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Police Militarization and Local Elections*

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Abstract

US local law enforcement agencies have been receiving substantial military equipment through the "1033 Program" during the last decades. Sheriffs, one of the agencies requesting such transfers, are directly accountable to voters for their actions, so one may wonder: how do military equipment transfers in a given county affect the re-election prospects of the county's sheriff? We construct a unique dataset on local electoral races covering 6,218 sheriff elections in 2,381 counties between 2006 and 2016 and reveal the causal effect of military transfers on sheriffs' re-election probabilities: an increase in military transfers in a given county (from none to the median value) results in an increase in the probability the county's sheriff is re-elected (by 3.6 to 9.9 percentage points). This result explains sheriffs' strong support for the "1033 Program" and suggests that the image of a "tough" sheriff in town seems to be rewarded, overall providing fresh evidence on voters' responsiveness in local office elections.

Keywords: Accountability, Local Elections, Militarization, Sheriffs, 1033 Program

JEL Classification: D72, H56, H76, K42

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"Today the President signed an executive order revoking the previous administration's injudicious ban on lifesaving equipment. We applaud the President's actions, and we are encouraged to see him acting on this important issue that we have vocally advocated for. [...] The equipment Sheriffs receive through [the 1033] program include equipment they could not otherwise afford including additional bullet-proof vests and Kevlar helmets, upgraded safety equipment, as well as larger equipment such as helicopters and robotics. By reinstating this program the President will provide more resources to local law enforcement to keep their communities safe without any additional cost to the tax-payer. This has been a top priority for America's Sheriffs, so we praise President Trump and Attorney General Sessions for delivering on this important initiative."

Statement by The National Sheriffs' Association President Harold Eavenson, Rockwall County, TX and Executive Director and CEO Jonathan Thompson, Monday, August 28, 2017.

1 Introduction

In 2015, the Obama administration banned access of sheriffs and other law enforcing agencies to "lifesaving equipment" (from the quote above) previously allocated to them through the "1033 Program". The detailed list of banned items includes tracked armored vehicles; weaponized aircraft, vessels, and vehicles; grenade launchers; camouflage uniforms used for urban settings; and bayonets. Additionally, President Obama ordered more scrutiny and usage oversight of certain other categories of equipment, while his executive order left most of the program largely unaffected. Obama's decision to review some of its aspects and put in place these restrictions on certain categories of equipment followed heavy criticism that the program contributed to excessive police militarization. Such criticism became particularly widespread and topical after the 2014 Ferguson uprising, where the program was blamed for transforming a small town into a large battleground.

Notably though, the 1033 Program has been running since 1997 when Congress authorized the Department of Defense to transfer excess military equipment to local law enforcement agencies supporting them in local operations. And this program follows in the steps of the earlier "1028 Program" initiated by President George H. W. Bush in 1990. Since then, law enforcement agencies responsible for the enforcement of federal, state and local laws have by and large taken advantage of these transfers to support their missions. Nevertheless, transfers are not allocated randomly and there is significant variation of transfers across the country, resulting in heterogeneous levels of "militarization" (see Figure A1 in the Appendix for a graphical illustration). A natural question then arises: how do citizens perceive these transfers? To answer this question, we focus on sheriffs, one of the most important law enforcement agencies and the only ones that are directly accountable to voters through regular elections. In fact, the large majority of sheriffs are elected at the same time as Presidential and mid-term elections, thus attracting a potentially large number of voters. Our aim is to uncover the causal effect, if any, of the transfer of items under the 1033 Program on the probability that sheriffs are re-elected. Do citizens reward or punish sheriffs for the presence of such equipment in a given county? In a nutshell, our empirical analysis supports sheriffs' enthusiasm for the program: equipment transfers increase the probability of sheriffs being re-elected. Our estimates indicate that if a sheriff running for re-election in a county that had not received transfers was instead allocated the median value of equipment allocated in counties in our sample, their re-election probability would increase by 3.6 to 9.9 percentage points.

The office of sheriff is among the most important local offices. It exists in nearly every county and independent city in the United States, with a total of 3,085 offices across the country (Reaves, 2011). These offices have more than 350,000 sworn or civilian employees, employing slightly more than 30% of the total workforce in local law enforcement agencies (Reaves, 2011). The majority of sheriffs' offices are full-service countywide law enforcement agencies (Reaves, 2011), with almost identical responsibilities to any other law enforcement agency citizens encounter in their daily life, most notably "the police" (DeHart, 2020). However, a distinctive feature of this office is sheriffs' direct accountability to voters while most other law enforcement agencies are headed by appointed or professional bureaucrats. Actually, sheriffs have historically been, and are still today, the most widespread elected county office in the US, as compared to judges, prosecutors, constables, and marshals (DeHart, 2020). While the exact responsibilities for which voters may be able to hold sheriffs accountable slightly vary across counties and states, sheriffs are typically involved in all stages of law enforcement. According to the Office of Justice Programs (Department of Justice), *"most sheriffs' offices perform a wide variety of law enforcement functions, including response to criminal incidents, response to calls for service, patrol, crime investigation, arrest of criminal suspects, execution of warrants, traffic enforcement, traffic direction and control, accident investigation, drug enforcement, and crime prevention. Sheriffs' offices also have countywide responsibilities related to jail operation, prisoner transportation, process service, enforcement of court orders, and court security"*.[1] Given sheriffs' responsibilities, their strong support for the 1033 Program, and last but not least the program's own purpose of supporting local law enforcing agencies, one can reasonably wonder how transfers of military equipment in a given county affect the sheriff's electoral performance.

In addressing this research question, we contribute to two recent and growing strands of academic research. First, there is very little evidence on elections for local offices (Warshaw, 2019) and almost no systematic empirical research focusing on sheriffs' offices. Notable exceptions are Thompson (2020), who shows that Democrat and Republican sheriffs comply with federal requests to detain unauthorized immigrants at the same rate, and Zoorob (2019), who demonstrates a strong incumbency advantage for sheriffs. We complement this scarce work, not only by being the first to focus on a "determinant" of sheriffs' electoral performance, but also by constructing the largest dataset on sheriffs' electoral outcomes covering more than three quarters of the counties where sheriffs are elected. Second, we are contributing to a recent growing literature on causal effects of the 1033 Program on different outcomes.[2] Earlier work by Bove and Gavrilova (2017) and Harris et al. (2017) showed that equipment transfers have a negative effect on crime rates. Both works use an instrumental variable approach to tackle endogeneity concerns, and perform the analysis at the county level. Our paper adopts similar strategies. Gunderson et al.

[1]The latest census confirms that almost all sheriffs' offices performed traditional law enforcement functions (96%) and court related duties (98%), while a majority of sheriffs' offices (75%) were responsible for operating at least one jail (Reaves, 2011).

[2]There is also some quantitative literature not interested in causal effects of the program, as well as further qualitative literature on the 1033 Program, and more broadly police militarization. We refer only to a selected subset of closely related works that permit us to highlight our contribution more succinctly.

(2019) instead replicate the work of Bove and Gavrilova (2017) and Harris et al. (2017) using a later release of equipment data and show that at the county level, equipment transfers do not affect crime rates.[3] Masera (2021) performs a similar analysis at the local agency level, also further developing the instrumental variable approach, and shows that while transfers decrease crime rates at the local agency level, there is displacement of violent crime to neighboring areas. Same as Gunderson et al. (2019) and Masera (2021) we use the data released at the local agency level and aggregate it at the county level. While spillovers (Masera, 2021) and "ecological fallacy" concerns (Gunderson et al., 2019) may be present in our setting as well —i.e., neighborhoods across a given county may indeed be affected differently by equipment transfers and hence vote differently for the sheriff — the research question at hand requires us to work at the county level and alleviates such issues. That is, given that our outcome of interest is by definition at the county level (i.e., there is one elected sheriff per county), it is only natural to perform the analysis at the county level.

