	0.001	-0.001	0.002	0.002
	(0.006)	(0.148)	(0.006)	(0.003)	(0.002)	(0.002)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	P. Dens.	P. Dens.	P. Dens.	P. Dens.	Pop.	Pop.
	<100	<100	<100	<100	<30,000	<30,000
Extended Period	No	No	No	No	No	Yes
Observations	7559	7559	7559	15208	10447	12466
KP F-Stat.	15.888	15.888	15.888	49.849	27.593	29.951

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Employee and officer rates are measured as number of employees/officers per 1000 population. Officers killed or assaulted are measured relative to the total number of officers, scaled to lie between 0 and 100. Fatal police encounters are measured in the absolute number of incidents in a county. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$. The extended period sample covers the years 2000-2014, while the rest is restricted to 2005-2014. All estimates are based on a static fixed effects specification.

Table 46: *The Effect of Equipment on Officer Assault, Employment and Fatal Encounters – Using the Number of Guns as Militarisation Measure*

Dep. Var.	Off. Killed	Off. Ass.	Empl. Rate	Fatal Police Encounters		
ln(No. vehicles)	-0.114	-29.360***	0.799***	-0.227	0.112	0.159*
	(0.091)	(9.712)	(0.299)	(0.149)	(0.081)	(0.085)
Pop	0.002	0.562***	-0.009	0.013*	-0.002	0.000
	(0.002)	(0.212)	(0.006)	(0.007)	(0.008)	(0.006)
PopDens	-0.000	-0.396*	-0.032***	-0.006	-0.000	-0.000
	(0.002)	(0.232)	(0.009)	(0.006)	(0.000)	(0.000)
Income	0.000	-0.000	0.000***	-0.000*	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black	-0.000	0.011	-0.001	0.000	0.000	0.000
	(0.000)	(0.024)	(0.001)	(0.000)	(0.000)	(0.000)
VetPop	0.004	0.108	-0.033**	0.013*	-0.006	-0.004
	(0.008)	(0.464)	(0.017)	(0.008)	(0.005)	(0.004)
Poverty	-0.000	-0.420***	0.001	0.002	0.002	0.002
	(0.002)	(0.143)	(0.005)	(0.003)	(0.002)	(0.002)
Unemp.	0.001	-0.266	0.000	0.001	0.002	0.002
	(0.006)	(0.165)	(0.006)	(0.003)	(0.002)	(0.002)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	P. Dens.	P. Dens.	P. Dens.	P. Dens.	Pop.	Pop.
	<100	<100	<100	<100	<30,000	<30,000
Extended Period	No	No	No	No	No	Yes
Observations	7559	7559	7559	15208	10447	12466
KP F-Stat.	16.963	16.963	16.963	36.995	36.551	34.244

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Employee and officer rates are measured as number of employees/officers per 1000 population. Officers killed or assaulted are measured relative to the total number of officers, scaled to lie between 0 and 100. Fatal police encounters are measured in the absolute number of incidents in a county. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$. The extended period sample covers the years 2000-2014, while the rest is restricted to 2005-2014. All estimates are based on a static fixed effects specification.

Table 47: *The Effect of Equipment on Officer Assault, Employment and Fatal Encounters – Using the Number of Vehicles as Militarisation Measure*

I. Different Instrument Based on Military Expenditures – Robustness

Type of crime	Violent	Property	Agg. Ass.	Murder	Robbery
ln(value of equip. stock)	-3.881	6.721	-3.613	0.099	0.096
	(4.644)	(24.170)	(4.151)	(0.162)	(0.561)
Population density	-8.550**	-13.294	-7.156*	-0.108	-0.787
	(4.270)	(17.344)	(3.744)	(0.117)	(0.553)
Population	3.406	-17.750	2.910	0.114*	0.145
	(2.660)	(14.673)	(2.423)	(0.068)	(0.271)
Median HH income	0.002**	0.011**	0.002**	0.000	0.000
	(0.001)	(0.005)	(0.001)	(0.000)	(0.000)
Prop. black	-0.166	-1.633	-0.240	0.011	0.074*
	(0.305)	(1.304)	(0.278)	(0.009)	(0.041)
Prop. veterans	-1.495	-5.602	-1.703	-0.100	0.300
	(3.795)	(16.181)	(3.387)	(0.136)	(0.404)
Poverty rate	-1.958	-3.174	-1.666	0.008	-0.158
	(1.241)	(4.642)	(1.127)	(0.046)	(0.157)
Unemployment	-2.202	-0.862	-2.280	-0.068	0.103
	(2.353)	(9.127)	(2.164)	(0.093)	(0.264)
Pop. dens. <50	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes
State-year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	10415	10415	10415	10415	10415
Kleibergen-Paap F-Statistic	111.366	111.366	111.366	111.366	111.366

