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Article

Police militarization and local sheriff elections

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ABSTRACT

We investigate how transfers of military equipment in the United States through the 1033 Program impact the electoral performance of sheriffs that receive a significant share of equipment while directly accountable to voters. To address this question, we have compiled a unique dataset covering 7281 sheriff elections in 2714 counties between 2006 and 2016. Our findings indicate that an increase in military transfers to the sheriff's office, from no transfers to the 25th percentile, increases the probability of the incumbent being reelected by 5.8–12.5 percentage points. This is due to an increase in the number of votes cast for the incumbent while there is no effect on the total number of voters participating in the election. Our heterogeneity results demonstrate that voters tend to reward military equipment transfers, especially when local newspapers are present and in Republican-leaning small counties, providing novel insights into voter responsiveness in local elections (*JEL* D72, H56, H76, K42).

KEYWORDS: Accountability, Local Elections, Local Media, Militarization, Sheriffs, 1033 Program

> "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

The 1033 Program, authorized by Congress in 1997, facilitates the transfer of surplus military equipment from the Department of Defense to local law enforcement agencies (LEAs) responsible for enforcing federal, state, and local laws. In 2015, after the Ferguson uprising, the Obama administration implemented a ban on sheriffs and other LEAs accessing "lifesaving equipment" (from the quote above), with the list of banned items including tracked armored vehicles, weaponized aircraft, vessels, and vehicles, grenade launchers, camouflage uniforms used for urban settings, and bayonets. President Obama also directed increased scrutiny and oversight regarding the usage of certain other equipment categories, while his executive order left most of the program unaffected. In contrast, the executive order issued by President Trump in 2017 took a different approach by revoking previous restrictions on the 1033 Program, (also) responding to the demands of the National Sheriffs' Association.

Sheriffs have historically received a significant portion of transfers through the 1033 Program, approximately 50% of the total annual transfers in our data, to support their missions. However, these transfers are not uniform, resulting in varying levels of militarization among sheriffs across the country, as depicted in Figure 1. This raises the question: Do citizens reward or penalize sheriffs for such transfers? To investigate this, we compiled a unique dataset covering 7281 sheriff elections in 2714 counties between 2006 and 2016 and aimed to uncover the causal effect, if any, of items transferred to sheriffs under the 1033 Program on sheriffs' electoral performance. By doing so, we contribute to the literature on accountability within local offices (Warshaw 2019) and advance knowledge on sheriff elections (Thompson 2020; Zoorob 2022) and the 1033 Program (Bove and Gavrilova 2017; Harris et al. 2017; Gunderson et al. 2021; Masera 2021a, 2021b).

Our empirical analysis provides evidence that supports sheriffs' enthusiasm for the program: the transfer of equipment to county sheriffs increases their probability of being reelected. This conclusion is based on an instrumental variable strategy devised to address the possibility of endogeneity (e.g., correlation between attributes of sheriffs and probability of reelection, reverse causality, and omitted variables). The instruments we use are those commonly used in this literature and, in the same vein, identification comes from a pooled cross-section of sheriff elections over the period 2006–2016.

Our baseline estimates suggest that if a sheriff seeking reelection, who had not received any transfers, instead received equipment at the 25th percentile value or quantity allocated to sheriffs in our sample, their reelection probability would rise by 5.8–12.5 percentage points. This result is due to an increase in the vote share for incumbents who receive equipment, explained by an increase in the number of votes cast for the latter while not affecting the total number of votes cast for sheriffs. Such an increase in incumbents' vote share would be enough to turn the result of many elections lost by incumbents. In fact, often those incumbents who lost and did not receive any transfers did so by narrow margins (i.e., the median vote share of incumbents who lost reelection and did not receive any transfer is 0.43).

Our analysis further indicates that our main result is mainly driven by Republican-leaning counties, small counties, and those counties with the presence of a local newspaper. In small

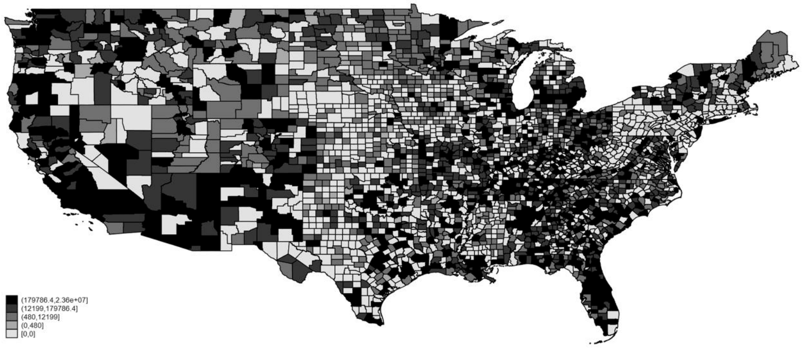

Figure 1. Cumulative value of total equipment transferred to sheriffs (by 2015)
Notes: Alaska and Hawaii not shown in the map because they do not elect sheriffs.

counties, the sheriff is not only a recognizable and important public figure, but also accountability may be more important than in larger counties where sheriffs' actions and responsibilities may be confounded by actions and transfers to other LEAs such as the police. The idea of accountability and recognition of sheriff actions is further supported by the fact that our main result is driven by counties with the presence of a local newspaper. As for party differences, transfers are rewarded more in counties with a Republican electoral base.

Our main result is also further supported by and robust to several modifications of our baseline specification. For instance, we provide alternative versions of our dependent variable focusing on the vote share for the incumbent instead of the reelection probability. We also provide alternative versions of our key regressors (e.g., varying types of transfers and time span) and assess the sensitivity of the results to alternative econometric strategies. In fact, in order to answer our research question with a causal interpretation of the results, we have to address the possible endogeneity of the transfers to the outcome of interest. This endogeneity may be due to omitted variables or reverse causality if sheriffs successful in their electoral bids alter their subsequent behavior in requesting items. This challenge is not new for papers, reviewed below, interested in examining the role of the 1033 Program on different outcomes. In common with this established literature, we use an instrumental variable approach relying on county-level but time-invariant characteristics interacted with a time-varying proxy of the overall availability of transfers. As discussed in detail in Section 2.1, we consider various instruments proposed in the literature and alternative identification strategies (based on different sources of variation in the data). The results from these different approaches consistently lead to the conclusion that military transfers affect the reelection of sheriffs, reinforcing each other in the causal interpretation of this effect.

By addressing this research question, our study makes valuable contributions to two recent areas of academic research. First, there is a lack of comprehensive evidence on elections for local offices in the United States, particularly with a focus on sheriffs' offices, where empirical research is limited (Warshaw 2019). While there have been some notable studies by Thompson (2020) on the compliance of Democratic and Republican sheriffs with federal requests regarding unauthorized immigrants, and by Zoorob (2022) on the incumbency advantage for sheriffs, our research extends this limited body of work. We not only investigate a determinant of sheriffs' electoral performance but also construct the largest dataset on sheriffs' electoral outcomes, covering 2714 counties where sheriffs are elected. This dataset provides comprehensive coverage of the 3085 sheriff offices nationwide, employing over

350 000 sworn or civilian employees and representing slightly over 30% of the local law enforcement workforce (Reaves 2011).

Second, our study contributes to the growing literature on the causal effects of the 1033 Program on various outcomes. Previous studies by Bove and Gavrilova (2017) and Harris et al. (2017) have shown that equipment transfers through the program have a negative effect on crime rates, and Bruce et al. (2019) demonstrated that equipment transfers do not significantly impact local public spending. These studies employed instrumental variable approaches to address endogeneity concerns and conducted their analyses at the county level, which aligns with the strategies adopted in our article. Gunderson et al. (2021) replicated the work of Bove and Gavrilova (2017) and Harris et al. (2017) using updated equipment data at the county level and found that equipment transfers do not affect crime rates.[1] Additionally, Masera (2021b) expanded the analysis to the local agency level, further refining the instrumental variable approach, and showed that while transfers decrease crime rates at the local agency level, there is a displacement of violent crime to neighboring areas. Furthermore, Masera (2021a) demonstrated that militarization increases police killings and reduces police safety.