The rest of the paper is structured as follows. Institutional details on the 1033 Program are described in Section 2. In Section 3 we discuss the econometric methodology while our data and summary statistics are provided in Section 4. Section 5 presents our results and Section 6 concludes.

2 The 1033 Program

The "1033 Program" borrows its name from the 1033 section of the National Defense Authorization Act (NDAA) for fiscal year 1997. The act was approved by Congress and signed into law by President Bill Clinton in 1996. The 1033 section grants permanent authority to the Secretary of Defense to transfer defense material to federal, state and local law enforcement agencies (LEAs). Similar to its predecessors (most notably the "1028 Program" signed into law by President George H. W. Bush in 1990 upon which the 1033 expanded), the 1033 Program aims at allocating excess military equipment for law enforcement purposes, particularly in counter-drug, counter-terrorism, and border security missions.

The Program is currently administered by the Defense Logistics Agency (DLA), the Department of Defense's combat logistics support agency. Among its responsibilities, DLA discards obsolete or unneeded excess equipment returned to the U.S. by military units around the world, and is hence responsible for the administration of the 1033 Program through the Law Enforcement Support Office (LESO). For a state to participate in the program, the governor appoints a state coordinator. State coordinators are responsible to first approve and certify LEAs such as police or Sheriff's departments applying to participate in the program, and thereafter supervise their participation. Participating LEAs can review online the available excess inventory and make requests for transfers through the state coordinator providing a justification of need. LEAs do not need to pay for the allocated items but are responsible for covering all shipping and storage costs.

The 1033 Program is ongoing and is often gaining attention both in the press and the political agenda.

[3] This result is in line with evidence by Mummolo (2018) who uses the census of "special weapons and tactics" (SWAT) in Maryland and shows that there is no negative correlation between police militarization and crime.

After the uprising in Ferguson in 2014, and the widespread criticism of the Program possibly resulting in an excessive police militarization, President Obama requested a review of the program. As a result, in 2015 President Obama prohibited the transfer of certain items and introduced additional control measures for the transfer of "controlled" equipment (Executive Order 13688).[4] Among other measures, LEAs requests for "controlled" equipment had to go through a review and authorization process by local government, and LEAs were also required to certify their responsibility for training their personnel in the proper use, maintenance, and repair of the allocated equipment. In 2017, President Trump revoked Obama's executive order and directed all executive departments and agencies "to cease implementing [*Obama's*] recommendations and, if necessary, to take prompt action to rescind any rules, regulations, guidelines, or policies implementing them" (Executive Order 13809). As of June 2020, around 8,200 federal, state and local law enforcement agencies from 49 states and four U.S. territories are participating in the program. Since the program started, the total original acquisition value of property transferred to LEAs is \$7.4 billion (Defense Logistics Agency, 2020).[5]

3 Econometric methodology

Our objective is to uncover the causal effect, if any, of the transfer of items under the 1033 Program on the probability that sheriffs are re-elected. To do so, we estimate the following linear probability model:

$$\textit{Re-election}_{ict} = \beta_1 \ln(equip_{ct-1}) + \beta_2 X_{ict} + \alpha_s + \nu_{decade} + \epsilon_{ict} \quad (1)$$

where our dependent variable, $\textit{Re-election}_{ict}$, is equal to 1 if sheriff i from county c (in state s) has been re-elected in year t and 0 otherwise. Our main regressor of interest is given by the log of $equip_{ct-1}$, as a measure of the transfer of items that a county has received. Depending on its formulation, it can be expressed in terms of quantities or values and over different time horizons (with $\ln(equip_{ct-1})$ denoting the (quantity or value) of equipment received from the inception of the 1033 Program until year $t-1$). Matrix X_{ict} includes a series of controls that may vary at the sheriff (e.g., political affiliation) or county (e.g., population) level, as described in Section 4. State (α_s) and decade (ν_{decade}) fixed effects are included to control for the substantial heterogeneity across states and for changes over time.[6] In our preferred specifications we do not include county fixed effects because a county appears, on average, only 2 times (and 5 times at most) in our estimations and for some of them we never observe a sheriff that does not win re-election (conditional on being a candidate). Still, we will consider county fixed effects in

[4]Equipment on the prohibited list included tracked armored vehicles; weaponized aircrafts, vessels and vehicles; .50-caliber firearms and ammunition; bayonets; camouflage uniforms and grenade launchers. Equipment on the controlled equipment list included manned aircrafts; unmanned aerial vehicles; armored and tactical wheeled vehicles; command and control vehicles; under .50-caliber firearms and ammunition; explosives and pyrotechnics; breaching apparatus; riot batons, helmets, and shields.

[5]These figures and some of the information in this section come directly from the DLA. For more information on the program, particularly for considerations unrelated to our analysis, the reader is referred to the DLA website.

[6]In some robustness checks, we will also use year fixed effects but it is the case that in some years we have few observations where the dependent variable is equal to 0, making the identification of such year fixed effects difficult. Given the many fixed effects included in our specifications, we use a linear probability model throughout the analysis. Still, the qualitative results would be unchanged when using a probit estimator.

a robustness check. Finally, ϵ_{ict} denotes the error term, with standard errors clustered at the state level to allow for arbitrary correlation among elections within a state over time.

The econometric challenge in estimating the specification in (1) is the possibility of endogeneity of our regressor of interest to the outcome variable, in that the likelihood of a county receiving a transfer of equipment is correlated with the attributes of a sheriff that also drive the probability of re-election. Bove and Gavrilova (2017) and Harris et al. (2017) face the same concern in their studies and we follow their strategy of applying an instrumental variable approach to deal with this issue. In particular, we employ the four instruments used by Harris et al. (2017). Two of them exploit the variation in the geographical location of counties and how they relate to the site of the facilities from which the equipment must be transported. The other two instruments are the size of a county and whether it is designated as a High Intensity Drug Trafficking Area (HIDTA), as large and HIDTA counties are encouraged to apply for items. These variables are obviously time invariant but are interacted with the stock of overall (i.e., US) transfer of equipment to generate time-variant and county-specific instruments. As a result, our first stage is defined as follows:

$$\ln(equip_{ct-1}) = \delta_1 \left(\ln(equip_{USt-1}) \otimes [1, D_c^1, D_c^6, HIDTA_c, \ln(land_c)]\right) + \delta_2 X_{ict} + \alpha_s + \nu_{decade} + \epsilon_{ict}. \quad (2)$$

In particular, $\ln(equip_{USt-1})$ is the equivalent of the (endogenous) regressor of choice but at the US level. D_c^1 and D_c^6 are the inverse of the distance from the centroid of county c to the closet and 6^{th} closest facility where items may be located, respectively. $HIDTA_c$ is a dummy variable indicating whether a county is designed as HIDTA and $\ln(land_c)$ is the log of the area of a county.

Bove and Gavrilova (2017) rely on a different source of variation to construct an instrument for the same regressor, based on the observation that the transfer of equipment is due to the surplus of items at the Department of Defense. Thus, they interact US military spending (time varying at the US level) with a county-specific measure of the probability of receiving any transfer. This probability is calculated as the share of years during the sample in which a county has received any item. As such, we are concerned that it is also potentially endogenous if the observed frequency of such transfers is correlated with some traits of sheriffs. This is more so in our setting than not for Bove and Gavrilova (2017), as we do not include county fixed effects in our preferred specifications that would alleviate the "mechanical positive correlation [of the instrument] to the dependent variable in the first stage" (Bove and Gavrilova, 2017, page 8). Because of this concern, we do not include such an instrument in our main specifications but we will consider it in a robustness check.

