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Essays on Law Enforcement Incentives

Shannon R. Graham
Clemson University, sgraham1994@yahoo.com

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Shannon Rae Graham
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Dr. Michael D. Makowsky, Committee Chair
Dr. Paul W. Wilson
Dr. F. Andrew Hanssen
Dr. Yichen Zhou

ABSTRACT

Law enforcement incentives have become a relevant discussion topic in recent years. Starting with sheriff offices, elections may introduce different incentives for sheriffs, impacting arrest rates and other behaviors around election periods. My first chapter examines the impact of both the primary and general elections on arrest rates in sheriff jurisdictions. Results show that offices led by a sheriff running for re-election see a decrease of 0.19 arrests per 1,000 capita following a loss in the primary. Offices with incumbents who win the primary but proceed to lose the general show 0.62 fewer arrests per 1,000 capita in the month following the general election. Further results indicate the decline following the loss in the general election stems mostly from drug arrests. These results suggest the electoral structure influences sheriff behavior, negatively impacting sheriff productivity, indicating substantial costs to elections, and possibly indicating different incentives across sheriffs during the election season.

Revenue generated through the criminal justice system has also been shown to cause a change in the incentive structure of law enforcement agencies. I show evidence of this first in my second chapter through a second-stage estimation of the effect of civil asset forfeiture laws on estimated law enforcement technical efficiency scores. Agencies allowed to retain proceeds from asset seizure have an incentive to generate revenue. I estimate how counties utilize police personnel and expenditures to maximize crime deterrence and incident clearances. Technical efficiency estimates are computed at the county level from 2007 and 2012 data

using the variable returns to scale data envelopment analysis estimator. To reduce estimation error and to allow for faster convergence, I use dimension reduction to estimate the Farrell output measure of police efficiency for county-level data. After testing for separability, results of a second-stage truncated regression show that the higher the allowed percentage of retained assets for a county, the more inefficient the county is at making arrests and deterring crime.

I conclude in my third chapter by discussing the growing law enforcement revenue generation incentive and how law enforcement revenues have become a key component of local government budgets across the United States. While numerous restrictions exist to constrain traditional sources of revenue, only recently have legislators introduced checks on the fiscal profitability of fines, fees, forfeitures, and asset seizures. Left unrestricted, fiscal incentives have demonstrably manifested in the enforcement patterns and discretionary decisions of police. The transformation of officers into agents of revenue creation leads to increased targeting of minority populations and out-of-towners, with emphasis on arrests that yield potential property seizure, with negative consequences for both community trust and the provision of public safety. Those burdened with legal financial obligations are disproportionately poor, positioning the criminal justice system as a pointedly regressive form of taxation. We discuss the mechanisms behind criminal justice revenue generation, the consequences to law enforcement outcomes, and policies designed to reform and mitigate revenue-driven law enforcement.

DEDICATION

To my Bunch of Coconuts, and even Padre. But all the more to my Jerm Bug.

Without your tremendous support I would be nowhere.

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CHAPTER 1

FIRED WITH EIGHT MONTHS' NOTICE

1.1 Introduction

The county sheriff is the only chief law enforcement officer to be elected to his position. Elections throw a wrench into the regular day-to-day of a sheriff's office, creating unique incentives and potentially impacting sheriff behavior in varying ways.

Entering an election cycle, the current sheriff has two choices: he can either choose to run or not run for re-election. Those who decline to run for re-election could be retiring or taking another job. In these cases, the sheriff may lose the incentive to answer to constituents or to continue working diligently.

A sheriff who plans on maintaining his status as sheriff, however, may substitute his time away from running the sheriff's office to campaigning for re-election. In order to secure his position in the next term, the sheriff needs to appeal to voters and garner support—a time-consuming task that potentially leads to neglect of the current office. Another consideration is that the actions he takes while in office could have an impact on his election chances, as voters ultimately decide who will be sheriff.

Post-election, policing incentives may depend on the results. Losing the election would be comparable to being fired. The losing sheriff may no longer worry about appeasing constituents. He could potentially be an unwilling or angry lame duck until the newly elected sheriff takes office—up to ten months—or he may lack the desire to execute his regular responsibilities. He also now has to find another job, possibly taking time away from his current job during the lame duck period.

Changes in behavior could show a decline in sheriff office productivity. A sheriff neglecting his office or his duties for any reason, whether to take time away to campaign or simply to shirk his regular responsibilities, is a danger to the productivity of policing by the sheriff's office. Risking sheriff productivity begs the question, exactly how significant are the costs to electing the chief law enforcement officers in counties?

This paper uses arrests per capita by a sheriff's office as a measure of changing sheriff behaviors to determine the costs of elections. I examine the monthly patterns surrounding both the primary and general elections for sheriff offices, distinguishing between sheriffs running and sheriffs not running in the next election. My main findings show that sheriffs in office who lose either the primary or the general election make significantly fewer arrests following the lost election. In the months following a loss in the primary, the sheriff makes about 0.19 fewer arrests per 1,000 capita. Similarily, those sheriffs who win the primary but go on to lose the general election make 0.62 fewer arrests per 1,000 capita in the month following the general election—before the newly elected sheriff takes office. Further results show the decline following the general election stems primarily from drug arrests—arrests that are more discretionary by nature.

This paper outlines some of the costs associated with sheriff elections. Significant negative changes in sheriff behavior during the election cycle would indicate higher costs, stemming specifically from differing incentives during the election cycle. Costs associated with the elections may include a decline in job performance—i.e. fewer arrests. There is also the potential for turnover costs. Following an election loss or the retirement of a sheriff, the winner of the election steps in. It may take him some time to adjust to running the office, potentially leading to fewer arrests during the first few months in office.

One examination of the costs of sheriff elections is that of Greenblatt (2018), discussing the incentives of "bad sheriffs." He analyzes the strong incentive for sheriffs to abuse their powers. A vast amount of money goes through the sheriffs' hands, ranging from fines and fees to asset seizures, without a secondary budgetary officer. There are often wrongful death lawsuits or excessive force accusations, but given the nature of their position, it is difficult to get rid of a sheriff (Greenblatt, 2018). Local government officials can fire local police chiefs, but since they are elected by the public, there is usually no way to fire a sheriff between elections. And oftentimes the sheriff has so much influence there are threats of harassment and intimidation for anyone who might dare get in the way. Despite having to answer to voters, often they can be in office for more than 20 years (Greenblatt, 2018). Especially in small counties, few challengers step up; average sheriff tenure is estimated at 11 years, while estimates of average police chief tenure range from two-and-a-half years to six years (Zoorob, 2020).

Elected sheriffs also have an incentive to be lulled into corruption. For example, since 2010, ten county sheriffs in the state of South Carolina have been convicted of crimes while in office, and two others are awaiting trial (Collins, 2020). One of them, suspended Chester County Sheriff Alex Underwood, filed for re-election in the 2020 election. As he has pleaded not guilty (Dys & Derickson, 2020), he is legally allowed to run again as long as he is not convicted at trial (CN2 News, 2020).

This is not to say there are not benefits associated with elected sheriffs. The National Sheriff's Association (NSA) believes election is the best option as "An 'Elected' Office of Sheriff is 'Directly' accountable and responsible to the will of the people in our Representative Democracy in providing public safety/law enforcement for their local communities."[1] As evidence, sheriffs make fewer seizures than appointed police chiefs (Mughan et al., 2019). Property seizures, allowed by asset forfeiture laws, are revenue enhancing (Benson et al., 1995; Baicker & Jacobson, 2007; Worrall & Kovandzic, 2008; Holcomb et al., 2011; Makowsky et al., 2019; Graham & Makowsky, forthcoming) and generally have negative associations by voters (Mughan et al., 2019). By making fewer seizures, sheriffs are following the will of the people—in both election and non-election years—providing evidence to support the NSA's claim.

This paper contributes to the literature by empirically documenting the shirking of sheriffs following a loss at the polls. To my knowledge, no other work estimates sheriff arrest patterns on a monthly basis during election season. I also add to the electoral structure literature. Prior work regarding elected judges and

[1] See National Sheriffs' Association 2010 Resolutions.

attorneys indicates a change in behavior toward more punitive actions nearing the election cycle (Huber & Gordon, 2004; Nadel et al., 2017). My research suggests sheriffs are different. A more punitive pattern for sheriffs would mean more arrests during election season, but results indicate, especially for those that lose the primary or general elections, arrests by the sheriff's office tend to decrease drastically.

1.2 The County Sheriff

Sheriffs in colonial America were modeled after the English "shire-reeve," who was appointed by governors as a royal officer. The English political figure served and protected the King's interests in the shire (Falcone & Wells, 1995; Facchini et al., 2020). The modern sheriff in the United States (US) is similar to the colonial sheriff, with the biggest difference being that the modern US sheriff is elected. Post-revolution, many states included the sheriff as an elected office in their constitutions in order to make sheriffs accountable to the local community. Under the new sheriff election model, the sheriff needed a good relationship with voters so they would re-elect him in the next election (Falcone & Wells, 1995; Facchini et al., 2020).

Sheriffs today are still elected in 46 states, mandated by most of their constitutions. There are no sheriffs in Alaska or Hawaii, the office was abolished in Connecticut in 2000, and the governor appoints the sheriffs in Rhode Island. In addition, a select few counties independently opt to appoint their sheriff.[2] The

[2] See Elected Office of the Sheriff: Executive Summary.

elected sheriff answers to the voters; he cannot have his powers or responsibilities restricted by county boards or commissioners (Falcone & Wells, 1995). According to Falcone & Wells (1995), the sheriff's office "has remained one of the most viable policing institutions in the United States" and "is a necessary and effective general purpose police agency and will most likely continue to be such as long as county-level government exists." Most of these sheriff elections are partisan elections with four-year terms. Five states—California, Louisiana, Minnesota, Oregon, and Tennessee—have nonpartisan elections, while the remaining 41 hold partisan elections. The states with sheriff offices not having four-year terms are as follows: Arkansas and New Hampshire have two-year terms, New Jersey has a three-year term, and Massachusetts has a six-year term.[3]

Sheriff offices also range in size. The smallest offices have one to two employees, while the largest sheriff's office is that of Los Angeles county, where they employed a total of 16,766 full-time employees, 9,351 of which are full-time sworn employees, in 2016 (Brooks, 2019). The majority of sheriff offices, though, are small, with about 55% of sheriff's offices employing less than 25 full-time sworn officers in 2016 (Brooks, 2019). Only 4% employed more than 250 full-time sworn officers, and that top 4% employed almost half (47%) of all full-time sworn officers nationwide (Brooks, 2019). The offices that are very large tend to form units that specialize in different tasks (Falcone & Wells, 1995). The office is, in a way, separated into smaller sub-offices to complete each different type of duty, making the largest offices fundamentally different from the others.

[3] See Office of Sheriff State-by-State Elections Information.

Though duties vary by county and state, the majority of sheriffs are the chief law enforcement officers of the county, with primary jurisdiction in the unincorporated parts of the county. Some counties also have jurisdiction in incorporated areas of the counties or are contracted out to incorporated areas (National Sheriffs' Association and others, 1977; Falcone & Wells, 1995; Bulman, 2019). As of 1977, 93% of all county sheriffs perform law enforcement functions (National Sheriffs' Association and others, 1977). In addition to the four states lacking elected sheriffs, sheriffs in Delaware, DC, Massachusetts, and Pennsylvania are restricted from law enforcement duties (Bulman, 2019). The majority of sheriffs also have control of the county jails, operating 85% of jails in the US (Zoorob, 2020). Many also perform court function, e.g. bailiff duties, in addition to law enforcement duties. Others solely provide security for the courthouse (Greenblatt, 2018). Other duties for some sheriffs include collecting county fees and taxes or selling licenses and permits (Falcone & Wells, 1995; Facchini et al., 2020).

Brown (1978) describes four different types of sheriff organizational models that have emerged, summarized well by Falcone & Wells (1995):

> *The "full-service model" carries out law enforcement, judicial and correctional duties. The "law enforcement model" (Multnomah County, Oregon) carries out only law enforcement duties, with other duties assumed by separate civil process and correctional agencies. The "civil-judicial model" involves only court-related duties (counties in Connecticut[4] and Rhode Island). Finally, the "correctional-judicial model" (San Francisco County) involves all functions except law enforcement.*

[4]As previously mentioned, Connecticut's sheriffs' offices were abolished in 2000.

The most common, or the traditional model is the "full-service model" (Falcone & Wells, 1995).

A sheriff office is structured such that there exists a boss (the sheriff) and workers (sheriff deputies). It seems to reason that a decrease in productivity from the sheriff himself may impact the productivity of his deputies (and thus show a reduction in arrests for the office as a whole). Including managerial talent multiplicatively in a standard production function leads to the marginal product of labor depending on the managerial talent. Using firms in the technology sector, Lazear et al. (2015) show that higher quality bosses increase total productivity substantially. Applying this theory to the sheriff election setting, managerial talent is depicted as the sheriff's own effort, while output would be measured as arrests by the sheriff's office. Thus by this theory, the productivity of the sheriff's deputies would decline when the sheriff's own effort declines around an election. This is evidence supporting the idea that a decline in effort by a sheriff surrounding an election could cause a decline in productivity throughout the entire sheriff office. Another channel through which a decline in sheriff effort leads to a decline in office productivity is through possible political pressure. Under pressure to increase or decrease arrests, the sheriff may dictate those pressures to his deputies, thus either increasing or decreasing arrests by the office according to the political pressure.

1.3 Literature

Even with the list of costs associated with sheriff elections, there is not a lot of prior literature regarding the effects of the electoral process on sheriff behavior. The most closely related research is that of Hill & Zoorob (n.d.). They also examine the effects of electoral incentives structure on the behavior of elected sheriffs. Using yearly data from sheriff offices and local police departments in Arkansas between 2005–2015, they estimate the effect of being a sheriff in an election year. They found that sheriff discretionary arrests declined by around 15% in election years, while there were no changes for homicide and manslaughter arrests. My paper, however, uses monthly data for a wider range of states to target more specific patterns in the months surrounding the elections. Zoorob (2020) also has a working paper that estimates the incumbency advantage for sheriffs. He finds that incumbents have an estimated 43 percentage point increased probability of winning the next election, which is a larger advantage than for most other offices.

Several other papers use arrest patterns to show racial disparities in law enforcement. When blacks gained the right to vote from the Voting Rights Act in 1965, there were more registered black voters who had a say in who their sheriff was (Facchini et al., 2020). Sheriffs were held more accountable for their actions, and thus black arrest rates fell in the counties covered by the legislation and having larger shares of black to white populations (Facchini et al., 2020). The voting rights in turn led to the improved treatment of minority groups by elected law enforcement offices. Sheriff offices also change their behavior when sheriffs' deputies gain the right to collectively bargain (Bulman, 2019). Comparing sheriffs' offices to police departments who were unaffected by the judicial decision to allow sheriffs' deputies to organize, Dharmapala et al. (2019) show collective bargaining led to an increase in violent misconduct incidents at sheriff's offices. Sheriff behavior is also affected by civil asset forfeiture laws. Compared to local police departments, sheriff offices seize less property because there are electoral costs for sheriffs associated with property seizures—voters do not favor forfeiture laws (Mughan et al., 2019). There are also slight differences in behaviors based on personal views held by the sheriffs or even the sheriff's attitude. Attitudes of political leaders influence both violence against women policies (Farris & Holman, 2015) and immigration policies (Farris & Holman, 2017).

Bulman (2019) shows the ratio of Black-to-White arrests is significantly higher under White sheriffs, using racial transitions between different sheriffs. These results are also driven by offenses where there is greater discretion by law enforcement and where the sheriff has greater control over hiring and policing strategies. Thompson (2020) explores differences in sheriff offices based on the sheriff's party affiliation. Using sheriff compliance with federal requests to detain unauthorized immigrants to determine if parties behave differently, he finds that Republicans and Democrats comply at nearly the same rate, showing law enforcement officers make similar choices across parties, at least in terms of immigration enforcement.

1.4 Data

Arrest data are available from the National Incident-Based Reporting System (NIBRS), accessed via the FBI's Crime Data Explorer. Total arrests were calculated from the arrestee files for each sheriff's office by ORI code at the month level. I also extracted total arrests for each of four categories: drug arrests, DUI arrests, violent arrests, and property arrests. In addition, I calculated total incidents for the violent and property categories in order to calculate arrest to incident ratios for each month. Violent crimes include murder/nonnegligent manslaughter, rape, robbery, and aggravated assault. Property crimes include burglary/breaking and entering, pocket-picking, purse-snatching, shoplifting, theft, and all other larceny. The county's estimated annual population is also included in the NIBRS data. Any observation with missing population data is dropped. Law enforcement data are summarized in Table 1.1.[5] All three of Arrests/Cap., Drug Arrests/Cap., and DUI Arrests/Cap. are scaled by 1,000. Included in this table is the percent of observations that are 0. This percentage is much lower, 5.5%, for total arrests per capita than for any of the individual crime categories. The distributions for each category are also depicted as histograms in Figure 1.1. It is very clear that any model for the individual crime categories should take into account the large number of 0's. I suspect the main reason there are so many 0's for each category is because each sheriff's office may not investigate the same types of crimes.

Sheriff election data are not available in one centralized location. Depend-

[5] All tables except Table 1.3 were created using the stargazer package in R (Hlavac, 2018).

ing on the state or county, election results are available on the state's secretary of state website or each individual county clerk or election commission website. Since not all states report to NIBRS, I target those states and counties that do report to NIBRS for election data collection.

Most sheriff elections take place every four years, not all of them in the same year as the presidential election. Panel a of Figure 1.2 shows the distribution of the election year for the elections in my sample. All of the states in my sample have sheriff elections every four years, with the exception of New Hampshire. For the New Hampshire counties, I have a set of election results for both 2014 elections and 2016 elections. Some states also have counties with elections in different years. For example, South Carolina and Maine have some counties with elections in 2014, and other counties with elections in 2016.

My sample includes elections between 2014–2016. For the election year in my sample period for each county, I gathered the name of the winner and any opponent names in the general election, each candidate's party (if available), the number of votes for each candidate, total votes cast, and the month of the election. All of the general elections took place at the beginning of the month. I also collected information from the same year's primary election for each party if that county had a primary for the sheriff elections. This information includes which parties had primaries, whether each candidate had an opponent in his primary, and the primary month. The date of the primary election varies by county; some take place at the beginning, some in the middle, and some at the end of the month. I then collected the name of the winner from the previous general election to determine if there was an incumbent running in either the primary or the general election in my sample period. For example, sheriff elections in Nebraska occur every four years—in 2014 within my sample. For each of the counties with available election information in Nebraska, I collected 2014 candidate general and primary information, and then collected the winner of the election in 2010 to determine if there was an incumbent running in the 2014 general and primary elections.

For those counties where candidate party information were unavailable, I supplemented the data with party information from news articles about that candidate. In finding the supplemental party information, I came across a few sheriffs who left office in between the normal election cycle. The two types of cases where this happened were 1) if the sheriff was corrupt and he resigned or was removed from office, or 2) if the sheriff decided to retire early—before the next election cycle. Depending on the county and when the sheriff left office between election cycles, the county either held a special election or the governor appointed a sheriff to fill the slot until the next election. For the special elections, I dropped the observations in the period just before the sheriff left office up through the period after the newly elected sheriff took office, since the goal of this paper is to gain a better understanding of the regular election cycle. In the case of the governor appointing a new sheriff, I only dropped the monthly observations before the sheriff left office up through after the appointed sheriff took office.

A summary of the elections in my sample is shown in Figure 1.2 and Table 1.2. Overall, there are 618 elections in my sample. In about 77% of the elections, the sheriff in office runs in the election—there is an incumbent running.

And the sheriff is re-elected in about 68% of the elections. The incumbent loses in either the primary or the general in about 9% of elections. Panel b of Figure 1.2 shows the distribution of the party of the sheriff in office during the election cycle, with about 49% of sheriffs affiliating with the Republican Party and 34% of sheriffs affiliating with the Democratic Party. About 8% of elections are in a county that requires the sheriff elections to be nonpartisan, and I am missing the party information for about 0.8% of the sheriffs.

Panel c of Figure 1.2 shows the distribution of primary months. For those counties with primary elections, the primaries occur in varying months, the earliest being March and the latest being September. The majority of primaries take place in May or June. Panel d shows the overwhelming majority of general elections take place in November. Tennessee holds their sheriff elections in August, making up 18 of the 618 elections in my sample. The distinctions in primary months and general months causes the amount of time between primaries and generals to vary among different counties. Panel e shows the distribution of the length of the lame duck period for the losing sheriffs in my data sample. For the purposes of this paper, I define the "lame duck period" as the period after which a sheriff lost an election (either the primary or the general election) until the new sheriff takes office. Of the 60 sheriffs who lost an election, the majority (46.7%) lost the general election—leaving two months for the lame duck period until the new sheriff took office. For those sheriffs who lost the primary election, the average lame duck period lasted about eight months, while the longest lame duck period lasted ten months.

This paper focuses specifically on what is referred to here as the election cycle. A depiction of the election cycle is shown in Figure 1.3. In the figure and throughout, since elections occur in different months across counties, the primary is denoted as u, while the general election is denoted as t. For the purposes of this study, the election cycle begins five months before the primary election, or at time period $(u-5)$. From this point up until the primary election, the sheriff is either running or never running for re-election. After the primary, the running sheriff either wins the election and continues running for office, or loses the primary election and becomes a lame duck. After the general election, the running sheriff either won and continues running the county office or lost the general election and became a lame duck. Depending on the specification, the sheriffs who lost the general and the sheriffs who lost the primary may or may not be included in the same lame duck indicator variable after the general election. The new sheriff then takes office two months after the general election takes place. He replaces the previous sheriffs in three situations: 1) when the incumbent sheriff lost the primary, 2) when the incumbent sheriff lost the general election, and 3) when the sheriff was never running. I include up to five months after the general election in the election cycle.

Most states also allow sheriffs to be re-elected without term limits. The exceptions include some counties in Colorado, with varying term limits of two to four terms, and all counties in West Virginia—limited to two terms. Each state's term limit laws are available from the National Sheriff's Association. For those counties with term limits, I was able to sift through previous election results, calculating how many terms the sheriff had served, ascertaining if the previous sheriff was forced out of office by term limit. While there may be a difference in sheriffs who are not running in the next election because of term limits versus their own decision to retire or not run again, there were not enough observations in my sample, ten elections out of the 618, for meaningful inference. But their existence should be noted.

I do not have election information for several counties for various reasons. In some instances, the counties either did not have a website, or they just did not have any election results on their websites. Other times, the election data did not go back far enough to collect the name of the winner from the previous election.

I merged the arrest data with the election information for the years 2012–2017. There were no sheriff elections in 2013, and I dropped all of the observation in 2012 except for those where there were no elections at the end of 2012. The final data set contains 36,971 sheriff-month observations. The majority of observations in the panel are non-election cycle observations. I assume any county in the sample with a missing arrest observation has 0 arrests.[6]

Table 1.3 shows the total number of observations for each group in each time period. Most of the observations come from the incumbent sheriffs who are running for the upcoming election. When the incumbent sheriff wins the primary, he is considered to be running for the general election. It should be noted that my panel is not a balanced panel so the observations may not be the same month

[6] I also test specifications where I restrict the sample size to only counties with populations below 40,000. Results were similar across specifications.

to month. In this table, and in the expanded model, those elections without a primary are only coded as before and after the general, so there is a slight uptick in observations for the general election portion. However, for the base model, the observations without primaries are coded in the “before primary” and “after general” categories, and are excluded from the “between primary and general” category.[7]

1.5 Base Model

The goal of this paper is to study the effect of the regular election cycle on sheriff behavior. In order to capture differences in sheriff behavior, I use two approaches. Both empirical models use the arrests by the sheriff’s office per capita, scaled by 1,000, as the outcome variable. The first model (base model) uses a simplified version of a monthly event study with collapsed coefficients, while the second (expanded model) expands the first model into month-by-month coefficients.

As previously noted, the office of the sheriff is one with political power. Sheriffs tend to be in office for several terms, and oftentimes they do not have a challenger in the election. The majority of my sample has both a primary election and a general election. The primary election is just as important—if not more important—as the general election, since it is often the case that an incumbent sheriff faces a strong opponent within his party, but then is unopposed in the general election. Given the importance of both the primary election and the general

[7] I tested a specification where the counties without primaries were instead included in the “between primary and general” category, and the results were consistent.

election and the relatively small amount of election data I have, for sheriff s in year-month m, I build the base model with three time periods—before the primary (B), between the primary and general (M), and after the general (A),

$$\begin{aligned}
y_{sm} = \beta_0 + &\beta_1(running_{sm} \times B_{sm}) + \beta_2(running_{sm} \times M_{sm}) + \\
&\beta_3(wongeneral_{sm} \times A_{sm}) + \beta_4(lostprimary_{sm} \times M_{sm}) + \\
&\beta_5(lostelection_{sm} \times A_{sm}) + \beta_6(neverrunning_{sm} \times B_{sm}) + \\
&\beta_7(neverrunning_{sm} \times M_{sm}) + \beta_8(neverrunning_{sm} \times A_{sm}) + \\
&\beta_9(newsheriff_{sm}) + \beta_{10}log(population_{cy}) + \alpha_s + \tau_m + \varepsilon_{sm}.
\end{aligned} \tag{1.1}$$

The main outcome variable, y_{sm}, is the sheriff office's monthly arrests per capita, scaled by 1,000. The variable representing before the primary, B_{sm}, indicates five months before the primary election up to the primary month (exclusive) in that county. Between the primary election and general election, M_{sm}, includes any observation that occurs after the primary month (inclusive) and before the general election (exclusive). After the election, A_{sm}, includes any observation for that sheriff that occurs up to five months after the general election (inclusive). It should be noted the outgoing sheriff, whether he lost the election or he was never running, does not stay in office for five months after the election. In every state in my sample except for Louisiana, the newly elected sheriff takes office at the beginning of the second month after the election—most commonly the beginning of January following a November election.

