Patronage, Deputy Turnover, and Crime: Evidence from Sheriff Elections

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Abstract

Political transitions can disrupt state capacity when incoming leaders prioritize patronage over merit. I exploit the institutional setting of U.S. county sheriffs – where incoming elected officials retain broad authority to fire deputies for political reasons – to estimate the organizational costs of leadership turnover. Using a sharp regression discontinuity design based on close elections, I find that crime increases by nine percent when incumbent sheriffs are narrowly defeated relative to when they narrowly retain office. Moreover, instrumenting sheriff turnover with term limits yields nearly identical estimates. The increase in crime is concentrated in smaller offices, where courts are more likely to permit politically motivated dismissals of deputies. Specifically, sheriff turnover triggers an immediate spike in deputy departures, eroding the jurisdiction-specific human capital essential for effective policing and highlighting the importance of civil service protections in mitigating the organizational costs of political transitions.
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1 Introduction

The effect of leadership turnover on government performance depends fundamentally on who executes the policies. While replacing incumbents can improve outcomes when actions are taken directly by elected leaders (e.g., Marx et al. 2025), it can severely disrupt organizational capacity when public goods provision relies on a skilled bureaucratic workforce. If incoming

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politicians prioritize political loyalty over merit, leadership transitions threaten to deplete institutional knowledge and degrade public services.

This paper develops a dynamic model of patronage to explain how the mere threat of politically motivated dismissals shapes bureaucratic investment. Unlike environments with deterministic spoils systems, I model a setting where public employees face uncertainty about the clientelistic nature of future leaders. The model shows that workers who face a greater risk of politically motivated dismissal have weaker incentives to invest in jurisdiction-specific human capital, since the returns to such investments depend on continued employment. Consequently, leadership transitions can generate persistent reductions in organizational capacity.

I test these predictions by examining U.S. county sheriffs, the chief law enforcement officials in most suburban and rural communities. Unlike municipal police chiefs – whose personnel decisions are constrained by civil service protections or collective bargaining agreements – sheriffs generally retain broad authority to hire, demote, and dismiss deputies. This environment is particularly well suited for analyzing the impact of political patronage on leadership transitions in advanced economies. If concerns about politically motivated dismissals discourage deputies from investing in jurisdiction-specific knowledge, sheriff turnover should reduce organizational effectiveness and increase crime.

To identify the causal effect of sheriff turnover on local public safety, I employ two complementary empirical strategies. First, I exploit close elections to compare county-level crime trends in jurisdictions where incumbent sheriffs narrowly lost reelection with those where incumbents narrowly retained office. Second, I instrument for sheriff turnover using variation in sheriff term-limit laws.

Both empirical approaches produce remarkably similar results. In the baseline regression discontinuity specification, the narrow electoral defeat of an incumbent sheriff increases crime by approximately ten percent. The instrumental variables estimates closely mirror this magnitude, suggesting that the post-defeat rise in crime reflects a structural consequence of leadership turnover rather than an artifact of heightened electoral conflict or local political instability.

To demonstrate that subordinates' exposure to patronage-based dismissals drives the decline in government performance, I exploit variation in sheriff's office size, which judicial doctrine has linked to the permissibility of political firings. Courts recognize that effective law enforcement in small offices depends on close interpersonal trust between sheriffs and their deputies, and accordingly have been more willing to uphold politically motivated dismissals in smaller departments. In contrast, in larger agencies, deputies are shielded from political retaliation by civil service rules and collective bargaining agreements.

This institutional distinction generates a sharp empirical prediction: the crime increase following an incumbent's defeat should be concentrated in smaller offices. I find exactly this pattern: in smaller offices, where politically motivated dismissals are more likely to be legally permissible, an incumbent's defeat raises crime by 14 percent. In larger agencies, where deputies are better insulated from patronage-driven dismissals, sheriff turnover has a smaller or statistically insignificant effect on crime. This heterogeneity supports the hypothesis that, in smaller communities, incoming sheriffs prioritize political loyalty over law enforcement effectiveness.

To establish this causal chain directly, I analyze the immediate impact of political transitions on personnel stability. Using a regression discontinuity design focused on a three-month post-election window, I find that an incumbent sheriff's narrow defeat triggers a sharp, immediate six percentage point spike in deputy departures. Crucially, in line with the legal boundaries of patronage, this personnel shock is concentrated within smaller departments, where immediate deputy turnover jumps by seven percentage points. This direct evidence of politically induced separations confirms that leadership turnover actively depletes workers' human capital, providing the empirical link between an incumbent's defeat and the subsequent decline in public safety.