Similarly to Gunderson et al. (2021) and Masera (2021a, 2021b), our study utilizes the data released at the local agency level and aggregates it at the sheriffs' office level. Although concerns about spillover effects (Masera, 2021a, 2021b) and the ecological fallacy (Gunderson et al. 2021) may also be relevant in our setting, where neighborhoods within a given county may be differently impacted by equipment transfers and, consequently, vote differently for the sheriff, the research question we address necessitates a county-level analysis and mitigates these concerns. Since our outcome of interest is inherently at the county level, with one elected sheriff per county, it is only natural to perform the analysis at this level. Instead, it is worth mentioning other limitations of our study. Although we believe that the different sets of fixed effects that we employ are already very demanding, the limited number of repeated observations for each county prevents us from using county fixed effects. Furthermore, the heterogeneity analysis that we present sheds interesting light on various aspects of the electoral process but is only indicative of which channels may lead to the conclusion that the transfer of equipment increases the probability of reelection. We leave it for further research to try to investigate these channels in more depth, as a way to explain the relatively large effect we find.

The rest of the article is structured as follows. We start with a short overview of the 1033 Program while Section 2 discusses the econometric methodology. Section 3 presents our data and summary statistics. Section 4 presents our results and Section 5 concludes.

1.1 The 1033 program

The 1033 Program derives its name from the 1033 section of the National Defense Authorization Act (NDAA) for fiscal year 1997. This act was approved by Congress and signed into law by President Bill Clinton in 1996. The 1033 section provides permanent authority to the Secretary of Defense to transfer defense material to federal, state, and local LEAs. Building upon previous programs such as the 1028 Program initiated by President George H. W. Bush in 1990, the 1033 Program's objective is to allocate surplus military equipment to support law enforcement efforts, particularly in areas such as counter-drug operations, counter-terrorism, and border security missions.

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

The administration of the 1033 Program falls under the responsibility of the Defense Logistics Agency (DLA), which is the combat logistics support agency of the Department of Defense. The DLA oversees the program through the Law Enforcement Support Office. As part of its role, the DLA manages the disposal of obsolete or unneeded excess equipment that is returned to the United States by military units worldwide.

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 has been a subject of attention in the media and political discussions, particularly following the events in Ferguson in 2014, which sparked criticism of excessive police militarization. In response to these concerns, President Obama initiated a review of the program, leading to certain restrictions and control measures being implemented in 2015 through Executive Order 13688.[2] However, in 2017, President Trump revoked President 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).

In June 2020, approximately 8200 federal, state, and local LEAs from 49 states and 4 US territories were participating in the 1033 Program. Since its inception, the program has transferred property to LEAs with a total original acquisition value of $7.4 billion (Defense Logistics Agency 2020).[3]

2. ECONOMETRIC METHODOLOGY

Our objective is to uncover the causal effect, if any, of the transfers of equipment to sheriffs available through the 1033 Program on their reelection probability. To do so, we estimate the following linear probability model:

$$Re-election_{i,c,s,t} = \alpha_{s,t} + \beta \ln(equip_{i,c,s,t}^{\text{last 4 years}}) + \epsilon_{i,c,s,t} \tag{1}$$

where our dependent variable, $Re-election_{i,c,s,t}$, is equal to 1 if sheriff i from county c (in state s) has been reelected in year t and 0 otherwise. Our main regressor of interest is the log of $equip_{i,c,s,t}^{\text{last 4 years}}$ where $equip_{i,c,s,t}^{\text{last 4 years}}$ measures the aggregate transfers to the sheriff's office in county c over the period $t-1$ and $t-4$ (i.e., aggregate transfers for 4 years that, by and large, correspond to the mandate of a sheriff).[4] In our main specifications, we will express transfers in terms of total quantities and total values but we will engage in robustness checks varying the definition of our key dependent variable (e.g., the time horizon and the definition of transfers). State and year or state-by-year fixed effects ($\alpha_{s,t}$) are included to control

[2] Equipment on the prohibited list included tracked armored vehicles; weaponized aircraft, vessels and vehicles; 0.50-caliber firearms and ammunition; bayonets; camouflage uniforms and grenade launchers. Equipment on the controlled equipment list included manned aircraft; unmanned aerial vehicles; armored and tactical wheeled vehicles; command and control vehicles; under 0.50-caliber firearms and ammunition; explosives and pyrotechnics; breaching apparatus; riot batons, helmets, and shields.

[3] These figures and some of the information in this section come directly from the DLA website.

[4] Given that many counties never receive transfers, we take the log of 1 plus the total transfer; the results are essentially identical if we were to use 0.5 or 1.5.

for the substantial heterogeneity across states and for changes over time, and they determine our identification strategy as discussed in the next subsection.[5] Finally, $\epsilon_{i,c,s,t}$ denotes the error term, with standard errors clustered at the state level to allow for arbitrary correlation among elections within a state over time. 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.

2.1 Identification strategy

The econometric challenge in estimating the specification in equation (1) is the possibility of endogeneity of our regressor of interest to the outcome variable, in that the likelihood of a sheriff receiving a transfer of equipment may be correlated with the attributes of a sheriff that also drive the probability of reelection. For example, it may be the case that "stronger" incumbents are more likely to request transfers. We also cannot exclude the possibility of reverse causality, whereby reelected sheriffs may request fewer items because now enjoying an incumbency advantage. Thus, an increasing incumbency advantage over time may lead to a higher likelihood of reelection while being associated with fewer transfers. Reverse causality may also go in the opposite direction, as sheriffs with more experience may be more successful in their requests. In conclusion, ordinary least square (OLS) estimation may lead to inconsistent results.

Several papers in the literature have faced similar challenges when using data from the 1033 Program to study a variety of outcomes. The studies closer to our analysis are Bove and Gavrilova (2017), Harris et al. (2017), and Masera (2021a, 2021b). They all share the use of an instrumental variable approach to be able to reach a causal interpretation of their results. Furthermore, they all make use of county-level and time-invariant variables interacted with a US-wide but time-varying regressor as instruments. In our analysis, we follow the exact same approach as in these papers and we employ the variables used by Harris et al. (2017), as we believe that these better fit the purpose of our article (as explained below). Still, in the robustness section, we show that our conclusions are robust to the use of the instruments proposed by Bove and Gavrilova (2017) and Masera (2021a, 2021b).

Harris et al. (2017) use four county-level but time-invariant variables as instruments. Two of them exploit the variation in the geographical location of counties and how they relate to the location of the DLA Disposition Services from which the equipment must be transported. The motivation behind these instruments is two-fold. First, local sheriff offices are responsible for the cost of shipping any item from the disposition center. Second, not every disposition center keeps all items, so the relevant transportation cost to consider is not necessarily from the closest center. Hence, Harris et al. (2017) chose to use two distance measures, related to the closest and sixth-nearest center (with the latter measure implicitly capturing information about the five closest facilities). The other two instruments are the land area of a county and whether it is designated as a High-Intensity Drug Trafficking Area (HIDTA), motivated by the fact that geographically large counties and HIDTA counties were actively encouraged to participate in the 1033 Program (Harris et al. 2017). In order to proxy for the availability of items through the 1033 Program, we use the total transfers of equipment across the United States in the past 4 years. Interacted with the county characteristics, we obtain time-variant county-specific instruments. More fundamentally, these interactions capture the fact that sheriffs face different incentives regarding transfers depending on

[5] We drop 2007 and 2013 from the sample because of the few observations (i.e., 48 and 66, respectively) with at most one incumbent losing an election in these years.

the overall availability of items because of the fixed costs of the transfers (e.g., for applications, transportation costs, and inspection visits).

Based on the discussion above, our first stage is defined as follows:

$$\ln(equip_{i,c,s,t}^{\text{last 4 years}}) = \alpha_{s,t} + \delta\Big(\ln(equip_t^{\text{US, last 4 years}}) \otimes [1, D_{c,s}^1, D_{c,s}^6, HIDTA_{c,s}, \ln(land_{c,s})]\Big) + \eta_{i,c,s,t} \quad (2)$$

where $\ln(equip_t^{\text{US, last 4 years}})$ denotes the aggregate 4-year transfers at the US level. $D_{c,s}^1$ and $D_{c,s}^6$ are the inverse of the distance from the centroid of county c to the closest and sixth closest disposition center where items may be located, respectively. $HIDTA_{c,s}$ is a dummy variable indicating whether a county is designed as HIDTA and $\ln(land_{c,s})$ is the log of the area of a county. If Harris et al. (2017) have already demonstrated that these instruments are relevant to explain county-level transfers, we will verify whether this is the case also in our sample and also experiment with other instruments and approaches used in the literature. As a preview, it is the case that our results are robust to different sets of fixed effects and instruments, as discussed in the robustness section, providing support for the robustness of our conclusions.