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. All specifications include county and state-year fixed effects. Dependent variable is measured as reported crimes per 100,000 population. Controls include year fixed effects, demographic (black and veteran population, pop. density) and economic (poverty and median income) variables.The log of the equipment stock value was instrumented by interacting the total (lagged) US military expenditure in a given year with the relative frequency of equipment take-up in the 2006-2014 period, analogous to Bove and Gavrilova (2017).

Table 48: *The Effect of Equipment on Crime Rates – Low Pop. Dens. – Using Bove and Gavrilova (2017)-type Instrument*

Type of crime	Violent	Property	Agg. Ass.	Murder	Robbery
ln(value of equip. stock)	-6.233	-44.223	-1.654	-0.076	-4.625***
	(6.331)	(33.390)	(5.448)	(0.131)	(1.772)
Population density	-0.116***	-0.550*	-0.051**	-0.003**	-0.058***
	(0.036)	(0.304)	(0.020)	(0.001)	(0.020)
Population	-0.410***	-2.924***	-0.159**	-0.007***	-0.233***
	(0.113)	(0.838)	(0.067)	(0.002)	(0.057)
Median HH income	0.002***	0.007**	0.001***	0.000	0.001***
	(0.000)	(0.003)	(0.000)	(0.000)	(0.000)
Prop. black	1.308***	3.667**	0.567**	0.008	0.711***
	(0.338)	(1.751)	(0.230)	(0.006)	(0.139)
Prop. veterans	3.099	40.889***	0.529	0.144*	2.082**
	(2.507)	(14.345)	(2.020)	(0.074)	(0.869)
Poverty rate	-0.342	-5.675	0.128	-0.024	-0.449*
	(0.794)	(4.265)	(0.668)	(0.031)	(0.240)
Unemployment	-2.247	-4.605	-1.645	-0.038	-0.259
	(1.521)	(9.335)	(1.192)	(0.048)	(0.496)
Pop. dens. >50	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes
State-year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	12527	12527	12527	12527	12527
Kleibergen-Paap F-Statistic	89.578	89.578	89.578	89.578	89.578

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. All specifications include county and state-year fixed effects. Dependent variable is measured as reported crimes per 100,000 population. Controls include year fixed effects, demographic (black and veteran population, pop. density) and economic (poverty and median income) variables.The log of the equipment stock value was instrumented by interacting the total (lagged) US military expenditure in a given year with the relative frequency of equipment take-up in the 2006-2014 period, analogous to Bove and Gavrilova (2017).

Table 49: *The Effect of Equipment on Crime Rates – High Pop. Dens. – Using Bove and Gavrilova (2017)-type Instrument*