I further evaluate the patronage mechanism through several empirical tests designed to rule out alternative explanations. One possibility is that the increase in crime reflects changes in partisan control or ideological priorities, rather than the loss of the incumbent sheriff. To examine this possibility, I analyze open-seat elections in which the incumbent sheriff does not seek reelection. I find that when the candidate from the incumbent sheriff's party loses an open-seat race, there is no corresponding increase in deputy turnover or crime. These results suggest that the disruption is tied specifically to the defeat of the incumbent sheriff, rather than to a broader electoral mandate for policy change.

An alternative explanation is that the results are driven by the managerial inexperience of incoming sheriffs. However, the data indicate that the newly elected sheriffs are typically highly experienced law enforcement professionals. Moreover, candidates with prior experience as a sheriff do not achieve better outcomes than candidates without such experience.

Further, the observed increase in crime may reflect changes in reporting practices rather than a genuine deterioration in public safety. Incumbent sheriffs, for example, may have incentives to underreport offenses in order to portray their administrations more favorably. Under this interpretation, the increase in crime following an incumbent's defeat would primarily capture more complete reporting by the incoming administration rather than an actual rise in criminal activity.

To evaluate this possibility, I examine whether an incumbent sheriff's defeat alters the

composition of reported offenses. Following Levitt 1998, crime categories differ in the extent to which their reporting depends on police resources and discretion. Murders and motor vehicle thefts, for instance, are generally difficult to conceal and are therefore less sensitive to reporting practices. In contrast, offenses such as burglary, rape, and assault are more susceptible to variation in police effort and administrative discretion. If changes in reporting behavior drive the estimated effects, then incumbent defeat should produce systematic shifts in the relative shares of these offense categories. The evidence provides little support for this hypothesis.

Finally, the regression discontinuity results could be due to systematic differences between counties in which incumbents narrowly lost versus counties where incumbents narrowly won reelection. I support the identifying assumption by showing that neither the incumbent's vote share density nor the covariate values exhibit discontinuities as the incumbent's vote share approaches the majority cutoff.

The estimated effects of incumbent defeat are economically and socially substantial. The increase in crime following an incumbent sheriff's defeat persists for approximately four to five years before returning to baseline levels. In smaller jurisdictions, where effective policing depends on local knowledge and strong interpersonal trust, rebuilding depleted organizational capacity may take several years. Given estimates that the social costs of crime account for roughly 13 to 18 percent of U.S. gross domestic product (Anderson 2021), these prolonged periods of organizational disruption impose significant welfare costs on local communities.

More broadly, this paper contributes to the literature on the microeconomic foundations of state capacity. This paper focuses on how government employees rationally underinvest in jurisdiction-specific human capital when facing a credible threat of politically motivated dismissal. Thus this paper is closely related to Aneja and Xu 2024, who study the staggered extension of civil service protections to U.S. Post Office workers between 1878 and 1905 under the Pendleton Act. They find that the reform reduced postal delivery errors by 15-20 percent, operating primarily through a reduction in politically motivated bureaucratic turnover rather than through improved worker selection. This paper complements their findings along four important dimensions.

First, the time period is fundamentally different. Aneja and Xu 2024 examine the Gilded Age, when the United States was first building a formal civil service system. The present study examines the modern era (1976–2020), when federal merit protections have long been in place for most public employees. The persistence of politically motivated dismissals in sheriff's offices thus reveals a contemporary gap in civil service coverage that continues to impose welfare costs on local communities.

Second, the performance measures capture distinct dimensions of public service. Aneja and Xu 2024 measure errors in postal delivery, whereas this paper measures local crime rates, a higher-stakes outcome with direct and quantifiable welfare costs.

Third, and most importantly from a theoretical perspective, the welfare costs in Aneja and Xu 2024 arise in a setting where the patronage regime is effectively deterministic. Under the spoils system, workers could reasonably expect dismissal following a change in political control, rendering prior beliefs about the nature of the regime largely irrelevant. In contrast, this paper develops a dynamic Bayesian model in which workers are uncertain whether future officeholders will be clientelistic or meritocratic and update these beliefs according to Bayes' rule. This process of belief updating implies that episodes of political firing can generate persistent effects on workers' incentives to invest in jurisdiction-specific human capital, producing long-lasting organizational consequences even after the initial political transition has passed.