Depending on whether state and year or state-by-year fixed effects are included, the identification strategy relies on exploiting different levels of variation in the data. In the former case, we are relying on a sort of pooled specification for sheriffs within a state while controlling for aggregate US-wide shocks. Specifications with state-by-year fixed effects are more demanding in that they only exploit the variation across elections in a given state and year. County and year fixed effects would be the ideal strategy to follow but notwithstanding the data collection effort we pursued, the limited number of repeated observations by county (i.e., at most 5 and on average 2.4) and overall few cases of incumbent losing elections makes such an identification extremely challenging. However, we do experiment with county and decade fixed effects and report the results in the robustness section. The motivation behind this specification is that there are elections in every year of our sample so that decade fixed effects are less demanding than year fixed effects but still control for some aggregate shocks. As the results will show, these alternative strategies provide very similar point estimates, which is reassuring.

Bove and Gavrilova (2017) chose their instrument 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 that alleviate the "mechanical positive correlation [of the instrument] to the dependent variable in the first stage" (Bove and Gavrilova 2017: 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.

Finally, Masera (2021a, 2021b) exploits the change in the number of US soldiers in Afghanistan and Iraq to measure the availability of military equipment to be transferred. This US-wide and time-varying variable is interacted with the distance from disposition centers to obtain the required instrument. Since the level of his analysis is police departments, he is also concerned that the proximity of such departments to military bases may threaten his identification strategy. Thus, he always controls for the distance between police departments and military bases (interacted with year fixed effects). We consider both strategies in our robustness checks and verify that they do not affect our conclusions. We do not use boots on the ground as our main instrument because it never works better than our current strategy (possibly because in our data, there are some missing values for the presence in Iraq). Controlling for military bases does not make a difference in our results either, possibly because we have many fewer sheriff offices (i.e., only one per county) than police departments (i.e., more than 10 000) and they are most often located in the county seat of each county, which is based on historical considerations.

3. DATA AND SUMMARY STATISTICS

To verify whether the transfers of equipment through the 1033 Program affect the reelection probability of sheriffs, two main sets of data are needed: data on the elections of sheriffs and detailed information about the equipment transfers 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 in Panels A and B of Table 1. Panel C instead summarizes the main variables in our regression sample.

3.1 Electoral data

We have compiled an original dataset consisting of 7281 sheriff elections conducted in 2714 counties between 2006 and 2016, making it the largest dataset of sheriff elections utilized in the existing literature.[6] In most states where sheriffs are elected, their term spans 4 years, and the elections coincide with either the Presidential or mid-term elections for the US Congress (although some elections occur in every year of our sample).[7] While 11 states centralize these elections, the remaining states delegate the responsibility to county-level electoral boards. Consequently, data for such elections are not consistently available at the state level, necessitating data collection at the county level. Even at the county level, the release of information varies considerably. While a few counties make some data available on the county's electoral board website (often limited to the most recent election), we frequently had to directly contact county clerks via email to acquire the data. Although a time-consuming process, many county clerks responded to our inquiries. The electoral data for the 2714 counties in our dataset encompass approximately 86% of counties in states with elected sheriffs and represent 92% of the population in those counties (as of 2010). Figure 2 visually depicts the availability of our electoral data, with certain counties missing from specific states in the South and the Midwest (similar to Thompson 2020).

[6] Our dataset complements the data employed in prior studies such as Thompson (2020) and Zoorob (2022). Thompson (2020) collected data on 3500 elections in 1395 counties spanning from 2003 to 2016. Their research focused exclusively on states conducting partisan sheriff elections, and they also included data from counties with a population exceeding 100 000 when state-level data were unavailable. In contrast, we did not impose such criteria and expanded the database to almost double the number of counties. Zoorob (2022) constructed a dataset of 5604 elections in 1303 counties. Although they collected data from a similar number of counties as Thompson (2020), their research question required earlier data, resulting in an unbalanced panel that occasionally included observations dating back to 1958.

[7] The National Sheriff's Association provides comprehensive information on sheriff elections. Notably, Alaska, Connecticut, Hawaii, and Rhode Island do not conduct county sheriff elections. In Alaska, law enforcement outside of cities is handled by the Alaska State Troopers, Connecticut replaced elected county sheriffs with State Marshals in 2000, Hawaii does not have county sheriffs, and Rhode Island maintains a statewide sheriff division whose members are not elected. Additionally, some counties outside of these states do not have elected sheriffs (e.g., the sheriffs and undersheriffs of NYC). The terms for sheriffs last two years in Arkansas and New Hampshire, six years in Massachusetts, and three years in New Jersey (with terms staggered across counties so that elections occur each year).

Table 1. Summary statistics.

	Observations	**Mean**	**St. dev.**	**Min**	**Max**
Panel A: Electoral data (2006–2016)					
Winner identity	7281				
Winner percentage	6934	0.808	0.197	0.209	1
Winner Democratic	7281	0.310	0.463	0	1
Winner Independent	7281	0.114	0.317	0	1
Winner Libertarian	7281	0.0001	0.012	0	1
Winner Republican	7281	0.425	0.494	0	1
Winner unknown party	7281	0.151	0.359	0	1
Panel B: 1033 Program data (1991–2015)					
$Total\ values_{c,s,t}$	78 575	\$8208	\$107 082	\$0	\$15 748 490
$Total\ quantities_{c,s,t}$	78 575	3.945	101.644	0	11 063
Panel C: Regression sample (2007–2016)					
$Re\text{-}election_{i,c,s,t}$	3905	0.937	0.242	0	1
$Change\ in\ vote\ share_{i,c,s,t}$	3611	0.027	0.225	−0.835	0.985
$Change\ in\ votes\ for\ incumbent_{i,c,s,t}$	3482	1230	14 481	−189 750	229 075
$Change\ in\ total\ votes_{c,s,t}$	3484	309	20 424	−392 430	408 724
$ln(total\ values_{i,c,s,t}^{\text{last 4 years}})$	3905	3.941	4.986	0	15.943
$ln(total\ quantities_{i,c,s,t}^{\text{last 4 years}})$	3905	1.008	1.510	0	9.556
$Population_{c,s,t}$	3905	0.115	0.331	0.000	9.819
$Past\ Dem\ share_{c,s,t}$	3904	0.388	0.163	0	0.920
$Newspaper\ dummy_{c,s}$	3905	0.744	0.436	0	1
$Median\ household\ income_{c,s,t}$	3682	46.713	12.197	22.264	125.900
$Share\ black\ population_{c,s,t}$	3682	0.119	0.164	0	0.859
$Past\ vote\ share_{i,c,s,t}$	3682	0.798	0.197	0	1
$Change\ crime_{c,s,t}$	3682	0.001	0.033	−0.411	0.656
$Received\ transfers_{i,c,s,t}^{\text{last 4 years}}$	3905	0.411	0.492	0	1

Notes: The 78 575 observations for the 1033 Program data are for 3143 counties. Summary statistics in panel C based on estimating sample with state and year fixed effects. Values and quantities of transfers refer to items allocated sheriffs' offices only.

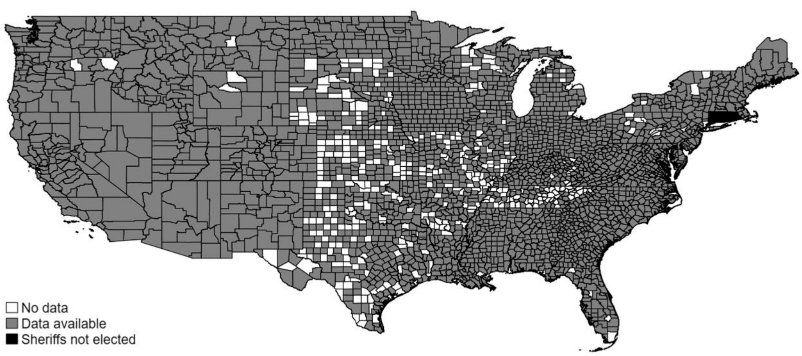

Figure 2. Availability of electoral data, by counties

Notes: Data availability based on 7281 observations; Alaska and Hawaii (not shown in the map) do not elect sheriffs.