Dep. Var.	Off. Killed	Off. Ass.	Empl. Rate	Fatal Police Encounters		
ln(Equip.)	-0.005	0.069	0.028	0.010	0.018***	0.008
	(0.009)	(0.556)	(0.021)	(0.010)	(0.007)	(0.006)
Pop. Dens.	0.003	-0.060	-0.047***	-0.004	0.000	-0.000
	(0.003)	(0.137)	(0.009)	(0.006)	(0.000)	(0.000)
Pop.	-0.001	-0.006	0.007*	0.008	-0.007	-0.003
	(0.001)	(0.094)	(0.004)	(0.007)	(0.009)	(0.007)
Income	0.000	0.000***	0.000***	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black	-0.000	0.017	0.000	-0.000	0.000	0.000
	(0.001)	(0.019)	(0.001)	(0.001)	(0.000)	(0.000)
Vet. Pop.	0.006	-0.818***	-0.018	0.005	-0.008	-0.006
	(0.007)	(0.285)	(0.016)	(0.007)	(0.006)	(0.005)
Poverty	0.002	-0.256***	0.001	0.002	0.003	0.002
	(0.002)	(0.096)	(0.005)	(0.003)	(0.002)	(0.002)
Unemp.	0.000	-0.095	-0.006	-0.002	-0.000	-0.000
	(0.008)	(0.160)	(0.007)	(0.004)	(0.004)	(0.003)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	P. Dens.	P. Dens.	P. Dens.	P. Dens.	Pop.	Pop.
	<100	<100	<100	<100	<30,000	<30,000
Extended Period	No	No	No	No	No	Yes
Observations	7559	7559	7559	15208	10447	12466
KP F-Stat.	66.909	66.909	66.909	167.701	127.086	124.201

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Employee and officer rates are measured as number of employees/officers per 1000 population. Officers killed or assaulted are measured relative to the total number of officers, scaled to lie between 0 and 100. Fatal police encounters are measured in the absolute number of incidents in a county. The log of the equipment stock value was instrumented by interacting the total (lagged) US military expenditure in a given year with the relative frequency of equipment take-up in the 2006-2014 period, analogous to Bove and Gavrilova (2017). The extended period sample covers the years 2000-2014, while the rest is restricted to 2005-2014.

Table 50: *The Effect of Equipment on Officer Assault, Employment and Fatal Encounters – Using Bove and Gavrilova (2017)-type Instrument*

Arr. Rate for	Agg. Ass.	Murder	Robbery
ln(value of equip. stock)	2.465	-0.006	-1.423**
	(2.791)	(0.138)	(0.636)
Population density	-0.028**	0.000	-0.016***
	(0.014)	(0.001)	(0.005)
Population	0.016	-0.002	-0.017*
	(0.034)	(0.002)	(0.010)
Median HH income	0.000	0.000	0.000**
	(0.000)	(0.000)	(0.000)
Prop. black	0.253*	0.010	0.162***
	(0.142)	(0.008)	(0.036)
Prop. veterans	-0.596	0.191***	0.284
	(1.088)	(0.067)	(0.285)
Poverty rate	-0.628	-0.033	-0.183*
	(0.430)	(0.035)	(0.102)
Unemployment	-0.076	-0.133**	0.064
	(0.664)	(0.056)	(0.195)
Pop. dens. >50	Yes	Yes	Yes
State-year FE	Yes	Yes	Yes
Observations	12527	12527	12527
KP F-Statistic	89.578	89.578	89.578

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. All specifications include county and state-year fixed effects. The log of the equipment stock value was instrumented by interacting the total (lagged) US military expenditure in a given year with the relative frequency of equipment take-up in the 2006-2014 period, analogous to Bove and Gavrilova (2017).

Table 51: *The Effect of Equipment on Arrest Rates – High Pop. Dens. – Using Bove and Gavrilova (2017)-type Instrument*

Arr. Rate for	Agg. Ass.	Murder	Robbery
ln(value of equip. stock)	-2.032	0.275	0.300
	(2.682)	(0.180)	(0.320)
Population density	-0.884	0.099	0.061
	(1.674)	(0.150)	(0.379)
Population	0.296	-0.041	-0.012
	(1.217)	(0.091)	(0.233)
Median HH income	0.001***	0.000	-0.000*
	(0.000)	(0.000)	(0.000)
Prop. black	-0.056	0.023*	0.049**
	(0.129)	(0.012)	(0.023)
Prop. veterans	-0.224	-0.019	-0.077
	(1.775)	(0.130)	(0.380)
Poverty rate	-0.412	0.048	-0.219**
	(0.430)	(0.057)	(0.093)
Unemployment	-2.187**	-0.118	0.230
	(1.025)	(0.098)	(0.197)
Pop. dens. <50	Yes	Yes	Yes
State-year FE	Yes	Yes	Yes
Observations	10415	10415	10415
KP F-Statistic	111.366	111.366	111.366

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. All specifications include county and state-year fixed effects. The log of the equipment stock value was instrumented by interacting the total (lagged) US military expenditure in a given year with the relative frequency of equipment take-up in the 2006-2014 period, analogous to Bove and Gavrilova (2017).