I include an indicator variable, $running_{sm}$, signalling that the sheriff currently in office is running in the upcoming election, whether that be the primary election or the general election—i.e. the current sheriff is an incumbent in the upcoming election. The variable $wongeneral_{sm}$ indicates an incumbent sheriff who ran for re-election and subsequently won the general election (and previously won the primary). I also include $lostprimary_{sm}$ to indicate an incumbent sheriff who lost the primary election, and $lostelection_{sm}$ to indicate an incumbent sheriff who lost either the primary or general election. I include $neverrunning_{sm}$ to indicate that the sheriff currently in office was never running in the election that year.

A new sheriff takes office following either a loss or a sheriff who was never running for office; he is indicated by $(newsheriff)_{sm}$. The new sheriff variable is only indicated after the new sheriff takes office, two months after the general election up until five months after the general election. Each type of sheriff is interacted with the different time periods. I am careful to only include the winning and losing indicators after the election is over to avoid any reverse causation effects. I also include $log(population_{cy})$, but the estimated population data are only at the year y level for the county c. Sheriff fixed effects are included as α_s, and year-month fixed effects are included as τ_m. The error term is ε_{sm}.

1.5.1 Results

Results for the base model are shown in Table 1.4. Estimated by (1.1), the coefficients with standard error bars for each group in each time period are depicted in Figure 1.4. Figure 1.4 corresponds to Specification 1 in Table 1.4. Specifications 1 and 2 use the full data sample, while Specifications 3 and 4 exclude any observations with 0 arrests per capita. The comparison group for every coefficient is the sheriff's office not during an election year. Results across all four specifications are fairly robust. A few of the coefficients (e.g. Ran and Lost Primary and Ran and Lost Election) are slightly larger when excluding the 0's. Every coefficient is negative, indicating that, typically, sheriffs make fewer arrests during election years compared to non-election years. This can be seen visually in Figure 1.4. This result is consistent with the results by Hill & Zoorob (n.d.), who find that arrests are down during election years. A few introductory specifications are given in Appendix Table A1. They are all consistent with the base model results.

Looking at Specification 1 in Table 1.4, before the primary election, the average running sheriff makes a statistically significant 0.08 fewer arrests per 1,000 capita, and between the two elections, the sheriffs who are running for re-election make a statistically significant 0.17 fewer arrests per 1,000 capita than sheriffs outside the election cycle. After the general election, the sheriffs who won make a statistically significant 0.12 fewer arrests per 1,000 capita compared to non-election years.

Following a loss in the primary election, the sheriff's office makes roughly a statistically significant 0.19 fewer arrests per 1,000 capita between the two elections. The average arrests per 1,000 capita in the monthly time frame is about 1.85. So, for the average month, the sheriff's office is making about 10.3% fewer arrests following a lost primary. Keep in mind the average sheriff that lost a primary had a lame duck period of eight months, so that is a 0.19 decrease in arrests per 1,000 capita over an average of eight months, sometimes up to ten months. This drop does not appear to be statistically different than the effects for the sheriffs who are still running and the sheriffs who were never running in that time period. The biggest decline in arrests per capita comes from losers after the general election. Sheriffs who lost either the primary or the general election made a statistically significant 0.36 fewer arrests per 1,000 capita following the general election. This effect does appear to be statistically stronger than the other two groups in that time period (those who won the general election and those who were never running). For the average month (mean 1.85 arrests per 1,000 capita), the sheriff's office makes about 19.5% fewer arrests per capita. Since the elections take place within the first week of the election month, the election months themselves are included in the "after election" time period. Therefore, the decline of 0.36 arrests per 1,000 capita following the general election exists for two months.

Sheriffs who are never running for re-election during the election cycle have varying results. I would not expect to see a decline in arrests until closer to the end of their term, but the average sheriff who is never running makes a statistically significant decline in arrests per 1,000 capita of about 0.22 in the five-month time period before the primary election. Then between the two elections and after the general election, the sheriff who is never running makes a statistically significant 0.14 and 0.15 fewer arrests per 1,000 capita, respectively.

There is also some semblance of turnover costs when the new sheriff takes office. In his first four months, the average new sheriff makes a statistically significant 0.26 fewer arrests per 1,000 capita, compared to the rest of his term. I suspect this is indicative of the time necessary to adjust to the new position. The sign of the coefficient on $log(population)$ is negative, but the coefficient is not statistically significant.

1.6 Crime Stratification

I am also interested in determining which crime categories are those where sheriff offices are making fewer arrests. I separate the crimes into drug, DUI, violent, and property crimes. The drug and DUI categories are both arrests per 1,000 population, while the violent and property categories are arrest to incident ratios. Results of the base model (OLS) on each independent variable are shown in Table A6 and Figures A1, A2, A3, and A4 in the Appendix. There are also several introductory regression results in Appendix Tables A2, A3, A4, and A5. Again, as shown in Figure 1.1 and Table 1.1, there is a significantly larger portion of 0's in each of these dependent variables. Given the excess 0's (upwards of 30%), I use an independent double hurdle model in place of the base model. Each of the independent variables is the same as the base model, but instead of estimating OLS, I estimate with the first hurdle as a probit model, and then the second hurdle as a zero-truncated model.

Results of the double hurdle model are shown in Table 1.5, with the marginal effects from the probit model in the first half, and the marginal effects from the truncated model in the second half. The marginal effects from the probit model would indicate how much more or less likely the average sheriff's office is to make at least one arrest in each month. The effects from the truncated portion can be read as the linear effect for each independent variable on the dependent variable. There are not many significant results from the probit hurdle (only two statistically significant coefficients from all four regressions). My hypothesis is that the marginal decision to make the first arrest is comparable to any other marginal arrest, thus the probability of making the first arrest would not likely be affected by the election cycle. There are, however, several statistically significant coefficients from the truncated portion of the model for both drug arrests per 1,000 capita and DUI arrests per 1,000 capita.

The regression results from the truncated model are in Part II of Table 1.5. The coefficients are also depicted graphically for each crime category in Figures 1.5, 1.6, 1.7, and 1.8. Looking at Specification 1 in Part II of Table 1.5, there seems to be an unexpected positive effect for the sheriffs who won the general election after the election. The average winning sheriff makes about 0.05 *more* drug arrests per 1,000 capita following an election win. There is also a statistically significant effect when the running sheriff loses one of the elections in the time period after the general. The average losing sheriff makes about 0.23 fewer drug arrests per 1,000 capita after the general election. The standard deviation for the truncated portion of the drug variable is 0.46. So, the effect of the sheriff election loss is half a standard deviation. This is a relatively large effect, especially since it persists for usually two months. This result can also be seen in Figure 1.5, as the bar extending the farthest below 0. It appears the decline in arrests from an election loss shown in the base model in Table 1.4 is mostly from drug arrests. Drug arrests are relatively discretionary compared to other reported crimes, such as violent crimes, thus it is easier to let the drug arrests decline. There are also statistically significant negative results for drug arrests for the never-running sheriffs. Never-running sheriffs make about 0.06 and 0.15 fewer drug arrests per 1,000 capita both before the primary election and between the two elections.

Specification 2 in Part II of Table 1.5 shows results from another relatively discretionary type of crime, DUI's. These results can also be seen in Figure 1.6. Before both the primary and the general elections, the average running sheriff tends to make a statistically significant 0.02 and 0.05 *more* DUI arrests per 1,000 capita, respectively. Though relatively small, (the standard deviation of the DUI variable is 0.32), these positive effects could be indicating that getting drunk drivers off the road is important to voters. There also tends to be a subsequent decline in DUI arrests per 1,000 capita following an election win. Interestingly, sheriffs who were never running tend to have an office that makes about a statistically significant 0.13 more DUI arrests per 1,000 capita between the two elections, but make about 0.09 fewer DUI arrests per 1,000 capita after the general election. There is also a small, negative, statistically significant effect for the new sheriffs making DUI arrests.

None of the election variables are statistically significant for the violent and property arrest to incident ratio specifications. The violent and property specifications are presented graphically in Figures 1.7 and 1.8. These results are consistent with those of Hill & Zoorob (n.d.), who found that the crime categories that were most affected by elections were the discretionary crimes, and the violent crimes were unaffected. The only variable that was statistically significant for the property arrest specification was the log of the population.

Population appears to be a significant factor for all crime specifications except violent crimes. The effect is the largest for the drug arrests, where increasing the population by 10% leads to a decrease of about 0.19 drug arrests per 1,000 capita. The DUI and property specifications also have negative results, but the magnitude of the coefficients is much smaller.

1.7 Expanded Model

For the second model, I extend the coefficients in the base model to month-by-month coefficients to confirm the base model results. I estimate

$$\begin{aligned} y_{sm} = \beta_0 + \boldsymbol{\beta_1} \sum_{i=-5}^{-1} (u+i) \times running_{sm} + \\ \boldsymbol{\beta_2} \sum_{i=0}^{3} (u+i) \times lostprimary_{sm} + \boldsymbol{\beta_3} \sum_{i=-3}^{-1} (t+i) \times running_{sm} + \\ \boldsymbol{\beta_4} \sum_{i=0}^{1} (t+i) \times lostgeneral_{sm} + \boldsymbol{\beta_5} \sum_{i=0}^{5} (t+i) \times wongeneral_{sm} + \\ \boldsymbol{\beta_6} \sum_{i=-5}^{3} (u+i) \times neverrunning_{sm} + \\ \boldsymbol{\beta_7} \sum_{i=-3}^{1} (t+i) \times neverrunning_{sm} + \boldsymbol{\beta_8} \sum_{i=2}^{5} (t+i) \times newsheriff_{sm} + \\ \beta_9 log(population_{cy}) + \alpha_s + \tau_m + \varepsilon_{sm}, \end{aligned} \tag{1.2}$$

where the only outcome variable y_{sm} is arrests per capita, scaled by 1,000, for sheriff s in year-month m. The variables u and t denote the primary and the general election months, respectively, and i denotes the number of months before or after that election. For example, $(t-3)$ is an indicator variable indicating that the observation is three months before the general election. The remaining variables are as previously described. Note the $lostelection$ and $neverrunning$ variables are only included up to one month after the general election. There are not enough observations from Louisiana to include up to five months after the general election. In this model, of those who lost an election, I only include those who lost the general election after the general election as $lostgeneral$.

Since the amount of time between the primary and the general election varies, there is a question of how many months to include after the primary and before the general. I settled on three-month windows for both, after having considered the overlap between the two elections. If, for example, one county only has five months between the primary and the general election, all five-month indicators after the primary would overlap with the five-month indicators before the general election. Using a window of three months, only the middle month overlaps in this example. Also in an effort to minimize overlap, I test specifications with varying numbers of months for winning sheriffs following primaries. The sheriffs who won the primaries are the same sheriffs as those who are running in the general election, so I look to minimize that overlap as well.

1.7.1 Results

Specification 1 in Table 1.6 shows the results from (1.2) and is depicted in Figure 1.9. Like the results from the base model, Specifications 1 and 2 include the full data sample, while Specifications 3 and 4 exclude the 0's in the dependent variable. The comparison group is still the sheriff's office not during an election year.

The results compare well to the base model results. Not every month is statistically significant, but before the primary, for the two coefficients that are statistically significant, the sheriffs who are running make between 0.10–0.13 fewer arrests per 1,000 capita, slightly larger than the base model's estimate of 0.08.

The sheriffs who lost the primary have an unexpected positive coefficient in the month of the primary. Looking at Figure 1.9, they have a slightly positive coefficient in the month of the primary and then decline through the three months after the primary. However, of those coefficients for those who lost the primary, only the coefficient for three months after the primary is statistically significant. In that month, the average sheriff is expected to make 0.17 fewer arrests per 1,000 capita. This coefficient is slightly smaller than that of the base model. Again, that drop could persist until two months after the general election.

Including only the statistically significant coefficients, the sheriffs who are still running after the primary tend to make about 0.13–0.15 fewer arrests per 1,000 capita in the months before the general election. These coefficients are slightly less than the base model counterpart of 0.17. After having won the general election, the sheriffs also tend to make somewhere between 0.13–0.17 fewer arrests per 1,000 capita. When they have lost, however, sheriffs make 0.50 fewer arrests per 1,000 capita in the month of the general election, and 0.62 fewer in their last month in office. Given a standard deviation of 1.88, the decline is a third of a standard deviation in the last month in office. These results are almost twice the magnitude of the base model results.

The results for the never-running sheriffs vary. Looking at Figure 1.9, the never-running sheriffs see large decreases in arrests at the beginning of the election cycle, statistically significant until the primary election. Then they level out between the primary and general elections. I suspect this is partially because of the aforementioned overlap between the months after the primary and the months before the general election. Then in the month of the general election, the never-running sheriff sees about 0.16 fewer arrests per 1,000 people, presumably because he is nearing the end of his term. The last month, however, is unexpectedly not statistically significant.

Tracking well with the base model coefficient of 0.26, the new sheriff, after he takes office, starts with a statistically significant 0.30 fewer arrests per 1,000 capita, and then for the most part trends upward to 0.25 fewer in the fifth month after the election (his fourth month in office). Again, I suspect this is likely in part due to turnover costs as it takes some time for the new sheriff to adjust to his new position. The coefficient on $log(population)$ is also almost identical to the base model.

1.8 Party Affiliation Extension

Using the base model, I extend the variables into three groups: Republicans, Democrats, and all other party affiliations. Included in the "other" category are independents, counties who hold non-partisan elections, and those for whom I could not find a party affiliation. The distribution among these groups is shown in Panel b of Figure 1.2. For this extension, I estimate the same equation as the base model, except including interactions of each variable with indicators for both the Republicans and the Democrats, leaving the "other" category and non-election counties as the comparison group. The second specification I show includes only the observations with Republican and Democratic sheriffs, omitting the observations in the "other" party category.

Results from the party affiliation estimation are shown in Table 1.7. Column 1 includes all observations, while Column 2 only includes the observations for Republican and Democratic sheriffs. Coefficients from Specification 1 are depicted in a graph in Figure 1.10 with standard error bars. The first thing to notice is that more of the Republican coefficients are statistically significant than the Democratic coefficients. The effects from the Republican sheriffs also tend to be larger in most cases than those of the Democratic sheriffs, though not statistically significantly different. It is easier to see this difference in Figure 1.10. However, after the general election, the effects from the losing Democrats and the never-running Democrats appear slightly larger than the effects for the Republicans in the same categories. Overall, it appears the Republican sheriffs may be impacted slightly more strongly by the election cycle than Democrats, towards the beginning of the cycle, while Democrats appear slightly more impacted near the end of the cycle.

1.9 Conclusions

This paper tests the effects of the election cycle on sheriff behavior using monthly arrest data and yearly election data between 2013–2016. Results were similar across both the base model and the expanded model, showing an overall decline in arrests during the election cycle and indicating some of the costs of election in the sheriff's office. More specifically, there is a decline in arrests per capita before the primary election for sheriffs running for re-election. This could support the theory that they take time away from their job responsibilities to campaign for the election. Interestingly, there also tends to be a slight increase in DUI arrests for those running before the elections, possibly indicating that drunk driving is an important issue for voters.

Both models also show that the losers of both the primary and general election decrease arrests following the lost election, though the results for those who lose the general are more solidified by the expanded model. Losing sheriffs after the primary make about 0.19 fewer arrests per 1,000 capita in each month since the time they lost the election. On average, this large drop off in arrests could last eight months before the new sheriff takes office, though it could be up to ten months. Though the amount of time spent as a lame duck is shorter for sheriffs who lose the general election, the magnitude of the drop off in arrests is larger. Sheriffs who lose the general election make an average of about 0.50 fewer arrests per 1,000 capita in the month of the election, and about 0.62 fewer the month after the election. The decline in arrests for those who lost the general election appears to come mostly from drug arrests. Whether the sheriff lets arrests fall because he lacks a desire to attend to his responsibilities or the election causes them to decrease for another reason, sheriff productivity is at risk. The decrease in arrests could be an indication that the sheriff's office has put less effort into getting criminals off the streets and shirked its responsibilities, showing the high costs of elections for the sheriff's office.

Results for sheriffs not running for re-election are more mixed, as they make fewer arrests before the primary, but between the two elections, they increase arrests again. In their last lame duck months after the general election, however, they do decrease arrests again, similar to the losing sheriffs. The decline, though, is much less, about 0.15 arrests per 1,000 capita. This drop in arrests is also likely a sign of shirking, but by sheriffs who were never-running instead of sheriffs who lost an election.

I also find evidence of turnover costs from the new sheriffs. Over their first four months in office, the average sheriff makes about 0.26 fewer arrests per 1,000 capita compared to the rest of his term. While not necessarily directly associated with the elections (turnovers could occur for any newly sworn-in officer), these turnover costs also show lower sheriff productivity at the start of a term.

Results towards the beginning of the election cycle seem to be stronger (more of them are statistically significant) and only slightly larger for Republicans than for Democrats (not statistically significantly different). As Republicans are known to be tougher on crime, I find this result unexpected. I would expect the Republican sheriffs to be increasing arrests before elections in an effort to attract voters through the Republican Platform. It appears, though, that Republican sheriffs decrease arrests even more so than Democrats, albeit only slightly. However, the roles are reversed toward the end of the election cycle. The Democrats appear of have slightly larger coefficients after the general election, though again not statistically different than Republicans.

My research could be extended in the future in a number of ways. The loss of an election could be used as an instrument of effort levels in a sheriff's office. Given the extreme decline in arrests following an election loss, it is plausible to expect there may be some ramifications. For example, crime incidents may increase, as criminals may see the sheriffs putting in less effort.

There is also work to be done examining differences across stratified populations. In smaller counties, sheriffs are the only law enforcement presence, while in larger counties with cities, the sheriffs have fewer law enforcement responsibilities. It is not unreasonable to think there may be differences in effects of a decrease in effort following an election loss in counties with different population levels.

Another future research possibility would include adding asset forfeiture laws. Varying effects could be identified based on the allowance of forfeited asset revenues being returned to the sheriff's office. An office in a state allowing them to retain 100% of revenues from forfeited assets may show a different effect from decreased effort by sheriff's offices than an office in a state where they are allowed to retain 0% of revenues from forfeited assets.

These are just a few examples of possible future research. If election results prove a viable indicator of policing effort in sheriff's offices, there can be a multitude of paths for future research.

Table 1.1: Law Enforcement Summary Statistics

Statistic	n	Mean	St. Dev.	% 0	Min	Pctl(25)	Pctl(75)	Max
Arrests/Cap.	36,971	1.8479	1.8788	5.50%	0.0000	0.6904	2.4169	44.0005
Drug Arrests/Cap.	36,971	0.2481	0.4192	31.3%	0.0000	0.0000	0.3247	15.4211
DUI Arrests/Cap.	36,971	0.2022	0.3011	30.0%	0.0000	0.0000	0.2690	4.2755
Violent A/I Ratio	21,217	0.4695	0.3826	27.4%	0.0000	0.0000	1.0000	1.0000
Property A/I Ratio	33,026	0.1245	0.1684	36.1%	0.0000	0.0000	0.1800	1.0000
Population	36,971	28,644	39,936	0%	750	8,532	31,531	357,041

Notes: Arrest categories are all scaled by 1000. The arrest to incident ratio for violent and property crimes is denoted by A/I Ratio.

Table 1.2: Election Summary Statistics (n = 618)

Statistic	Mean	St. Dev.
Incumbent Running	0.7735	0.4189
Incumbent Won Primary and General	0.6764	0.4682
Incumbent Won Primary and Lost General	0.0453	0.2081
Incumbent Lost Primary	0.0518	0.2218
Sheriff Never Running	0.2265	0.4189

Table 1.3: Observations by Time Period for Each Election Type

<table>
<tr><th colspan="11">Primary Election</th></tr>
<tr><th></th><th></th><th>(u-5)</th><th>(u-4)</th><th>(u-3)</th><th>(u-2)</th><th>(u-1)</th><th>(u)</th><th>(u+1)</th><th>(u+2)</th><th>(u+3)</th></tr>
<tr><td rowspan="2">Running</td><td>Won</td><td rowspan="2">399</td><td rowspan="2">399</td><td rowspan="2">399</td><td rowspan="2">400</td><td rowspan="2">400</td><td>369</td><td>369</td><td>367</td><td>300</td></tr>
<tr><td>Lost</td><td>32</td><td>32</td><td>32</td><td>30</td></tr>
<tr><td>Never Running</td><td></td><td>122</td><td>122</td><td>122</td><td>121</td><td>121</td><td>121</td><td>121</td><td>121</td><td>121</td></tr>
<tr><th colspan="11">General Election</th></tr>
<tr><th></th><th></th><th>(t-3)</th><th>(t-2)</th><th>(t-1)</th><th>(t)</th><th>(t+1)</th><th>(t+2)</th><th>(t+3)</th><th>(t+4)</th><th>(t+5)</th></tr>
<tr><td rowspan="2">Running</td><td>Won</td><td rowspan="2">378</td><td rowspan="2">380</td><td rowspan="2">380</td><td>418</td><td>418</td><td>408</td><td>408</td><td>408</td><td>408</td></tr>
<tr><td>Lost</td><td>28</td><td>28</td><td></td><td></td><td></td><td></td></tr>
<tr><td>Never Running</td><td>New Sheriff</td><td>140</td><td>140</td><td>140</td><td>140</td><td>140</td><td>213</td><td>213</td><td>213</td><td>213</td></tr>
</table>

Notes: This table shows the number of observations for each group and time period in the data sample. The primary election is denoted by (u), the general election is denoted by (t), and each increment is one month.

Table 1.4: Base Model - Arrests per Capita

	Dependent variable:			
	Arr/Cap	asinh(Arr/Cap)	Arr/Cap	asinh(Arr/Cap)
	(1)	(2)	(3)	(4)
(Running)×(B)	-0.0805^{*}	-0.0456^{***}	-0.0877^{**}	-0.0480^{***}
	(0.041 8)	(0.015 6)	(0.042 8)	(0.014 7)
(Running)×(M)	-0.1743^{***}	-0.0655^{***}	-0.1861^{***}	-0.0692^{***}
	(0.048 3)	(0.018 5)	(0.047 1)	(0.016 9)
(Ran and Won General)×(A)	-0.1210^{***}	-0.0421^{***}	-0.1338^{***}	-0.0469^{***}
	(0.040 3)	(0.014 8)	(0.041 6)	(0.014 4)
(Ran and Lost Primary)×(M)	-0.1902^{**}	-0.0661	-0.2382^{***}	-0.0961^{**}
	(0.077 8)	(0.040 8)	(0.084 8)	(0.042 5)
(Ran and Lost Election)×(A)	-0.3614^{***}	-0.1754^{***}	-0.3971^{***}	-0.1943^{***}
	(0.114 9)	(0.046 2)	(0.119 4)	(0.045 3)
(Never Running)×(B)	-0.2185^{***}	-0.0818^{***}	-0.2243^{***}	-0.0834^{***}
	(0.057 7)	(0.021 5)	(0.059 9)	(0.021 2)
(Never Running)×(M)	-0.1414^{**}	-0.0388	-0.1686^{**}	-0.0547^{**}
	(0.068 4)	(0.026 2)	(0.071 0)	(0.025 7)
(Never Running)×(A)	-0.1547^{**}	-0.0747^{**}	-0.1374^{**}	-0.0635^{**}
	(0.073 5)	(0.030 5)	(0.068 7)	(0.026 0)
(New Sheriff)	-0.2552^{***}	-0.0888^{***}	-0.2536^{***}	-0.0835^{***}
	(0.050 6)	(0.020 3)	(0.051 0)	(0.018 5)
log(Population)	-1.0107	-0.2908	-1.5979^{*}	-0.6072^{**}
	(0.751 4)	(0.247 9)	(0.853 3)	(0.252 0)
Constant	10.4764	3.3163	16.3427^{*}	6.4803^{***}
	(7.496 5)	(2.472 9)	(8.512 8)	(2.514 3)
Month/Year FE	Yes	Yes	Yes	Yes
Sheriff FE	Yes	Yes	Yes	Yes
Clustered By:	Sher.	Sher.	Sher.	Sher.
Observations	36,971	36,971	34,938	34,938
Adjusted R^2	0.7469	0.7511	0.7499	0.7604

Notes: This table shows regression results from the base model, (1.1). The variable B is an indicator for up to five months before the primary, M is an indicator for between the primary and the general, and A is an indicator for up to five months after the general election. Significance is denoted at the 1%, 5%, and 10% level by ***, **, and *. Standard errors are robust and clustered by sheriff.