Fourth, the two papers differ in their empirical designs. Aneja and Xu 2024 exploit the staggered extension of civil service protections across post offices under the Pendleton Act, controlling for whether a presidential election occurred but not for whether the incumbent administration was returned to office. By contrast, this paper's regression discontinuity design identifies the causal effect of sheriff turnover by comparing counties where incumbent sheriffs barely won reelection to those where they barely lost, explicitly conditioning on the incumbent's electoral outcome rather than the mere occurrence of an election. This distinction allows the present paper to isolate the specific role of incumbent defeat—and the patronage-based dismissals that follow—from the broader electoral disruptions that an election-year control would capture.

The paper proceeds as follows. Section 2 develops a dynamic model of patronage and worker human capital. Section 3 describes the institutional background, data, and sample. Section 4 presents the empirical framework. Section 5 reports the main result, namely, the 9 to 10 percent increase in crime following an incumbent sheriff's defeat. Section 6 investigates the mechanisms that could be driving this increase, including deputy turnover, resentment in sheriff offices after narrow losses, and changes in reporting practices. Section 7 assesses economic significance, and Section 8 presents robustness tests. Formal proofs are collected in an online appendix.

2 A Dynamic Model of Patronage and Worker Human Capital

This section models an environment consistent with prior empirical work in which the political supporters of winning candidates are rewarded with government positions for which they may be unqualified (Colonnelli et al. 2020). Moreover, when public employees anticipate that political turnover may lead to their dismissal, their incentives to invest in jurisdiction-specific skills decline.

The model departs from the framework implicit in Aneja and Xu 2024 in one key respect: workers must form beliefs about whether future government officials are clientelistic. In the historical setting of Aneja and Xu 2024, the patronage system was pervasive and its replacement by formal civil service rules was a publicly observable national event. Workers did not need to infer the political character of their employer; the regime type was known. In the present setting, however, incoming government officials may or may not prioritize political loyalty. Thus, workers' human capital investments depend on the likelihood they will face a clientelistic official in the future.

There are infinitely many time periods $t \in \{0, 1, 2, \ldots\}$. Within each period, there is one active worker and the following events occur in sequence:

1. The worker learns their inherited human capital stock (h_t), whether the government official is clientelistic ($s_t = 1$), and the cost of human capital investments (p_t).

2. The worker chooses an investment level, $i_t \in [0, d]$ where $d > 0$, leading to a net period payoff

$$U(h_t, i_t) = h_t - i_t p_t.$$

3. An election occurs, leading to the elected official being reelected ($x_t = 0$) or replaced ($x_t = 1$). The probability that the elected official retains office is $q \in (0.5, 1)$.

4. If the incumbent official is replaced, the type of the new government official s_{t+1} (clientelistic vs. meritocratic) determines whether the incumbent worker is fired or retained.

5. A retained worker's human capital becomes

$$h_{t+1} = i_t + h_t,$$

while a fired worker is replaced with a new worker with baseline human capital $h_{t+1} = h_0$.

Each worker maximizes expected discounted utility, defined as the present value of all future periods' payoffs:

$$E\left[\sum_{t=0}^{\infty} \beta^t U(h_t, i_t) 1(\text{employed at } t)\right],$$

where $\beta \in (0,1)$ is the discount factor and the indicator function "1(employed at t)" equals one for all periods when the worker is employed. If the worker is fired, they cannot benefit from their accumulated jurisdiction-specific human capital, h_t, which has no value outside the current jurisdiction. Because this expertise is non-transferable, the worker's continuation value upon dismissal is normalized to zero.

A government official is of type $s_t \in \{0,1\}$, where $s_t = 1$ denotes clientelistic and $s_t = 0$ meritocratic. If the incumbent is replaced ($x_t = 1$), the new government official is of the same type as the incumbent with probability $r \in (0.5, 1)$ and of a different type with probability $1 - r$; thus r denotes the regime persistence. When an incumbent is replaced, the incoming official retains the inherited worker if meritocratic and dismisses the worker if clientelistic; dismissal thus occurs if and only if $s_{t+1} = 1$.