4 Data and summary statistics

In order to verify whether the transfer of equipment through the 1033 Program affects the re-election probability of sheriffs, two main sets of data are needed: data on the elections of sheriffs and detailed information about the equipment received by counties over time. Both building blocks involve substantial challenges, in that some data are not readily available or are not presented at the geographical level needed for the analysis. In the following, we describe the steps taken to construct the dataset and present some descriptive statistics, starting with the electoral and equipment data.

4.1 Electoral data and 1033 Program data

We constructed an original dataset of 6,218 sheriff elections in 2,381 counties between 2006 and 2016, making it the largest dataset of sheriffs elections (to the best of our knowledge) used in the literature.[7] In most states where sheriffs are elected, they serve a 4-year mandate with the election taking place at the same time as the the elections for the US Congress (in the year of Presidential elections or with mid-term elections).[8] While eleven states run these elections centrally, county-level electoral boards are responsible for the remaining states. This explains why data for such elections are not systematically made available at the state level and the need to collect data at the county level. Even at the county level, the release of information varies dramatically. While few counties make (some) data available on the county's electoral board website (often those of the last election), we most often had to directly contact county clerks via email to obtain them.

Although a long process, many county clerks did respond to our queries. The 2,381 counties for which we have electoral data represent around 75% of counties for states electing sheriffs and 87% of the population in such counties (in 2010). Graphically, Figure 1 illustrates the geographical availability of our electoral data, with most counties missing from certain states of the South and the Midwest (same as in Thompson 2020).

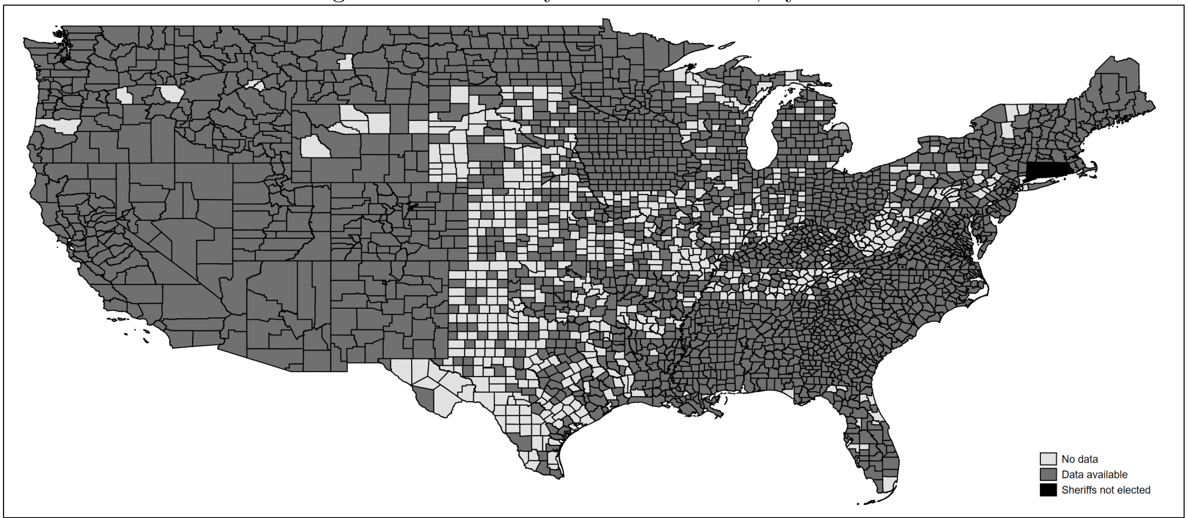
Although our electoral data covers the period 2006-2016, we do not have data on all elections within this period for some counties. The starting year has been chosen because of the increase, documented below, in the transfer of items that has occurred in the late 2000s. The final year is chosen because of the availability of the 1033 Program data from the Department of Defense (as discussed later). For each election, we know the names of the candidates, their political affiliation, and the votes they received in the election. As shown in Panel A of Table 1, we know who won for 6,218 elections although we can calculate the winner's percentage of the votes in slightly fewer cases. Winners achieve very good electoral outcomes with an average of more than 80% of the votes. Party affiliation, which is known for around 85% of the observations, reveals that most sheriffs are either Republican or Democrat (i.e., around 40%

[7] In so doing, we complement the data used in Thompson (2020) and Zoorob (2019). Closer to us, Thompson (2020) collected data on 3,500 elections in 1,395 counties between 2003 and 2016. The research question in Thompson (2020) permitted the author to focus only on states holding partisan sheriff elections (and also collected data from counties with a population larger than 100,000 when data was not available at the state level). Instead, we did not impose any such criteria, and our database includes an additional 1,000 counties. Zoorob (2019) instead constructed a dataset of 5,604 elections in 1,303 counties. Although he collected data on a similar number of counties as Thompson (2020), his research question required earlier data, resulting in an unbalanced panel that sometime contains observations dating back to 1958.

[8] All details on sheriff elections are obtained from the National Sheriff's Association. Alaska, Connecticut, Hawaii, and Rhode Island do not hold county sheriff elections. In Alaska, law enforcement outside cities is handled by the Alaska State Troopers, Connecticut replaced elected county sheriffs with State Marshals in 2000, Hawaii has no county sheriffs, and Rhode Island maintains a statewide sheriff division but its members are not elected. Sheriffs are also not elected in a small number of counties outside these states (e.g., the sheriffs and undersherrifs of NYC). Sheriffs are elected for 2 years in Arkansas and New Hampshire, for 6 years in Massachusetts, and for 3 years in New Jersey (with terms staggered across counties so that every year there are some elections).

and 31%, respectively). Notice that in 6 states (i.e., California, Louisiana, Minnesota, North Dakota, Oregon, Tennessee) sheriffs are non-partisan but our electoral data still report a clear party affiliation (i.e., Democrat or Republican) for 18% of their observations (with the remaining sheriffs being classified as Independent or of unknown party).

Figure 1: Availability of electoral data, by counties



Notes: Data availability based on 6,218 observations; Alaska and Hawaii (not shown in the map) do not elect sheriffs.

When it comes to the econometric analysis, our dependent variable is a dummy variable, $Re\text{-}election_{ict}$, equal to 1 if sheriff i from county c has been re-elected in year t and 0 otherwise. With the high shares of votes already mentioned above, it is no surprise that most sheriffs are re-elected, when they decide to run for another term. The summary statistics in Panel C of Table 1, which are based on the sample used in the econometric analysis, show that they are re-elected in almost 94% of the cases. It is important to notice that the number of observations for our dependent variable is much smaller than the data discussed earlier. In fact, in order to define whether a sheriff has been re-elected or not, we need to observe two consecutive elections (e.g., our dependent variable cannot be defined for the year 2006) and it has to be the case that the name of the incumbent is among the losers in the following election.[9] Notice that few states impose term limits on sheriffs, mostly in the form of at most two consecutive terms.[10] Excluding sheriffs facing such term limits would raise the probability of re-election only marginally to 0.940 and results in the loss of 91 observations.

As for the other building block of our dataset, we use the data on the 1033 Program released by the Department of Defense (in 2014, and updated in 2015) for the period 1991-2015.[11] The data include all transfers of items with information about the goods being transferred (defined by the 13-digit code

[9]It may be the case that a sheriff does not run for re-election because of the expectation of losing. However, it would be unreasonable to assume that this happens in most of the situations where a sheriff is not re-elected and is not among the losers.

[10]Indiana, New Mexico, and West Virginia have two-term limits. Most counties in Colorado do not have term limits, except for 12 counties with two-term limits, 5 counties with three-term limits, and one county with a four-term limit.

[11]This data is publicly available through the GitHub page of the Washington Post; see `https://github.com/washingtonpost/data-1033-program`. The data cover until September 2015 (included).