Table 1.5: Stratified Crime - Hurdle Models

	Dependent Variable:			
Part I: Probit	Drug (1)	DUI (2)	Viol A/I (3)	Prop A/I (4)
(Running)×(B)	-0.0039	-0.0058	-0.0016	0.0083
	(0.0071)	(0.0094)	(0.0114)	(0.0091)
(Running)×(M)	-0.0044	-0.0093	-0.0392^{***}	-0.0018
	(0.0078)	(0.0103)	(0.0140)	(0.0094)
(Ran and Won General)×(A)	-0.0098	0.0013	-0.0022	-0.0137
	(0.0074)	(0.0088)	(0.0108)	(0.0086)
(Ran and Lost Primary)×(M)	-0.0104	0.0157	0.0272	-0.0625
	(0.0279)	(0.0285)	(0.0288)	(0.0384)
(Ran and Lost General)×(A)	-0.0280	0.0203	-0.0145	-0.0350
	(0.0371)	(0.0246)	(0.0577)	(0.0421)
(Never Running)×(B)	-0.0079	-0.0008	0.0075	0.0098
	(0.0108)	(0.0140)	(0.0203)	(0.0126)
(Never Running)×(M)	-0.0131	-0.0111	-0.0030	0.0047
	(0.0143)	(0.0206)	(0.0245)	(0.0166)
(Never Running)×(A)	-0.0038	-0.0133	-0.0372	0.0094
	(0.0162)	(0.0264)	(0.0349)	(0.0193)
(New Sheriff)	-0.0256^{*}	-0.0117	0.0284	0.0195
	(0.0139)	(0.0175)	(0.0180)	(0.0125)
log(Population)	0.0830	-0.0511	-0.0291	0.1307
	(0.0853)	(0.1247)	(0.1572)	(0.1151)
Month/Year FE	Yes	Yes	Yes	Yes
Sheriff FE	Yes	Yes	Yes	Yes
Observations	36,971	36,971	21,217	33,026
	Dependent Variable:			
Part II: Zero-Truncated	Drug (1)	DUI (2)	Viol A/I (3)	Prop A/I (4)
(Running)×(B)	0.0195	0.0237^{*}	-0.0022	0.0021
	(0.0216)	(0.0122)	(0.0094)	(0.0059)
(Running)×(M)	-0.0326	0.0526^{***}	-0.0134	0.0061
	(0.0233)	(0.0126)	(0.0103)	(0.0065)

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Part II: Zero-Truncated	*Dependent Variable:* Drug (1)	DUI (2)	Viol A/I (3)	Prop A/I (4)
(Ran and Won General)×(A)	0.0516^{***}	-0.0256^{**}	-0.0119	0.0068
	(0.0194)	(0.0120)	(0.0086)	(0.0053)
(Ran and Lost Primary)×(M)	-0.0683	0.0068	-0.0158	-0.0047
	(0.0701)	(0.0461)	(0.0276)	(0.0208)
(Ran and Lost General)×(A)	-0.2284^{***}	-0.0728	-0.0035	0.0341
	(0.0884)	(0.0503)	(0.0362)	(0.0228)
(Never Running)×(B)	-0.0596^{*}	0.0295	-0.0093	0.0077
	(0.0358)	(0.0183)	(0.0151)	(0.0093)
(Never Running)×(M)	-0.1509^{***}	0.1279^{***}	-0.0100	-0.0011
	(0.0406)	(0.0190)	(0.0170)	(0.0110)
(Never Running)×(A)	0.0180	-0.0878^{***}	-0.0321	-0.0116
	(0.0556)	(0.0323)	(0.0242)	(0.0148)
(New Sheriff)	-0.0305	-0.0346^{*}	0.0127	0.0118
	(0.0327)	(0.0196)	(0.0152)	(0.0092)
log(Population)	-1.1925^{***}	-0.4883^{***}	-0.1712	-0.1490^{*}
	(0.1992)	(0.1197)	(0.1166)	(0.0804)
Month/Year FE	Yes	Yes	Yes	Yes
Sheriff FE	Yes	Yes	Yes	Yes
Observations	25,384	25,865	15,410	21,120
Log-Likelihood	7,481.2	14,884	2,630.6	20,675

Notes: This table shows the results from the hurdle model for stratified crime types. Both the Drug and DUI specifications are arrests per 1000 population, while the arrest to incident ratio is denoted by A/I. The variable B is an indicator for up to five months before the primary, M is an indicator for between the primary and the general, and A is an indicator for up to five months after the general election. Significance is denoted at the 1%, 5%, and 10% level by ***, **, and *. Standard errors are robust and clustered by sheriff.

Table 1.6: Expanded Model - Arrests per Capita

	Dependent variable:			
	Arr/Cap	asinh(Arr/Cap)	Arr/Cap	asinh(Arr/Cap)
	(1)	(2)	(3)	(4)
(u-5)×(Running)	-0.1292^{***}	-0.0616^{***}	-0.1245^{***}	-0.0517^{***}
	(0.0456)	(0.0194)	(0.0482)	(0.0180)
(u-4)×(Running)	-0.0584	-0.0440^{**}	-0.0517	-0.0366^{*}
	(0.0450)	(0.0193)	(0.0459)	(0.0189)
(u-3)×(Running)	-0.0619	-0.0386^{**}	-0.0617	-0.0365^{*}
	(0.0515)	(0.0194)	(0.0541)	(0.0189)
(u-2)×(Running)	-0.1002^{*}	-0.0417^{**}	-0.1242^{**}	-0.0551^{***}
	(0.0537)	(0.0196)	(0.0560)	(0.0183)
(u-1)×(Running)	0.0100	-0.0237	-0.0039	-0.0362^{*}
	(0.0899)	(0.0212)	(0.0928)	(0.0195)
(u)×(Lost Primary)	0.0492	0.0484	0.0166	0.0233
	(0.1248)	(0.0595)	(0.1289)	(0.0597)
(u+1)×(Lost Primary)	-0.1173	-0.0466	-0.1603	-0.0736
	(0.0961)	(0.0561)	(0.1039)	(0.0591)
(u+2)×(Lost Primary)	-0.1464	-0.0544	-0.1892	-0.0841
	(0.1078)	(0.0522)	(0.1153)	(0.0547)
(u+3)×(Lost Primary)	-0.1693^{*}	-0.0657	-0.2157^{**}	-0.0932^{*}
	(0.0891)	(0.0509)	(0.0958)	(0.0534)
(t-3)×(Running)	-0.1504^{**}	-0.0539^{**}	-0.1532^{**}	-0.0555^{**}
	(0.0654)	(0.0251)	(0.0607)	(0.0222)
(t-2)×(Running)	-0.0856	-0.0398^{*}	-0.0755	-0.0267
	(0.0561)	(0.0232)	(0.0557)	(0.0214)
(t-1)×(Running)	-0.1268^{*}	-0.0379^{*}	-0.1519^{**}	-0.0464^{**}
	(0.0748)	(0.0225)	(0.0770)	(0.0212)
(t)×(Won General)	-0.1376^{***}	-0.0523^{**}	-0.1483^{***}	-0.0536^{***}
	(0.0517)	(0.0205)	(0.0532)	(0.0201)
(t+1)×(Won General)	-0.0760	-0.0277	-0.1020^{**}	-0.0392^{**}
	(0.0501)	(0.0203)	(0.0518)	(0.0197)
(t+2)×(Won General)	-0.1723^{***}	-0.0504^{**}	-0.1905^{***}	-0.0602^{***}
	(0.0554)	(0.0201)	(0.0560)	(0.0191)
(t+3)×(Won General)	-0.1281^{**}	-0.0345^{*}	-0.1345^{**}	-0.0344
	(0.0556)	(0.0210)	(0.0578)	(0.0209)

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	Dependent variable:			
	Arr/Cap	asinh(Arr/Cap)	Arr/Cap	asinh(Arr/Cap)
	(1)	(2)	(3)	(4)
(t+4)×(Won General)	-0.1346^{*}	-0.0594^{***}	-0.1477^{**}	-0.0665^{***}
	(0.068 7)	(0.021 3)	(0.070 4)	(0.020 4)
(t+5)×(Won General)	-0.0562	-0.0167	-0.0516	-0.0119
	(0.056 9)	(0.020 6)	(0.059 3)	(0.020 4)
(t)×(Lost General)	-0.5015^{**}	-0.1956^{*}	-0.5282^{**}	-0.2031^{**}
	(0.255 5)	(0.106 9)	(0.237 9)	(0.088 8)
(t+1)×(Lost General)	-0.6242^{***}	-0.3037^{***}	-0.5981^{**}	-0.2826^{***}
	(0.221 8)	(0.085 0)	(0.233 8)	(0.080 4)
(u-5)×(Never Running)	-0.2042^{**}	-0.0593^{**}	-0.2419^{***}	-0.0861^{***}
	(0.071 6)	(0.029 0)	(0.075 1)	(0.029 8)
(u-4)×(Never Running)	-0.2796^{***}	-0.1139^{***}	-0.2730^{***}	-0.1064^{***}
	(0.081 0)	(0.034 6)	(0.083 9)	(0.034 6)
(u-3)×(never running)	-0.2538^{***}	-0.1038^{***}	-0.2600^{***}	-0.1048^{***}
	(0.081 4)	(0.031 5)	(0.086 3)	(0.031 6)
(u-2)×(never running)	-0.1951^{***}	-0.0865^{**}	-0.1859^{**}	-0.0767^{**}
	(0.074 6)	(0.033 6)	(0.076 4)	(0.030 6)
(u-1)×(Never Running)	-0.1804^{**}	-0.0517	-0.1922^{**}	-0.0569
	(0.088 8)	(0.036 6)	(0.091 7)	(0.034 8)
(u)×(Never Running)	-0.0189	-0.0015	-0.0556	-0.0265
	(0.095 1)	(0.033 1)	(0.099 9)	(0.033 9)
(u+1)×(Never Running)	-0.0859	-0.0241	-0.0884	-0.0259
	(0.089 9)	(0.035 3)	(0.092 4)	(0.034 0)
(u+2)×(Never Running)	-0.0939	-0.0335	-0.1185^{*}	-0.0519^{*}
	(0.065 6)	(0.029 1)	(0.067 4)	(0.028 5)
(u+3)×(Never Running)	-0.0550	-0.0143	-0.1140	-0.0571^{**}
	(0.070 1)	(0.028 1)	(0.077 0)	(0.028 6)
(t-3)×(Never Running)	-0.1712^{**}	-0.0581^{**}	-0.1712^{**}	-0.0541^{*}
	(0.070 0)	(0.029 4)	(0.072 5)	(0.029 4)
(t-2)×(Never Running)	-0.0731	-0.0278	-0.0690	-0.0208
	(0.072 8)	(0.032 1)	(0.072 6)	(0.030 1)
(t-1)×(Never Running)	-0.0733	-0.0029	-0.0901	-0.0086
	(0.087 6)	(0.032 5)	(0.090 6)	(0.032 1)
(t)×(Never Running)	-0.1627^{**}	-0.0801^{**}	-0.0962	-0.0324

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	Dependent variable:			
	Arr/Cap	asinh(Arr/Cap)	Arr/Cap	asinh(Arr/Cap)
	(1)	(2)	(3)	(4)
	(0.0794)	(0.0346)	(0.0753)	(0.0296)
(t+1)×(Never Running)	−0.1189	−0.0610*	−0.1378*	−0.0758**
	(0.0796)	(0.0339)	(0.0802)	(0.0314)
(t+2)×(New Sheriff)	−0.3048***	−0.1100***	−0.3152***	−0.1125***
	(0.0656)	(0.0261)	(0.0659)	(0.0246)
(t+3)×(New Sheriff)	−0.2605***	−0.0959***	−0.2521***	−0.0821***
	(0.0761)	(0.0301)	(0.0784)	(0.0279)
(t+4)×(New Sheriff)	−0.2340***	−0.0764***	−0.2359***	−0.0728***
	(0.0685)	(0.0268)	(0.0695)	(0.0257)
(t+5)×(New Sheriff)	−0.2453***	−0.0794***	−0.2344***	−0.0728***
	(0.0617)	(0.0264)	(0.0628)	(0.0241)
log(Population)	−1.0674	−0.3139	−1.6732*	−0.6382**
	(0.7579)	(0.2515)	(0.8543)	(0.2526)
Constant	11.0541	3.5497	17.1062**	6.7929***
	(7.5603)	(2.5082)	(8.5224)	(2.5198)
Month/Year FE	Yes	Yes	Yes	Yes
Sheriff FE	Yes	Yes	Yes	Yes
Clustered By:	Sher.	Sher.	Sher.	Sher.
Observations	36,971	36,971	34,938	34,938
Adjusted R^2	0.7466	0.7508	0.7495	0.7600

Notes: This table shows the results from the expanded model, (1.2). The primary is indicated by (u), and the general is indicated by (t), and each interval is a one month time period. In each specification, the dependent variable is Arrests per Capita. Significance is denoted at the 1%, 5%, and 10% level by ***, **, and *. Standard errors are robust and clustered by sheriff.

Table 1.7: Party Affiliation

	Dependent variable:	
	Arrests per Capita	
	(1)	(2)
(Rep)×(Running)×(B)	-0.0985^{***}	-0.0979^{***}
	(0.0294)	(0.0302)
(Rep)×(Running)×(M)	-0.1214^{***}	-0.1224^{***}
	(0.0345)	(0.0356)
(Rep)×(Ran and Won General)×(A)	-0.1067^{***}	-0.1239^{***}
	(0.0325)	(0.0332)
(Rep)×(Ran and Lost Primary)×(M)	-0.1216	-0.1276
	(0.1122)	(0.1130)
(Rep)×(Ran and Lost Election)×(A)	-0.1645	-0.1783
	(0.1244)	(0.1243)
(Rep)×(Never Running)×(B)	-0.1063^{***}	-0.1111^{***}
	(0.0382)	(0.0390)
(Rep)×(Never Running)×(M)	-0.0556	-0.0641
	(0.0546)	(0.0559)
(Rep)×(Never Running)×(A)	-0.0600	-0.0765
	(0.0492)	(0.0503)
(Dem)×(Running)×(B)	-0.0269	-0.0280
	(0.0342)	(0.0356)
(Dem)×(Running)×(M)	-0.0801^{**}	-0.0831^{**}
	(0.0374)	(0.0388)
(Dem)×(Ran and Won General)×(A)	-0.0215	-0.0349
	(0.0351)	(0.0360)
(Dem)×(Ran and Lost Primary)×(M)	-0.0504	-0.0591
	(0.0920)	(0.0927)
(Dem)×(Ran and Lost Election)×(A)	-0.2758^{***}	-0.2929^{***}
	(0.0946)	(0.0950)
(Dem)×(Never Running)×(B)	-0.0784^{*}	-0.0848^{*}
	(0.0456)	(0.0463)
(Dem)×(Never Running)×(M)	-0.0281	-0.0372
	(0.0517)	(0.0528)
(Dem)×(Never Running)×(A)	-0.1272^{**}	-0.1445^{**}

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	Dependent variable:	
	Arrests per Capita	
	(1)	(2)
	(0.0630)	(0.0636)
(Rep)×(New Sheriff)	−0.0815**	−0.0856**
	(0.0354)	(0.0371)
(Dem)×(New Sheriff)	−0.0842*	−0.0866*
	(0.0471)	(0.0479)
(Rep)	−0.1209	−0.4406
	(0.1577)	(0.2775)
(Dem)	0.2396	
	(0.1617)	
log(Population)	−0.6488**	−0.7905
	(0.2874)	(0.5381)
Constant	5.2584*	6.9075
	(2.8873)	(5.3689)
Month/Year FE	Yes	Yes
Sheriff FE	Yes	Yes
Clustered By:	Sher.	Sher.
Observations	36,971	29,975
Adjusted R^2	0.6848	0.6995

Notes: This table shows the results of the base model, adding interactions for both the Republican (Rep) and Democratic (Dem) parties. The letters B, M, and A stand for the three different time periods, before the primary (B), between the primary and general (M), and after the general (A). (New Sheriff) is only indicated for his first four months in office. Specification 1 includes all observations, and Specification 2 only includes the observations with a Republican or Democratic sheriff. Significance is denoted at the 1%, 5%, and 10% level by ***, **, and *. Standard errors are robust and clustered by sheriff.

Figure 1.1: Histograms of Dependent Variables

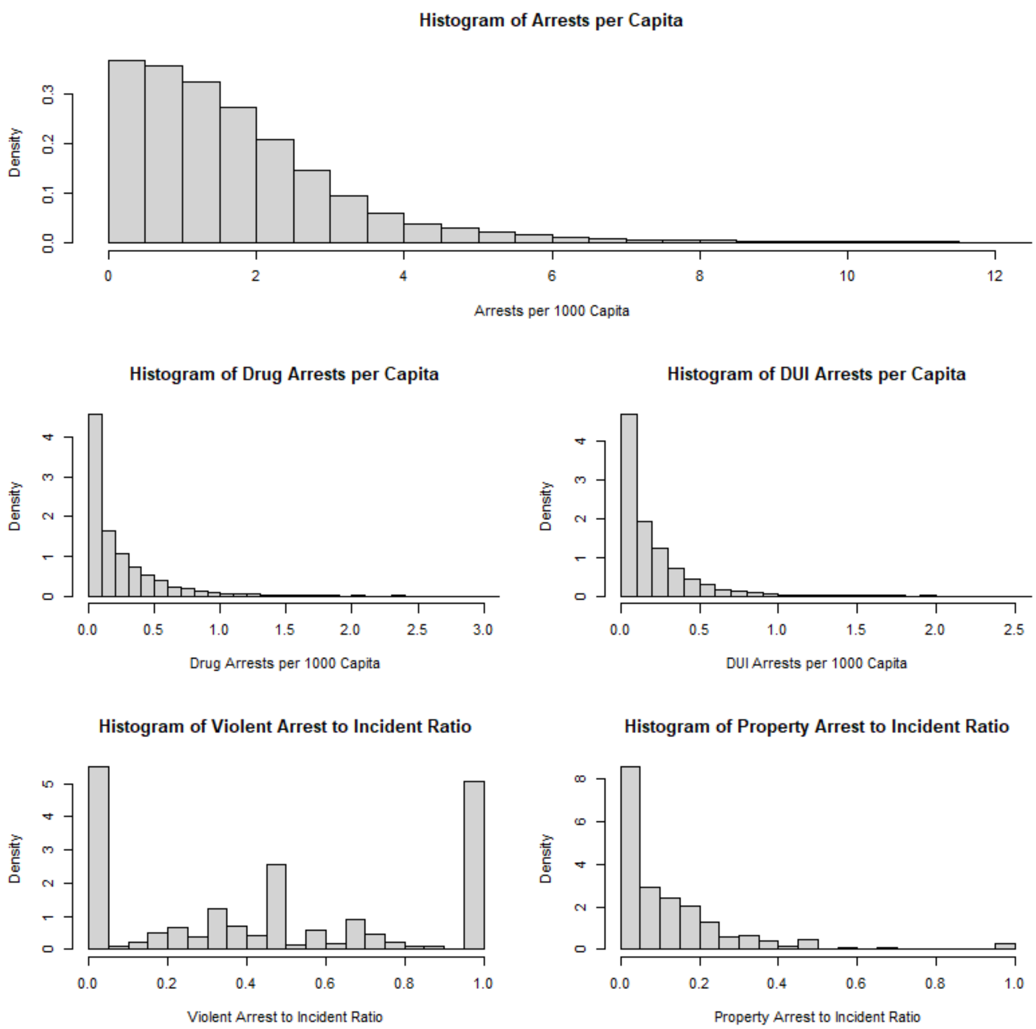
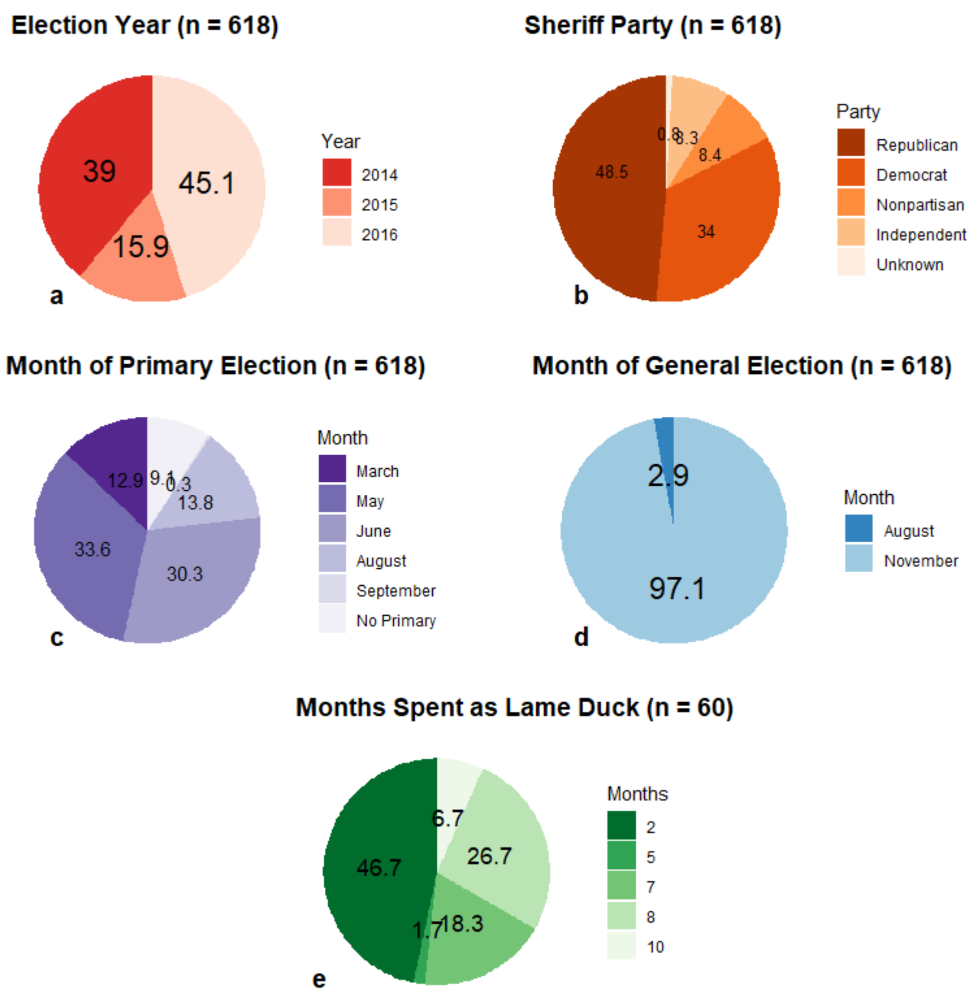


Figure 1.2: Pie Charts Representing Election Data

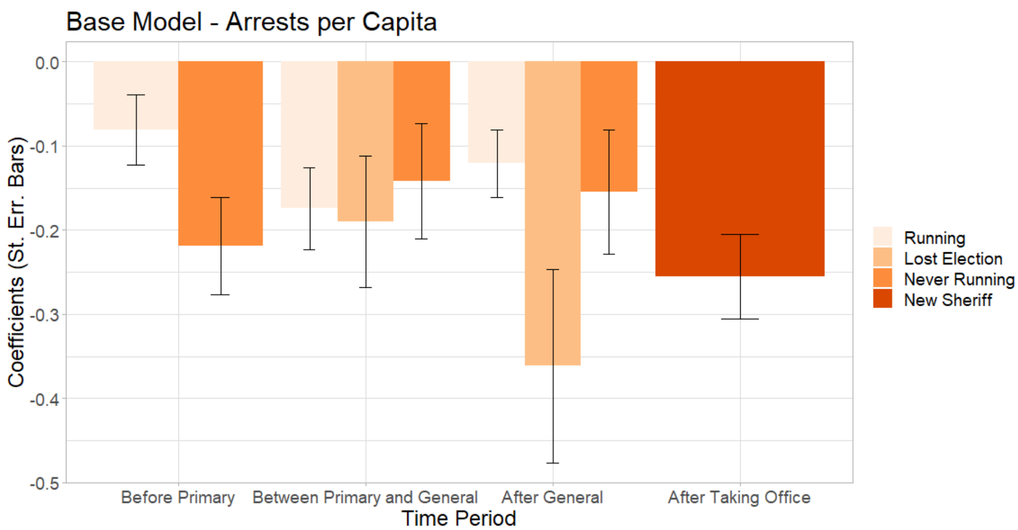


Notes: a: Each portion represents the percentage of elections in my data sample taking place in each year. b: Each portion represents the percentage of elections in my sample where the sheriff affiliates with the indicated party. c: Each portion represents the percentage of elections in my sample with primaries in each month. d: Each portion represents the percentage of elections in my sample with generals in each month. e: Each portion represents the number of months each losing sheriff in my sample spent as a lame duck.

Figure 1.3: Election Cycle Timeline

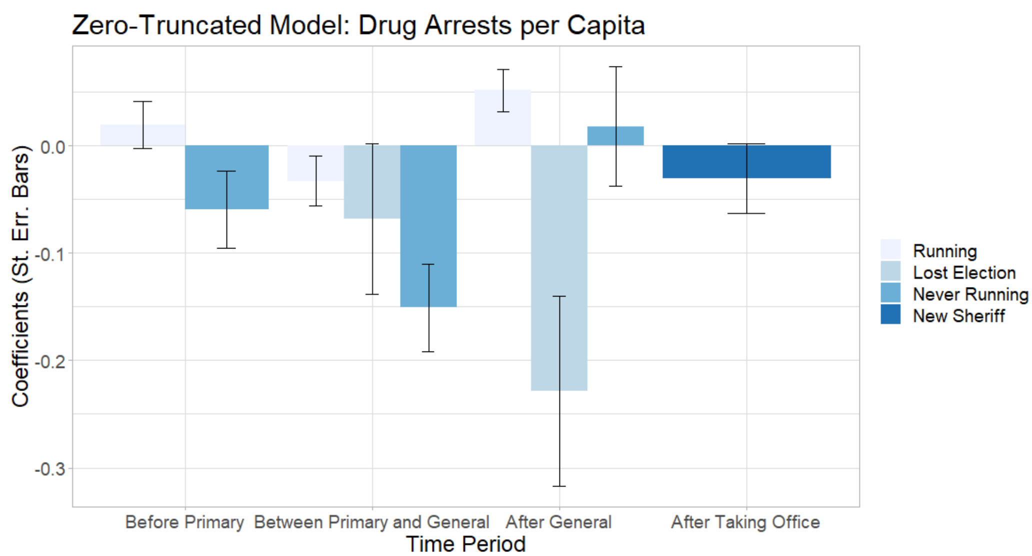
Notes: This figure depicts sheriffs during each time period in the election cycle. The primary election is in month (u), and the general election takes place in month (t). Each numerical unit is one month. In some specifications, those who lost the general election and lost the primary are included as the same indicator variable after the general election, as they would both be lame ducks with similar incentives.

Figure 1.4: Base Model Coefficients



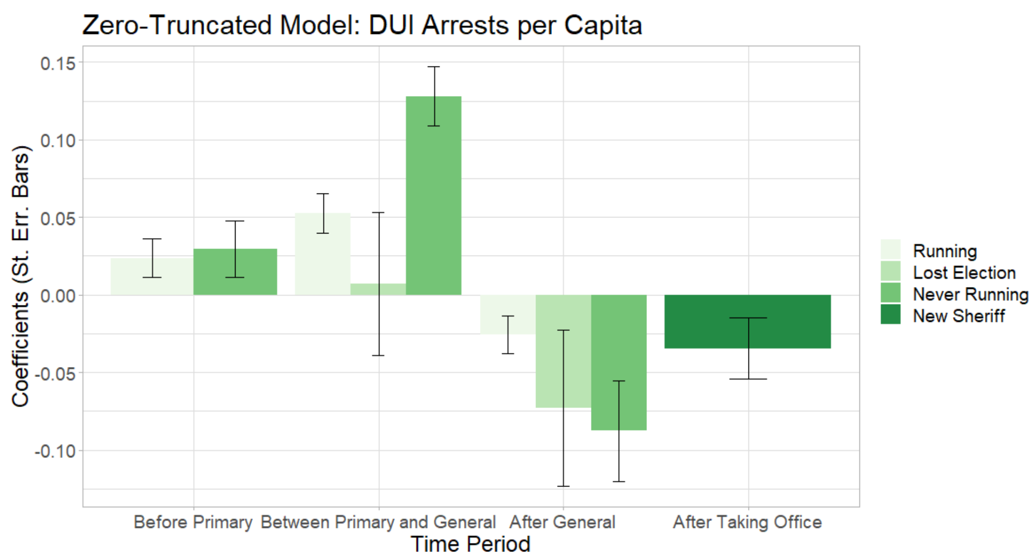
Notes: This figure shows the regression coefficients from the base model, (1.1), with standard error bars. The dependent variable is Arrests per Capita, and each section depicts a different time period for each variable. Standard error bars are robust and clustered by sheriff.