A clientelistic official ($s_t = 1$) always dismisses an active worker; a meritocratic official ($s_t = 0$) retains the worker. That is, conditional on an incumbent being replaced, dismissal occurs if and only if the state is $s_t = 1$. This deterministic mapping means that dismissal and survival are fully revealing of the government official type.

Thus the "effective" discount factor β_i for a worker employed by an official of type $i \in \{0,1\}$ accounts for the likelihood that the worker is retained in the next period:

$$\beta_0 = \beta\Big(1 - (1-q)(1-r)\Big) \quad \text{and} \quad \beta_1 = \beta\Big(1 - (1-q)r\Big).$$

Because a one-unit investment raises the payoff by one unit in every period the worker remains employed, its present value to a worker currently facing a meritocratic official, denoted v_0, depends only on the effective discount factor β_0. The reason is that such a worker is only ever employed by meritocratic officials: the moment a clientelistic official arrives, the worker is dismissed and the continuation value falls to zero. Survival therefore keeps the worker in state 0, and since β_0 already embeds the per-period probability of retention, the

value of the investment is the geometric sum

$$v_0 = \frac{\beta_0}{1-\beta_0}.$$

Deriving the corresponding expression for a type-1 worker is more involved because future employment depends on the type of official in office. Unlike type-0 workers, type-1 workers may be employed under either clientelistic or meritocratic administrations, so the returns to human-capital investment must account for uncertainty over future political regimes. Nevertheless, the discounted value of a one-unit investment can be expressed as

$$v_1 = \frac{\beta q}{1-\beta q} + \frac{\beta(1-q)(1-r)}{(1-\beta q)(1-\beta_0)}.$$

Thus, a worker employed by an official of type i invests in human capital in period t if the discounted value of the investment exceeds its cost, $v_i \geq p_t$. The costs of human capital investments, $\{p_t\}$, are assumed to be independent and uniformly distributed on $[u_L, u_U]$.

Assumption. The cost bounds satisfy $u_l \leq v_1 < v_0 < u_U$, so both investment thresholds lie strictly inside the support of the cost distribution. Hence investment occurs with positive probability but is not certain under either type of official.

Given that a fired worker obtains a continuation value of 0, the Bellman equation is

$$V(h_t, 0, p_t) = h_t + \max_{0 \leq i_t \leq d} \Big\{ -i_t p_t + \beta_0 V(h_{t+1}, s_t, p_{t+1}) \Big\}$$
$$V(h_t, 1, p_t) = h_t + \max_{0 \leq i_t \leq d} \Big\{ -i_t p_t + \beta q V(h_{t+1}, 1, p_{t+1}) + \beta(1-q)(1-r) V(h_{t+1}, 0, p_{t+1}) \Big\}.$$

The next proposition characterizes the optimal human capital investment policy, as well as showing that the value function for a worker employed by an official of type i is the sum of the value of the existing stock, $(1+v_i)h_t$, the value of future investment opportunities, F_i, and the current-period investment option value, $d \max\{0, v_i - p_t\}$.

Proposition 1

1. *The value function is*

$$V(h_t, 0, p_t) = (1+v_0)h_t + F_0 + d \max\{0, v_0 - p_t\}$$
$$V(h_t, 1, p_t) = (1+v_1)h_t + F_1 + d \max\{0, v_1 - p_t\},$$

where

$$F_0 = \frac{\beta_0 d(v_0 - u_L)^2}{2(1-\beta_0)(u_U - u_L)}$$
$$F_1 = \frac{d}{2(u_U - u_L)}\Big[\frac{\beta q}{1-\beta q}(v_1 - u_L)^2 + \frac{\beta(1-q)(1-r)}{(1-\beta q)(1-\beta_0)}(v_0 - u_L)^2\Big].$$

2. *The optimal investment is a threshold strategy:*

$$i^*(s_t, p_t) = \begin{cases} d & \text{if } p_t \leq v_{s_t} \\ 0 & \text{if } p_t > v_{s_t}. \end{cases} \tag{1}$$

3. *A worker is more likely to invest in human capital if a meritocratic official is in office, compared to when a clientelistic official is in office.*

(Proof in Online Appendix A.)

Because workers invest in jurisdiction-specific human capital, their incentives are directly tied to job security. Thus, with a clientelistic official ($s_t = 1$) the threat of a patronage-based dismissal destroys the long-term expected returns on their investment. The reduction in human capital investments is akin to the findings of Iyer and Mani 2012, who show that inexperienced bureaucrats underinvest in general competence because cultivating political alignment yields higher payoffs.