Table 52: *The Effect of Equipment on Arrest Rates – Low Pop. Dens. – Using Bove and Gavrilova (2017)-type Instrument*

Type of Crime	Agg. Ass.	Murder	Robbery
ln(value of equip. stock)	-0.191	0.194	-0.818**
	(0.125)	(3.120)	(0.413)
Population density	-0.006	0.112	-0.265**
	(0.013)	(0.403)	(0.134)
Population	-0.065*	-4.436***	-0.328
	(0.036)	(1.262)	(0.258)
Median HH income	0.000	0.002***	0.000*
	(0.000)	(0.001)	(0.000)
Prop. black	0.003	-0.042	0.061**
	(0.005)	(0.181)	(0.029)
Prop. veterans	-0.048	-3.255	-0.028
	(0.103)	(2.610)	(0.325)
Poverty rate	0.016	-0.966	-0.190
	(0.034)	(0.897)	(0.132)
Unemployment	-0.070	-0.195	0.178
	(0.045)	(1.213)	(0.168)
Pop. dens. <100	Yes	Yes	Yes
State-year FE	Yes	Yes	Yes
Observations	14472	14472	14472
KP F-Statistic	81.875	81.875	81.875

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. All specifications include county and state-year fixed effects. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t-1$.

Table 53: *The Effect of Equipment on Crime Rates – Low/med. Pop. Dens. – Excluding Counties <60 Miles from Distribution Site*

Arr. Rate for	Agg. Ass.	Murder	Robbery
ln(value of equip. stock)	-3.746**	-0.471***	-1.573***
	(1.776)	(0.122)	(0.332)
Population density	-0.168	-0.049*	-0.065
	(0.306)	(0.027)	(0.072)
Population	-1.447**	-0.051	-0.200
	(0.626)	(0.045)	(0.137)
Median HH income	0.001***	0.000	0.000
	(0.000)	(0.000)	(0.000)
Prop. black	0.188**	0.029***	0.077***
	(0.077)	(0.007)	(0.020)
Prop. veterans	-1.627	-0.010	-0.404
	(1.372)	(0.108)	(0.286)
Poverty rate	-0.289	0.032	-0.236***
	(0.372)	(0.046)	(0.077)
Unemployment	-1.828***	-0.086	-0.026
	(0.535)	(0.058)	(0.126)
Pop. dens. <100	Yes	Yes	Yes
State-year FE	Yes	Yes	Yes
Observations	14472	14472	14472
KP F-Statistic	81.875	81.875	81.875

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. All specifications include county and state-year fixed effects. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t - 1$.

Table 54: *The Effect of Equipment on Arrest Rates – Low/med. Pop. Dens. – Excluding Counties <60 Miles from Distribution Site*

Dep. Var.	Off. Killed	Off. Ass.	Empl. Rate	Fatal Police Encounters		
ln(Equip.)	-0.010	-2.775***	0.077***	-0.015	0.013	0.016**
	(0.008)	(0.789)	(0.029)	(0.013)	(0.008)	(0.008)
Pop	0.000	0.112	0.002	0.010	-0.004	-0.001
	(0.001)	(0.113)	(0.003)	(0.007)	(0.009)	(0.007)
PopDens	0.000	-0.349*	-0.036***	-0.006	-0.000	-0.000*
	(0.002)	(0.202)	(0.009)	(0.006)	(0.000)	(0.000)
Income	0.000	0.000	0.000***	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black	-0.000	-0.035	0.001	0.000	0.000	0.000
	(0.000)	(0.023)	(0.001)	(0.000)	(0.000)	(0.000)
VetPop	0.001	-0.639*	-0.019	0.010	-0.007	-0.004
	(0.009)	(0.359)	(0.015)	(0.007)	(0.005)	(0.004)
Poverty	0.000	-0.325***	0.000	0.001	0.003	0.003
	(0.002)	(0.125)	(0.005)	(0.003)	(0.002)	(0.002)
Unemp.	0.002	-0.079	-0.003	0.001	0.000	0.000
	(0.006)	(0.139)	(0.006)	(0.003)	(0.003)	(0.002)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	P. Dens.	P. Dens.	P. Dens.	P. Dens.	Pop.	Pop.
	<100	<100	<100	<100	<30,000	<30,000
Extended Period	No	No	No	No	No	Yes
Observations	7262	7262	7262	14472	10055	11995
KP F-Stat.	25.797	25.797	25.797	81.875	41.493	43.314