Figure 1.5: Hurdle Model Coefficients: Drug Arrests



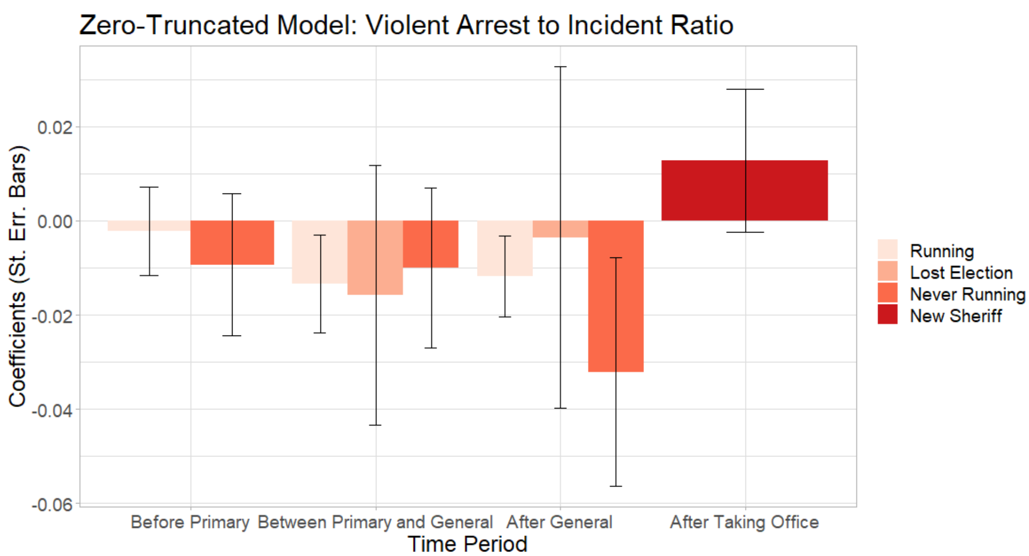
Notes: This figure shows the regression coefficients from the truncated regression portion of the hurdle model for drug arrests, with standard error bars. The dependent variable is Drug Arrests per Capita, and each section depicts a different time period for each variable.

Figure 1.6: Hurdle Model Coefficients: DUI Arrests



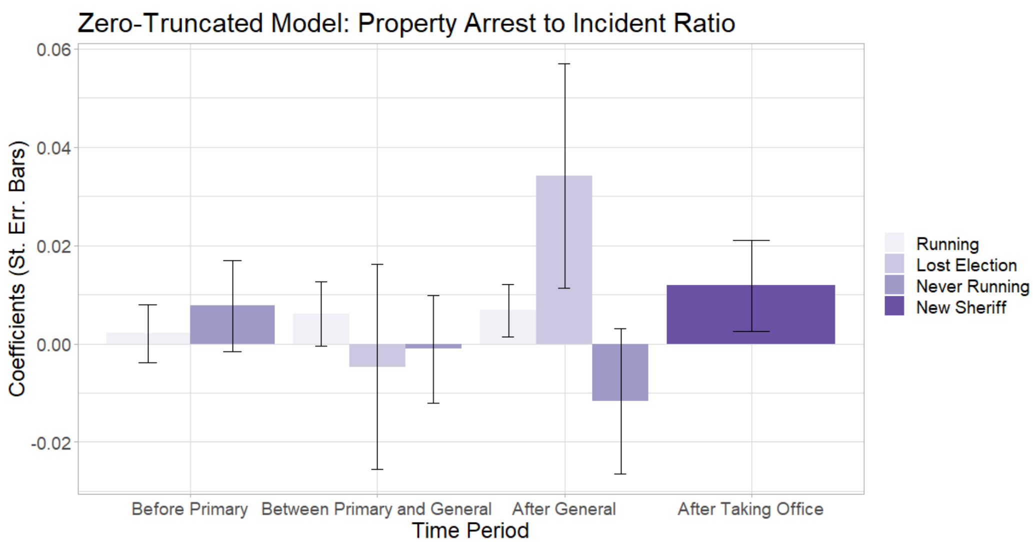
Notes: This figure shows the regression coefficients from the truncated regression portion of the hurdle model for DUI arrests, with standard error bars. The dependent variable is DUI Arrests per Capita, and each section depicts a different time period for each variable.

Figure 1.7: Hurdle Model Coefficients: Violent Arrests



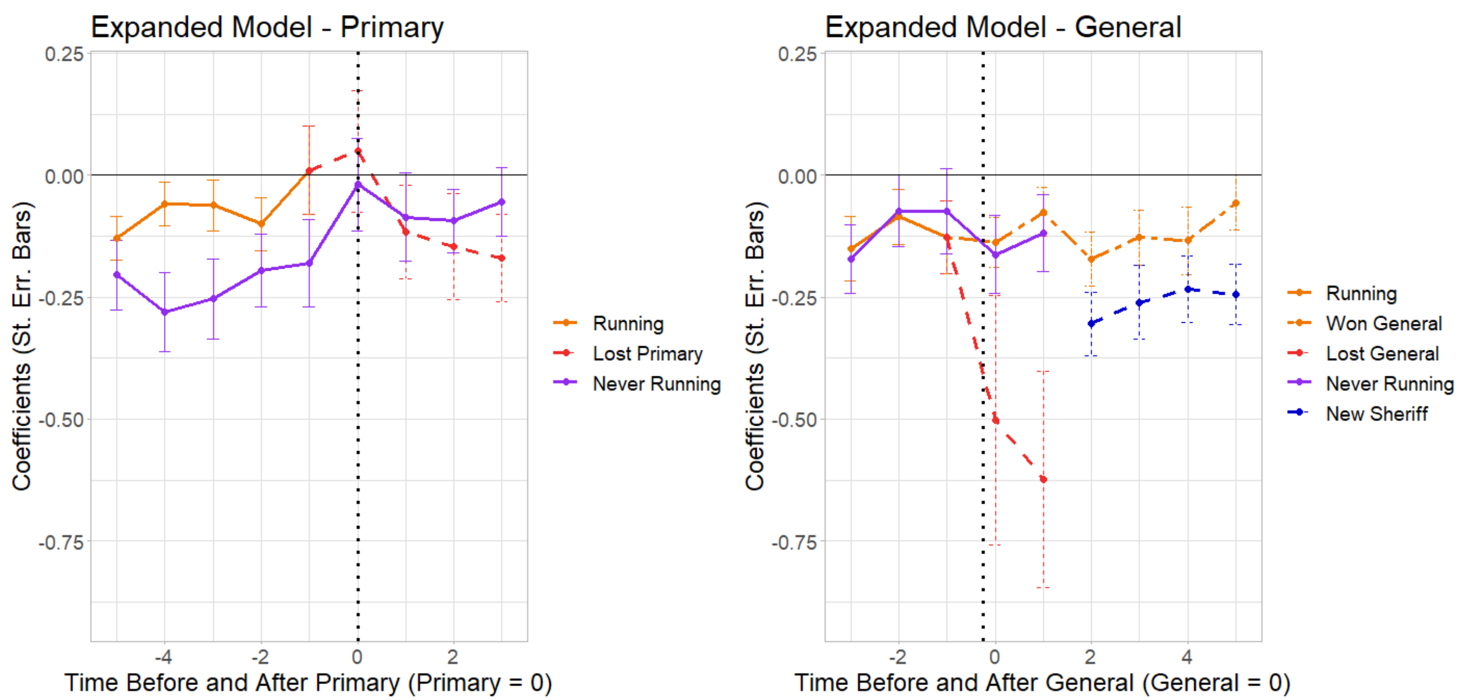
Notes: This figure shows the regression coefficients from the truncated regression portion of the hurdle model for violent arrests, with standard error bars. The dependent variable is the Violent Arrest to Incident Ratio, and each section depicts a different time period for each variable.

Figure 1.8: Hurdle Model Coefficients: Property Arrests



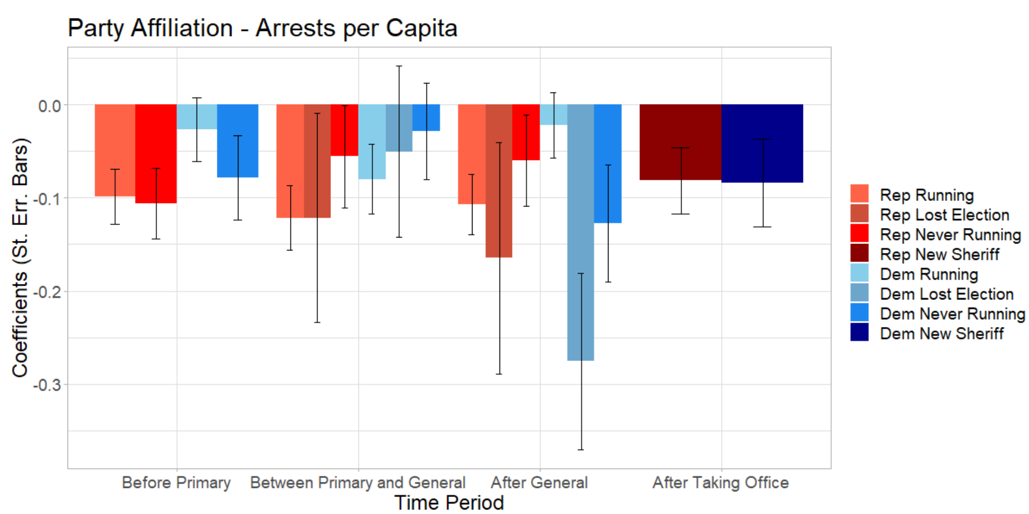
Notes: This figure shows the regression coefficients from the truncated regression portion of the hurdle model for property arrests, with standard error bars. The dependent variable is the Property Arrest to Incident Ratio, and each section depicts a different time period for each variable.

Figure 1.9: Expanded Model Coefficients



Notes: This figure plots the regression coefficients for the expanded model, (1.2), with standard error bars. The general elections are held at the beginning of the month, thus the dotted line representing the election is shifted to slightly before 0. Standard errors are robust and clustered by sheriff.

Figure 1.10: Base Model Coefficients: Party Extension



Notes: This figure shows the regression coefficients from the party affiliation extension with standard error bars. Coefficients correspond with Column 1 in Table 1.7. Standard errors are robust and clustered by sheriff.

CHAPTER 2

EFFICIENCY FORFEITED: A FRONTIER ANALYSIS OF LAW ENFORCEMENT AND REVENUE GENERATION

2.1 Introduction

One criticism of asset forfeiture is that forfeiture laws provide law enforcement agencies with the incentive to change police behavior in a way that generates more revenue for the agency. Any type of property, cash, etc. believed to be connected to an illegal activity can be taken away by a law enforcement officer, i.e. seized. Individuals have the right to contest seizures in a trial process, though the majority remain uncontested.[1] After seizure, the value from what is not destroyed can be used to fund drug education programs, school budgets, health departments, etc. (Williams, 2002; Holcomb et al., 2011). In a majority of states, some or all of the proceeds are allowed to be returned to the law enforcement agencies and used by the department, resulting in the "policing for profit" argument (Blumenson & Nilsen, 1998; Worrall & Kovandzic, 2008; Holcomb et al., 2011; Makowsky et al., 2019; Graham & Makowsky, forthcoming). Figure 2.1 shows the varying percent allowance of retained assets through forfeiture laws across the United States (US). Some states allow agency retention of 0 percent, while the remaining states have retention rates between 50 percent and 100 percent. For those agencies allowed to

[1] See the FBI's asset forfeiture description.

keep the majority of the proceeds from seized assets, there is a financial incentive to actively seek arrests for illegal activities typically resulting in seizures.

Previous literature analyzes the incentives from forfeiture laws for law enforcement agencies to gear their efforts toward revenue generation. In times of fiscal distress and when forfeiture laws allow the law enforcement agency to retain funds from seized assets, arrest rates increase for blacks and Hispanics committing drug, DUI, and prostitution violations (Makowsky et al., 2019). Local deficits compounded with the forfeiture laws affect the behavior of law enforcement officers and agencies (Makowsky et al., 2019). The behavior of law enforcement agents is also impacted through the federal equitable sharing program. State and local agencies have the ability to transfer forfeited assets to federal law enforcement. A percentage of the federal forfeiture fund can then be returned to state and local departments in the form of federal equitable sharing payments. Those agencies in states with more restrictive forfeiture laws tend to collect more in federal equitable sharing payments (Worrall & Kovandzic, 2008; Holcomb et al., 2011, 2018). These agencies are incentivized to use the federal equitable sharing program to maximize their revenue generation potential. Other evidence of forfeiture laws benefitting police departments through financial incentives includes higher police expenditures following an increase in property seizures (Benson et al., 1995). The agencies that seize more property have more money at their disposal. Similarly, Baicker & Jacobson (2007) find that local governments decrease the allocation of funds to police departments following a period of higher property seizures by those departments. The police departments respond by increasing drug arrest rates, as drug crimes are the most lucrative in terms of collecting cash seizures.

Forfeitures are not the only legal asset police departments have at their fingertips. They also use fines and fees to their monetary advantage (Garrett & Wagner, 2009; Makowsky & Stratmann, 2009; Goldstein et al., 2020; Harvey, 2020). And along with the fiscal incentives set by fines, fees, and forfeitures comes some ancillary effects, both negative and positive. Negative effects include shifted effort from violent and property crime clearances (Goldstein et al., 2020) to drug crimes (Mast et al., 2000; Baicker & Jacobson, 2007; Kelly & Kole, 2016), while positive effects include fewer traffic accidents (Makowsky & Stratmann, 2011) and safer driving (Harvey, 2020). For more information regarding fines, fees, and forfeitures and their effects on police incentives and behaviors, see Graham & Makowsky (forthcoming).

This paper uses non-parametric frontier analysis to estimate technical efficiency scores of law enforcement agencies at the county level. I then use a second-stage model to test the effects of civil asset forfeiture laws on the estimated efficiencies of law enforcement. Technical efficiency is defined as how effectively departments transform a set of police production inputs into desired police outputs, relative to other departments. Defined more precisely in Section 2.2, there are two common types of efficiency estimators used to measure law enforcement efficiency: the free-disposal hull (FDH) (Deprins & Simar, 1984) and data envelopment analysis (DEA) (Farrell, 1957; Banker et al., 1984) estimators. Both types of estimators use a set of inputs and outputs to estimate the efficiency scores. Minimal work in police efficiency exists using the FDH estimator. Drake & Simper (2003) compare the scores produced from the FDH estimator with those from stochastic frontier analysis and data envelopment analysis (DEA) estimators in English and Welsh police forces between 1996 and 1999. The DEA estimators require the assumption of convexity of the production frontier, while the FDH estimator is consistent under convexity or non-convexity. After Drake & Simper (2003) published their work, Kneip et al. (2015, 2016) provided central limit theorems and a statistic for testing convexity. Incorporating their work, I test for convexity to assist in determining the best estimator of law enforcement efficiency.

Research on police efficiency is much more expansive outside of the US. Thanassoulis (1995) analyzed police forces in England and Wales, and Carrington et al. (1997) studied the New South Wales Police Service. Sun (2002) and Wu et al. (2010) analyzed police forces in Taiwan, while research in Spain was done by Diez-Ticio & Mancebon (2002), García-Sánchez (2009), and García-Sánchez et al. (2013). It appears Nyhan & Martin (1999) and Gorman & Ruggiero (2008) are the only studies to examine US law enforcement using DEA. Nyhan & Martin (1999) examine the efficiency of 20 US cities and then use population and median income as environmental variables affecting the magnitude of the efficiency level in each city in a second-stage estimation. They use department cost and staff as inputs and total crimes, response time, and crime clear up rate as outputs in their estimation. Nyhan & Martin (1999) try to show both population and income are important environmental factors in determining the state's police efficiency by introducing them into the frontier estimation as “uncontrollable” inputs.

Gorman & Ruggiero (2008) evaluate the efficiency of the 48 continental US states and DC and use a three-stage DEA technique to examine the impact of environmental variables for the year 2002. They first estimate the DEA scores, and then they use standard OLS and Tobit models in the second stage to test the effect of several environmental variables on the efficiency estimates. Their third stage estimates a cost index. Gorman & Ruggiero (2008) use the number of sworn officers, the number of other employees, and the number of vehicles, while their outputs include the rate of murders, the rate of other violent crime, and the rate of all property crime. They find that roughly 70 percent of states are technically efficient. Their three-stage technique also leads them to believe several environmental factors, population being the most important, are causing the inefficiencies.

Simar & Wilson (2007) describe problems with multi-stage estimation on efficiency estimates. Environmental variables must be separable from the production set to obtain meaningful efficiency scores. This separability condition requires that the true production set or frontier cannot change because of the environmental variables. In other words, an environmental factor cannot be a reason the production frontier moves to another location in the input-output space. Gorman & Ruggiero (2008) do not examine the plausibility of their assumption of separability when they do their multi-stage estimations. Nyhan & Martin (1999) try to use their environmental variables as “uncontrollable” inputs, further decreasing the power of their model with a sample size of 20. This paper tests for the separability condition using the test introduced by Daraio et al. (2018).

A technique for dimension reduction was introduced by Wilson (2018). Several of the aforementioned law enforcement efficiency studies could have benefitted from this technique by reducing the dimensions of the problem and reducing estimation error. This paper takes advantage of the Wilson (2018) method to examine law enforcement efficiency.

In order to determine whether police departments capitalize on incentives to generate revenue, I split crime incidents into two categories: crime where the incident is reported by some victim, such as murder or theft, and crime where the incident is not victim-reported (e.g., drug crimes, etc.). Non-victim-reported crimes are the majority of crimes where police are able to seize property that can potentially be used for their department. By not including the non-victim-reported crimes in the efficiency estimation, I show that agencies are substituting away from solving victim-reported crimes, probably in favor of the potentially lucrative non-victim-reported crimes. The counties that are substituting away from victim-reported crimes should have lower efficiency scores.

Because of their nature, it is more difficult to alter the rate of arrests for victim-reported crimes, while victimless crime arrests are more easily manipulated. Law enforcement officers have an incentive to increase the rate of arrests for victimless crimes because of the potential for revenue generation. My hypothesis is that officers exposed to higher asset retention rates spend more time generating revenue through victimless crimes, thus decreasing the focus on victim-reported crime. The effort put forth by law enforcement officers is scarce, and thus it is expected that the counties in states with a 0 percent asset forfeiture rate should more efficiently solve incidents reported by a victim.

2.2 Methodology

The goal of this paper is to estimate the effect of civil asset forfeiture laws on estimated law enforcement technical efficiencies. To first estimate the police efficiency scores, I use non-parametric frontier analysis. Technical efficiency scores signify how effectively a firm transforms their p inputs $\boldsymbol{x} \in \mathbb{R}^p_+$ into their q outputs $\boldsymbol{y} \in \mathbb{R}^q_+$, relative to the other firms, i.e. how close they are to the overall production frontier.

Assume a firm's production set

$$\Psi = \{(\boldsymbol{x}, \boldsymbol{y}) \mid \boldsymbol{x} \text{ can produce } \boldsymbol{y}\} \tag{2.1}$$

containing every feasible combination of inputs and outputs. This paper uses the following standard assumptions about production (Shephard, 1970; Färe, 1988; Simar & Wilson, 2000b). The production set is assumed closed, and any production requires the use of some positive amount of inputs, i.e. $(\boldsymbol{x}, \boldsymbol{y}) \notin \Psi$ if $\boldsymbol{x} = 0$ and $\boldsymbol{y} \geq \boldsymbol{0}$, $\boldsymbol{y} \neq \boldsymbol{0}$, where inequalities hold element by element here and throughout. Free-disposability is also assumed for both inputs and outputs; if $(\boldsymbol{x}, \boldsymbol{y}) \in \Psi$, then for any $(\boldsymbol{x'}, \boldsymbol{y'})$ such that $\boldsymbol{x'} \geq \boldsymbol{x}$ and $\boldsymbol{y'} \leq \boldsymbol{y}$, $(\boldsymbol{x'}, \boldsymbol{y'}) \in \Psi$.

Technical efficiency of a firm is measured by the distance to the estimated production frontier in the input-output space in one of several directions. Most commonly, efficiency is measured in either the horizontal input direction, or the vertical output direction. In the input direction, the efficiency measure proportionately scales all inputs to their minimum level, keeping outputs constant. Similarly, in the output direction, the efficiency measure proportionately scales all outputs to their maximum level, given some level of inputs. Given that police departments in the county have their input levels predetermined by officials outside the department, I employ the output distance function. It makes more sense to ask how police outputs can be maximized given an already chosen level of police inputs, rather than asking how police inputs can be minimized given a fixed level of police outputs.

The Farrell (1957) output measure of technical efficiency is defined as

$$\theta(\boldsymbol{x}, \boldsymbol{y}) = \sup\{\theta \mid (\boldsymbol{x}, \theta\boldsymbol{y}) \in \Psi\}. \tag{2.2}$$

Technical efficiency is achieved when $\theta(\boldsymbol{x}, \boldsymbol{y}) = 1$. The firm is considered technically inefficienct when $\theta(\boldsymbol{x}, \boldsymbol{y}) > 1$. This measure of technical efficiency proportionately scales all outputs, holding inputs constant.

Proposed by Deprins & Simar (1984), the free-disposal hull (FDH) estimator estimates the true frontier Ψ by

$$\widehat{\Psi}_{FDH}(S_n) = \{(\boldsymbol{x}, \boldsymbol{y}) \in \mathbb{R}_{+}^{p+q} \mid \boldsymbol{y} \leq Y_i, \boldsymbol{x} \geq X_i, (X_i, Y_i) \in S_n\}, \tag{2.3}$$

where $(S_n) = \{(X_i, Y_i)\}_{i=1}^{n}$ is the data sample. The FDH estimator makes no assumption of the convexity of Ψ. The data envelopment estimators (DEA) (Farrell, 1957; Banker et al., 1984), on the other hand, assume convexity of the true frontier. Most common in the literature is the variable returns to scale (VRS) DEA estimator, estimated by

$$\begin{aligned}\widehat{\Psi}_{VRS}(S_n) = \{(\boldsymbol{x}, \boldsymbol{y}) \in \mathbb{R}_+^{p+q} \mid \boldsymbol{y} \leq \sum_{i=1}^{n} \gamma_i Y_i, \\ \boldsymbol{x} \geq \sum_{i=1}^{n} \gamma_i X_i, \sum_{i=1}^{n} \gamma_i = 1, \gamma_i \geq 0 \; \forall i = 1, ..., n\}\end{aligned} \tag{2.4}$$

and the convex hull of $\widehat{\Psi}_{FDH}(S_n)$. Similarly, the constant returns to scale (CRS) DEA estimator is estimated by (2.4), but relaxes the $\sum_{i=1}^{n} \gamma_i = 1$ constraint, and is the conical hull of $\widehat{\Psi}_{FDH}(S_n)$. Then for any $j \in \{FDH, VRS, CRS\}$, the Farrell (1957) output measure $\theta(\boldsymbol{x}, \boldsymbol{y})$ is estimated with $\widehat{\theta}_j(\boldsymbol{x}, \boldsymbol{y} \mid S_n)$ by replacing Ψ in (2.2) with $\widehat{\Psi}_j(S_n)$.

The convergence rate for such estimators is n^{κ}. Depending on the estimator,

$$\kappa = \begin{cases} \frac{1}{p+q} & \text{under FDH} \\ \frac{2}{p+q+1} & \text{under VRS} \\ \frac{2}{p+q} & \text{under CRS} \end{cases}, \tag{2.5}$$

and thus the convergence rate clearly depends on the number of inputs p and outputs q—the curse of dimensionality. In order to reduce estimation error, I use Wilson's (2018) method of dimension reduction. Raising the number of inputs or outputs exacerbates bias and causes a larger number of firms to lie on the estimated frontier (Wilson, 2018). Wilson (2018) shows strong evidence for reduced estimation error via dimension reduction in many circumstances. The reduction technique exploits the probable multicollinearity between inputs and outputs to decrease the dimensions by eigensystem decomposition. Though any subset of total inputs or outputs can be reduced, in this paper the total number of police inputs and outputs are decreased to $p = q = 1$. The reduced input is a weighted average of inputs, while the reduced output is a weighted average of outputs. For each of the inputs and the outputs, the ratio of the largest eigenvalue to the sum of all eigenvalues for the eigenvalue decomposition of the standardized moment matrices corresponds to the amount of linear, independent information captured in the reduction. That ratio for each of the inputs is given by

$$R_X = \frac{\lambda_{x_1}}{\sum_{k=1}^{p} \lambda_{x_k}}, \tag{2.6}$$

where λ_{x_k} is the k^{th} eigenvalue from the decomposition of the moment matrix. An R_Y value can be calculated similarly for the outputs. Wilson (2018) runs several simulations to determine the trade-off between reduced estimation error and loss of information for varying sample sizes and dimensions. His results show dimension reduction becomes beneficial for R_X and R_Y values in the 0.75–0.80 range for my sample sizes. Finding values above that range for both R_X and R_Y, I reduce the dimensions for my sample to $p = q = 1$. To calculate the reduced input and output, I use the reduce.dim() function in Wilson's (2008) Frontier Efficiency Analysis with R (FEAR) package in R.