Table 1: Summary statistics

	Observations	Mean	St. dev.	Min	Max
Panel A: Electoral data					
Winner identity	6,218				
Year	6,218	2011.2	3.305	2006	2016
Winner percentage	6,117	0.807	0.196	0.209	1
Winner Democrat	6,218	0.312	0.463	0	1
Winner Independent	6,218	0.131	0.338	0	1
Winner Libertarian	6,218	0.0002	0.013	0	1
Winner Republican	6,218	0.409	0.492	0	1
Winner unknown party	6,218	0.147	0.355	0	1
Panel B: 1033 Program data					
Quantity gears	78,575	6.723	323.831	0	62,868
Quantity vehicles	78,575	0.381	9.525	0	1,528
Quantity weapons	78,575	4.191	206.686	0	53,286
Value gears	78,575	$4,928	$277,408	$0	$72,376,400
Value vehicles	78,575	$14,123	$148,894	$0	$20,033,534
Value weapons	78,575	$878	$16,173	$0	$2,424,497
Panel C: Regression sample					
$Re\text{-}election_{ict}$	3,403	0.939	0.239	0	1
$Democrat_{ic}$	3,403	0.323	0.468	0	1
$Republican_{ic}$	3,403	0.411	0.492	0	1
Quantities:					
$\ln(gears_{ct-1})$	3,403	0.677	1.562	0	11.776
$\ln(vehicles_{ct-1})$	3,403	0.470	0.886	0	7.155
$\ln(weapons_{ct-1})$	3,403	2.105	1.715	0	11.010
Values:					
$\ln(gears_{ct-1})$	3,403	2.240	4.211	0	18.199
$\ln(vehicles_{ct-1})$	3,403	3.886	5.756	0	17.098
$\ln(weapons_{ct-1})$	3,403	6.036	4.026	0	14.846

Notes: The 78,575 observations for the 1033 Program data are for 3,143 counties over 25 years.

of the National Stock Number classification and a brief description) and relative quantity and total cost. As argued by Gunderson et al. (2019), these data are less likely to include incorrect or misleading observations than previously released data already aggregated at the county level. In fact, they provide disaggregate data about each transfer of item to each agency (e.g., it appears that this source records transfers to agencies located in counties for which there is no observation in the aggregate version of the dataset). However, the challenge in using these data is that they report the state and the name of the enforcement agency that received material (in a given year) but not the exact address of that agency, which is needed to assign the transfer of equipment to a specific county. After dropping LEAs in Guam, Puerto Rico and the Virgin Islands, the data list 191,459 transfers (i.e., observations) of material, distributed over 7,738 LEAs. In order to link an agency with a county, we make use of the Census of State and Local Law Enforcement Agencies (United States Department of Justice, 2008), which provides information on the addresses of LEAs. After dropping 51 LEAs whose address spanned more than one county, we were able to exactly match the name of recipients of equipment with an address for about half of the observations (i.e., 99,243 for a total of 4,752 LEAs). For the remaining observations, we first considered whether the name of the agency contains any reference to a county (i.e., mentioning 'county' or any abbreviation of 'county'). For these agencies, we assumed that they are located within the county mentioned in the title; for example, we assigned "Calhoun Co Sheriff Department" of Alabama to the Calhoun county in the state. In this way, we matched approximately a further quarter of the total observations (i.e., 50,336, or 1,772 agencies). For the remaining LEAs, we used a process of fuzzy matching. Working by state, the algorithm compared the names of LEA in the 1033 dataset with the name of each LEA in the addresses data, and for each of them it calculated a similarity score. We then manually examined the higher similarity scores to see if there were obvious matches. For example, "Barre City Police Department" (in Vermont) in the 1033 dataset was matched with "Barre Police Department" in the addresses dataset. For the agencies with no obvious match, we checked to see if their name gave any hint about their address. If it did, we assigned the corresponding county; for example, "Fromberg Police Department" in Montana had no obvious match in the address dataset but Fromberg is in Carbon County and we were able to assign this county. After this multi-step process, we were left with 85 agencies (for a total of 7,142 transfer observations) that could not be matched to a county (e.g., agencies spanning multiple counties) and such observations, representing 1.1% (3.7%) of LEAs (transfer observations) of the original dataset, had to be dropped from the subsequent analysis.

With all transfers assigned to a county, we aggregated these transfers at the county-year level distinguishing quantities and unit values (i.e., total cost divided by quantity). We make use of the classification used in Bove and Gavrilova (2017) to further distinguish between gears, vehicles, and weapons. It is important to keep in mind that such groupings contain quite different items; for example, weapons include almost $67,000$ rifles (5.66 millimeter) valued at $430 each and 4 robots for explosive ordnance disposal with a unit value of $135,000. Thus, our measures of transfers of equipment in terms of quantities and values complement each other, in that values can be summed across products (but few items may drive the variation) while quantities may implicitly weigh for the value of items although summing up wildly different goods. Keeping this caveat in mind, Panel B in Table 1 provides summary statistics for the quantities and values of these groups of goods, for all counties over the period 1991-2015.[12] One striking feature of these data is the much higher standard deviation of each of these variables compared to their mean. This is due to the many zeros (i.e., at least 90% of the observations of each variable) in the data, which reflects the fact that few disbursements took place in the initial years of the 1033 Program and the fact that some counties never received anything (e.g., only 2,537 counties ever received any transfer). In terms of variation over time, most transfers took place in the most recent years with a substantial increase in the second half of the 2000s (see Table A1 in the Appendix for a detailed breakdown). In fact, less than 1%, 2% and 10% of the total quantities of gears, vehicles, and weapons have been transferred by 2006 (i.e., the first year for which we collected electoral data). The comparison between quantities

[12]Comparing our data with Table 6 in Gunderson et al. (2019) shows that we match them well. They report the average value and quantity of all transfers to be $28,990.3 and 13.6, respectively. The equivalent figures in our dataset are $27,454.5 and 8.55, with the larger difference in quantity over value possibly due to a larger number of low-value items recorded by Gunderson et al. (2019). Compared to our data construction, one important difference is that they assign LEAs to counties using Google Maps API instead of the matching we described above.

and values confirms that these groups of items are significantly different in terms of unit values. Focusing on the observations with positive transfers, the average value of weapons is only \$10,187 compared with an average of \$128,668 and \$288,277 for gears and vehicles, respectively. Panel C of Table 1 shows the relevant statistics on equipments for the econometric analysis, where we use the log of (one plus) the transfer of the various categories (accumulated from 1991 till year $t-1$).

4.2 Other data

Starting with the data needed to construct the instruments, we take D_c^1, D_c^6, and $HIDTA_c$ from the replication files made available from Harris et al. (2017) while $land_c$ and population ($population_{ct}$) come from the US Census. For the instrument employed by Bove and Gavrilova (2017), we take nominal US military spending from the World Development Indicators (and use the gross national expenditure deflator, always from the World Development Indicators, to obtain real values). Data on county unemployment was taken from the US Bureau of Labor Statistics and county demographic data as well as data on county poverty and income were taken from the US Census Bureau. As for the data used in various robustness checks, they are described when they are used in such exercises.