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Standard errors (clustered at the county level) in parentheses. Employee and officer rates are measured as number of employees/officers per 1000 population. Officers killed or assaulted are measured relative to the total number of officers, scaled to lie between 0 and 100. Fatal police encounters are measured in the number of incidents per 100,000 inhabitants in a county. The log of the equipment stock value was instrumented by interacting the first principal component of the distances to the 10 closest distribution sites with the log of the sum of equipment issued until year $t - 1$. The extended period sample covers the years 2000-2014, while the rest is restricted to 2005-2014. All estimates are based on a static fixed effects specification.

Table 55: *The Effect of Equipment on Officer Assault, Employment and Fatal Encounters - Excluding Counties <60 Miles from Distribution Site*

J. Links to Individual Data Sources

Variables	Source/Agency
Equipment Distributed to Law Enforc. (30/09/17 release)	DLA-LESO
http://www.dla.mil/DispositionServices/FOIA/EFOIALibrary.aspx	
Distribution Center Location	DLA-LESO
http://www.dla.mil/DispositionServices/Contact/FindLocation.aspx	
Crime and Police Agency Data	FBI-UCR/ICSPR
https://www.icpsr.umich.edu/icpsrweb/NACJD/discover-data.jsp	
County Land Area	US Census Bureau
https://www.census.gov/support/USACdataDownloads.html	
County Population Centers	US Census Bureau
https://www.census.gov/geo/reference/centersofpop.html	
County Income and Poverty Rate	US Census Bureau
https://www.census.gov/programs-surveys/saipe/data.html	
Demographics (Sex, Population, Race) - pre 2010	US Census Bureau
https://www.census.gov/support/USACdataDownloads.html	
Demographics (Sex, Population, Race) - 2010+	US Census Bureau
https://www.census.gov/data/tables/2016/demo/popest/counties-total.html	
Veteran Population by County	Dep. of Vet. Aff./Sagestats
http://data.sagepub.com/sagestats/document.php?id=4450	
County HITDA Designation	NHAC
https://web.archive.org/web/20140809221723/http://www.nhac.org/news/HIDTA_Counties.htm	
Unemployment	BLS
https://www.bls.gov/lau/	
Data on Fatal Encounters	Independent Collaboration
http://www.fatalencounters.org/	
Agency Data to Census Crosswalk	ICSPR
https://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/35158	

Table 56: *Links to Data Sources - Links Last Checked: 12 March 2018*

K. Principal Components – Details

The principal components used in this these have the following loadings (standardised to sum up to one):

Instrument - $DistPC1_c*$	$ln(N^o\_TotFlow_t)$	$ln(N^o\_TotStock_t)$	$ln(\$TotStock_t)$	$ln(\$TotFlow_t)$
Distance to closest cent.	0.0667	0.0651	0.0647	0.0654
" 2nd closest cent.	0.0970	0.0968	0.0968	0.0968
" 3rd closest cent.	0.1023	0.1024	0.1025	0.1024
" 4th closest cent.	0.1043	0.1045	0.1046	0.1045
" 5th closest cent.	0.1050	0.1053	0.1053	0.1052
" 6th closest cent.	0.1051	0.1053	0.1054	0.1053
" 7th closest cent.	0.1051	0.1054	0.1054	0.1053
" 8th closest cent.	0.1050	0.1052	0.1053	0.1052
" 9th closest cent.	0.1048	0.1050	0.1051	0.1050
" 10th closest cent.	0.1042	0.1044	0.1044	0.1044

Table 57: *Principal Component Loadings*

N^o indicates that the number of items instead of their dollar value ($) was used. The difference between stock and flow variables is that $TotStock_t$ comprises all equipment distributed up to (and including) year t, while $TotFlow_t$ only sums over equipment shipped in t.