Consideration also needs to be taken for bias in efficiency estimates. Noted in Simar & Wilson (1998) and Simar & Wilson (2000a), efficiency estimates from DEA estimation are inherently biased, as they are estimates based on an unobserved true frontier. Simar & Wilson (2000a) provide a general method to estimate the inherent bias in DEA efficiency estimates for each observation. I use the boot.sw98() function in Wilson's (2008) FEAR package in R setting CI.TYPE $= 2$ to use this method to calculate the bias-corrected efficiency estimates for use in the second stage model.

2.2.1 Test for Convexity of the Production Set

Kneip et al. (2015, 2016) provide central limit theorems for inference and develop a statistic for testing convexity of the production frontier. The test statistic described in Kneip et al. (2016) and Simar & Wilson (2020) compares means of the FDH estimates to the VRS estimates over N sample splits. The FDH estimator allows for non-convexity or convexity, and is thus consistent in both cases, while the VRS estimator is consistent only under convexity. The null hypothesis of convexity can be rejected if there is a sufficiently large difference in the bias-adjusted means. The sample is randomly split into two groups of observations with sample sizes n_1 and n_2. Since the test is heavily dependent on this random split, I employ Simar & Wilson's (2020) bootstrap method for multiple sample splits. Following Kneip et al. (2016), since the FDH estimator and the VRS estimator have different rates of convergence, n_1 and n_2 are chosen so as to offset the differences. Referring back to (2.5) and solving $n_1^{2/(p+q+1)} = n_2^{1/(p+q)}$ and $n_1 + n_2 = n$ by simple bisection yields the appropriate sample sizes.

The VRS estimator is used on sample 1, S_{1,n_1}, while the FDH estimator is used on the larger sample 2, S_{2,n_2}, since the FDH estimator has the slower convergence rate. Estimating the Farrell (1957) output measure of efficiency for each sample, let the sample mean and sample variance of each group be

$$\widehat{\mu}_{j,n_\ell} = n_\ell^{-1} \sum_{(X_i,Y_i)\in S_{\ell,n_\ell}} \widehat{\theta}_j(X_i, Y_i \mid S_{\ell,n_\ell}) \tag{2.7}$$

and

$$\widehat{\sigma}^2_{j,n_\ell} = n_\ell^{-1} \sum_{(X_i,Y_i)\in S_{\ell,n_\ell}} \left[\widehat{\theta}_j(X_i, Y_i \mid S_{\ell,n_\ell}) - \widehat{\mu}_{j,n_\ell}\right]^2 \tag{2.8}$$

for estimator $j \in \{VRS, FDH\}$ and $\ell \in \{1, 2\}$.

Kneip et al. (2016) describe two different test statistics that depend on the value of $(p+q)$. Since I use the dimension reduction technique, I have $(p+q) = 2$, so I require the use of the convexity test statistic for $(p + q) \leq 3$,

$$\widehat{\tau}_C = \frac{(\widehat{\mu}_{FDH,n_2} - \widehat{\mu}_{VRS,n_1}) - (\widehat{B}_{FDH,\kappa,n_2} - \widehat{B}_{VRS,\kappa,n_1})}{\sqrt{\frac{\widehat{\sigma}^2_{FDH,n_2}}{n_2} + \frac{\widehat{\sigma}^2_{VRS,n_1}}{n_1}}} \xrightarrow{d} N(0, 1), \tag{2.9}$$

which converges in distribution to the standard normal distribution by Theorem 4.2 in Kneip et al. (2015). The bias for each sample, $\widehat{B}_{j,\kappa,n_\ell}$, is estimated using the generalized jackknife method described in Kneip et al. (2016).

The convexity statistic tests H_0: Ψ convex versus H_1: Ψ not convex. For each test, I use the test.convexity() function in Wilson's (2008) FEAR package in

R, using 10 sample splits with 1000 replications each. The test statistic is averaged over all sample splits, and the p-values are estimated via the bootstrap method in Simar & Wilson (2020).

2.2.2 Test for Returns to Scale of the Production Set

If I fail to reject convexity, then I also need to test for the returns to scale of the production function. Kneip et al. (2016) and Simar & Wilson (2020) describe such a test statistic, similar to the convexity test statistic. Here, the statistic compares means of the VRS estimates with the means of the CRS estimates. If the difference is sufficiently large, then the null of CRS is rejected in favor of VRS. I again employ the N sample split method of Simar & Wilson (2020). The returns to scale test statistic is

$$\widehat{\tau}_{RTS} = \frac{(\widehat{\mu}_{VRS,n_1} - \widehat{\mu}_{CRS,n_2}) - \left(\widehat{B}_{VRS,\kappa,n_1} - \widehat{B}_{CRS,\kappa,n_2}\right)}{\sqrt{\left(\frac{\widehat{\sigma}^2_{VRS,n_1}}{n_1}\right) + \left(\frac{\widehat{\sigma}^2_{CRS,n_2}}{n_2}\right)}} \xrightarrow{d} N(0,1), \quad (2.10)$$

where the sample means $\widehat{\mu}_{j,n_\ell}$ and sample variances $\widehat{\sigma}^2_{j,n_\ell}$ are computed as in (2.7) and (2.8) for $j \in \{VRS, CRS\}$ and $\ell \in \{1,2\}$. Bias estimates $\widehat{B}_{j,\kappa,n_\ell}$ are again calculated using the generalized jackknife method described in Kneip et al. (2016).

The returns to scale statistic tests H_0: CRS versus H_1: VRS. For each test, I use the test.rts() function in Wilson's (2008) FEAR package in R, using 10 sample splits with 1000 replications each. The test statistic is averaged over all sample splits, and the p-values are estimated via the bootstrap method in Simar & Wilson (2020).

2.2.3 Test of Separability

In this paper, I test the effects of an environmental variable, state forfeiture law percentages, on the efficiency estimates. In order to do this, the “separability“ condition discussed in Simar & Wilson (2007) must hold. There are two ways an environmental variable can impact production. An environmental variable can either affect the distribution of efficiency estimates, or it can affect the production frontier itself. The first case is an ideal situation where separability is satisfied and second-stage regressions can be used for inference. For the second case, inference cannot be made with second-stage regressions.

Daraio et al. (2018) introduce a statistic to test the aforementioned separability condition. Similar to the tests of convexity and returns to scale, the idea is to test the difference in the sample means of the unconditional and the conditional estimators. The unconditional efficiency estimator is the sample mean of the estimates unconditional on the environmental variable, forfeiture law retention percentages. The conditional efficiency estimator is the sample mean of the efficiency estimates, conditional on the forfeiture law percentages. For the conditional estimator, the production set is assumed dependent on the environmental variable. The null hypothesis for this test is separability (the production frontier is not affected by the environmental variable, and the unconditional and the conditional sample means are equal), while the alternative is that the environmental variable is non-separable (the environmental variable affects the production frontier, and the difference between the unconditional and the conditional sample means is sufficiently large).

From Daraio et al. (2018), the test statistic for values of $(p + q)$ such that $\kappa \geq 1/3$ for FDH estimators or $\kappa \geq 2/5$ for DEA estimators is

$$\widehat{\tau}_{sep} = \frac{(\widehat{\mu}_{n_1} - \widehat{\mu}_{c,n_{2,h}}) - (\widehat{B}_{\kappa,n_1} - \widehat{B}^c_{\kappa,n_{2,h}})}{\sqrt{\left(\frac{\widehat{\sigma}^2_{n_1}}{n_1}\right) + \left(\frac{\widehat{\sigma}^{2,h}_{c,n_2}}{n_{2,h}}\right)}} \xrightarrow{d} N(0,1), \tag{2.11}$$

where $\widehat{\mu}_{n_1}$ and $\widehat{\mu}_{c,n_{2,h}}$ are the unconditional and the conditional sample means for either the FDH or DEA estimators (using the same estimator for both samples). The estimated unconditional and conditional sample variances are $\widehat{\sigma}^2_{n_1}$ and $\widehat{\sigma}^{2,h}_{c,n_2}$. The unconditional and conditional bias estimates, $\widehat{B}_{\kappa,n_1}$ and $\widehat{B}^c_{\kappa,n_{2,h}}$, are again estimated using the generalized jackknife method described in Kneip et al. (2016). Estimates of the conditional estimator require a smoothed estimator, which uses a bandwidth h for each environmental variable. See Daraio et al. (2018) for more information.

The separability test statistic tests the null hypothesis of separability against non-separability. For each separability test, I use the test.sep.cont() function in Wilson's (2008) FEAR package in R with 1000 replications. The p-values are then estimated by the bootstrap method in Simar & Wilson (2020).

2.2.4 Second-Stage Empirical Strategy

The second-stage model estimates the effect of the environmental variable, state civil asset forfeiture law percentages, on the estimated technical efficiencies using a left-truncated model. Note that the second-stage model is only meaningful if the separability condition holds. Simar & Wilson (2007) mention problems with using simple OLS estimation and suggest a truncated model instead. Using maximum-likelihood estimation, I estimate

$$\widehat{\theta}_{ist} = \boldsymbol{Z}_{ist}\boldsymbol{\beta} + \sigma \frac{\phi\left(\frac{1-\boldsymbol{Z}_{ist}\boldsymbol{\beta}}{\sigma}\right)}{1-\Phi\left(\frac{1-\boldsymbol{Z}_{ist}\boldsymbol{\beta}}{\sigma}\right)} + \varepsilon_{ist}, \tag{2.12}$$

where

$$\boldsymbol{Z}_{ist}\boldsymbol{\beta} = \beta_0 + \beta_1 RetentionRate_s \tag{2.13}$$

for county i in state s and year t. Left-truncation occurs at one, and the second stage truncated model assumes separability of the environmental variables from the technical efficiency. The efficiency estimates are denoted by $\widehat{\theta}_{ist} \in [1, \infty)$, the standard normal density and distribution are denoted by ϕ and Φ, and the error term is ε_{ist}. The truncated model is estimated using the truncreg command in Stata.

The variable $RetentionRate_s$ is the percentage of revenue from forfeited assets that law enforcement agencies in the state are allowed to retain for their agency. The retention rate varies at the state level, so I cannot include state or county fixed effects. The rates also do not change over time, thus I exclude time fixed effects. I estimate (2.12) for each population quintile, including two specifications—one that does not include $log(population_{ist})$ and one that does.

2.3 Data

According to the US Bureau of Justice Statistics, law enforcement is for the ”prevention, detection, and investigation of crime, and the apprehension and detention of individuals suspected of law violation.“[2] Previous research in police efficiency has used various measures of inputs $\boldsymbol{x}$ and outputs $\boldsymbol{y}$ to analyze how effectively police departments adhere to these duties. Inputs tend to include measures of labor and capital, while outputs are mainly the number or percentage of solved crimes (clearance measures). García-Sánchez et al. (2013) analyze the common inputs and outputs used in DEA estimation of police efficiency and conclude the most common inputs used are the number of police members, number of vehicles, police costs, and the number of different crimes committed. The most common outputs include the number or percentage of solved crimes with varying specifications. Following the majority of existing work, this paper uses the rate of police employees per 1000 people in the county (including civilian and sworn officers) and the police expenditure per 1000 people as the two inputs. I use the rates per 1000 people as a way to control for the population in the counties. For outputs, I add to the existing literature by using both a measure of clearances and a measure of deterrence. Two outputs constitute the clearance measures—the arrest to reported incident ratios for violent crimes and for property crimes. Two more out-

[2] See https://www.bjs.gov/index.cfm?ty=ty=tp&tid=7.

puts constitute the deterrence measures—the rates of incidents per capita for violent crimes and for property crimes. I standardize each variable by subtracting the minimum value from each observation and dividing by the range. Then, since incidents per capita should have a negative relationship with a county's law enforcement efficiency—having fewer crimes would make the county more efficient—I take the additive inverse of the standardized observations for both the violent and property incidents per capita variables and add one. Using both the clearance and deterrence measures as outputs, my efficiency estimates will be more versatile compared to others used in the literature. I then use the dimension reduction technique to reduce the total of two inputs and four outputs to one input and one output.

The data come from the US Census Bureau's Census of Governments, the FBI's Uniform Crime Report (UCR), and the FBI's National Incident-Based Reporting System (NIBRS). Pierson et al. (2015) create a cleaned database for all the available Census of Governments data. I pull the 2007 and 2012 data from their database to obtain total police expenditure by county. The Census of Governments collects comprehensive state and local finance data from individual governmental units every five years (COG, 1977–2012). The governmental units include the county, city, township, and special district. I aggregate the governmental units to get a measure of police expenditure at the county level. The police expenditures include spending on current operations, construction, and other capital outlay.

The number of employed law enforcement officers is taken from the Law Enforcement Officers Killed and Assaulted (LEOKA) data collection from the

UCR (FBI, 2007, 2012a). The LEOKA collection reports data at the agency level, and I again aggregate to the county level to match the expenditure data.

Arrest and reported incident data are from the NIBRS database (FBI, 2007, 2012b). I calculate the arrest to reported incident ratio for each of two groups of incidents. Violent incidents include murder and nonnegligent manslaughter, negligent manslaughter, justifiable homicide, kidnapping, rape, sodomy, sexual assault with an object, forcible fondling, robbery, and aggravated assault. Incidents of property crime are arson, burglary/breaking and entering, pocket-picking, purse-snatching, shoplifting, theft, and all other larceny. Each incident in NIBRS is recorded with a primary, secondary, and tertiary incident. I look at the primary incident but exclude any incidents where the secondary or tertiary incident is a victimless crime. As an example, if the primary incident is manslaughter while the secondary incident is a drug violation, the incident observation is not counted.

The NIBRS file has an observation for each incident reported from each reporting agency, and each department has an agency-specific Originating Agency Identifier (ORI). For both the violent crimes and the property crimes, the total number of incidents and arrests is combined at the agency level by the ORI code. To aggregate the employment and arrest data by FIPS code, and to then combine with the expenditure data, I use the Law Enforcement Agency Identifiers crosswalk series (ICPSR, 2012). After merging all data and dropping the outliers as discussed in Section 2.2, I have a total of 2,478 counties coming from 2007 and 2012. It is important to note that each county-year observation is treated as a separate firm, e.g. Platte County, NE in 2007 is different than Platte County, NE in 2012. This means there is no captured information when comparing the same county in 2007 to 2012.

I use asset forfeiture rates from Holcomb et al. (2011). They collect the asset retention rates for each state in the US. Refer back to Figure 2.1 for a heat map of the retention rates in the data. The majority of states have a 100 percent retention rate. There are no states with retention rates between 0 percent and 50 percent. I do not have data for the hashed states in the figure.

Data are separated into population quintiles. Tables 2.1–2.5 show summary statistics for each subsample.[3] As previously discussed, I include the expenditure per capita and employment per capita as inputs. Outputs are the violent and property arrest to incident ratios and the violent and property incidents per capita. In addition, Figure 2.2 plots the data for the full sample and for each population quintile. The x-axes for all plots are the reduced inputs and the y-axes are the reduced outputs, calculated by the dimension reduction technique for each subsample. The final subsamples exclude outliers detected by Wilson's (1993) method for outlier detection for non-parametric frontier models.[4] The first quintile includes counties with populations from 578–9,301.2, the second quintile includes counties with populations from 9,301.2–18,019.6, the third quintile includes counties with populations from 18,019.6–33,805.6, the fourth quintile includes counties with populations from 33,805.6–79,016.8, and the fifth quintile includes counties with populations from 79,016.8–4,538,028.

[3]Summary statistic tables are all created with the Stargazer package in R by Hlavac (2018).
[4]I use the ap() function in Wilson's (2008) FEAR package in R to detect the outliers.

2.4 Results

The R values, in (2.6), from the full sample dimension reduction technique are shown in the first row of Table 2.6. An R_X of 0.904 and R_Y of 0.967 are well above Wilson's (2018) suggested range of 0.75–0.80. Following the dimension reduction, I test for convexity, returns to scale, and separability of the full sample. The first row in each panel of Table 2.7 shows the results of each of these tests for the full sample. I fail to reject convexity with a p-value of 0.228, thus pointing away from the FDH estimator and toward DEA estimators. Because the convexity test shows the production frontier of my data is probably convex, I test the returns to scale. For the full sample, CRS is strongly rejected in favor of VRS. Given the results of these two tests for the full sample, I use the VRS estimator to test the separability of the retention rate variable. The first row in the bottom panel shows the result from this test, failing to reject separability. For the separability test in the second row, I include both the retention rate variable and population as environmental variables in the same test. The test rejects separability when I include population. Since separability is rejected when population is included, but fails to be rejected for forfeiture laws alone, I take the following strategy. First, I separate the data into population quintiles. Then, for each of the population quintiles, I estimate the bias-corrected VRS estimates. I next incorporate the truncated regression model, and estimate (2.12) with two different specifications for each quintile.

Before estimating the truncated regression, I confirm the convexity, returns to scale, and separability tests for each population quintile. These results are also shown in Table 2.7. Each test result is consistent with the test results from the full sample, so I use the VRS estimator to produce the bias-corrected technical efficiency estimates.[5] Table 2.8 summarizes the bias-corrected DEA efficiency scores. They are estimated separately for each group, allowing for different frontiers across the different population quintiles. These estimates are next used as the dependent variable in estimating (2.12).

Tables 2.9 and 2.10 show the truncated regression results for each population quintile. In both sets of results, the dependent variables are all the bias-corrected DEA efficiency estimates. In Table 2.9, I only include the forfeiture retention rate as the independent variable, while in Table 2.10, I also include the log of the population as an independent variable. Keep in mind that I rejected separability on the full sample when I added population as a second environmental variable, so the results in Table 2.10 should be taken with a grain of salt. Coefficients on the forfeiture retention rates are similar between the two specifications. The coefficients represent the marginal effects for the independent variables. The retention rate is statistically significant in all population quintiles except the second. Remember that a larger efficiency score means the county's law enforcement is less technically efficient, so the positive coefficients signify a decrease in efficiency. Referring to Table 2.9, a one percentage point increase in the retention rate leads to a 0.000739 point decrease in technical efficiency for the first quintile.

[5]Note the separability tests for each quintile test separability with the forfeiture retention rates as the only environmental variable.

Therefore, increasing a county's retention rate by one standard deviation, 30.55 percentage points, leads to a decrease in efficiency of 0.023 points. For the fifth quintile, increasing the county's retention rate by one standard deviation, 36.329 percentage points leads to a decrease in efficiency of 0.133 points. Given the standard deviations of the efficiency scores for these two population quintiles, 0.170 and 0.340, the effects are rather small, though larger in the fifth quintile—almost half of a standard deviation. Though across the board, the coefficients are positive, indicating that the counties that are not allowed to retain any of the forfeited assets are more technically efficient than those counties that are allowed to retain forfeited assets.

2.5 Conclusions

The results from the truncated regressions indicate there is a negative relationship between the forfeiture retention rate of a county and the efficiency of the law enforcement in that county. This is evidence supporting the theory that police output in the form of arrests and deterrence is depleted by the presence of civil asset forfeiture laws. The technical efficiency estimates are higher (less efficient) for the counties with higher forfeiture retention rates. Given a set amount of funding and officers, the counties with no ability to retain forfeited assets have higher levels of clearance and deterrence measures.

When counties are allowed to retain some percentage of the forfeiture revenues, they have the financial incentive to target crimes with a monetary payoff, most commonly drug crimes. In doing so, the violent and property arrest to incident ratios, as well as the amount of deterrence, appear to suffer. The results in this paper support the other evidence piling up in the ”policing for profit“ literature that financial incentives cause police agencies to change their behavior.

Table 2.1: Summary Statistics - First Population Quintile

Statistic	N	Median	Mean	St. Dev.	Min	Max
Expenditure/Cap	496	99.187	120.139	83.887	0.000	568.225
Employment/Cap	496	3.066	3.439	2.399	0.129	26.210
Viol A/I	496	500	532.210	304.528	0	1,000
Prop A/I	496	107.143	161.742	191.852	0.000	1,000
Viol I/Cap	496	1.320	1.593	1.182	0.109	8.342
Prop I/Cap	496	12.835	14.932	10.324	0.186	54.767
Red. Input	496	0.101	0.119	0.074	0.003	0.489
Red. Output	496	1.367	1.377	0.174	0.918	1.799
Retention Rate	496	100	84.744	30.550	0	100
Population	496	5,874	5,596.647	2,279.101	578	9,300

Notes: This table shows summary statistics for the first population quintile. Per capita is denoted as "/Cap", and the arrest to incident ratios are denoted as "A/I." The violent and property arrests to incident ratios and incidents per capita are all scaled by 1000. The reduced (Red.) input and output are the results of the dimension reduction technique discussed in Section 2.2. Retention Rate is the percent of forfeited assets retained by law enforcement agencies in that county.

Table 2.2: Summary Statistics - Second Population Quintile

Statistic	N	Median	Mean	St. Dev.	Min	Max
Expenditure/Cap	491	77.853	88.221	51.377	0.000	342.216
Employment/Cap	491	2.502	2.883	2.392	0.449	26.968
Viol A/I	491	500	496.450	226.198	0.000	1,000
Prop A/I	491	120	140.039	93.247	0.000	666.667
Viol I/Cap	491	1.655	1.960	1.523	0.062	12.979
Prop I/Cap	491	16.187	18.563	11.434	0.299	64.893
Red. Input	491	0.081	0.090	0.046	0.018	0.297
Red. Output	491	1.329	1.328	0.155	0.559	1.704
Retention Rate	491	100	91.601	23.201	0	100
Population	491	13,448	13,551.200	2,563.012	9,304	18,006

Notes: This table shows summary statistics for the second population quintile. Per capita is denoted as "/Cap", and the arrest to incident ratios are denoted as "A/I." The violent and property arrests to incident ratios and incidents per capita are all scaled by 1000. The reduced (Red.) input and output are the results of the dimension reduction technique discussed in Section 2.2. Retention Rate is the percent of forfeited assets retained by law enforcement agencies in that county.

Table 2.3: Summary Statistics - Third Population Quintile

Statistic	N	Median	Mean	St. Dev.	Min	Max
Expenditure/Cap	494	71.408	78.897	46.210	0.000	284.033
Employment/Cap	494	2.594	2.707	1.091	0.306	9.126
Viol A/I	494	429.2	441.797	193.041	0	1,000
Prop A/I	494	126.423	139.506	77.197	0.000	611.111
Viol I/Cap	494	2.122	2.552	2.068	0.033	16.460
Prop I/Cap	494	24.540	25.691	14.426	0.355	81.315
Red. Input	494	0.076	0.082	0.038	0.007	0.286
Red. Output	494	1.256	1.247	0.173	0.342	1.679
Retention Rate	494	100	86.775	29.261	0	100
Population	494	24,757.5	25,175.260	4,361.385	18,023	33,783

Notes: This table shows summary statistics for the third population quintile. Per capita is denoted as "/Cap", and the arrest to incident ratios are denoted as "A/I." The violent and property arrests to incident ratios and incidents per capita are all scaled by 1000. The reduced (Red.) input and output are the results of the dimension reduction technique discussed in Section 2.2. Retention Rate is the percent of forfeited assets retained by law enforcement agencies in that county.

Table 2.4: Summary Statistics - Fourth Population Quintile

Statistic	N	Median	Mean	St. Dev.	Min	Max
Expenditure/Cap	494	63.1	70.291	38.814	0	210
Employment/Cap	494	2.392	2.561	1.087	0.643	13.188
Viol A/I	494	389.267	402.885	167.091	0.000	1,000
Prop A/I	494	133.657	140.781	75.143	9.709	976.190
Viol I/Cap	494	2.198	2.551	2.031	0.034	16.543
Prop I/Cap	494	26.897	28.412	15.787	0.419	86.821
Red. Input	494	0.070	0.074	0.031	0.010	0.186
Red. Output	494	1.244	1.220	0.166	0.326	1.737
Retention Rate	494	100	76.164	39.629	0	100
Population	494	47,827.5	51,010.740	12,838.9	33,896	79,015

Notes: This table shows summary statistics for the fourth population quintile. Per capita is denoted as "/Cap", and the arrest to incident ratios are denoted as "A/I." The violent and property arrests to incident ratios and incidents per capita are all scaled by 1000. The reduced (Red.) input and output are the results of the dimension reduction technique discussed in Section 2.2. Retention Rate is the percent of forfeited assets retained by law enforcement agencies in that county.

Table 2.5: Summary Statistics - Fourth Population Quintile

Statistic	N	Median	Mean	St. Dev.	Min	Max
Expenditure/Cap	494	63.1	70.291	38.814	0	210
Employment/Cap	494	2.392	2.561	1.087	0.643	13.188
Viol A/I	494	389.267	402.885	167.091	0.000	1,000
Prop A/I	494	133.657	140.781	75.143	9.709	976.190
Viol I/Cap	494	2.198	2.551	2.031	0.034	16.543
Prop I/Cap	494	26.897	28.412	15.787	0.419	86.821
Red. Input	494	0.070	0.074	0.031	0.010	0.186
Red. Output	494	1.244	1.220	0.166	0.326	1.737
Retention Rate	494	100	76.164	39.629	0	100
Population	494	47,827.5	51,010.740	12,838.9	33,896	79,015

Notes: This table shows summary statistics for the fourth population quintile. Per capita is denoted as "/Cap", and the arrest to incident ratios are denoted as "A/I." The violent and property arrests to incident ratios and incidents per capita are all scaled by 1000. The reduced (Red.) input and output are the results of the dimension reduction technique discussed in Section 2.2. Retention Rate is the percent of forfeited assets retained by law enforcement agencies in that county.

Table 2.6: R Values

	R_X	R_Y
Full Sample	0.904	0.967
Q1	0.926	0.945
Q2	0.862	0.971
Q3	0.942	0.977
Q4	0.927	0.976
Q5	0.868	0.981

Notes: This table contains the R_X and R_Y values from the dimension reduction technique, defined in (2.6). Each Q is a population quintile.