Although our electoral data cover the period 2006–2016, we do not have data on all elections within this period for some counties. For each election, we know the names of the candidates, their political affiliations, and the votes they received in the election. As shown in

Panel A of Table 1, we know who won for 7281 elections although we can calculate the winner's percentage of votes in slightly fewer cases. Winners achieve good electoral outcomes with an average of more than 80% of the votes although there are many elections decided with small margins. We have also collected data on party affiliation, which is known for around 85% of the observations, with most sheriffs either Republican or Democrat (i.e., around 42% and 31%, respectively).[8]

When it comes to the econometric analysis, our main dependent variable is a dummy variable, $Re\text{-}election_{i,c,s,t}$, equal to 1 if sheriff i from county c (in state s) has been reelected in year t and 0 otherwise. With the high vote shares already mentioned above, it is no surprise that most sheriffs are reelected when they decide to run for another term (although those who lose reelection usually do so by a small margin). The summary statistics in Panel C of Table 1, which are based on the sample used in the econometric analysis, show that they are reelected in almost 94% of the cases.[9] However, this overall rate masks quite some variation including the "low" values of 0.79 and 0.84 in West Virginia and North Dakota, respectively, to 1 in Vermont. 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 reelected 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.[10]

Given the richness of the electoral data we collected, we define three more dependent variables used as robustness checks to corroborate the evidence from the benchmark specifications. We will use the $Change\ in\ vote\ share_{i,c,s,t}$ together with $Change\ in\ votes\ for\ incumbent_{i,c,s,t}$ and $Change\ in\ total\ votes_{c,s,t}$ to understand how the impact of transfers on reelection probability materializes at the voting booth. In fact, if the effect of transfers is positive, the vote share of the incumbent should increase but this can come through a change in the composition of the votes with or without an overall effect on total votes. As the summary statistics in Panel C of Table 1 suggest, there does not seem to be any large change in the total votes cast in the elections while incumbents seem to gain votes and, consequently, vote shares.

3.2 1033 program data

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 to a given agency with information about the goods being transferred (defined by the 13-digit code of the National Stock Number classification and a brief description) and relative quantity and total cost. As pointed out by Gunderson et al. (2021), the data we use are less prone to containing incorrect or misleading observations compared to previously aggregated data at the county level (e.g., it appears that this source

[8] Our assumption is that most of the observations where we have no party affiliation recorded are due to some states (or counties) holding non-partisan elections. Of course, we cannot exclude that some missing observations may be due to data collection issues (e.g., county clerks' offices not providing us back this information or counties' websites not reporting the latter).

[9] Notice that few states impose term limits on sheriffs, mostly in the form of at most two consecutive terms. Excluding sheriffs facing such term limits would not change the overall probability of reelection (i.e., 94%). 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.

[10] It may be the case that a sheriff does not run for reelection 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 reelected and is not among the losers.

[11] These data are 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). See Gunderson et al. (2021) and their Appendix 1.1.1 for a detailed description of different data releases.

records transfers to agencies located in counties for which there is no observation in the aggregate version of the dataset).

In our study, we assigned transfers to counties by leveraging the Census of State and Local LEAs (United States Department of Justice 2008), which provides information on the addresses of LEAs. In Appendix A1, we provide a thorough description of the process we followed to link each agency to its respective county. Once the transfers were successfully assigned to their corresponding counties, we proceeded to identify transfers allocated to sheriffs' offices specifically. For this purpose, we employed a straightforward criterion: if a LEA's name in our dataset contained the word "sheriff," we considered it to be a sheriff's office.

Panel B in Table 1 provides summary statistics for the aggregate quantities and values of transfers to sheriffs for all counties over the period 1991–2015. 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, implying that some counties have no registered transfers. Finally, we consider transfers to sheriffs at the county–year level distinguishing quantities and unit values (i.e., total cost divided by quantity). A similar consideration applies to the log of the transfers (of values of quantities) used in the econometric analysis (see Panel C of Table 1).

3.3 Other data

Summary statistics for most variables used in the econometric analysis are reported in Panel C of Table 1. As for the sources and starting with the data needed to construct the instruments, we take $HIDTA_{c,s}$ from the replication files made available from Harris et al. (2017) while $land_{c,s}$ and $population_{c,s,t}$ come from the US Census. Data on the locations of DLA disposition centers are obtained from the DLA[12] and distances are calculated with respect to the centroid of counties. Regarding military bases, their locations are obtained from the Military Bases dataset, which is part of the US Department of Transportation/Bureau of Transportation Statistics's National Transportation Atlas Database.[13] For the instrument employed by Masera (2021a, 2021b), figures on US troops stationed abroad are taken from a report by the Congressional Research Service (2021); annual figures calculated as averages of quarterly data.[14] To calculate the variable used 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 come 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. Data on the availability of local newspapers are obtained from the Official Directory of US Newspapers.[15] Finally, data to calculate the political leaning of a county were kindly provided by Mayda et al. (2022). Crime data are retrieved from the Uniform Crime Reporting Program run by the US Department of Justice.[16] As for the remaining data

[12] The list of locations was compiled on April 27, 2023 from the list of locations available at https://www.dla.mil/Disposition-Services/Find-Location/.

[13] The list of locations is as of May 21, 2019 and was compiled on April 27, 2023 from https://public.opendatasoft.com/explore/dataset/military-bases/information/.

[14] The report number is R44116 dated February 22, 2021 and available at https://sgp.fas.org/crs/natsec/R44116.pdf (accessed on May 22, 2023).

[15] The data was downloaded on July 28, 2023 from https://www.usnpl.com/.

[16] Our data are the compilation of the annual reports on "Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest." See for instance the 2010 report available at https://www.icpsr.umich.edu/web/NACJD/studies/33526.

used in the various robustness checks, they are described when they are utilized in the respective specifications.

4. RESULTS

Table 2 reports our benchmark results for two versions of the main dependent variable, $Reelection_{i,c,s,t}$, distinguishing whether we look at the total quantity or value of the items received and whether we include state and year or state-by-year fixed effects. The point estimates are essentially identical (for a given version of the key regressor) with the two different sets of fixed effects and they are all statistically significant at a 5% level. This stands in contrast to OLS results (shown in Table A2 in the Appendix) in which the estimated coefficients are never significant. Thus, OLS estimates suffer from a negative bias that could be due to reverse causality if, for example, a reelected sheriff enjoying increasing incumbency advantage feels more secure in their position and will be requesting fewer transfers than otherwise.

Table 2 also shows first-stage estimates with the tests at the bottom of the table indicating that the instruments are overall not weak (i.e., the Kleibergen–Paap *F*-stat is always above 10) although it is mostly the size of a county and its HIDTA characterization to perform well.[17] 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.

Having established that transfers under the 1033 Program have a positive impact on the electoral performance of sheriffs, it is important to understand their economic significance in affecting the reelection prospects of incumbent sheriffs. To answer this question, we engage in a quantification exercise where we consider how different the likelihood of reelection would have been for sheriffs (around 59% of the observations) who did not receive transfers. In particular, we calculate the value of transfers at the 10th and 25th percentile for every year of counties with positive transfers. We then use these values and the estimated coefficients from Table 2 to see how much the probability of reelection would increase if sheriffs without transfers would have received a transfer of such magnitude.

Table 3 shows that the probability of reelection would increase between 3.7 and 10.2 percentage points if a sheriff that did not receive any transfer in the past four years preceding the election year would have instead obtained the quantity or value at the 10th percentile. The equivalent increase for moving to the 25th percentile would be between 5.8 and 12.5 percentage points. The table also reports the values of these percentiles in 2016: these statistics are a clear reminder of the substantial variation (in quantity and values) across the distribution of transfers.

Given the richness of our electoral data, we can further interpret the increase in sheriffs' reelection probability by focusing on incumbents' electoral performance. First, it is important to consider that the median vote share of incumbents who lost reelection and did not receive any transfer is 0.43. Hence, the changes in reelection probabilities displayed in Table 3 can be explained by the many elections where the incumbent lost by a small margin, with our results suggesting that the outcome of several elections would be different if sheriffs had been successful in acquiring some transfers. Second, in Section 4.2, we use the change

[17] Point estimates would be almost identical while tests would improve if the two distance measures were removed from the set of instruments. However, they are significant in some specifications and hence the choice to keep them in all specifications.

Table 2. Benchmark results.