L. Categorisation of Law Enforcement Agencies and Military Equipment Data

The equipment data has been categorized according to 13 digit Nato Stock Numbers (NSN's). These numbers or federal supply groups (the first two numbers of the NSN) have been allocated as follows:

- **Agencies, organisations dropped from sample:** Homeland Security Immigration and Customs Enforcement, Homeland Security Transport Security Authorities, the Homeland Security Federal Protective Service, Justice academies, the FBI, forest services, any assembly sergeant in arms, tribal law enforcement, Dep. of Housing and Urban Development entities, university/campus police, the Naval Criminal Investigative Service, airport authorities, the US Capitol Police, metropolitan transit police, Department of Veteran Affairs, Treasury/inspector general, any fish or wildlife enforcement bodies, the Department of Natural Resources, the DOJ Alcohol and Firearms, the US Postal Inspection Service,the US Customs and Border Protection, the US Marshall Services (but I do include local marshal offices), any other entity without a clear link to local law policing (e.g. hostpitals, veteran homes, prison administration etc.).

- **Classified as consistently measured**: Items with demilitarisation codes *B*, *C*, *D*, *F*, *G* and *Q* (only if integrity code = 3) and/or one of the NSN's listed below. Code *E* was omitted because it only contained an insignificant number of very low value items (mainly normal clothing), and the distribution over time didn't appear as though these items were tracked systematically. The quantitative implications of adding them would be virtually non-existent.

- **Guns**: Includes NSN 1005-00-726-5655, 1005-00-677-9150, 1005-01-094-7045, 1005-00-937-5840, 1005-00-921-5483, 1005-00-856-6885, 1005-01-545-9853, 1005-00-589-1271, 1005-DS-GUN-LE30, 1005-01-073-2368, 1005-00-072-5011, 1005-01-D17-2240, 1095-01-529-4911, 1005-00-073-9421, 1005-01-042-9820, 1005-01-128-9936, 1095-01-529-4911, 1095-01-545-5743, 1095-01-526-7860, 1005-00-179-0300, 1095-01-543-2189

- **Vehicles (narrow definition)**: Includes NSN 1510-01-005-5461, 1510-DS-FIX-WNGA, 1520-00-087-7637, 1520-00-133-9286, 1520-00-169-7137, 1520-00-918-1523, 1520-01-020-4216, 1520-01-043-4949, 1520-01-385-3844, 1520-00-758-0289, 1520-01-520-6275, 1520-01-520-6276, 1520-DS-HEL-ICOP, 1540-DS-GLI-DERS, 1940-00-287-6962, 1940-00-294-2300, 1940-00-529-7277, 1940-01-089-4487, 1940-01-277-0069, 1940-01-450-