Table 2.7: Initial Test Results

Convexity Test Results

	$\widehat{\tau}_C$	P-Value
Full Sample	-0.600	0.228
Q1	-0.870	0.441
Q2	0.516	0.175
Q3	-0.109	0.288
Q4	-5.357	0.782
Q5	-2.941	0.982

Returns to Scale Test Results

	$\widehat{\tau}_{RTS}$	P-Value
Full Sample	215.000***	0.000
Q1	92.022***	0.000
Q2	46.725***	0.000
Q3	66.421***	0.000
Q4	66.388***	0.000
Q5	32.844***	0.000

Separability Test Results

	$\widehat{\tau}_{sep}$	P-Value
Full Sample	0.131	0.560
Full Sample†	1.076***	0.010
Q1	-0.072	0.785
Q2	0.548	0.438
Q3	0.587	0.254
Q4	1.735	0.302
Q5	1.124	0.205

† The second full sample separability test includes both retention rate and population as environmental variables.

Notes: Significance is denoted at the 1%, 5%, and 10% level by *, **, and ***. This table shows the test results from the convexity, returns to scale, and separability tests for the full sample and each population quintile. In the top panel, the convexity test tests the null hypothesis of convexity against the alternative of non-convexity. In the middle panel, the returns to scale test tests the null hypothesis of CRS against the alternative of VRS. In the bottom panel, the separability test tests the null hypothesis of separability of the environmental variable(s) against the alternative of non-separability. The separability tests were estimated with VRS efficiency estimates.

Table 2.8: Bias-Corrected DEA Efficiency Estimates

	N	Min.	1Q	Median	Mean	St. Dev.	3Q	Max.
Full Sample	2478	1.010	1.282	1.390	1.437	0.281	1.517	5.276
Q1	496	1.008	1.215	1.317	1.331	0.170	1.446	1.975
Q2	491	1.009	1.196	1.289	1.310	0.179	1.387	3.057
Q3	494	1.017	1.218	1.318	1.364	0.290	1.415	4.945
Q4	494	1.033	1.311	1.399	1.459	0.309	1.525	5.495
Q5	496	1.021	1.287	1.429	1.502	0.340	1.618	4.599

Notes: This table summarizes the bias-corrected DEA efficiency estimates for the full sample and for each quintile. A county is technically efficient with a score of 1, and the farther from 1, the less technically efficient a county is.

Table 2.9: Truncated Regression Results

	Dependent Variable:				
	Bias-Corrected DEA Scores				
	Q1	Q2	Q3	Q4	Q5
Retention Rate	0.000739**	0.000124	0.00324*	0.00301***	0.00366***
	(0.000320)	(0.000499)	(0.00177)	(0.000758)	(0.000925)
Constant	1.248***	1.255***	0.537**	1.045***	1.014***
	(0.0296)	(0.0481)	(0.251)	(0.0846)	(0.100)
Sigma	0.188***	0.213***	0.529***	0.416***	0.459***
	(0.00805)	(0.0107)	(0.0566)	(0.0266)	(0.0296)
Observations	496	491	494	494	496
Log Likelihood	195.0536	180.1395	23.1953	-50.2023	-96.7128

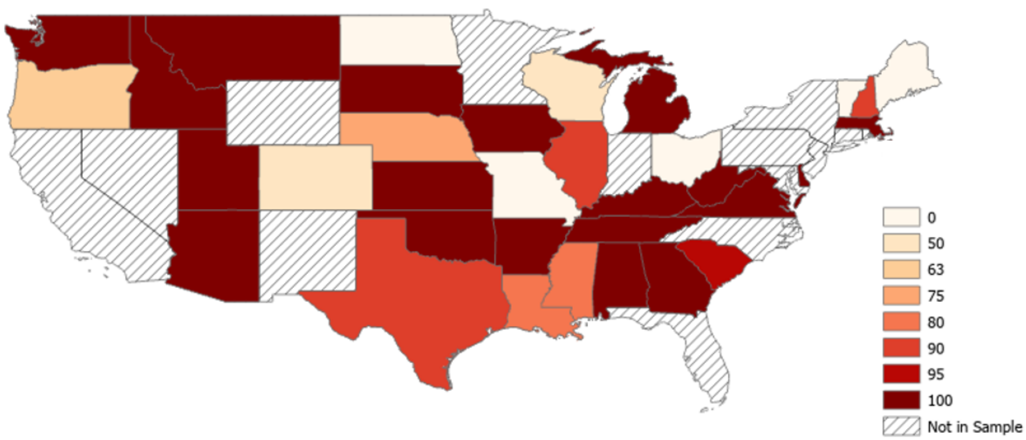
Notes: Significance is denoted at the 1%, 5%, and 10% level by ***, **, and *. The dependent variable in each column is the bias-corrected DEA efficiency score. Each column contains the observations from a different population quintile. Standard errors are in parentheses.

Table 2.10: Truncated Regression Results with Population

	Dependent Variable:				
	Bias-Corrected DEA Scores				
	Q1	Q2	Q3	Q4	Q5
Retention Rate	0.000650**	0.000127	0.00366**	0.00302***	0.00369***
	(0.000320)	(0.000499)	(0.00171)	(0.000755)	(0.000916)
log(population)	0.0408**	0.0131	0.690***	0.120	0.0688**
	(0.0185)	(0.0594)	(0.262)	(0.103)	(0.0348)
Constant	0.909***	1.131**	-6.448**	-0.250	0.174
	(0.158)	(0.567)	(2.750)	(1.127)	(0.448)
Sigma	0.187***	0.213***	0.515***	0.414***	0.455***
	(0.00798)	(0.0107)	(0.0530)	(0.0265)	(0.0291)
Observations	496	491	494	494	496
Log Likelihood	197.505	180.164	27.199	-49.528	-94.763

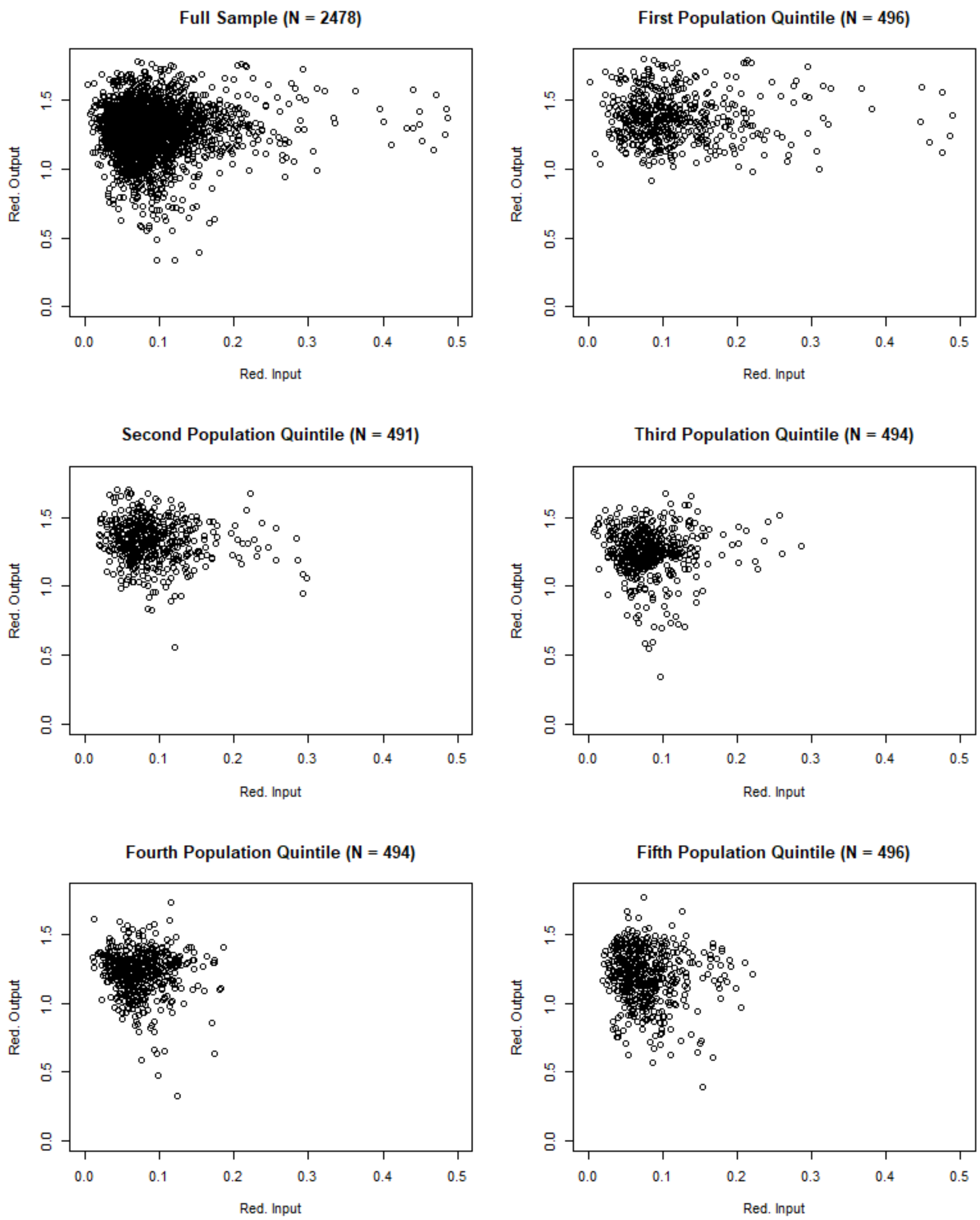
Notes: Significance is denoted at the 1%, 5%, and 10% level by ***, **, and *. The dependent variable in each column is the bias-corrected DEA efficiency score. Each column contains observations from a different population quintile. Standard errors are in parentheses. Note these regressions are only meaningful if the environmental variables are separable.

Figure 2.1: Forfeiture Retention Rates by State



Notes: This figure shows the percentage of forfeited assets that the agencies in the state can retain. The hashed states are not in my data sample, while increasing darkness indicates increasing retention percentages.

Figure 2.2: Data Plots for Sample and Subsamples



Notes: This figure plots the data observations for the full sample and each of the population quintiles. The reduced (Red.) input and output resulting from the dimension reduction technique are plotted on the x- and y-axes.

CHAPTER 3

LOCAL GOVERNMENT DEPENDENCE ON CRIMINAL JUSTICE REVENUE AND EMERGING CONSTRAINTS[1]

3.1 Introduction

The elected officials managing local governments in the United States are rarely given sufficient credit for the budgetary tightrope they must navigate. Dependence on property taxes directly connects their revenue base to the vagaries of both the local real estate market and the broader economy. They cannot inflate away their problems by printing money, and many states have tied their local governments to the fiscal mast, constitutionally committing them to tax and expenditure limits that can go so far as refunding every surplus dollar collected (Joyce & Mullins, 1991). Come election season, they will face voters that prefer higher spending, lower taxes, and a budget balanced by debt financing not always accessible for smaller municipal governments (Banzhaf & Oates, 2012). Even if they navigate all of these hurdles, should they fall short in their provision of preferred public goods or overshoot in the levied tax burden for key constituents, *their tax base may simply choose to leave* (Tiebout, 1956). Given these conditions, it should be expected that elected officials and their bureaucratic agents will welcome with open arms any and all sources of revenue unobstructed by constitutional and political constraints.

[1]The bulk of this chapter was written with Michael D. Makowsky and is forthcoming in the *Annual Review of Criminology* (Graham & Makowsky, forthcoming).

The criminal justice system, over the last thirty years, has become a common means by which local governments balance their budgets, with many municipalities going so far as to become dependent on fines and fees revenue to maintain solvency (Carpenter et al., 2019; Colgan, 2017a; Maciag, 2019; Makowsky, 2019). In a case study of Morrow, Riverdale, and Clarkston, Georgia, Carpenter et al. (2019) found that each collected between 14%–25% of their total revenues from fines and fees between 2012–2016. Revenues collected were predominantly from traffic and city ordinance violations that posed little to moderate risk to public safety, suggesting a strong revenue motivation from law enforcement. In the wake of the Department of Justice investigation into Ferguson, Missouri and the local government's fiscal dependence on fines and fees levied on African-Americans, the US Commission on Civil Rights identified 38 US city governments whose budgets were more dependent than Ferguson on similar revenues (US Commission on Civil Rights (USCCR), 2017); Maciag (2019) identified 284 such jurisdictions.

The 2012 Census of Governments reports local government fine and forfeiture revenues were equivalent to 7.4% of all law enforcement and court expenditures within the middle quintile of reporting counties (Liu et al., 2019). Local governments in the top 5% of counties were able to offset roughly half of these expenditures through criminal justice collections (see Figure 3.1). These numbers include all fines and penalties, as well as conviction-contingent fees. They do not, as classified, include the yield from confiscated property sales, processing fees, and supervision or incarceration fees, which are often far greater than the principal fines.[2] Baicker & Jacobson (2007) estimated that US Department of Justice and state seizures amount to roughly \$3 per capita on average (with a standard deviation of \$5). Including such revenues, we expect that far more local governments employ a police department that generates revenues in excess of costs. For this minority of local governments, law enforcement has become a source of revenue depended on for fiscal solvency.[3]

Not only are fiscal incentives prevalent at the municipality level, but they are also widespread in sheriff offices. Most commonly funded by county governments, sheriff offices may find themselves greedy and with an ability to cut as many corners as possible. However, since the sheriff is elected, he needs to find a balance between pleasing voters and taking advantage of his power and funding resources from the county and state.

Greenblatt (2018) analyzes sheriff power and enumerates tactics used to abuse that power. Sheriffs have been caught using only a small fraction of allotted

[2] Within the Census of Governments classification manual for reporting governments, revenues reported as fines and forfeitures (code U30) are directed to include "Revenue from penalties imposed for violations of law; civil penalties (e.g., for violating court orders); court fees if levied upon conviction of a crime or violation; court-ordered restitution to crime victims where government actually collects the monies; and forfeits of deposits held for performance guarantees or against loss or damage (such as forfeited bail and collateral)" and should exclude "Penalties relating to tax delinquency... library fines... and sale of confiscated property (use code U99)" (US Census Bureau, 2006). It appears that, in practice, revenues from confiscated property sales (especially prior to 2005) were likely accounted within two separate miscellaneous categories, as well as fines and forfeitures. We can be confident in the assumption that "fines and forfeitures," as a category, regularly underestimates the total revenues from law enforcement for any government entity.

[3] While our focus here is on local governments, it should be noted the federal government also profits from public enforcement. In FY 2012, federal agencies collected \$4.152 billion from health care fraud lawsuits, financial sanctions, and civil and criminal penalties (Lemos & Minzner, 2013).

funds for inmate meals, and instead, for example, investing the rest in a used car lot. The money coming through the sheriff office tends to have little oversight by anyone other than the sheriff, so it is relatively easy to succomb to these financial incentives. Given that they can feed inmates as they please, it is tempting to skim from the food fund, and purchase, say, a $70,000 muscle car (Pishko, 2019) or a $740,000 beach house (Pfaff, 2018). While sheriff power appears to assist monetary incentives in tempting sheriffs, fines and fees can work on their own in other local governments.

Maciag (2019), in his construction and analysis of local government revenues, finds that fines and fees have become a critical source of revenue. For each town, city, and municipal government identified, Maciag (2019) calculates the fines as a share of general revenues and the total fines per adult resident, reporting the number of local governments over certain thresholds for each state. The top ten states and totals including all states are reported in Table 3.1. Some of these local governments collect up to 80%–90% of their general revenues from fines and forfeitures, and others collect more than $500 per resident, suggesting the majority of fine revenue comes from out-of-towners in those localities. These states tend to be concentrated in the south, where there are more rural towns. Maciag (2019) argues the biggest impact is on the smaller localities because they have smaller tax bases and long-developed dependencies. Several of these smaller localities also tend to circumvent legal restrictions on fine revenue. Missouri, Georgia, Maryland, and Texas all have caps on fine-generated revenue, but there are ways to get around legal restrictions, and often states simply do not enforce them.[4]

Given that fines and fees have become a critical revenue source, adverse effects put the public at risk. Fiscal incentives force law enforcement agents to target traffic citations and lucrative drug crimes instead of clearing and deterring victim-reported violent and property crimes. Fiscal incentives create such a tradeoff between lucrative crimes and public safety (Kantor et al., 2017). Assuming the goal of law enforcement is to protect the public and deter crime with their given funding, this tradeoff could greatly reduce the level of efficiency with which law enforcement agencies solve and deter crime. This decrease in efficiency is discussed further in Chapter 2.

With given resources, sheriff offices see vast differences in incentives surrounding an election cycle. Election incentives lead to a decline in productivity during election season, and thus also put the public at risk. This decline in productivity is discussed further in Chapter 1, which gives a broad description of the political economy of sheriff elections.

Liu et al. (2019) show that spending on police, judicial and legal services, and corrections have increased substantially over time. Between 1982 and 2015, expenditures within these categories increased from \$388 per capita to \$937 per capita (Liu et al., 2019). There are counties whose fine and fee revenue regularly exceeds police and judicial expenditures. A large portion of assets collected via forfeiture, at both the federal and state levels, are seized without a criminal con-

[4]For example, Maciag (2019) found several localities in West Virginia that had not conducted their required annual audits for at least five years.

viction. Bail is too expensive for the average household income. Partially due to the inability to post bail (Reaves, 2013), about 460,000 people are incarcerated daily without having been convicted of a crime. Roughly two-thirds of those who are incarcerated are charged some sort of fine or fee. Within Alabama at least half of individuals with a felony conviction carry more than $5,000 in criminal debt, much of which will likely never be collected (Liu et al., 2019), and those with criminal debt have higher recidivism rates.

3.1.1 The Return of Tax Farming

The explicit establishment of law enforcement as agents of revenue generation, particularly in sub-national governments, is not a new practice, going back to ancient Rome (Webber & Wildavsky, 1986). Fines collected in the feudal and seignorial courts of Europe were a significant source of revenue that lords collected through their private court systems (Coşgel et al., 2011). The privilege of retaining tax and fine revenues was typically delegated to the same entity by provincial governors in the Ottoman Empire (Coşgel et al., 2012). Profit motives, in the form of piecemeal employment incentives, were an explicit part of the US criminal justice system prior to World War I (Parrillo, 2013). Tax agents received a share of additional remittances resultant of any evasion they uncovered. Prosecutors were rewarded with additional fees per conviction. In the early 20^{th} century, however, financially-motivated over-enforcement became a significant public concern and most of these type of rewards vanished (Parrillo, 2013).

Johnson & Koyama (2014) demonstrate within an historical model how the piecemeal contracting of tax collection to individuals and monopsonistic cabals, i.e. tax farming, proved crucial in 17^{th} century Europe to the expanding of governments operating under significant capacity constraints. Giving collection agents a direct share of the proceeds collected is a means to efficient taxation of income and other property that is otherwise infeasible for both historic macro states and modern, smaller, sub-national governments. Incentivized as the budget-maximizing agents of vote-maximizing principals, law enforcement in smaller cities and municipalities in the United States have, in effect, been recast as the tax farmers from antiquity, providing a second-best solution for capacity constrained governments.

True to historical form, it is within the smallest local governments that we observe the greatest dependence on revenue generated by the criminal justice system. Government revenues characterized by the largest shares from fines, non-property seizure related forfeitures, and court fees are predominantly observed in counties in the lowest population quartile (Figure 3.2). Similarly, the local governments most dependent on criminal justice revenues presented in Table 3.6 from Maciag (2019) are predominantly from rural areas with smaller constituent tax bases and limited government resources. From the point of view of government officials seeking to sustain solvency and, in turn, the continued existence of their own elected and paid positions, dependence on criminal justice revenues is not just a function of opportunity, but also necessity born of limited alternatives.

3.2 Revenue Mechanisms Within Criminal Justice

Financial sanctions are an economically sound form of punishment (Becker, 1968; Ehrlich, 1996), but the possibility of overzealous enforcement, particularly in regards to "victimless" crimes, complicates any model of optimal deterrence (Landes & Posner, 1975). Virtually every step of the criminal justice process can generate revenue at the expense of alleged offenders. The prospects for generating revenue via the criminal justice system have become so fully integrated into local political economy that fiscal motivations cannot be left out of any model of welfare-maximizing law enforcement.

The broad categories of sanctions that can be levied on individuals pulled into the criminal justice system fall under the umbrella category of "legal financial obligations" (LFOs) (Logan & Wright, 2014; Ruback, 2015; Pleggenkuhle, 2018). Within the Revised Model Penal Code are six types of LFOs: (1) victim restitution, (2) fines, (3) costs, (4) fees, (5) assessments, and (6) asset forfeitures (American Law Institute, 2017). Victim restitution is given priority over all other legal financial obligations; is paid by the offender, post-conviction, to the victim(s) of his crime; and, in contrast to the other five categories, cannot be used for government expenses (American Law Institute, 2017).[5]

It is with the other five categories and their viability as revenue for local governments that we concern ourselves here. Researchers, journalists, and

[5] The ostensible goal of restitution is not to punish the offender, but instead to restore the victim of economic, emotional, or psychological losses (Ruback, 2015; Appleman, 2016; Martin et al., 2018).

government officials have compiled data on the revenues generated within each category, to differing degrees of success. In regards to each, we will take care to discuss what is known, while also pointing out the limitations within the available data.

3.2.1 Fines

While costs, fees, and assessments are ostensibly imposed to cover expenses incurred by the criminal justice system, the stated goal of fines is punishment and deterrence (Logan & Wright, 2014; Ruback, 2015; Appleman, 2016). Fines, or financial penalties for a crime or violation, are typically set by statute. The amount of the fine is based on the severity of the crime, as well as the harm suffered by the victim and the offender's ability to pay (Ibid.). Fines do not have to be the sole punishment for a crime; the offender may be charged a fine in conjunction with another punishment, such as prison time (Polinsky, 2006; Bannon et al., 2010; Ruback, 2015; Martin et al., 2018). Many fines also tend to have surcharges added on at the outset of the fine (Logan & Wright, 2014; Appleman, 2016). Surcharges can be flat rates or percentages of the fine, and are again a source of revenue for the criminal justice system (Appleman, 2016).

3.2.2 Fees, Costs, and Assessments

Fees, costs, and assessments, used interchangeably here and throughout, are the most common types of LFOs (Ruback, 2015). They all refer to an economic sanction used as a revenue source to reimburse the criminal justice system for operating costs. They include LFOs such as administrative expenses, as well as costs for services issued by the court and other expenditures (Ruback, 2015; Appleman, 2016).

The prevalence of fees and the dollar amounts charged have increased substantially in recent decades (Beckett et al., 2008; Bannon et al., 2010; Beckett & Harris, 2011). Florida, for instance, has added in excess of 20 types of financial obligations since 1996, while also increasing several of the current fees. Fees are charged on top of other fine and restitution charges, and are often in excess of three times combined fine and restitution charges (Bannon et al., 2010). A woman convicted of a drug crime in Pennsylvania in 2009 incurred fines of $500 and restitution charges of $345, while her 26 different fees totaled $2,464 (Bannon et al., 2010). In Alabama, depending on the city or county, a simple $20 base fine for running a stop sign or red light can turn into $190 (Birmingham Municipal Court)–$263 (Walker County Municipal Court) with the addition of the penalties and surcharges (Lawyers' Committee for Civil Rights (LCCR), 2017).

Various types of fees arise before a conviction. Varying by state and locality, fees can be charged pre-, mid-, and post-trial. Pre-trial fees include those such as bail charges, booking fees (Logan & Wright, 2014), or public defender fees (Logan & Wright, 2014; Appleman, 2016). Some localities even offer optional fees in place of going to trial, effectively buying a clean record (Ibid.). Another option in some places for minor cases is a "deferred prosecution agreement" (Logan & Wright, 2014). The suspect can agree to completing community service, a drug program, etc. in exchange for the prosecutor filing charges. Following conviction, fees can take the form of court or prosecution costs (Logan & Wright, 2014), jury fees (Appleman, 2016), a multitude of supervision fees (Logan & Wright, 2014; Ruback, 2015; Appleman, 2016), and jail fees that could include telephone charges or room and board (Logan & Wright, 2014; Appleman, 2016). For an in-depth review of fines, fees, and costs, see Martin et al. (2018).

3.2.3 Property Forfeiture and Seizure

The US Dept of Justice (2009) defines forfeiture as "the taking of property derived from a crime, involved in a crime, or that which makes a crime easier to commit or harder to detect without compensating the owner" (pg. 8). The forfeiture process begins with a "seizure", or what is effectively the changing of hands of the property in question. The forfeiture that follows falls into one of a few categories depending on the jurisdiction and the value of the seized property (Holcomb et al., 2011).

At the federal level, agencies file property forfeitures as either criminal or civil. Criminal forfeitures were first authorized in 1970 with the Racketeer Influenced and Corrupt Organizations Act (RICO) and the Controlled Substances Act (US Dept of Justice and Office of the Attorney General, 1990; Solomon, 1993; Warchol et al., 1996; Blumenson & Nilsen, 1998). Criminal forfeiture had initially been passed with very basic guidance and only for racketeering and drug kingpin offenses, finding limited application. As part of the Comprehensive Crime Control Act of 1984, the Comprehensive Forfeiture Act adjusted the legislation to make criminal forfeiture a stronger asset. With motivation to stop drug trafficking, essentially any asset used in a crime or purchased with proceeds from a crime could now be forfeited (Warchol et al., 1996). The Anti-Drug Abuse Act of 1986 authorized the criminal forfeiture of "substitute assets." If the actual property or cash used in a criminal action is missing, law enforcement can forfeit other property of the same value as the missing property (US Dept of Justice and Office of the Attorney General, 1990).[6]

State laws place additional strictures on seized property, predominantly with regard to the final destination of revenues generated. Statutes include requirements that dedicated portions of proceeds go toward paying off debt or educational line items (Williams, 2002). The majority of states allow some portion to go back to the forfeiting law enforcement agency, whether the property itself is kept and used by the department or the proceeds pay for forfeiture expenses (Williams, 2002; Holcomb et al., 2011). Eight states prohibit local law enforcement from retaining any proceeds from seized property, while the remaining 42 allow agencies to retain between 50%–100% of revenue (Holcomb et al., 2011). State statutes mandate varying levels of standards of proof, setting a burden of

[6]Criminal forfeitures proceed *in personam*, or "against the person". In order for the law enforcement agency to legally obtain the asset of a person, that person must be convicted of a crime beyond a reasonable doubt (Solomon, 1993; Warchol et al., 1996; Warchol & Johnson, 1996; Holcomb et al., 2011). These circumstances include due process protections for the defendant and the forfeiture does not take place until conviction (i.e., forfeiture and conviction happen at the same time) (US Dept of Justice and Office of the Attorney General, 1990). Civil forfeitures fall under *in rem* jurisdiction, or are filed "against a thing", where the target of the forfeiture is the property instead of the person, and do not require a formal hearing. These are typically forfeitures of contraband, when the property is illegal in all circumstances (Warchol et al., 1996). Administrative forfeitures are more common. With a currently capped value of $500,000, a law enforcement agency can seize cash or other property associated with illegal activity, given probable cause. Regardless of value, means of transportation used to carry controlled substances are also subject to seizure (US Dept of Justice and Office of the Attorney General, 1990).

proof more restrictive than the federal requirement of a preponderance of the evidence (Holcomb et al., 2011).