5 Results

We begin by estimating a parsimonious version of our model without any control variable, to obtain a first pass on the performance of the instruments and the results. Table 2 reports the results for six versions of the dependent variable, distinguishing whether we look at the quantity or value of the items received. Even a cursory look at the results indicate that the re-election prospects of sheriffs are positively enhanced the more equipment their county has received over the years (since 1991 and until year $t-1$). Although the magnitudes are different (especially, as expected, between quantities and values) and we will evaluate their economic significance later, the effects are all statistically significant (at least at 5% level and most times at 1% level). This stands in contrast to OLS results (not reported to save on space but available upon request) in which the estimated coefficients are always positive but much smaller in size and significant only in two of the six specifications (for quantity and value of weapons), denoting a negative bias in the OLS estimates. Table 2 also shows the first-stage estimates with the tests at the bottom of the table showing that the instruments are not weak (i.e., the Kleibergen-Paap F-stat is always above the 5% critical value in all but the specifications for gears where it is above the critical value for 10% maximal IV size, and always higher than the value of 10 that is usually used as a rule of thumb). Having more instruments than endogenous regressors allows us to test for overidentifying restrictions, and the Hansen J Stat shows that we never reject the null hypothesis that the instruments are valid and are correctly excluded from the second stage.[13] Differently from Harris et al. (2017) who

[13]In the absence of county fixed effects, we could include D_c^1, D_c^6, $HIDTA_c$, and $\ln(land_c)$ in levels too. If we were to do so, most instruments would be insignificant in the first stage while they would still pass the Kleibergen-Paap and Hansen J statistics, indicating the presence of substantial multicollinearity. The qualitative results on our regressors of interest

use the same set of instruments, the inverse of the distance from the 6^{th} closest federal facility is never significant. Excluding it from the regressions does not change the point estimates of the second-stage coefficients but improves the Kleibergen-Paap statistics, which pass the 5% critical value also for gears. Given this consideration, we will not include D_c^6 in the specifications that follow.[14]

Table 2: Benchmark results

	Quantity			Value		
	Gears	Vehicles	Weapons	Gears	Vehicles	Weapons
Second stage						
$\ln(equip_{ct-1})$	0.021***	0.039***	0.019**	0.010***	0.007***	0.011**
	(0.008)	(0.012)	(0.008)	(0.003)	(0.002)	(0.005)
First stage						
$\ln(equip_{USt-1})$	0.221***	0.165***	0.281***	0.644***	1.794***	0.881***
	(0.078)	(0.046)	(0.072)	(0.160)	(0.320)	(0.190)
$\ln(equip_{USt-1}) \times D_c^1$	0.379	0.029	0.638***	0.404	0.060	0.848***
	(0.252)	(0.077)	(0.130)	(0.343)	(0.369)	(0.182)
$\ln(equip_{USt-1}) \times D_c^6$	8.717	-1.934	2.466	12.989	-3.785	18.031
	(19.378)	(15.104)	(25.251)	(21.823)	(39.615)	(43.083)
$\ln(equip_{USt-1}) \times \ln(land_c)$	0.009	0.013***	0.016**	0.019**	0.036***	0.025***
	(0.007)	(0.004)	(0.006)	(0.009)	(0.009)	(0.008)
$\ln(equip_{USt-1}) \times HIDTA_c$	0.094***	0.063***	0.115***	0.138***	0.170***	0.132***
	(0.012)	(0.009)	(0.013)	(0.017)	(0.021)	(0.019)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Decade fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,403	3,403	3,403	3,403	3,403	3,403
Kleibergen-Paap F-stat	17.3	26.0	25.7	16.0	41.6	19.3
Hansen J Stat p-value	0.91	0.86	0.54	0.92	0.84	0.54

Notes: Standard errors in parenthesis clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

In Table 3 we add socio-economic and political controls. It is not immediately obvious which variables we should include for the former group, as sheriffs are not responsible for any specific economic policy. Thus, the controls we include at this stage are mostly driven by their statistical relevance, either as a determinant of re-election or as an explanatory variable in the first stage along with the instruments.[15] Based on this consideration, we include median household income, population, and the share of black population in the county. As for the political controls, we include the political affiliations of the sheriffs with a focus on Democrats and Republicans, with the 13% of Independents and around 15% of unaffiliated sheriffs (and 1 Libertarian sheriff) constituing the omitted category. We also include a dummy variable for Republican governors (and notice that in our sample we only have Democrat and Republican governors), as the 1033 Program's state coordinator responsible for the program's oversight in a given state is appointed by the governor.

would be unchanged (except for the insignificance for the quantity of gears, with a *p-value* of 0.13).

[14] If we were to maintain it as an instrument, the results in the next tables would be qualitatively unchanged but we would observe a slight worsening in the performance of the instruments.

[15] Estimates from the first first stage are not reported from this table onward to save on space but are available upon request.

The first observation is that the estimated coefficients of our key regressors and their significance is broadly unchanged compared to our previous parsimonious specifications, indicating that the instruments are performing well for the possibility of omitted variables. The addition of these controls reduces the values of the Kleibergen-Paap F-stat, with all passing at least at the 10% critical value (and two at the 5% level, and all above the 10).[16] The *p-values* of Hansen J Stat continue to remain far from any conventional level of significance. As for the role of these controls, it seems that sheriffs have a higher likelihood of re-election the higher the median household income (but not when it comes to weapons) and the share of black population in their county. This last result is consistent with the evidence presented by Mummolo (2018) who shows that militarized police units are more often deployed in communities with a large share of black citizens. This interpretation reinforces the idea, already captured by our key regressors, that voters reward sheriffs for the higher accumulation of military equipment. Instead, the overall size of their constituency seems to play only a negative and marginally significant role. In terms of political controls, Democrat and Republican sheriffs are more likely to be re-elected (compared to Independent and unaffiliated sheriffs) with point estimates higher for Democrats (but not statistically different from their Republican counterparts). Instead, the political affiliation of the Governor does not matter, not even in the first stage in explaining the transfer of equipment.[17]

Table 3: Adding controls

	Quantity			Value		
	Gears	Vehicles	Weapons	Gears	Vehicles	Weapons
$\ln(equip_{ct-1})$	0.019**	0.037***	0.021*	0.009**	0.008***	0.012*
	(0.009)	(0.013)	(0.012)	(0.004)	(0.003)	(0.007)
$Median\ household\ income_{ct}$	0.001**	0.001**	0.001	0.001**	0.001**	0.001*
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)
$Population_{ct}$	-0.033	-0.035*	-0.043*	-0.038*	-0.039*	-0.040*
	(0.020)	(0.019)	(0.024)	(0.022)	(0.022)	(0.023)
$Share\ black\ population_{ct}$	0.053**	0.065***	0.058**	0.051*	0.071***	0.067***
	(0.025)	(0.023)	(0.024)	(0.026)	(0.023)	(0.023)
$Democrat_{ic}$	0.065***	0.065***	0.061***	0.065***	0.061***	0.059***
	(0.015)	(0.015)	(0.015)	(0.015)	(0.016)	(0.015)
$Republican_{ic}$	0.046***	0.043**	0.040**	0.044**	0.040**	0.039**
	(0.016)	(0.017)	(0.017)	(0.016)	(0.017)	(0.017)
$Republican\ Governor_{st}$	-0.014	-0.014	-0.018	-0.011	-0.011	-0.018*
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.010)
State and decade FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,403	3,403	3,403	3,403	3,403	3,403
Kleibergen-Paap F-stat	12.9	15.9	28.1	10.9	20.6	17.1
Hansen J Stat p-value	0.91	0.91	0.76	0.93	0.96	0.68

Notes: Standard errors in parenthesis clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

[16]If we were to add the socio-economic and political controls separately, the point estimates of our regressors of interest would not change but their significance and the the Kleibergen-Paap F-stat would all increase.

[17]The results for our key regressors would be unchanged if we were to drop the 6 states where sheriffs are non-partisan (at a cost of about 450 observations).

As shown by the last table, the addition of several controls does not alter the qualitative results on the positive causal effect of the transfer of military equipment on the re-election of sheriffs. Having established this statistical result, it is important to understand how economically significant it is in affecting the re-election prospects of incumbent sheriffs. To answer this question, we engage in a quantification exercise where we consider how different the prospect of re-election could have been for sheriffs whose counties did not receive transfers.