	(1)	(2)	(3)	(4)
Second stage				
$\ln(equip_{i,c,s,t}^{\text{last 4 years}})$	0.016^{**}	0.046^{**}	0.014^{**}	0.043^{**}
	(0.007)	(0.019)	(0.007)	(0.020)
First stage				
$\ln(equip_{t}^{\text{US, last 4 years}}) \times D_{c,s}^{1}$	0.005	0.115	0.007	0.122
	(0.218)	(0.148)	(0.245)	(0.142)
$\ln(equip_{t}^{\text{US, last 4 years}}) \times D_{c,s}^{6}$	−1.429	5.616	−5.863	3.414
	(25.487)	(13.065)	(25.637)	(13.895)
$\ln(equip_{t}^{\text{US, last 4 years}}) \times \ln(land_{c,s})$	0.021^{**}	0.009^{**}	0.016^{*}	0.008
	(0.009)	(0.005)	(0.009)	(0.005)
$\ln(equip_{t}^{\text{US, last 4 years}}) \times HIDTA_{c,s}$	0.075^{***}	0.050^{***}	0.073^{***}	0.049^{***}
	(0.015)	(0.008)	(0.016)	(0.008)
Category of equipment	Total values	Total quantities	Total values	Total quantities
State and year fixed effects	Yes	Yes	No	No
State-by-year fixed effects	No	No	Yes	Yes
Observations	3905	3905	3889	3889
Kleibergen–Paap F-stat	11.5	13.3	10.3	13.0
Hansen J Stat p-value	0.26	0.21	0.24	0.25

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

Table 3. Quantification exercise.

	(1)	(2)	(3)	(4)
Change in probability of re − election				
From no transfer to 10th percentile	0.102^{**}	0.040^{**}	0.094^{**}	0.037^{**}
	(0.043)	(0.017)	(0.044)	(0.017)
10th percentile of equipment (in 2016)	$2134	2	$2134	1
From no transfer to 25th percentile	0.125^{**}	0.062^{**}	0.114^{**}	0.058^{**}
	(0.053)	(0.026)	(0.054)	(0.027)
25th percentile of equipment (in 2016)	$35389	4	$35000	3
Category of equipment	Total values	Total quantities	Total values	Total quantities
State and year fixed effects	Yes	Yes	No	No
State-by-year fixed effects	No	No	Yes	Yes

Notes: Calculations based on the estimates in Table 2 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 vote shares and number of votes as dependent variables reporting positive and significant results, in line with our benchmark results in Table 2. Our quantification exercise performed on the coefficients reported in Table 6 would suggest that a sheriff going from no transfers to the 10^{th} and 25^{th} percentile of total transfers would experience an increase of around 0.08 and 0.10 points of the vote share, respectively (for the specification with state and year fixed effects and transfers expressed as total values). Again, these results suggest that transfers could have a role in flipping the outcome in a significant number of those elections where the incumbent sheriff lost the race and did not receive any equipment.

In conclusion, the result on the role of equipment on the reelection outcome is statistically and economically significant. In the next subsections, we discuss some heterogeneity results and verify the robustness of these estimates through an extensive battery of sensitivity checks.

4.1 Heterogeneity

Given data availability and the question at hand, pinpointing the exact channels driving our results is not a straightforward task. We therefore rely on our data to empirically explore whether our main result is driven by specific county characteristics. These results are summarized in Table 4 where we interact our key regressors with variables capturing different considerations. The first two panels are inspired by the debate on accountability in local elections (Warshaw 2019), where we aim at understanding the heterogeneous responses of voters depending on the county size, in terms of population, and the availability of local media (Hopkins and Pettingill 2018; Warshaw 2019). The third panel instead focuses on the heterogeneity in voters' preferences where in a pure Downsian logic we hypothesize that transfers are rewarded more in Republican-leaning counties (Downs 1957).

Panel A shows that our qualitative conclusions are at play in counties that are not too large. Although the interaction term between transfers and population is not significant, the sum with the main effect becomes statistically insignificant for very large counties. In fact, the *F*-test at the bottom of the panel shows that the effect is still significant at the 95^{th} percentile of the distribution. Still, there are very large counties (with a population in the millions) where the effect completely disappears. This result hints at a heterogeneous response of voters (at least as far as equipment transfers are concerned) depending on counties' size. Voters in small counties may be better able than voters in large counties to distinguish sheriffs' actions from those of 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 larger in smaller counties. 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-thirds of counties where the sheriff's office is the largest LEA (Zoorob 2022), one could expect most of those counties to be relatively small.

We pursue further the idea of accountability in Panel B where we exploit the presence of at least one local newspaper in a given county. In this case, the main effect is insignificant while the interaction with the dummy variable for the presence of local newspapers (i.e., based in the county) is always positive and significant.[18] Given that the sum of the two effects is always significant (as shown by the *F*-test at the bottom of the panel), we can conclude that the presence of a local newspaper does play an important role.[19] This result aligns with the ongoing discourse emphasizing the significance of local media in ensuring accountability in local elections (Hopkins and Pettingill 2018; Warshaw 2019). While accountability may be weak in counties where citizens cannot be informed about their sheriffs' actions, responsiveness at the booth can be more pronounced in counties with the presence of a local newspaper.

Finally, in Panel C, we consider the overall ideological leaning of a county, defined as the average share of votes for Democratic candidates in the most recent election.[20] The results

[18] We would obtain similar qualitative conclusions if we were to use the count of newspapers in a county.

[19] Note that the correlation between $Newspaper\ dummy_{c,s}$ and population is only 0.14, and it would be –0.22 with a dummy for small counties based on the cut-off for a population of 100 000 (in 2015), which is the threshold used by Thompson (2020) and hence the compatibility of the results in Panels A and B.

[20] Depending on the year, it may be election only for House, House and Senate, House and President, or House, Senate and President at once.

Table 4. Heterogeneity.

	(1)	(2)	(3)	(4)
Panel A: size of counties				
$\ln(equip_{i,c,s,t}^{last\ 4\ years})$	0.020***	0.089***	0.019***	0.083***
	(0.007)	(0.027)	(0.007)	(0.024)
$\ln(equip_{i,c,s,t}^{last\ 4\ years}) \times Population_{c,s,t}$	0.023	0.069	0.022	0.059
	(0.029)	(0.067)	(0.025)	(0.056)
$Population_{c,s,t}$	−0.146	−0.207	−0.145	−0.179
	(0.175)	(0.143)	(0.157)	(0.126)
Category of equipment	Total values	Total quantities	Total values	Total quantities
State and year fixed effects	Yes	Yes	No	No
State-by-year fixed effects	No	No	Yes	Yes
Observations	3905	3905	3889	3889
Kleibergen–Paap *F*-stat	2.46	1.99	2.27	2.25
Hansen *J* Stat *p*-value	0.33	0.57	0.31	0.52
F-test *p*-value	0.05	0.02	0.03	0.01
Panel B: newspaper availability				
$\ln(equip_{i,c,s,t}^{last\ 4\ years})$	−0.038	−0.107	−0.038	−0.100
	(0.026)	(0.087)	(0.025)	(0.075)
$\ln(equip_{i,c,s,t}^{last\ 4\ years}) \times Newspaper\ dummy_{c,s}$	0.063**	0.171*	0.062**	0.164**
	(0.026)	(0.088)	(0.026)	(0.078)
$Newspaper\ dummy_{c,s}$	−0.213**	−0.145**	−0.208**	−0.138**
	(0.086)	(0.068)	(0.090)	(0.063)
Category of equipment	Total values	Total quantities	Total values	Total quantities
State and year fixed effects	Yes	Yes	No	No
State-by-year fixed effects	No	No	Yes	Yes
Observations	3905	3905	3889	3889
Kleibergen–Paap *F*-stat	1.92	1.48	2.14	2.41
Hansen *J* Stat *p*-value	0.45	0.26	0.42	0.35
F-test *p*-value	0.00	0.00	0.02	0.02
Panel C: Democratic-leaning counties				
$\ln(equip_{i,c,s,t}^{last\ 4\ years})$	0.018*	0.083***	0.014	0.063**
	(0.010)	(0.027)	(0.011)	(0.031)
$\ln(equip_{i,c,s,t}^{last\ 4\ years}) \times Past\ Dem\ share_{c,s,t}$	−0.010	−0.089	−0.007	−0.053
	(0.024)	(0.060)	(0.027)	(0.071)
$Past\ Dem\ share_{c,s,t}$	0.073	0.113*	0.070	0.088
	(0.102)	(0.065)	(0.109)	(0.074)
Category of equipment	Total values	Total quantities	Total values	Total quantities
State and year fixed effects	Yes	Yes	No	No
State-by-year fixed effects	No	No	Yes	Yes
Observations	3904	3904	3888	3888
Kleibergen–Paap *F*-stat	6.29	13.1	5.93	11.2
Hansen *J* Stat *p*-value	0.40	0.54	0.34	0.49
F-test *p*-value	0.07	0.06	0.15	0.08