Even in states with more restrictive standards of proof and limitations on revenue retention, there remains opportunity for revenue-motivated law enforcement. The federal equitable sharing program was created in 1984 with the passing of the Comprehensive Forfeiture Act. State and local agencies can seize any property associated with a felony crime (even if no charges are actually levied) and then transfer it to federal agencies, who then return the property to the seizing agency via the appropriate federal equitable sharing fund—the Asset Forfeiture Fund or the Treasury Fund (Holcomb et al., 2011).

Depending on the type of case, the state and local agencies can receive up to 80% of the proceeds back from the federal fund (Blumenson & Nilsen, 1998; Holcomb et al., 2011). Under adoptive forfeitures, the state or local agencies seize the property for state crimes. However, federal agencies can adopt these forfeitures with a transfer from the state and local agencies if the crime is also a federal crime (Blumenson & Nilsen, 1998; Holcomb et al., 2011). State and local agencies can then receive their 80% of the proceeds back, while the government keeps the remaining 20% for costs associated with operating the federal funds (Holcomb et al., 2011). Budget-maximizing incentives are likely to be a concern in nearly any property seizure context (Carpenter et al., 2019). Such concerns, however, are especially heightened with regards to equitable sharing forfeitures—proceeds can *only* be used to fund law enforcement activities or officer salaries, as long as the payment is going toward positions that were created to fill slots that were opened up when another officer was moved to a task force (US Dept of Justice, 2009; Holcomb et al., 2011).

3.3 The Revenue-Motivated Law Enforcement Hypothesis

The capacity for revenue generation changes the incentives of law enforcement agents and the government principals they serve. That said, an observed increase in revenue generated within the criminal justice system does not necessarily imply that fiscal motivations are a meaningful determinant of criminal justice outcomes. Estimating the salience of these incentives on outcomes requires identifying effects separate from agent responsibilities to respond to observed conditions, deter future crime, and provide broad public safety. Researchers within this growing literature use several strategies to estimate the impact of revenue motivations on officer discretion and the allocation of law enforcement resources. Table 3.2 presents a breakdown of this literature, including the object of focus within the criminal justice system, identification strategy, and observed outcomes.

A simple political economy model of law enforcement as budget-maximizing entities within local governments serves as a sufficient starting point. Such a model finds considerable support within the literature. Benson et al. (1995), Worrall & Kovandzic (2008), Holcomb et al. (2011), and Holcomb et al. (2018) each investigate whether law enforcement budgets are, in fact, sensitive to the manner in which officers carry out their duties. Benson et al. (1995) observe higher non-capital expenditure in police departments that collect more forfeited assets. Controlling for drug and other arrests and levels of crime, they find that the forfeiture revenues are not simply substituted for other general revenues by public officials, allowing officers to enhance their agency's budget. Worrall & Kovandzic (2008), Holcomb et al. (2011), and Holcomb et al. (2018) identify the effects of the state asset forfeiture laws on measures of the federal equitable sharing payments to agencies or municipalities. They test the hypothesis that police entities in states that do not allow departmental retention of revenue from forfeitures are nonetheless able to circumvent their state laws and generate revenue through equitable sharing. Each similarly demonstrate that states with more restrictive forfeiture laws have higher federal equitable sharing payments. In a similar vein, Mughan et al. (2019) examine the difference in revenue incentives between sheriff departments and municipal departments. They observe that those in the elected office do not have as strong a response to the financial incentive as their appointed counterparts, seizing far fewer assets.

Budgetary effects are persistent and self-reinforcing, as local governments and their police departments grow dependent on the criminal justice revenues for which they can be directly and indirectly credited (Worrall, 2001; Baicker & Jacobson, 2007; Beck & Goldstein, 2017). Police officials, for all their efforts, are likely to find themselves on little more than a budgetary treadmill. As law enforcement succeeds in generating revenue, the expectation of self-funding enters into the budget, eventually displacing previous support from general funds towards other expenditure line items. Each year they increase their revenue, higher government officials will have the opportunity to reduce general fund allocations for law enforcement, leaving police increasingly dependent on their own revenue generation just to maintain their budgetary status quo. Gains to the broader municipality may be limited as well. Carroll (2009) finds that increased non-tax diversification of revenue sources fails to increase year-to-year local fiscal stability.

Given the established relationship between budget incentives and revenue generation, it logically follows that officer discretion and deployment will be sensitive to the costs and benefits associated with different law enforcement outcomes. Garrett & Wagner (2009) find that traffic tickets and citations depend on local fiscal conditions—towns ramp up tickets while enduring budgetary shortfalls. Further in this vein, Makowsky & Stratmann (2009, 2011) observe officer discretion depends on the residency status (in-town or out-of-town) of drivers in conjunction with fiscal conditions. Harvey (2020) exploits variation in laws in Canadian towns in Saskatchewan, identifying sharp discontinuities across town borders depending on the share of citation revenues that the towns in question are able to retain in their budgets.

Several papers explore the relationship between state-level forfeiture revenue retention laws and drug arrest rates (Mast et al., 2000; Baicker & Jacobson, 2007; Bishopp & Worrall, 2009; Kelly & Kole, 2016; Kantor et al., 2017; Makowsky et al., 2019). Kantor et al. (2017) build an identification strategy around the implementation of the Comprehensive Crime Control Act of 1984 (implementation of federal equitable sharing) and the state forfeiture retention rates. They find that in states that otherwise limit police retention of proceeds from seized property, the establishment of federal equitable sharing redirected police effort towards drug enforcement. These states subsequently produced 37% more drug arrests while experiencing a 17% reduction in reported crime and a 22% increase in roadway fatalities. Makowsky et al. (2019) find increased rates of arrests for drug crimes, DUI, and prostitution, and higher rates of property seizure—these increases, however, are only observed for black and Hispanic arrest rates. Sances & You (2017) similarly observe that dependence on fine and forfeiture revenues in local governments is increasing with the size of the constituent African-American population.

If revenue motivations lead to greater prioritization of drug, DUI, or prostitution arrests, departments may, in turn, reduce the resources applied toward violent and property crime related enforcement. If police agents target revenue-generating activity, police effort may be substituted away from other crimes. However, departments might be generating enough revenue to put it back toward solving more violent and property crimes. Goldstein et al. (2020) study municipality police use of own-source revenue from fines and fees towards clearing reported violent and property crimes, using commuting zones in an instrumental variables identification strategy. Their results support the dominance of substitution effects; violent and property crime clearances are lower where fines and fees constitute a greater share of total revenue. Kelly & Kole (2016) and Makowsky et al. (2019) find weak relationships between seizure-related revenues on violent crimes, suggesting that substitution of enforcement towards revenue generation is unlikely to be at the expense of the investigation and deterrence of the highest profile and most serious crimes.

3.4 Suboptimal Deterrence, Bias, and Deteriorating Trust

Prior research has demonstrated the distortion of law enforcement and, in turn, its deviation from the optimal provision of public safety when revenue concerns enter the decision-making calculus of enforcement agents (Garoupa & Klerman, 2002). These deviations from optimal enforcement manifest from the prioritization of fiscal profitability, which includes not just the expected yield of individual arrests, but also the probability that arrest proceeds will be retained and the expected costs and benefits of associated adjudication. Goldstein et al. (2020) find that clearance rates of violent criminal incidents reported to police decrease when the proportion of local government revenue from fines and fees increases. The observed effect is predominantly driven by smaller municipalities.[7]

It can be difficult to estimate how much debt individuals with different criminal convictions typically incur. Fees are often not located in a single place in the statutory code and are not collected at a single point in an individual's criminal proceeding, making it difficult to calculate exactly how much debt a criminal conviction might engender. Louisiana, for example, has dozens, if not hundreds, of assessments sprawled throughout its code (Bannon et al., 2010). In FY 2018, outstanding federal criminal debt was $126.7 billion and outstanding civil debt

[7]This is not to say that the directing of police resources towards areas where enforcement yields revenue is without positive effect in the area of emphasis chosen. Makowsky & Stratmann (2011) demonstrate that when a municipality experiencing fiscal distress has incentive to increase the number of traffic citations issued, drivers respond by driving more conservatively, leading to fewer traffic collisions. **?** report similar findings.

was \$18.5 billion, making the total outstanding federal debt \$145.2 billion (Office of the US Attorneys, 2018). While both prisoners and non-prisoners can accumulate debt, prisoners are at higher risk for longer-term financial difficulties (Bannon et al., 2010; Pleggenkuhle, 2018). Link (2019) analyzes prisoner criminal justice debt using survey data from prisoners in the Returning Home Studies in metropolitan areas of Texas, Ohio, and Illinois. In FY 2018, 44% of the prisoners in the survey had accumulated some amount of debt, with a median amount for those with debt of \$260 (Link, 2019). This personal accumulation of LFO's can put individuals in significant financial distress (Mello, 2018) and even drive them towards crime: in a survey of individuals involved with the justice system conducted by AACLJ and the University of Alabama-Birmingham (Alabama Appleseed Center for Law and Justice (AACLJ), 2017), 38.3% of respondents indicated they had committed at least one crime to pay outstanding LFO's.

Byproduct of these distortions in law enforcement are biases against socially, politically, and financially vulnerable portions of the population (Anwar et al., 2012; Alesina & La Ferrara, 2014; Agan et al., 2018). Sances & You (2017) find that fine and forfeiture revenues increase with the size of a county's African American population and that this effect is mitigated by African American representatives on elected city councils. Makowsky et al. (2019) find that black and Hispanic drug and DUI arrests, and associated seizures of cash and automobiles, increase with local deficits when police can retain proceeds from forfeited property in their budgets, while comparable white arrests are unchanged. When combined with institutions that are racially biased (Antonovics & Knight, 2009;

Anwar et al., 2012; Goncalves et al., 2017), revenue-driven policing exacerbates broader racial bias in the criminal justice system (Alabama Appleseed Center for Law and Justice (AACLJ), 2017; US Commission on Civil Rights (USCCR), 2017).

Increased perception of law enforcement as agents of revenue generation, less beholden to fair application of the law, undermines the legitimacy of their authority (National Research Council and others, 2004; Katzenstein & Waller, 2015; Tyler et al., 2015). Murphy et al. (2008) and Murphy & Barkworth (2014) find that lower public estimates of police legitimacy correspond with reduced cooperation with police by victims or witnesses. In his review of the broad revenue motivations behind municipal law enforcement, McBride (2018) observes that if communities believe that "...police power is being used for illegitimate purposes, faith and trust in officers that exercise that power would be undermined and their ability to perform their legitimate functions would be stymied." Given the frequent bias in enforcement and the crushing financial burden it often imposes, the emergence of law enforcement as a regressive source of revenue generation stands a threat to law enforcement and its provision of public safety.

3.5 Proposed Legal and Policy Reforms

A variety of legislative efforts and policy proposals have emerged in recent years with the common goals of constraining revenue-motivated law enforcement. These efforts, however, have little choice but to exist on top of the elected leaders and municipal police departments of local governments that have grown dependent on subsidizing not just police budgets, but their broader general funds. In many cases, the fiscal solvency of an entire municipality stands threatened by limitations on the generation and retention of revenue from the criminal justice system.

In 2018, the city of Philadelphia reached a settlement with the Institute of Justice to reform its civil asset forfeiture laws. Within the settlement, the city agreed to two legally binding consent decrees that committed to both restrictions on civil forfeiture practices and to paying reparations to past victims of overzealous asset forfeiture (Wimer, 2018).[8] The city also agreed to remit all future property forfeiture proceeds to community-based drug prevention and rehabilitation programs. This final component is noteworthy for its attempt to mitigate the direct incentives for law enforcement to feed their own budgets through the confiscation of assets. It, however, also leaves as an open question whether law enforcement will remain indirectly rewarded for their ability to subsidize drug prevention and rehabilitation programs, freeing up otherwise committed funds to return to the general fund.

In the wake of the tragic death of Michael Brown in Ferguson, Missouri, and the subsequent investigation by the Department of Justice, the Missouri state legislature passed Senate Bill 5. The bill, amongst other things: placed limits on

[8] The settlement banned confiscation of property for drug possession and the seizure of any cash amount less than $1,000 without strong proof of criminal activity. Police officers must record in-depth summaries of all property seized and communicate the explicit process to retrieve seized property. Civilians must be granted a prompt hearing if they request the return of their assets. Reparations were to be made from a $3 million fund established by the city to compensate those whose property was wrongfully seized.

the percentage of revenues that municipalities could generate from traffic fines; banned "failure to appear" charges, placed limits on the combined costs of fines and fees, and banned jail sentences for minor traffic offenses; and eliminated the collection of court costs if a case is dismissed (Fines & Fees Justice Center (FFJC), 2014). Perhaps most importantly, it banned jailing of individuals unable to pay a fine, eliminating a mechanism that had effectively hailed the return of debtors prisons.[9] From a political economy point of view, the final provision of the settlement is perhaps the most interesting: citizens in Missouri were granted, under the bill, the ability to dissolve their local governments via referendum if they do not turn over excess traffic revenues to the state.

A number of policies enacted or under consideration would mitigate the burden of fines and fees, both on the grounds of their burden to low-income individuals and their relationship to the Excessive Fines Clause (Colgan, 2020). Colgan (2019)[10] proposes an adjustment to how economic sanctions are collected. Citing experiments in US localities—Staten Island, NY, Maricopa County, AZ, and Milwaukee, WI, to name a few—her proposal outlines a day-fine structure to account for ability to pay. The day-fine policy would use a self-reported base income to calculate ability to pay, and then multiply by the "penalty unit," or the degree of severity of the offense. In this vein, the proposed Florida SB 1328/HB 903 would eliminate driver's license suspension for unpaid fines and fees and allow for smaller structured payments (Fines & Fees Justice Center (FFJC), 2020).

[9] See S.B. 5, 98^{th} Gen. Assembly, 1^{st} Reg. Sess. (Miss. 2015).

[10] See also Colgan (2017b) for greater detail on the merits of graduating fines with ability to pay.

Contra Costa County, California adopted a moratorium on adult criminal justice fees for probation, indigent defense, and work release programs (Fines & Fees Justice Center (FFJC), 2019).

Makowsky (2019) lays a general framework to change fiscal incentives underlying revenue-motivated law enforcement. The core policy innovation is the remittance of all criminal justice revenue to the state government for redistribution back to municipal governments as per capita block grants, dampening the direct fiscal incentive behind any discretionary arrest decision and undermining the less than 5% of governments reliant on criminal justice revenues as a *de facto* form of regressive taxation. In doing so, a state can begin the steady process of weening local governments off of dependence on criminal justice revenue.[11]

3.6 Conclusions

Revenue generated through the criminal justice system has become a key component of municipal budgets for a growing number of local governments across the United States. Its value lies not just in its immediate value and flexibility, but its capacity for expropriating from those whose socioeconomic or residential status softens the political costs that would otherwise be expected from any tax borne by fully enfranchised constituents. Police departments are not just funding themselves—they are often subsidizing their entire municipal government.

The literature to date demonstrates the broader costs of revenue-motivated

[11] In a more nuanced version of the policy, Makowsky suggests that states require that any revenues generated via law enforcement be rebated to all citizens within the state that qualified for SNAP benefits.

law enforcement. First and foremost, the economic damage to the marginal felon arrestee is difficult to overstate: their lives are disrupted and expected lifetime earnings are irrevocably damaged with the acquisition of a criminal record. Even beyond felony and misdemeanor arrests, however, poor households rarely have means to absorb the potentially thousands of dollars in legal financial obligations often associated with a non-criminal citation. The secondary costs of revenue-driven law enforcement are equally disconcerting. As budgets become more dependent on the criminal justice system for revenue, the occupational incentives facing police officers at each node of discretion in their interactions with citizens shift more towards fiscal profit, and further from public safety.

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The authors are not aware of any affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.

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Table 3.1: Local Government Fines by State

Fines as a Share of General Revenues

State	Over 10%	Over 20%	Over 30%	Over 50%
Georgia	92	52	30	13
Texas	90	39	22	10
Louisiana	70	49	40	25
Oklahoma	55	42	29	14
Arkansas	44	14	11	3
New York	34	12	5	1
Illinois	33	11	4	1
Ohio	24	15	10	8
Tennessee	18	12	10	2
Missouri	18	6	2	-
TOTALS	583	284	179	80

Total Fines Per Adult Residents

State	Over \$100	Over \$200	Over \$300	Over \$500
Texas	147	77	40	22
Georgia	87	54	37	19
Louisiana	66	48	36	21
Oklahoma	53	33	22	14
Ohio	41	21	16	6
Illinois	41	14	11	4
New York	39	11	4	2
Tennessee	24	14	8	6
Arkansas	19	11	10	5
Florida	19	8	6	2
TOTALS	723	363	233	124

Notes:(Upper) Number of Local Governments in each state where the sum of fines, forfeitures, and other court revenue exceeds the stated percentage. Top 10 states are reported. Either FY2018 or FY2017.

(Lower) Number of Local Governments in each state where the sum of fines, forfeitures, and other court revenue per adult resident exceeds the stated dollar value. Top 10 states reported, FY2018 and FY2017. Both figures exclude governments i) reporting less than \$100,000 in fines or other court revenues, and ii) with insufficient public audit records that did not respond to requests for additional information.

Retrieved from Maciag (2019).

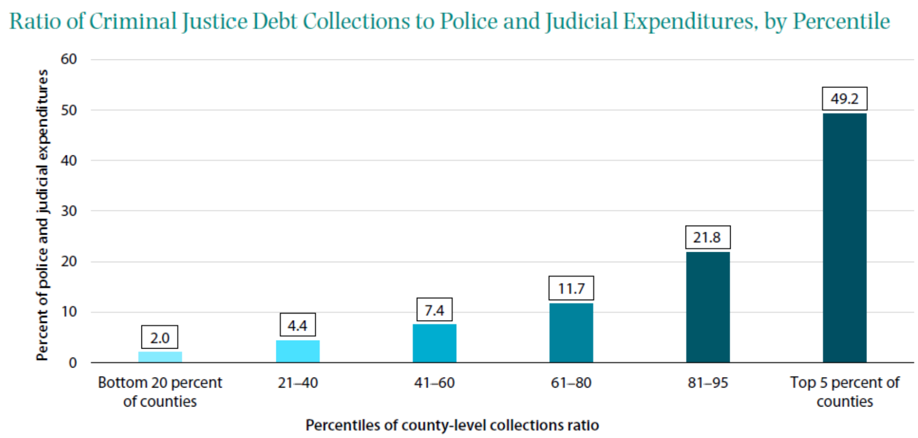
Table 3.2: Summary of Literature

Authors	Fines	Fees	Forf.	Eq. Sharing	Obs. Level	Dep. Var.	Expl. Vars.	Model	FE	Errors	Data Source	Results
Benson et al. (1995)			x		Agency	Non-capital expenditures of police	value of assets forfeited; Drug & other arrests	OLS		Standard	FDLE	More seizures lead to higher police expenditures; Consistent with agencies responding to monetary incentives
Mast et al. (2000)			x		City	Drug arrest ratio	Forfeiture laws	WLS	City; Time	Standard	DUFP; UCR	States allowed to keep proceeds have higher drug arrest to total arrest ratios by about 20% and higher drug arrest rates by about 18%
Baicker & Jacobson (2007)			x	x	County	Police budget; Drug arrest rate	Lagged seizures and deficits; Introduction of forfeiture laws	OLS	County; Year	Clustered	COG; DOJ; STRIDE; UCR	Local governments offset police seizures by reducing their general allocations to police; Police then increase drug arrest rates; heroin prices also increase
Worrall & Kovandzic (2008)			x	x	Municipality	Forfeiture proceeds; Equitable sharing payments	Forfeiture laws	log-log		Robust; Clustered	LEMAS; UCR	Fewer equitable sharing payments collected in states with higher state retention rates, thus maximizing their potential for revenue generation
Bishopp & Worrall (2009)			x		Municipality	Drug arrest rates	Forfeiture laws	OLS		Robust; Clustered	LEMAS; UCR	Contrary to Mast et al. (2000): no association between forfeiture laws and drug arrest rates; Contrary to Baicker & Jacobson (2007): past asset forfeitures do not affect drug arrest rates
Garrett & Wagner (2009)	x				County	Percent change in traffic tickets	Local government fiscal health	OLS	County; Time	Robust; Clustered	LINC	Increase in traffic tickets following a year of decreased local government revenue
Makowsky & Stratmann (2009)	x				Traffic stop	Tickets and citations	Fiscal indicators; Driver residency; Distance to court	Probit, LPM, Heckman	Officer	Clustered	BG; RMV	Municipal fiscal conditions affect the issuance of traffic tickets; Lower revenues from property taxes lead to more fines
Holcomb et al. (2011)			x	x	Municipal police; Sheriff agencies	Equitable sharing amounts	Aspects of state forfeiture laws	Left-censored Tobit	Region	Robust; Clustered	LEMAS; DOJ AFF	More restrictive state forfeiture laws lead to more federal equitable sharing proceeds

Authors	Fines	Fees	Forf.	Eq. Sharing	Obs. Level	Dep. Var.	Expl. Vars.	Model	FE	Errors	Data Source	Results
Makowsky & Stratmann (2011)	x				Tickets per month by city	Traffic tickets; Accidents	IV: indicator of fiscal distress; Tickets	IV			BG; RMV; HDHSD	Fewer accidents with more tickets issued; Fewer traffic injuries with more law enforcement
Kelly & Kole (2016)			x		Agency	Serious crime clearances; Drug arrests	forfeiture proceeds	OLS	Agency; Year	Standard	LEMAS; UCR	Weak support of forfeiture affecting police behavior; Forfeiture is not vital for police funds and incentives
Kantor et al. (2017)			x	x	County	Reported crime; Arrests; Police budget; Police strength	Forfeiture laws; CCCA	OLS	County; Year	Clustered	COG; UCR	Crime fell where states had smaller retention rates before CCCA; Shifted effort to drug crimes; Increased roadway fatalities
Sances & You (2017)	x	x			Municipality	Fine revenue per capita	Percent African-American	OLS	State; County	Robust	COG; US Census	More fines and fees collected in municipalities with higher African-American populations
Holcomb et al. (2018)			x	x	Jurisdiction	Equitable sharing proceeds	Index measure of state forfeiture laws	Multilevel tobit		Standard	LEMAS; DOJ AFF	Agencies in states with state laws that are more restrictive or less rewarding to police collect more in federal equitable sharing payments
Makowsky et al. (2019)			x		Jurisdiction	Drug, DUI, prostitution, and violent arrests	Deficits/state forfeiture laws interaction	OLS	State; Year	Robust; Clustered	COG; NIBRS	With fiscal distress: increase in African American and Hispanic drug, DUI, and prostitution arrests; increase in white prostitution arrests
Mughan et al. (2019)			x	x	Jurisdiction	Criminal, civil forfeiture revenues	Sheriff vs. municipality; Forfeiture law & forfeiture law	OLS	Year	Robust	LEMAS; DOJ ESP	Sheriff (elected) agencies seize less than municipal (appointed) agencies; Sheriffs less responsive to forfeiture laws
Goldstein et al. (2020)	x	x			Municipality	Violent and property clearances	Own-source revenue from fines and fees; Commuting times as IV	OLS; IV	County; Time	Clustered	COG; UCR; CSLLEA; ACS	Municipalities with higher shares of revenue from fines and fees have lower violent and property crime clearances
Harvey (2020)	x				Towns	Traffic citation rates	Whether a town receives fine revenue	RDD	Year	Robust; clustered	SMJ; SGI	Higher share of fine revenues leads to higher police effort and safer driving with only small effects on citation frequency

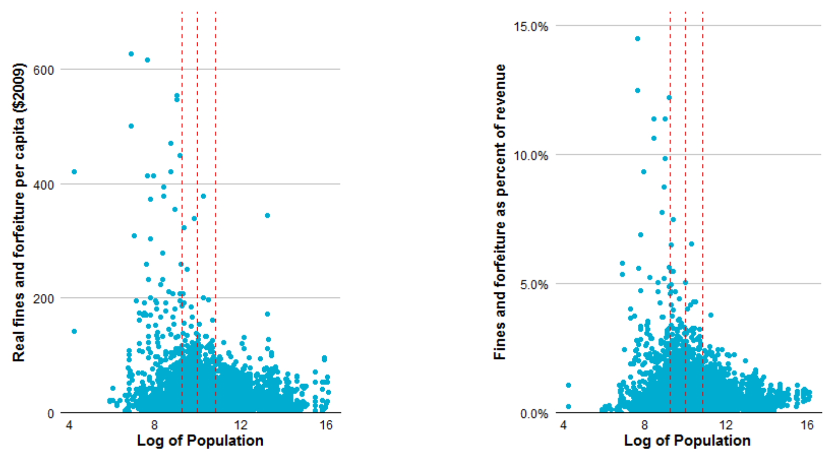
Notes: ACS: American Community Survey; BG: Boston Globe; CCCA: Comprehensive Crime Control Act of 1984; COG: Census of Governments; CSLLEA: Census of State and Local Law Enforcement Agencies; DOJ AFF: Department of Justice Asset Forfeiture Fund; DOJ ESP: Department of Justice Equitable Sharing Program; DUFP: Drug Use Forecasting Program (National Institute of Justice); FDLE: Florida Department of Law Enforcement; HDHSD: MA Highway Department and Highway Safety Division; LEMAS: Law Enforcement Management and Administrative Statistics; LINC: Log into North Carolina; NIBRS: National Incident-Based Reporting System; RMV: MA Registry of Motor Vehicles; SGI: Saskatchewan Government Insurance; SMJ: Saskatchewan Ministry of Justice; STRIDE: System to Retrieve Information from Drug Evidence; UCR: Uniform Crime Reporting;

Figure 3.1: US Debt Collections by Quintile



Notes: Reproduced here from Liu et al. (2019). Original Source: Census of Governments, US Census Bureau 2012. Revenue from fines and forfeits includes penalties imposed for violations of law, civic penalties, court fees if levied upon conviction, court-ordered restitutions to crime victims, and forfeits of deposits held (such as forfeited bail and collateral). Sale of confiscated property is not included. Police and court expenditures cover current operations, construction, land and existing structures, as well as equipment, all for police protection and judicial and legal functions. Data include observations at the city and county level, aggregated to the county level. Counties in higher quintiles have higher shares of criminal debt collection.