8597, 1940-01-477-8615, 1940-01-561-9777, 1940-99-075-1779, 1940-DS-BOA-T000, 1990-01-591-4178, 1990-DS-MIS-CVES, 2310-01-090-7829, 2310-01-091-1684, 2310-01-111-2274, 2310-01-111-2275, 2310-01-146-7194, 2310-01-371-9585, 2310-01-380-8225, 2310-01-380-8290, 2310-01-654-4105, 2310-DS-PAS-SMOT, 2320-00-072-1450, 2320-00-077-1616, 2320-00-077-1617, 2320-00-077-1631, 2320-00-077-1632, 2320-00-077-1633, 2320-00-077-1636, 2320-00-226-6081, 2320-00-455-8464, 2320-00-463-4580, 2320-00-463-4582, 2320-00-490-0860, 2320-00-892-5496, 2320-00-904-3277, 2320-00-926-0873, 2320-00-937-4036, 2320-00-989-7163, 2320-01-015-0399, 2320-01-028-4396, 2320-01-044-7133, 2320-01-047-8754, 2320-01-047-8756, 2320-01-074-7642, 2320-01-090-3436, 2320-01-090-7787, 2320-01-090-7797, 2320-01-090-7799, 2320-01-090-7823, 2320-01-090-7825, 2320-01-090-7890, 2320-01-090-7891, 2320-01-090-7892, 2320-01-090-7893, 2320-01-091-1661, 2320-01-091-1662, 2320-01-091-1722, 2320-01-091-1725, 2320-01-097-0249, 2320-01-097-0260, 2320-01-099-6426, 2320-01-100-7672, 2320-01-107-7153, 2320-01-107-7155, 2320-01-107-7156, 2320-01-113-4669, 2320-01-128-9551, 2320-01-128-9552, 2320-01-143-5782, 2320-01-146-7187, 2320-01-146-7188, 2320-01-146-7189, 2320-01-146-7190, 2320-01-146-7191, 2320-01-146-7193, 2320-01-165-2056, 2320-01-176-0467, 2320-01-178-0516, 2320-01-190-8556, 2320-01-204-7606, 2320-01-205-2665, 2320-01-205-2682, 2320-01-206-4088, 2320-01-230-0300, 2320-01-230-0302, 2320-01-230-0303, 2320-01-230-0304, 2320-01-230-0305, 2320-01-230-0306, 2320-01-230-0307, 2320-01-230-0308, 2320-01-239-1788, 2320-01-272-5028, 2320-01-313-3407, 2320-01-314-2912, 2320-01-318-9902, 2320-01-346-9317, 2320-01-371-9577, 2320-01-371-9579, 2320-01-371-9583, 2320-01-371-9584, 2320-01-372-3932, 2320-01-372-3933, 2320-01-380-8213, 2320-01-380-8229, 2320-01-380-8233, 2320-01-380-8604, 2320-01-389-7558, 2320-01-412-0143, 2320-01-413-3739, 2320-01-418-7400, 2320-01-431-9237, 2320-01-447-4938, 2320-01-447-8577, 2320-01-455-9593, 2320-01-456-1282, 2320-01-467-0677, 2320-01-488-6962, 2320-01-492-8215, 2320-01-493-3785, 2320-01-493-3789, 2320-01-498-6138, 2320-01-499-0015, 2320-01-511-0033, 2320-01-518-7330, 2320-01-518-7332, 2320-01-523-1106, 2320-01-523-1114, 2320-01-523-1314, 2320-01-523-1317, 2320-01-523-1429, 2320-01-523-1432, 2320-01-531-2623, 2320-01-531-9962, 2320-01-540-1993, 2320-01-540-2038, 2320-01-542-8463, 2320-01-549-8577, 2320-01-552-7745, 2320-01-552-7773, 2320-01-563-7052, 2320-01-566-8062, 2320-01-571-2446, 2320-01-572-1119, 2320-01-576-1801, 2320-01-576-4315, 2320-01-581-9536, 2320-DS-TRU-CKDC, 2320-DS-TRU-CKDQ, 2320-DS-VAN-0001, 2340-00-MUL-ES, 2340-01-384-8073, 2340-01-530-9371, 2340-01-574-6673, 2340-DS-ATV-4WHE, 2340-P0-003-6730, 2350-00-860-2349, 2350-01-045-1123, 2350-01-096-9356, 2350-01-132-9099, 2350-01-281-6451, 2350-01-554-8159, 2350-01-575-0646, 2355-00-168-2620, 2355-01-123-1606, 2355-01-552-4677, 2355-01-552-5344, 2355-01-552-5581, 2355-01-553-4634, 2355-01-554-8556, 2355-01-555-0908, 2355-01-558-1053, 2355-01-561-0281, 2355-01-562-6146, 2355-01-564-4340, 2355-01-590-1660, 2355-01-590-2719, 2355-01-602-3357, 2355-20-001-9922, 2355-21-913-4649, 2355-DS-

COM-BTV2, 2410-DS-TRA-CTO0, 2420-01-532-3399, 2420-01-535-4061, 2420-DS-TRA-CTO1

- **Weapons**: Federal Supply Groups 10,13,14
- **Vehicles**: Federal Supply Groups 15,16,17,19,20,23,24,25,26,28,29,30
- **Gear**: Federal Supply Groups 12,42,58,59,67,69,78,84

More information on federal supply groups can be found at the following website: *http://www.win-governmentcontracts.com/federal-supply-groups-fsg.htm*