Table 4 shows that the probability of re-election would increase between 3.6 or 9.9 percentage points, depending on the version of the our key variable, if a county that did not receive any transfer in a given year would have obtained the median quantity or value. In particular, we calculate the median value of transfers for every year for those counties with positive transfers. We then use this figure and the estimated coefficient from Table 3 to see how much the probability of re-election would increase if such counties had received a transfer of such (median) magnitude. The second row in Table 4 shows what these median values are in 2016 across the various categories, and these statistics are a clear reminder of the substantial variation (in quantity and values) across the equipment subject to transfers. For example, if a county had been successful over the years to have accumulated 66 units of gear, the likelihood of a sheriff's re-election in that county would have been 3.8 percentage points higher. This effect (and similarly for other specifications) is not a small change, in particular when it is considered in terms of values and the (large) amount of money spent (even) in local races.[18] Furthermore, while it is the case that sheriffs are elected with large margins (i.e., 0.81 on average, as shown in Table 1), those who lose do so by small margins (i.e., 0.14 on average with a median value of only 0.10) and hence the presence of transfers in some counties may have altered the winner of the election.

Table 4: Quantification exercise

	Quantity			Value		
	Gears	Vehicles	Weapons	Gears	Vehicles	Weapons
Change in prob. of re-election	0.038**	0.036***	0.058*	0.086**	0.093***	0.099*
	(0.017)	(0.013)	(0.013)	(0.035)	(0.031)	(0.055)
Median equipment (in 2016)	66	5	24	$41,489	$412,845	$6,955

Notes: Calculations based on the estimates in Table 3 for county-year observations with no transfers; standard errors (calculated using the delta method) in parenthesis clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

In conclusion, the results on the role of equipment on re-election outcome is statistically and economically significant. In the next section we verify the robustness of these estimates through an extensive battery of sensitivity checks.

[18]Due to data limitations on local campaign finances, this statement is a conjecture based on limited data available by VR systems for Florida (https://www.vrsystems.com/). For example, in the 2020 election in a large county such as Orange County, the incumbent sheriff (John Mina, DEM) incurred expenditures equal to $339,619. But also in smaller counties campaign expenditures are not negligible. For instance, in the 2020 election in Citrus County in Florida the new elected sheriff (Michael Prendergast, REP) run a campaign of $155,771.

5.1 Robustness checks

We begin assessing the robustness of our results by considering alternative methodologies to identify the role of transfers on electoral performance. Table 5 reports the results of re-estimating the specifications presented in Table 3 by, in turn, adding the instrument by Bove and Gavrilova (2017), using county fixed effects, and year fixed effects. Although we include the controls discussed above, which generally perform as in Table 3, we do not report their estimates to save on space.

Table 5: Robustness checks on methodology

	Quantity			Value		
	Gears	Vehicles	Weapons	Gears	Vehicles	Weapons
Panel A: adding instrument from Bove and Gavrilova (2017)						
$\ln(equip_{ct-1})$	0.015**	0.025**	0.008*	0.006**	0.004**	0.004*
	(0.007)	(0.010)	(0.005)	(0.003)	(0.002)	(0.002)
State and decade FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,403	3,403	3,403	3,403	3,403	3,403
Kleibergen-Paap F-stat	36.8	44.3	276.6	48.1	155.5	66.2
Hansen J Stat p-value	0.81	0.59	0.67	0.71	0.55	0.50
Panel B: county fixed effects						
$\ln(equip_{ct-1})$	0.019	0.057**	0.051**	0.010*	0.013**	0.035***
	(0.011)	(0.025)	(0.022)	(0.005)	(0.005)	(0.012)
County and decade FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,577	2,577	2,577	2,577	2,577	2,577
Kleibergen-Paap F-stat	16.3	14.6	25.2	25.4	21.0	11.1
Hansen J Stat p-value	0.04	0.07	0.14	0.05	0.11	0.24
Panel C: year fixed effects						
$\ln(equip_{ct-1})$	0.023	0.061*	0.019	0.012	0.009*	0.010
	(0.019)	(0.032)	(0.013)	(0.008)	(0.005)	(0.007)
State and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,403	3,403	3,403	3,403	3,403	3,403
Kleibergen-Paap F-stat	11.3	10.5	14.6	8.26	15.4	11.0
Hansen J Stat p-value	0.55	0.72	0.63	0.66	0.79	0.64
Panel D: year fixed effects without controls						
$\ln(equip_{ct-1})$	0.019	0.041**	0.015*	0.009*	0.007**	0.009*
	(0.013)	(0.020)	(0.008)	(0.005)	(0.003)	(0.005)
State and year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,403	3,403	3,403	3,403	3,403	3,403
Kleibergen-Paap F-stat	27.9	24.8	26.1	22.4	29.7	18.9
Hansen J Stat p-value	0.80	0.89	0.83	0.88	0.92	0.81

Notes: Socio-economic and political controls as in Table 3 included in all but Panel D; standard errors in parenthesis clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

Panel A of Table 5 shows that the addition of the instrument based on total US military spending and the probability of a county receiving some transfers does work well, in that the qualitative results do not change while the enlarged set of instruments comfortably passes the usual tests.[19] However, this instrument, which we fear may suffer from endogeneity of its own in our context, would perform less well

[19] Consistent with the other instruments, we also include the level of military spending as an instrument but not the county-specific and time invariant probability of a county receiving transfers.

if used on its own. In this case, the estimated coefficients for our regressors of interest would be significant in only three specifications, although always positive and not far away from conventional significance levels (i.e., *p-values* between 0.11 and 0.17) in the other specifications. In Panel B we substitute the state fixed effects with county fixed effects. Ideally, this would be the preferred specification as it identifies the coefficients of interest based on the time variation within a county, controlling for any time-invariant characteristics (e.g., rural versus urban counties). Considering that the same county is included in the sample on average only 2 times and that most sheriffs are re-elected, this amounts to a very demanding specification as the lack of variation in the dependent variable results in a loss of about 800 observations. Still the results in Panel B confirm our prior conclusions (with the exception of the quantity of gears), although the Hansen J Stat is sometimes significant in rejecting the hypothesis that the instruments are valid.[20] Finally, we consider year instead of decade fixed effects. The challenge of such strategy is that the sample includes few observations with the dependent variable equal to 0 in some years, which explains the choice of decade fixed effects in our main specifications. Nonetheless, Panel C shows the results with year fixed effects: the point estimates for all our key regressors are positive but only two coefficients reach a 10% significance level with sometimes low values of the Kleibergen-Paap F-stat (although always higher than 10). In order to alleviate the limited within-year variation, Panel D repeats the analysis dropping all the control variables. In this case, the instruments pass all the tests and the point estimates of all but one of the coefficients of the transfer of items are significant and of very similar magnitude as in Table 2.

Overall, the results of Table 5 show that the conclusions reached earlier are not sensitive to the chosen estimation strategy, especially in consideration of the demanding specifications when county or year fixed effects are included. We now move to consider alternative definitions of our key regressors and the definition of our sample.

In Panel A of Table 6 we restrict the focus to the transfers received by a county over the period $t-1$ and $t-5$ (i.e., 4 years that, by and large, correspond to the mandate of a sheriff) instead of over the whole 1033 Program. Interestingly, the results are preserved except for weapons, whose effects turn insignificant. Being the most common and cheapest items, this result suggests that they make a difference on the re-election probability only once a significant amount or value has been accumulated. In the last two panels we split the sample between small and large counties, with the cut-off for a population of $100,000$ (in 2015) —the choice of this population threshold is based on Thompson (2020). Panel B shows that our qualitative conclusions go through for small counties while Panel C demonstrates that they do not hold for large counties, where the coefficients of interest turn all negative (although far away from significant).[21] This result may hint at different levels of accountability (at least as far as equipment transfers are concerned) depending on the counties' size. Voters in small counties may be better able

[20] These results do not use the interaction with land as an instrument, as it would never be significant and its exclusion improves the Hansen J Stat without affecting the point estimate of any regressor.