Figure 3.2: County Fine and Forfeiture Revenues



Notes: (Left) Figure 2A. Per Capita Fine and Forfeiture Revenues and County Population, 1977–2012 (Right) Figure 2B. Fine and Forfeiture Share of Total Revenues and County Population, 1977–2012 *Source:* Census of Governments, US Census Bureau 1977–2012; author's calculations. *Notes:* In 2005, the Census of Governments expanded the sample to include smaller counties, generally those with populations less than 250,000. The dotted lines denote the 25^{th}, 50^{th}, and 75^{th} percentiles of county population in the 2007 Census of Governments.

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Appendices

APPENDIX A
CHAPTER 1

The following tables and figures show a few introductory specifications to the base model in Chapter 1, as well as the base model (OLS) specifications for the crime stratified variables.

Table A1: Introductory Estimations

	Dependent variable:				
	Arrests per Capita				
	(1)	(2)	(3)	(4)	(5)
Before Primary	−0.1064**		−0.0922***		
	(0.031 8)		(0.035 5)		
Between Primary and General	−0.1670***		−0.1536***		
	(0.041 9)		(0.045 2)		
After General	−0.1615***		−0.1571***		
	(0.034 3)		(0.034 8)		
Running		−0.1084***	−0.0051		
		(0.036 9)	(0.043 4)		
Never Running		−0.1676***	−0.0536		
		(0.057 2)	(0.060 6)		
(Won Primary)×(Before Primary)				−0.0747*	
				(0.042 8)	
(Won Primary)×(After Primary)				−0.1687***	
				(0.046 3)	
(Lost Primary)×(Before Primary)				−0.0061	
				(0.073 3)	
(Lost Primary)×(After Primary)				−0.1622**	
				(0.082 1)	
(Never Running)×(Before Primary)				−0.2161***	
				(0.057 5)	

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	Dependent variable:				
	Arrests per Capita				
	(1)	(2)	(3)	(4)	(5)
(Never Running)×(After Primary)				-0.1348^{**}	
				(0.0638)	
(Won General)×(Before General)					-0.1404^{***}
					(0.0457)
(Won General)×(After General)					-0.0914^{**}
					(0.0372)
(Lost General)×(Before General)					-0.4389
					(0.2696)
(Lost General)×(After General)					-0.5881^{***}
					(0.2213)
(Never Running)×(Before General)					-0.1726^{***}
					(0.0572)
(Never Running)×(After General)					-0.1512^{**}
					(0.0731)
log(Population)	-1.0028	-1.1443	-1.0129	-1.1290	-1.1000
	(0.7552)	(0.7799)	(0.7564)	(0.7782)	(0.7789)
Constant	10.4033	11.8169	10.5005	11.6518	11.3780
	(7.5340)	(7.7798)	(7.5459)	(7.7633)	(7.7705)
Month/Year FE	Yes	Yes	Yes	Yes	Yes
Sheriff FE	Yes	Yes	Yes	Yes	Yes
Clustered By:	Sher.	Sher.	Sher.	Sher.	Sher.

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	Dependent variable:				
	Arrests per Capita				
	(1)	(2)	(3)	(4)	(5)
Observations	36,971	36,971	36,971	36,971	36,971
Adjusted R^2	0.7468	0.7463	0.7468	0.7464	0.7466

Notes: This table shows the results from a few introductory regressions on the arrests per capita dependent variable. Significance is denoted at the 1%, 5%, and 10% level by ***, **, and *. Standard errors are robust and clustered by sheriff.

Table A2: Introductory Estimations - Drug Crimes

	Dependent variable:				
	Drug Arrests per Capita				
	(1)	(2)	(3)	(4)	(5)
Before Primary	−0.0057		−0.0131		
	(0.0091)		(0.0148)		
Between Primary and General	−0.0174*		−0.0260*		
	(0.0095)		(0.0148)		
After General	−0.0088		−0.0073		
	(0.0095)		(0.0097)		
Running		−0.0021	0.0155		
		(0.0094)	(0.0162)		
Never Running		−0.0300**	−0.0142		
		(0.0122)	(0.0160)		
(Won Primary)×(Before Primary)				0.0006	
				(0.0130)	
(Won Primary)×(After Primary)				−0.0095	
				(0.0108)	
(Lost Primary)×(Before Primary)				0.0199	
				(0.0314)	
(Lost Primary)×(After Primary)				−0.0089	
				(0.0360)	
(Never Running)×(Before Primary)				−0.0276**	
				(0.0127)	

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	Dependent variable:				
	Drug Arrests per Capita				
	(1)	(2)	(3)	(4)	(5)
(Never Running)×(After Primary)				−0.0328**	
				(0.0148)	
(Won General)×(Before General)					−0.0098
					(0.0105)
(Won General)×(After General)					−0.0026
					(0.0107)
(Lost General)×(Before General)					0.0731
					(0.0756)
(Lost General)×(After General)					−0.0395
					(0.0617)
(Never Running)×(Before General)					−0.0352***
					(0.0127)
(Never Running)×(After General)					−0.0060
					(0.0171)
log(Population)	−0.5415	−0.5553	−0.5481	−0.5531	−0.5514
	(0.3828)	(0.3866)	(0.3840)	(0.3863)	(0.3859)
Constant	5.3693	5.5070	5.4333	5.4837	5.4693
	(3.8096)	(3.8479)	(3.8219)	(3.8449)	(3.8411)
Month/Year FE	Yes	Yes	Yes	Yes	Yes
Sheriff FE	Yes	Yes	Yes	Yes	Yes
Clustered By:	Sher.	Sher.	Sher.	Sher.	Sher.

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	Dependent variable:				
	Drug Arrests per Capita				
	(1)	(2)	(3)	(4)	(5)
Observations	36,971	36,971	36,971	36,971	36,971
Adjusted R^2	0.4528	0.4528	0.4528	0.4528	0.4529

Notes: This table shows the results from a few introductory regressions on the arrests per capita dependent variable. Significance is denoted at the 1%, 5%, and 10% level by ***, **, and *. Standard errors are robust and clustered by sheriff.

Table A3: Introductory Estimations - DUI Crimes

	Dependent variable:				
	DUI Arrests per Capita				
	(1)	(2)	(3)	(4)	(5)
Before Primary	−0.0027		−0.0005		
	(0.0051)		(0.0074)		
Between Primary and General	−0.0009		0.0010		
	(0.0073)		(0.0085)		
After General	0.0009		0.0021		
	(0.0057)		(0.0057)		
Running		0.0010	0.0010		
		(0.0062)	(0.0084)		
Never Running		−0.0138	−0.0141		
		(0.0123)	(0.0146)		
(Won Primary)×(Before Primary)				0.0008	
				(0.0068)	
(Won Primary)×(After Primary)				−0.0006	
				(0.0087)	
(Lost Primary)×(Before Primary)				0.0090	
				(0.0121)	
(Lost Primary)×(After Primary)				0.0106	
				(0.0189)	
(Never Running)×(Before Primary)				−0.0141	
				(0.0114)	

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	Dependent variable:				
	DUI Arrests per Capita				
	(1)	(2)	(3)	(4)	(5)
(Never Running)×(After Primary)				−0.0135	
				(0.0144)	
(Won General)×(Before General)					0.0058
					(0.0078)
(Won General)×(After General)					0.0088
					(0.0064)
(Lost General)×(Before General)					−0.0696
					(0.0763)
(Lost General)×(After General)					−0.0406
					(0.0552)
(Never Running)×(Before General)					−0.0131
					(0.0116)
(Never Running)×(After General)					−0.0167
					(0.0196)
log(Population)	−0.2017	−0.2030	−0.2049	−0.2027	−0.2100
	(0.2739)	(0.2747)	(0.2741)	(0.2747)	(0.2750)
Constant	2.0460	2.0581	2.0769	2.0553	2.1264
	(2.7292)	(2.7369)	(2.7311)	(2.7367)	(2.7400)
Month/Year FE	Yes	Yes	Yes	Yes	Yes
Sheriff FE	Yes	Yes	Yes	Yes	Yes
Clustered By:	Sher.	Sher.	Sher.	Sher.	Sher.

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	Dependent variable:				
	DUI Arrests per Capita				
	(1)	(2)	(3)	(4)	(5)
Observations	36,971	36,971	36,971	36,971	36,971
Adjusted R^2	0.4949	0.4950	0.4949	0.4949	0.4951

Notes: This table shows the results from a few introductory regressions on the DUI arrests per capita dependent variable. Significance is denoted at the 1%, 5%, and 10% level by ***, **, and *. Standard errors are robust and clustered by sheriff.

Table A4: Introductory Estimations - Violent Crimes

	Dependent variable:				
	Violent Arrest to Incident Ratio				
	(1)	(2)	(3)	(4)	(5)
Before Primary	−0.0019		0.0306*		
	(0.0103)		(0.0183)		
Between Primary and General	−0.0307**		0.0038		
	(0.0125)		(0.0204)		
After General	−0.0044		−0.0032		
	(0.0102)		(0.0103)		
Running		−0.0234**	−0.0417**		
		(0.0102)	(0.0200)		
Never Running		−0.0124	−0.0282		
		(0.0187)	(0.0232)		
(Won Primary)×(Before Primary)				−0.0145	
				(0.0136)	
(Won Primary)×(After Primary)				−0.0452***	
				(0.0132)	
(Lost Primary)×(Before Primary)				0.0693*	
				(0.0385)	
(Lost Primary)×(After Primary)				0.0202	
				(0.0373)	
(Never Running)×(Before Primary)				−0.0006	
				(0.0221)	

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	Dependent variable:				
	Violent Arrest to Incident Ratio				
	(1)	(2)	(3)	(4)	(5)
(Never Running)×(After Primary)				−0.0232	
				(0.0225)	
(Won General)×(Before General)					−0.0302**
					(0.0141)
(Won General)×(After General)					−0.0130
					(0.0112)
(Lost General)×(Before General)					−0.0305
					(0.0440)
(Lost General)×(After General)					−0.0151
					(0.0731)
(Never Running)×(Before General)					−0.0028
					(0.0200)
(Never Running)×(After General)					−0.0530*
					(0.0322)
log(Population)	−0.1026	−0.1027	−0.0961	−0.0897	−0.0959
	(0.1795)	(0.1795)	(0.1785)	(0.1745)	(0.1780)
Constant	1.2930	1.2952	1.2279	1.1625	1.2289
	(1.7915)	(1.7916)	(1.7820)	(1.7419)	(1.7772)
Month/Year FE	Yes	Yes	Yes	Yes	Yes
Sheriff FE	Yes	Yes	Yes	Yes	Yes
Clustered By:	Sher.	Sher.	Sher.	Sher.	Sher.

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	Dependent variable:				
	Violent Arrest to Incident Ratio				
	(1)	(2)	(3)	(4)	(5)
Observations	21,217	21,217	21,217	21,217	21,217
Adjusted R^2	0.2302	0.2302	0.2303	0.2305	0.2302

Notes: This table shows the results from a few introductory regressions on the violent arrest to incident ratio dependent variable. Significance is denoted at the 1%, 5%, and 10% level by ***, **, and *. Standard errors are robust and clustered by sheriff.

Table A5: Introductory Estimations - Property Crimes

	Dependent variable:				
	Property Arrest to Incident Ratio				
	(1)	(2)	(3)	(4)	(5)
Before Primary	0.0024		0.0029		
	(0.0038)		(0.0057)		
Between Primary and General	−0.0017		−0.0012		
	(0.0040)		(0.0058)		
After General	0.0016		0.0017		
	(0.0036)		(0.0037)		
Running		0.0004	−0.0005		
		(0.0036)	(0.0061)		
Never Running		0.0003	−0.0008		
		(0.0063)	(0.0077)		
(Won Primary)×(Before Primary)				0.0031	
				(0.0050)	
(Won Primary)×(After Primary)				−0.0025	
				(0.0044)	
(Lost Primary)×(Before Primary)				−0.0010	
				(0.0132)	
(Lost Primary)×(After Primary)				−0.0146*	
				(0.0088)	
(Never Running)×(Before Primary)				−0.0024	
				(0.0073)	

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	Dependent variable:				
	Property Arrest to Incident Ratio				
	(1)	(2)	(3)	(4)	(5)
(Never Running)×(After Primary)				0.0020	
				(0.0084)	
(Won General)×(Before General)					−0.0016
					(0.0045)
(Won General)×(After General)					−0.0006
					(0.0040)
(Lost General)×(Before General)					−0.0108
					(0.0166)
(Lost General)×(After General)					0.0215
					(0.0237)
(Never Running)×(Before General)					0.0008
					(0.0065)
(Never Running)×(After General)					−0.0033
					(0.0114)
log(Population)	0.0039	0.0047	0.0038	0.0057	0.0058
	(0.0712)	(0.0716)	(0.0713)	(0.0716)	(0.0712)
Constant	0.0120	0.0042	0.0128	−0.0056	−0.0073
	(0.7101)	(0.7139)	(0.7115)	(0.7137)	(0.7104)
Month/Year FE	Yes	Yes	Yes	Yes	Yes
Sheriff FE	Yes	Yes	Yes	Yes	Yes
Clustered By:	Sher.	Sher.	Sher.	Sher.	Sher.

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	Dependent variable:				
	Property Arrest to Incident Ratio				
	(1)	(2)	(3)	(4)	(5)
Observations	33,026	33,026	33,026	33,026	33,026
Adjusted R^2	0.2555	0.2555	0.2555	0.2555	0.2555

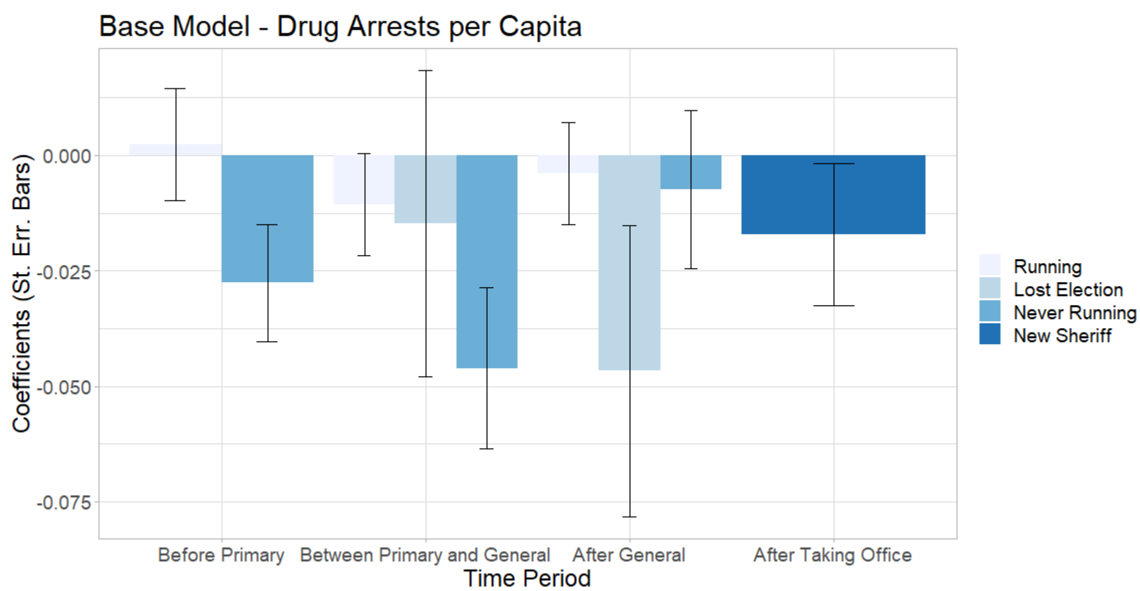
Notes: This table shows the results from a few introductory regressions on the property arrest to incident ratio dependent variable. Significance is denoted at the 1%, 5%, and 10% level by ***, **, and *. Standard errors are robust and clustered by sheriff.

Table A6: Crime Stratification - Base Model

	Dependent variable:			
	Drug	DUI	Viol A/I	Prop A/I
	(1)	(2)	(3)	(4)
(Running)×(B)	0.0024	0.0029	−0.0069	0.0030
	(0.0122)	(0.0065)	(0.0129)	(0.0048)
(Running)×(M)	−0.0106	0.0014	−0.0429***	−0.0029
	(0.0110)	(0.0087)	(0.0135)	(0.0043)
(Ran and Won General)×(A)	−0.0040	0.0080	−0.0122	0.0001
	(0.0110)	(0.0066)	(0.0115)	(0.0041)
(Ran and Lost Primary)×(M)	−0.0148	0.0095	0.0122	−0.0119
	(0.0331)	(0.0171)	(0.0397)	(0.0076)
(Ran and Lost Election)×(A)	−0.0466	−0.0116	−0.0022	0.0082
	(0.0316)	(0.0266)	(0.0463)	(0.0171)
(Never Running)×(B)	−0.0276**	−0.0134	−0.0015	−0.0024
	(0.0128)	(0.0114)	(0.0221)	(0.0073)
(Never Running)×(M)	−0.0461***	−0.0107	−0.0132	0.0042
	(0.0175)	(0.0138)	(0.0270)	(0.0094)
(Never Running)×(A)	−0.0073	−0.0162	−0.0536*	−0.0034
	(0.0171)	(0.0196)	(0.0322)	(0.0114)
(New Sheriff)	−0.0171	−0.0123	0.0429**	0.0080
	(0.0154)	(0.0112)	(0.0208)	(0.0075)
log(Population)	−0.5473	−0.2072	−0.0989	0.0035
	(0.3829)	(0.2736)	(0.1780)	(0.0714)
Constant	5.4252	2.1002	1.2546	0.0160
	(3.8110)	(2.7265)	(1.7773)	(0.7117)
Month/Year FE	Yes	Yes	Yes	Yes
Sheriff FE	Yes	Yes	Yes	Yes
Clustered By:	Sher.	Sher.	Sher.	Sher.
Observations	36,971	36,971	21,217	33,026
Adjusted R^2	0.4528	0.4950	0.2305	0.2555

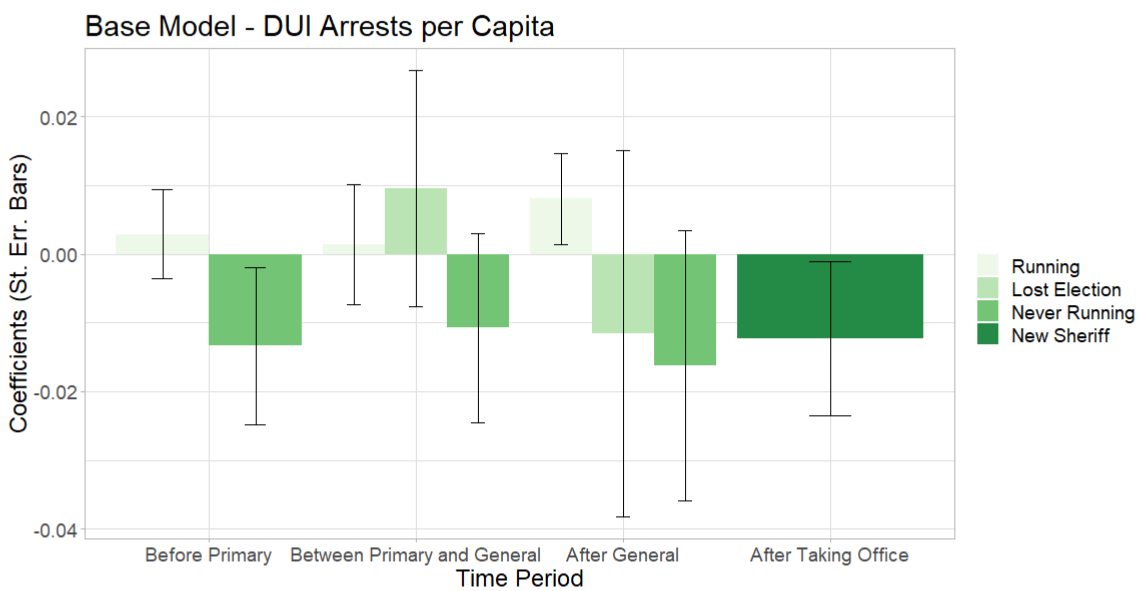
Notes: This table shows the results from the base model for stratified crime types. Both the Drug and DUI specifications are arrests per 1000 population, while the arrest to incident ratio is denoted by A/I. The variable B is an indicator for up to five months before the primary, M is an indicator for between the primary and the general, and A is an indicator for up to five months after the general election. Significance is denoted at the 1%, 5%, and 10% level by ***, **, and *. Standard errors are robust and clustered by sheriff.

Figure A1: Base Model (OLS) Coefficients: Drug Arrests



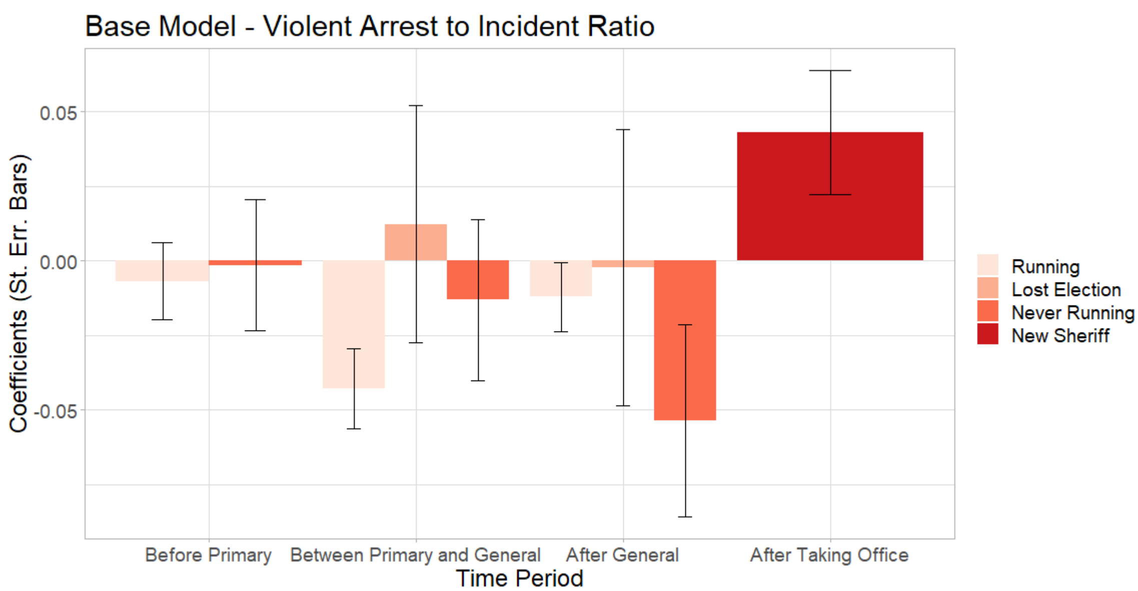
Notes: This figure corresponds to Specification 1 in Table A6.

Figure A2: Base Model (OLS) Coefficients: DUI Arrests



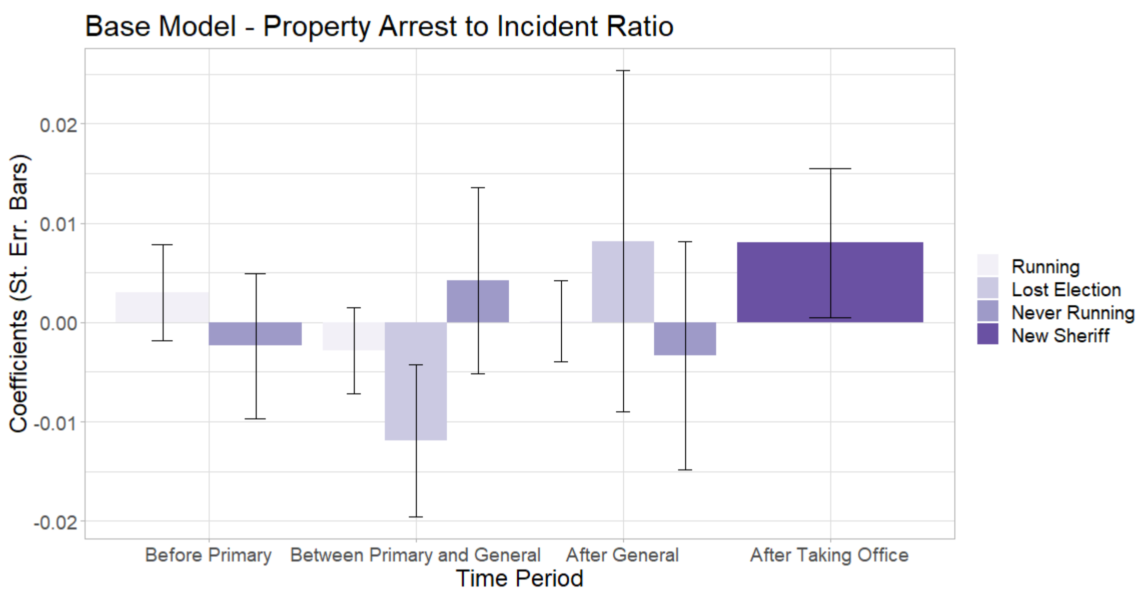
Notes: This figure corresponds to Specification 2 in Table A6.

Figure A3: Base Model (OLS) Coefficients: Violent Arrests



Notes: This figure corresponds to Specification 3 in Table A6.

Figure A4: Base Model (OLS) Coefficients: Property Arrests



Notes: This figure corresponds to Specification 4 in Table A6.