Notes: Standard errors in parenthesis clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively; *F*-test in Panel A reports the p-value for testing whether $\ln(equip_{i,c,s,t}^{last\ 4\ years}) + \ln(equip_{i,c,s,t}^{last\ 4\ years}) \times Population_{c,s,t} = 0$ when $Population_{c,s,t}$ takes the value corresponding to the 95^{th} percentile. *F*-test in Panel B reports the *p*-value for testing whether $\ln(equip_{i,c,s,t}^{last\ 4\ years}) + \ln(equip_{i,c,s,t}^{last\ 4\ years}) \times Newspaper\ dummy_{c,s}{=}0$; *F*-test in Panel C reports the *p*-value for testing whether $\ln(equip_{i,c,s,t}^{last\ 4\ years}) + \ln(equip_{i,c,s,t}^{last\ 4\ years}) \times Past\ Dem\ share_{c,s,t}{=}0$ when $Past\ Dem\ share_{c,s,t}$ takes the value corresponding to the 75^{th} percentile.

indicate that our conclusions go through for Republican-leaning counties but they become weaker for Democratic-leaning counties. That is, transfers to sheriffs play a less significant role when the county is Democratic leaning. Assuming that voters of the Republican party have stronger preferences for law and order, this result is in line with a standard Downsian framework of spatial modeling and proximity voting (Downs 1957). Formally, the *F*-test at the bottom of the table shows that the effect is only significant in three specifications and with *p*-values above 0.05 in the remaining ones when Democrats in the previous election were close to a 50% share (i.e., in around 25% of observations). Nevertheless, note that this result and the ones presented in Panels A and B may not be independent since county size, newspaper availability, and voters' preferences may be correlated.

Although all the specifications in Table 4 pass the test for overidentifying restrictions, the Kleibergen–Paap *F*-stat falls often below relevant critical values. This is somewhat to be expected, considering the interaction terms with the endogenous variable require interacting the instruments with the various county characteristics. Still, the results in Table 4 are suggestive of heterogeneous results in explaining our conclusion on the role of transfers in reelection. Moreover, these results are further supported by performing the analysis splitting the sample where the Kleibergen–Paap *F*-stat for some specifications also improves.

4.2 Robustness

This section assesses the robustness of our results by considering alternative specifications to identify the role of transfers on electoral performance. We start these robustness checks by adding several controls to our benchmark parsimonious specifications. Next, we consider alternative dependent variables to assess how the effect uncovered is translated to votes cast. Alternative versions of our key regressors are presented next and we conclude by assessing the sensitivity of the results to alternative identification strategies (varying the set of fixed effects and instruments).

In Panel A of Table 5, we add a series of socio-economic controls. We do so as a robustness check because their inclusion is not predicated by the identification strategy and their choice is not guided by a specific theoretical framework. In particular, we control for median household income, population, share of the black population, past vote share of the incumbent, and change in crime cleared by sheriffs. An incumbency advantage is clearly present, as reelection is more likely the higher the vote share that incumbent sheriffs earned in their previous election.

There is some evidence that sheriffs have a higher likelihood of reelection the higher the share of black population in their county (but only for total values of transfers). On the other hand, voters may reward sheriff's productivity in fighting crime. In fact, crime is one of the key policies sheriffs focus on during their electoral campaigns (Lublin 2004: 72). Moreover, past literature has elaborated on the link between crime and equipment transfers. For our crime variable, we focused on the number of violent crimes cleared by arrest by the sheriff's office of the county divided by the county population. As other papers in the literature have done (Masera, 2021b), we use the FBI's definition of a violent crime: murder, robbery, forcible rape, and aggravated assault. Following Marx et al. (2022), we calculate the change in cleared per capita crime as the average of crime rates in an election year and the 3 years before that minus the crime rate 4 years before the election year. The election year and the 3 years before that is a period of time coinciding with a sheriff's tenure in most cases. Therefore, this definition allows us to calculate the overall change in crime that is associated with a sheriff's tenure compared to the situation right before the sheriff got elected (or reelected). Our crime change variable, while always positive is barely significant. Across our specifications, it is only statistically significant at the 10% level and only in the specification

Table 5. Adding control variables.

	(1)	(2)	(3)	(4)
Panel A: socio-economic controls				
$\ln(equip_{i,c,s,t}^{\text{last 4 years}})$	0.021**	0.061**	0.016*	0.050*
	(0.009)	(0.027)	(0.009)	(0.027)
$Median\ household\ income_{c,s,t}$	0.001	0.000	0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.000)
$Population_{c,s,t}$	−0.003	−0.014	−0.001	−0.010
	(0.012)	(0.012)	(0.012)	(0.012)
$Share\ black\ population_{c,s,t}$	0.065**	0.042	0.066**	0.047
	(0.031)	(0.027)	(0.031)	(0.028)
$Past\ vote\ share_{i,c,s,t}$	0.208***	0.202***	0.203***	0.199***
	(0.031)	(0.031)	(0.032)	(0.032)
$Change\ crime_{c,s,t}$	0.237	0.249	0.237	0.252*
	(0.164)	(0.161)	(0.149)	(0.145)
Category of equipment	Total values	Total quantities	Total values	Total quantities
State and year fixed effects	Yes	Yes	No	No
State-by-year fixed effects	No	No	Yes	Yes
Observations	3682	3682	3667	3667
Kleibergen–Paap *F*-stat	6.56	6.60	5.39	5.19
Hansen *J* Stat *p*-value	0.33	0.28	0.32	0.30
Panel B: distance from closest military base				
$\ln(equip_{i,c,s,t}^{\text{last 4 years}})$	0.015**	0.047**	0.013**	0.043**
	(0.006)	(0.019)	(0.006)	(0.019)
Category of equipment	Total values	Total quantities	Total values	Total quantities
Year × distance military base	Yes	Yes	Yes	Yes
State and year fixed effects	Yes	Yes	No	No
State-by-year fixed effects	No	No	Yes	Yes
Observations	3905	3905	3889	3889
Kleibergen–Paap *F*-stat	11.6	12.8	10.2	12.0
Hansen *J* Stat *p*-value	0.18	0.16	0.16	0.18

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

of total quantities with state-by-year fixed effects. All in all, Panel A suggests that our benchmark results are unaffected by the addition of socio-economic controls.

Panel B of Table 5 is inspired by Masera (2021a, 2021b) who includes as a control the distance to the closest military bases (interacted with year fixed effects) because of the concern that police departments, his geographical level of analysis, may differ depending on their closeness to the bases. Our results are unaffected when adding such interaction terms (with the inverse of the distance).[21]

We also experimented adding other controls (one at a time on top of the specifications in Panel A), which turned out not to be statistically significant and neither do they affect our results (full results available upon request). We replaced the change in crime used above with the 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

[21] If we were to exactly follow Masera (2021a, 2021b) and use dummy variables for military bases within 20 km instead of a continuous version of the distance, we would obtain very similar point estimates but slightly lower Kleibergen–Paap *F*-stats.

obtained from Kaplan (2019).[22] Mass shootings can be used as a proxy capturing the salience of security and law enforcement in a given election year for each specific county. Hence, we included a dummy variable if a mass shooting in the previous year occurred in that county or in a border one.[23] Also, no evidence of any effect of educational attainment (i.e., percentage of adults having completed at least some years at college). 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 calculated and included the county-level 10-year changes in import competition but we did not find any effect.[24] We also included in the first two columns a dummy variable for Republican governors, as the 1033 Program's state coordinator responsible for the program's oversight in a given state is appointed by the governor. The dummy was never significant and its inclusion did not affect the other estimates.

Table 6 makes full use of our electoral data and shows results when using different dependent variables: the change in the vote share of the incumbent, the change in the number of votes cast for the incumbent, and the change in the total number of votes cast in the sheriff election. Panel A confirms that transfers provide an electoral boost by increasing incumbents' vote shares, consistent with our benchmark conclusions and the increase in their reelection probability. The following two panels indicate that this outcome is due to an increase in the number of votes in favor of the incumbent while there is no change in the overall number of people voting for the sheriff. Taken together, these results suggest that voters are persuaded by transfers—either previous voters change their minds or are replaced by voters persuaded by the equipment acquired by sheriffs.