[21] Admittedly, the Kleibergen-Paap F-stat mostly falls below relevant critical values when restricting the sample to only small counties. However, they would improve if we were to exclude HIDTA from the set of instruments while this change would not affect the point estimates in the second stage.

than voters in large counties to distinguish sheriffs' actions from those by other LEAs and hence hold sheriffs accountable. First, it is reasonable to assume that the sheriff's figure itself is more recognizable in small counties than in large counties. Second, one could further argue that the importance of the sheriff's office is negatively correlated with the county size. According to Reaves (2011) "while more than half of local police departments employed fewer than 10 full-time officers in 2008, less than a third (29%) of sheriffs' offices were this small". Hence, among the two-third of counties where the sheriff's office is the largest LEA (Zoorob, 2019), one could expect most of those counties to be relatively small.

Table 6: Robustness checks on regressor and sample

	Quantity			Value		
	Gears	Vehicles	Weapons	Gears	Vehicles	Weapons
Panel A: equipment received over past 4 years						
$\ln(equip_{ct-1/t-5})$	0.020**	0.040***	0.022	0.009**	0.009***	0.010
	(0.009)	(0.015)	(0.015)	(0.004)	(0.003)	(0.008)
State and decade FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,403	3,403	3,403	3,403	3,403	3,403
Kleibergen-Paap F-stat	10.9	11.9	11.7	10.7	15.3	12.2
Hansen J Stat p-value	0.90	0.87	0.64	0.88	0.82	0.44
Panel B: small counties						
$\ln(equip_{ct-1})$	0.039**	0.062***	0.052**	0.015**	0.010**	0.023**
	(0.015)	(0.023)	(0.020)	(0.006)	(0.004)	(0.010)
State and decade FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,596	2,596	2,596	2,596	2,596	2,596
Kleibergen-Paap F-stat	3.68	5.49	12.6	4.16	9.46	10.2
Hansen J Stat p-value	0.69	0.45	0.70	0.50	0.21	0.72
Panel C: large counties						
$\ln(equip_{ct-1})$	-0.002	-0.000	-0.016	-0.001	-0.001	-0.013
	(0.009)	(0.016)	(0.019)	(0.004)	(0.004)	(0.015)
State and decade FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	804	804	804	804	804	804
Kleibergen-Paap F-stat	15.1	23.3	22.9	22.5	28.9	18.2
Hansen J Stat p-value	0.76	0.74	0.71	0.72	0.70	0.69

Notes: Socio-economic and political controls as in Table 3 included in all panels; standard errors in parenthesis clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

In the next set of robustness checks, we augment our standard specification with other controls (added one at the time). Panel A of Table 7 includes per capita crime rate (defined to include arrests for murder, manslaughter, rape, robbery, aggravated assault, burglary, and vehicle theft) with crime rates at the county-level obtained from Kaplan (2019).[22] We consider crime as a natural control for our empirical setting. Not only past literature has elaborated on the link between crime and equipment transfers, but

[22]Two comments are in order: i) Kaplan (2019) provides a compilation of crime data released by the Uniform Crime Reporting (UCR) Program Data. That is, we also follow previous studies that use UCR data to control for crime (e.g., Bove and Gavrilova 2017; Harris et al. 2017; Masera 2021 among others). While we are aware of the potential issues when using *county-level UCR data* due to the way missing values are imputed (see e.g., Kaplan 2019), we do not see this as a concern in our analysis given that crime simply serves as an additional control and our results indicate that it does not affect our estimates. ii) Each of Bove and Gavrilova 2017; Harris et al. 2017; Masera 2021 use a slightly different definition of the crime variable. Our results are robust and quantitatively very similar using any of these definitions for our crime variable.

also crime rates may affect incumbents' electoral prospects as it is one of the key policies sheriffs focus during their electoral campaigns (Lublin, 2004, p.72). Instead, Panel B includes a dummy variable if a mass shooting in the previous year occurred in that county or in a border one.[23] This variable is used as a proxy capturing the salience of security and law enforcement in a given election year for each specific county. County-level data on mass shooting are from MotherJones (2020).[24] Finally, the vote share that a sheriff received in the previous election is included in Panel C of Table 7 (with few observations dropped because of lack of data). This variable captures the popularity of the incumbent. While these extra regressors all exert a positive and statistically significant effect on the probability of re-election, their inclusion does not qualitatively affect the estimates of our key regressors.

Table 7: Robustness checks with extra regressors

	Quantity			Value		
	Gears	Vehicles	Weapons	Gears	Vehicles	Weapons
Panel A: controlling for crime						
$\ln(equip_{ct-1})$	0.019**	0.037***	0.021*	0.009**	0.008***	0.012*
	(0.009)	(0.013)	(0.011)	(0.004)	(0.003)	(0.006)
$Crime\ rate_{ct}$	0.003***	0.003**	0.003***	0.003***	0.003***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
State and decade FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,403	3,403	3,403	3,403	3,403	3,403
Kleibergen-Paap F-stat	12.9	15.9	28.1	10.9	20.7	17.3
Hansen J Stat p-value	0.91	0.91	0.76	0.93	0.96	0.68
Panel B: controlling for shootings						
$\ln(equip_{ct-1})$	0.019**	0.037***	0.021*	0.009**	0.008***	0.011*
	(0.009)	(0.013)	(0.011)	(0.004)	(0.003)	(0.006)
$Shooting\ dummy_{ct-1}$	0.064***	0.064***	0.070***	0.066***	0.060***	0.073***
	(0.021)	(0.018)	(0.019)	(0.020)	(0.018)	(0.020)
State and decade FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,403	3,403	3,403	3,403	3,403	3,403
Kleibergen-Paap F-stat	12.7	15.6	28.3	10.9	20.4	17.4
Hansen J Stat p-value	0.90	0.90	0.74	0.92	0.95	0.66
Panel C: controlling for past vote share						
$\ln(equip_{ct-1})$	0.020**	0.039***	0.024*	0.010**	0.009***	0.014*
	(0.009)	(0.014)	(0.012)	(0.004)	(0.003)	(0.007)
$Past\ vote\ share_{ict}$	0.173***	0.173***	0.174***	0.175***	0.180***	0.175***
	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)
State and decade FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,325	3,325	3,325	3,325	3,325	3,325
Kleibergen-Paap F-stat	13.4	15.1	26.6	11.0	19.9	16.6
Hansen J Stat p-value	0.86	0.87	0.72	0.89	0.95	0.63

Notes: Socio-economic and political controls as in Table 3 included in all panels; past vote shares refer to the incumbent in the previous election; standard errors in parenthesis clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

We have also considered several other possible control variables. We do not report these results as

[23]Results would be virtually identical if we were to consider shootings in the previous 2 or 3 years. Instead, contemporaneous mass shootings do not appear to play a role.

[24]For other recent use of this data and more details on MotherJones as well as for alternative data sources on mass shootings, see Lagerborg et al. (2020).

these regressors are never significant and they do not affect our conclusions on the role of transfers (but they are available on request). In particular, there is no evidence of any effect for measures of educational attainment (i.e., percentage of adults with college degree or higher, or having completed at least some years at college) or the overall political leaning of a county (i.e., share of votes for Republican candidates in the last general election, which always include House representatives but may also refer to Senate and President races in some years). Considering the evidence (e.g., Autor et al. 2020) showing that exposure to trade shocks has had effects on various aspects of US elections (e.g., change of support for parties, increase in political contributions), we also include the county-level 10-year changes in import competition but we did not find any effect.[25]

Table 8: Robustness checks with different categories of transfers

	Quantity		Value	
	Non-lethal	Total	Non-lethal	Total
Panel A: estimates				
$\ln(equip_{ct-1})$	0.013	0.016**	0.008*	0.009**
	(0.011)	(0.008)	(0.004)	(0.004)
Panel B: quantification exercise				
Change in prob. of re-election	0.032	0.046**	0.086*	0.079**
	(0.028)	(0.022)	(0.046)	(0.033)
Median equipment (in 2016)	56	43	$70,384	$188,579
State and decade FE	Yes	Yes	Yes	Yes
Observations	3,403	3,403	3,403	3,403
Kleibergen-Paap F-stat	10.6	30.0	11.6	24.6
Hansen J Stat p-value	0.76	0.88	0.79	0.90

Notes: Socio-economic and political controls as in Table 3 included in all panels; quantification exercise for county-year observations with no transfers; standard errors (calculated using the delta method for quantification exercise) in parenthesis clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively.