So far, our results are based on two different, but conceptually very similar, measures of transfers. In Table 7, we use four alternative versions. In Panel A, we vary the time horizon across which we assume that voters could consider transfers as a determinant of their voting behavior in sheriff elections. Instead of focusing on the transfers in 4 years prior to the election, as they overlap with the tenure of most sheriffs, we consider that voters may have a shorter memory than in our benchmark specification, that is they hold the sheriff accountable only for the transfers requested during the past two years (i.e., between $t-1$ and $t-2$). In Panel B, we take the opposite view and consider the stock of military transfers since the inception of the 1033 Program in 1991. The results are very similar across the two panels and also in comparison to our benchmark specifications.

The conclusions are also robust when using transfers made to all agencies in a county, and not just to sheriffs' offices, as shown in Panel C. All transfers are meaningful variables to consider because while voters may not know or pay attention to which agency has requested the items, they could still associate them with the sheriff's office on the day of the election. These results are in line with previous conclusions, a result expected given that our data show that sheriffs regularly request transfers (i.e., their requests account on average for 50% of the total value and quantity of annual transfers). Still, these specifications indicate that voters cannot fully distinguish the source of the transfers, and in so doing sheriffs "gain"

[22] Kaplan (2019) provides a compilation of crime data released by the Uniform Crime Reporting (UCR) Program Data. 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.

[23] Data from MotherJones (2020). For other recent uses of these data and more details on MotherJones as well as for alternative data sources on mass shootings, see Lagerborg et al. (2023).

[24] 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).

Table 6. Other dependent variables.

	(1)	(2)	(3)	(4)
Panel A: change in vote share				
$\ln(equip_{i,c,s,t}^{\text{last 4 years}})$	0.013**	0.029**	0.012*	0.026*
	(0.006)	(0.013)	(0.006)	(0.014)
Category of equipment	Total values	Total quantities	Total values	Total quantities
State and year fixed effects	Yes	Yes	No	No
State-by-year fixed effects	No	No	Yes	Yes
Observations	3611	3611	3595	3595
Kleibergen–Paap *F*-stat	9.56	12.9	8.20	11.9
Hansen *J* Stat *p*-value	0.17	0.10	0.15	0.093
Panel B: change in votes for incumbent				
$\ln(equip_{i,c,s,t}^{\text{last 4 years}})$	3254.353***	8290.238***	3376.209***	8449.389***
	(907.061)	(2281.398)	(968.319)	(2362.070)
Category of equipment	Total values	Total quantities	Total values	Total quantities
State and year fixed effects	Yes	Yes	No	No
State-by-year fixed effects	No	No	Yes	Yes
Observations	3482	3482	3467	3467
Kleibergen–Paap *F*-stat	8.33	11.9	6.93	10.9
Hansen *J* Stat *p*-value	0.52	0.23	0.35	0.17
Panel C: change in total votes				
$\ln(equip_{i,c,s,t}^{\text{last 4 years}})$	1131.796	3684.612	1342.829	4179.081
	(1736.740)	(4591.824)	(1873.971)	(5006.261)
Category of equipment	Total values	Total quantities	Total values	Total quantities
State and year fixed effects	Yes	Yes	No	No
State-by-year fixed effects	No	No	Yes	Yes
Observations	3484	3484	3469	3469
Kleibergen–Paap *F*-stat	8.44	11.9	6.93	10.9
Hansen *J* Stat *p*-value	0.67	0.82	0.84	0.91

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

from the requests lodged by other agencies.[25] Note that the point estimates are smaller than in our baseline specifications, and this is consistent with overall larger amounts (both in terms of quantities and values) of equipment in Panel C compared to our benchmark specifications. Finally, the estimates in Panel D demonstrate that the extensive margin (i.e., a dummy variable for having received any amount of transfers) is also a significant determinant of reelection.

Moving to probe the robustness of our econometric methodology, we experimented using county and decade fixed effects. As discussed in Section 2.1, this is the most we can do when also using county fixed effects because of the limited number of repeated observations and the overall few numbers of incumbents losing an election. Such decade fixed effects control for some aggregate shocks, as there are elections every year in the sample. The estimates recovered with this strategy are reported in Table 8, and they are remarkably close to our benchmark specifications. Taken together with our two identification strategies based on state and year or state-by-year fixed effects, these results consistently lead to the conclusion that military transfers affect the reelection of sheriffs.

[25] If we were to only use transfers by agencies other than a sheriff's offices, the key results would hold although with smaller and less significant point estimates (i.e., sheriffs gain from transfers to other agencies but less than for those they request).

Table 7. Robustness checks on regressor.

	(1)	(2)	(3)	(4)
Panel A: equipment received over past 2 years				
$\ln(equip_{i,c,s,t}^{last\ 2\ years})$	0.017^{**}	0.053^{**}	0.016^{**}	0.050^{**}
	(0.007)	(0.024)	(0.007)	(0.024)
Category of equipment	Total values	Total quantities	Total values	Total quantities
State and year fixed effects	Yes	Yes	No	No
State-by-year fixed effects	No	No	Yes	Yes
Observations	3905	3905	3889	3889
Kleibergen–Paap *F*-stat	16.2	11.5	14.2	12.6
Hansen *J* Stat *p*-value	0.31	0.21	0.30	0.24
Panel B: equipment received over all past years				
$\ln(equip_{i,c,s,t}^{since\ 1991})$	0.012^{**}	0.035^{**}	0.012^{**}	0.033^{**}
	(0.005)	(0.013)	(0.005)	(0.013)
Category of equipment	Total values	Total quantities	Total values	Total quantities
State and year fixed effects	Yes	Yes	No	No
State-by-year fixed effects	No	No	Yes	Yes
Observations	3905	3905	3889	3889
Kleibergen–Paap *F*-stat	9.79	11.1	8.56	11.1
Hansen *J* Stat *p*-value	0.18	0.19	0.20	0.24
Panel C: equipment received by any agency over the past 4 past years				
$\ln(equip_{i,c,s,t}^{anyagency,\ last\ 4\ years})$	0.008^{**}	0.016^{**}	0.007^{*}	0.015^{*}
	(0.003)	(0.007)	(0.003)	(0.007)
Category of equipment	Total values	Total quantities	Total values	Total quantities
State and year fixed effects	Yes	Yes	No	No
State-by-year fixed effects	No	No	Yes	Yes
Observations	3905	3905	3889	3889
Kleibergen–Paap *F*-stat	41.3	38.6	50.7	42.8
Hansen *J* Stat *p*-value	0.20	0.14	0.20	0.21
Panel D: indicator variable for transfers received				
$Received\ transfers_{i,c,s,t}^{last\ 4\ years}$	0.203^{**}	0.209^{**}	0.187^{**}	0.186^{**}
	(0.085)	(0.088)	(0.085)	(0.088)
Category of equipment	Total values	Total quantities	Total values	Total quantities
State and year fixed effects	Yes	Yes	No	No
State-by-year fixed effects	No	No	Yes	Yes
Observations	3905	3905	3889	3889
Kleibergen–Paap *F*-stat	7.68	7.58	6.81	6.62
Hansen *J* Stat *p*-value	0.33	0.30	0.30	0.27

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

The last piece of robustness checks presented in Table 9 focuses on our instruments. In the absence of county fixed effects, we could also include $D_{c,s}^1$, $D_{c,s}^6$, $HIDTA_{c,s}$, and $\ln(land_{c,s})$ in levels in the first stage. The point estimates reported in Panel A of Table 9 show that their inclusion leads to only marginally smaller coefficients than our benchmark results; in fact, the tests at the bottom show that they are never statistically different from those. However, the Kleibergen–Paap F-stats are lower, possibly because of substantial multicollinearity in this extended set of instruments. Hence, we do not include the instruments in level in our benchmark specifications. On the other hand, even with the inclusion of year (or state-by-year) fixed effects throughout the analysis, it is technically possible to use the

Table 8. Results with county fixed effects.

	(1)	(2)
$\ln(equip_{i,c,s,t}^{\text{last 4 years}})$	0.017** (0.007)	0.054** (0.021)
Category of equipment	Total values	Total quantities
County and decade fixed effects	Yes	Yes
Observations	3024	3024
Kleibergen–Paap *F*-stat	11.2	9.35
Hansen *J* Stat *p*-value	0.10	0.049

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

(time-invariant) county-level instruments without interactions with the overall amount of transfers. The outcome of this alternative strategy is shown in Panel B, confirming the robustness of our results. However, this is not our preferred choice because it does not allow us to exploit varying levels of transfers over time that are due to the surplus returned by the military—a crucial consideration exploited by related papers in the literature (e.g., Bove and Gavrilova 2017; Harris et al. 2017; Masera 2021a, 2021b).

In Section 2.1, we argued in favor of following the instrumental variable strategy employed by Harris et al. (2017). Masera (2021a, 2021b) chooses to proxy for the availability of items to be distributed to local authorities in a different way. He uses boots on the ground (in Afghanistan and Iraq) deployed by the US military, instead of the total US amount of transfers. We did follow this strategy by replacing $ln(equip_t^{\text{US, last 4 years}})$ in equation (2) with the following expression

$$eqp_t = \frac{boots_{2007} - boots_t}{boots_{2007} - boots_{2016}} \tag{3}$$

where $boots_t$ stands for the military personnel stationed in Afghanistan in year t.[26] As discussed with regard to Panel B of Table 5, Masera (2021a, 2021b) includes an interaction between the distance to military bases and year dummies as control. We follow him also in this dimension and also use dummy variables (for distances lower than 20 km) instead of a continuous version of distances. The point estimates recovered with this approach are reported in Panel C of Table 9. Those estimates are slightly larger than our benchmark results but still very close while the Kleibergen–Paap *F*-stats are slightly lower. Along the lines of this approach, results would be very similar if we used our original strategy of continuous distance measures but replaced total US transfers with the formulation in equation (3), with or without controlling for distance from military bases. Finally, Panel D shows that the qualitative conclusions would continue to hold if we were to add the instrument used by Bove and Gavrilova (2017), based on total US military spending and the probability of a county receiving some transfers.

In conclusion, the estimates in Tables 5–9 have probed our main results in a number of dimensions but still confirm that transfers of equipment are a robust determinant of sheriffs' reelection probabilities.

[26] If we also use the data we found for Iraq, the instruments do not work well, possibly because we have missing values in 2012–2015 followed by years with positive values that may be not for military personnel.

Table 9. Robustness checks on instruments.

	(1)	(2)	(3)	(4)
Panel A: adding non-time varying instruments				
$\ln(equip_{i,c,s,t}^{\text{last 4 years}})$	0.014^{**}	0.041^{**}	0.012^{**}	0.040^{**}
	(0.006)	(0.018)	(0.006)	(0.019)
Category of equipment	Total values	Total quantities	Total values	Total quantities
State and year fixed effects	Yes	Yes	No	No
State-by-year fixed effects	No	No	Yes	Yes
Observations	3905	3905	3889	3889
Kleibergen–Paap F-stat	7.03	10.00	8.29	9.10
Hansen J Stat p-value	0.12	0.082	0.13	0.11
χ^2 test with benchmark (p-value)	0.40	0.26	0.22	0.30
Panel B: instruments only in level				
$\ln(equip_{i,c,s,t}^{\text{last 4 years}})$	0.015^{**}	0.047^{**}	0.015^{**}	0.046^{**}
	(0.006)	(0.018)	(0.006)	(0.019)
Category of equipment	Total values	Total quantities	Total values	Total quantities
State and year fixed effects	Yes	Yes	No	No
State-by-year fixed effects	No	No	Yes	Yes
Observations	3905	3905	3889	3889
Kleibergen–Paap F-stat	11.7	13.4	10.4	13.0
Hansen J Stat p-value	0.29	0.30	0.25	0.29
Panel C: using Masera's (2021a, 2021b) formulation				
$\ln(equip_{i,c,s,t}^{\text{last 4 years}})$	0.023^{**}	0.066^{**}	0.018^{**}	0.053^{**}
	(0.010)	(0.030)	(0.008)	(0.024)
Category of equipment	Total values	Total quantities	Total values	Total quantities
Year × distance (dummy) military base	Yes	Yes	Yes	Yes
State and year fixed effects	Yes	Yes	No	No
State-by-year fixed effects	No	No	Yes	Yes
Observations	3905	3905	3889	3889
Kleibergen–Paap F-stat	9.85	8.43	14.6	12.8
Hansen J Stat p-value	0.93	0.85	0.90	0.89
Panel D: adding Bove and Gravilova (2017) instrument				
$\ln(equip_{i,c,s,t}^{\text{last 4 years}})$	0.004^{**}	0.015^{**}	0.004^{**}	0.014^{**}
	(0.002)	(0.007)	(0.002)	(0.007)
Category of equipment	Total values	Total quantities	Total values	Total quantities
State and year fixed effects	Yes	Yes	No	No
State-by-year fixed effects	No	No	Yes	Yes
Observations	3905	3905	3889	3889
Kleibergen–Paap F-stat	98.8	102.4	98.7	97.3
Hansen J Stat p-value	0.19	0.20	0.20	0.25

Notes: Standard errors in parenthesis clustered by state; ***, **, * denote significance at the 1%, 5%, and 10% level, respectively; Panel B also includes $D_{c,s}^{1}$, $D_{c,s}^{6}$, $HIDTA_{c,s}$ and $\ln(land_{c,s})$ as instruments, and χ^2 test refers to the test of equality of estimated coefficient for $\ln(equip_{i,c,s,t}^{\text{last 4 years}})$ between Panel B and Table 2; in Panel C, dummies for distances lower than 20 km are used, as in Masera (2021a, 2021b).

5. CONCLUSION

Every two years, American citizens have the opportunity to express their preferences by electing a wide range of local officials, including sheriffs, school board members, county governments, judges, magistrates, and coroners. In this study, we focus on sheriffs, who hold one of the most pivotal local offices and provide new insights into how voters respond to the actions of locally elected officials. Our findings, in a broader context, contribute to the literature on accountability within local offices. Of particular interest are our heterogeneity results, which shed light on the impact of local newspaper availability. These results add to the discussion regarding the importance of local media accessibility in ensuring that voters are informed about the actions of their representatives, as highlighted in prior research (Hopkins and Pettingill 2018; Warshaw 2019).

In the context of sheriff elections, our prior understanding was primarily centered around the substantial advantages of incumbency (Zoorob 2022) and the absence of significant partisanship (Thompson 2020). However, until now, there have been no available results shedding light on factors that impact their electoral performance. Our findings help explain the enthusiastic support shown by sheriffs for Trump's executive order, which lifted restrictions from the Obama administration on equipment transfers. Unfortunately, our dataset did not allow us to pinpoint the precise mechanism driving this phenomenon. While we have uncovered interesting heterogeneity results, we cannot definitively determine why voters reward sheriffs for these equipment transfers.

In addition, our findings diverge from prior research indicating that militarization adversely impacts citizens' perceptions of the police (Mummolo 2018). Moreover, research has shown that in the aftermath of the killing of George Floyd in May 2020, the surge in Black Lives Matter protests was greater in counties with a high degree of militarization compared to those with no militarization (Mavridis et al. 2022). Therefore, our result on an improvement in sheriffs electoral performance through militarization highlights the potential for valuable insights through further investigation of sheriff elections. For instance, there exists limited knowledge regarding voter turnout in sheriff elections. Existing literature has posited that participation tends to be low in (other) local elections (Warshaw 2019), with a predominance of privileged voters, such as the affluent and elderly (Oliver and Ha 2007; Kogan et al. 2018). This raises the possibility that our findings may be influenced by a distinct subset of voters, markedly different from those actively protesting against police militarization on the streets or disapproving of police militarization.

APPENDIX A1: 1033 DATA

In order to assign an agency in the original 1033 dataset to a county, we made use of the Census of State and Local LEAs (United States Department of Justice 2008), which provides information on the addresses of LEAs. We first drop LEAs in Guam, Puerto Rico and the Virgin Islands to obtain 191 459 transfers (i.e., observations) of material, distributed over 7738 LEAs. After dropping 51 LEAs whose address spanned more than one county, we were first able to exactly match the name of the recipients of equipment with an address for about half of the observations (i.e., 99 243 for a total of 4752 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 1772 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 names gave any hint about their address. If they 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 7142 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.

APPENDIX A2: ADDITIONAL RESULTS

Table A2. OLS results.

	(1)	(2)	(3)	(4)
$\ln(equip_{i,c,s,t}^{\text{last 4 years}})$	0.000	0.002	−0.000	0.001
	(0.001)	(0.003)	(0.001)	(0.003)
Category of equipment	Total values	Total quantities	Total values	Total quantities
State and year fixed effects	Yes	Yes	No	No
State-by-year fixed effects	No	No	Yes	Yes
R^2	0.00	0.00	0.00	0.00
Observations	3905	3905	3889	3889

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

DATA AVAILABILITY

The data underlying this article are available in Harvard dataverse, at https://doi.org/10.7910/DVN/EII07A.

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