The final robustness check in Table 8 makes use of two extra categories of transfers. One refers to all sort of items that are classified by Bove and Gavrilova (2017) in a residual category of non-lethal items (e.g., high-tech cameras, office supplies). Interestingly, these goods are not robust determinants of re-elections, even though Bove and Gavrilova (2017) find that they have the biggest marginal effect on crime reduction. This may be taken to suggest that the effect that equipment transfers have on sheriff re-election probabilities does not go through crime reduction. This is consistent with our findings in Panel A of Table 7, in which crime rate variable is significant but including it leaves our key regressors unchanged. Finally, it is possible to sum up all transfers into one group. Since each individual category (apart from non-lethal items) is significant, it is no surprise that these total quantities and values also display a significant effect on our outcome variable. In Panel B, we repeat the quantification exercise

[25] Data controlling for education levels are obtained from the Economic Research Service of the Department of Agriculture, data used to construct changes in import penetration are from the US County Business Patterns (to calculate county-level employment by industry) and the US International Trade Commission (for import data), data on elections were kindly provided by Mayda et al. (2021).

we used to produce Table 4, applying it on quantity and value of non-lethal and total categories. With the exception of non-lethal equipment quantity, the changes in probability of re-election are significant in all other columns, and are consistent with our previous findings. While the interpretation of our total value is intuitive (i.e., $188,579 transferred to a county without transfers increase the sheriff's reelection probability by 7.9 percentage points), the interpretation of these results deserves some caution when summing quantities of dramatically different types of equipment (e.g., tractors and rifles).

In conclusion, our benchmark results are robust to variation in the methodology to identify the role of military transfers, the specific choice of dependent variable and sample, and addition of more control variables. The different results on the size of the counties also provides an interesting angle to interpret our overall results.

6 Conclusion

The 1033 Program has received a lot of (academic and popular) attention in recent years to understand the consequences of the militarization of local communities. In this paper, we consider what may be a "side" effect of this program, in that it was not a stated objective that such transfers of equipment should lead to changes in the likelihood of re-election for local officials. Nonetheless, we provide robust causal evidence that sheriffs, who are elected at the county level, have their re-election probability increased the more military transfers their county has received over the years. This result is very robust and economically significant, as the electoral boost provided by the arsenal of accumulated military equipment may have determined the identify of the winner in quite a few of close-called elections.

The relevance of this result is twofold. First, it is interesting on its own: despite existing studies showing that militarization negatively affects citizens' perception of the police (Mummolo, 2018), voters in militarized counties reward sheriffs in the electoral booth. This conclusion is in line with sheriffs' enthusiastic support for Trump's executive order lifting restrictions from the Obama administration on the transfer of certain items probably more adequate for military missions than public safety purposes. This conclusion is further reinforced by the results found when we focus on non-lethal items, which are never found to be a statistically significant determinant of re-election.

Second, at a more general level this paper advances our limited existing knowledge on the accountability of local officers (Warshaw, 2019). Every two years American citizens have the opportunity to express their preferences to elect a plethora of local officers, such as sheriffs, school boards, county governments, judges, magistrates, coroners, among others. Nevertheless, the literature on local elections is scarce, possibly also due to the lack of a comprehensive national dataset on local politics (Warshaw, 2019). By focusing on sheriffs (i.e., one of the most important local offices), we are able to shed light on the electoral determinants of such elections, providing fresh evidence on voters' response to actions taken by locally elected officials. To be able to address the research question at hand, we have assembled the largest dataset on sheriffs elections for the period 2006-2016, and hope that it will prove useful for other studies on the relevance of local elections (i.e., dataset to be made publicly available upon publication).

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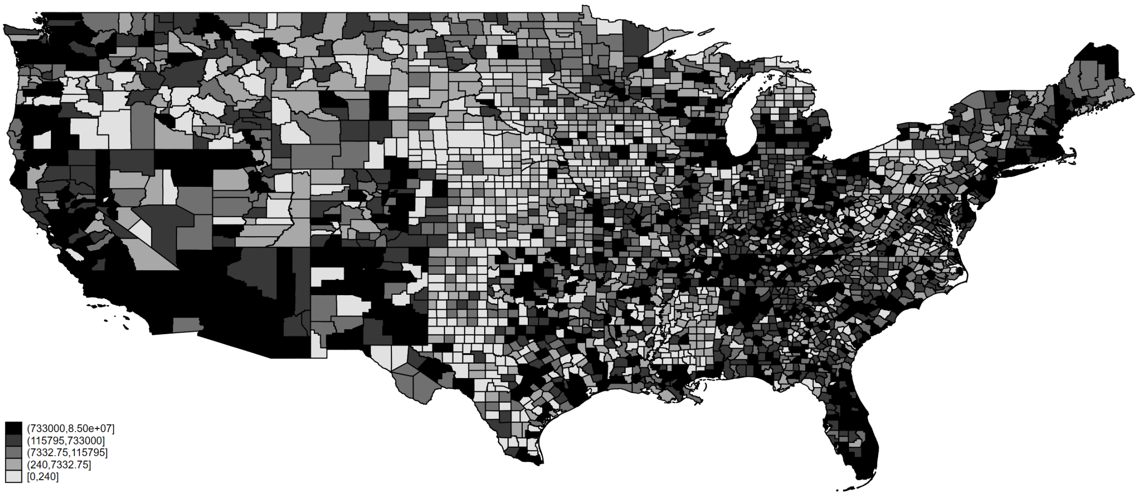
Appendix

Figure A1: Cumulative value of total equipment (by 2015)



Notes: Alaska and Hawaii not shown in the map because they do not elect sheriffs.

Table A1: Total quantities and values of items transferred

	Quantity			Values (in thousands)		
Year	Gears	Vehicles	Weapons	Gears	Vehicles	Weapons
1991	58	2	0	$238	$87	$0
1992	15	0	0	$56	$0	$0
1993	167	2	147	$668	$185	$20
1994	11	9	2	$25	$619	$0
1995	464	24	20	$1,142	$3,080	$53
1996	81	51	0	$428	$5,970	$0
1997	41	40	14	$175	$5,628	$1
1998	255	36	0	$1,087	$5,361	$0
1999	12	46	1	$39	$3,523	$0
2000	45	92	2	$180	$6,883	$0
2001	53	37	0	$205	$2,971	$0
2002	110	30	4	$326	$2,035	$2
2003	47	47	3	$233	$9,269	$150
2004	112	32	0	$703	$5,288	$0
2005	56	41	251	$185	$9,928	$83
2006	185	99	32,159	$387	$18,015	$12,200
2007	52	75	7,205	$209	$9,253	$2,619
2008	305	130	10,866	$1,332	$35,147	$3,637
2009	1,166	92	2,183	$995	$24,930	$470
2010	3,538	154	5,719	$3,377	$31,128	$1,005
2011	6,004	1,097	7,498	$4,398	$65,786	$2,681
2012	22,383	2,545	21,645	$19,725	$113,202	$10,086
2013	118,450	4,329	113,783	$57,375	$189,299	$12,368
2014	206,272	13,601	101,829	$240,601	$431,941	$16,053
2015	166,138	7,048	24,888	$126,483	$126,483	$7,339
Total	526,016	29,659	328,219	$384,941	$1,106,009	$68,768