Given that dismissing a worker eliminates accumulated human capital, it is straightforward to show that, if the accumulated stock is at least as high as what a new worker can accumulate in one period, then an electoral defeat in year t leads to a lower expected human capital stock in year $t+1$.

Proposition 2 *For all $h_t \geq h_0 + d$, $s_t \in \{0, 1\}$, and $p_t \in [u_L, u_U]$,*

$$E[h_{t+1} \mid x_t = 0, s_t, p_t] - E[h_{t+1} \mid x_t = 1, s_t, p_t] \geq (1-r)d > 0.$$

(Proof in Online Appendix B.)

Moreover, the negative impact of electoral defeat on human capital persists.

Proposition 3 *For all $h_t \geq h_0 + d$, $s_t \in \{0, 1\}$, and $p_t \in [u_L, u_U]$,*

$$E[h_{t+2} \mid x_t = 0,\ s_t, p_t] - E[h_{t+2} \mid x_t = 1,\ s_t, p_t] > d(1-r)(1-q)(2r-1) > 0.$$

(Proof in Online Appendix C.)

As demonstrated in Proposition 3, political transitions decrease human capital at time $t + 2$ because the regime is persistent ($r > 1/2$) and turnover is possible ($q < 1$). This two-period human-capital gap vanishes as r converges to $\frac{1}{2}$ reflecting that the gap survives only when clientelism persists across administrations. The intuition behind this persistence is central to understanding the long-term organizational costs of political turnover and is driven by two compounding forces.

First is a mechanical depletion: an incumbent's defeat and the subsequent patronage-based dismissals instantly destroy the accumulated, non-transferable expertise of the existing workforce, replacing experienced deputies with novices at the baseline human capital level, h_0. Second is a dynamic behavioral friction: because the incoming replacement workers operate in an environment where patronage dismissals have just been demonstrated, they face a credible threat that future administrations may also prioritize political loyalty. Consequently, newly hired deputies form more pessimistic expectations about their future tenure and invest less in jurisdiction-specific human capital than the workforce that would have remained in the absence of political turnover. This twofold process – the immediate destruction of accumulated organizational capital through personnel departures and the slower accumulation of replacement capital among new hires – generates persistent reductions in organizational capacity. It also provides the theoretical foundation for the dynamic treatment effects documented in Section 6, which show that the adverse consequences for public safety endure for four to five years before the agency's human capital stock is fully replenished.

The model yields three predictions that guide the empirical analysis. Proposition 1 states that the credible prospect of politically motivated dismissals following an incumbent's defeat should erode law enforcement effectiveness and raise crime. Proposition 2 indicates that this increase should be detectable beginning in the year immediately following the election, since dismissals eliminate the accumulated human capital stock on which effective policing depends. As demonstrated in Proposition 3, the increase in crime is persistent rather than transitory. Organizational damage endures because replacement workers enter with pessimistic expectations about job security and consequently underinvest in jurisdiction-specific human capital. This prediction of long-run persistence is borne out empirically in Figure 7, which shows that the crime effect remains elevated for four to five years before gradually dissipating.

3 Background, data sources and sample

3.1 Background

The model rests on two key institutional features of county law enforcement: first, that sheriffs retain full legal authority to dismiss deputies for political reasons; and second, that deputy sheriffs actively invest in specialized, jurisdiction-specific human capital. This section documents both conditions in U.S. sheriff's offices.

County sheriffs function as the chief law enforcement officers across most suburban and rural U.S. communities, with their deputies operating as the county-level equivalents of municipal police officers. Principal institutional distinctions between elected sheriffs and appointed municipal police chiefs are summarized in Table 1. Because sheriffs are political actors chosen by the electorate, substantial anecdotal evidence indicates that deputy sheriffs face intense pressure to actively support incumbent reelection campaigns and are uniquely vulnerable to retaliatory actions if an incumbent is defeated.[1]

Table 1: Comparisons between U.S. Sheriffs and Police Chiefs

Feature	**Sheriffs**	**Police Chiefs**
Jurisdiction	County	City
Selection	Elected	Appointed[a]
Subordinates	Deputies	Officers
Political firings	Permitted[b]	Prohibited

[a]A small number of police chiefs are elected. [b]The permissibility of politically motivated dismissals for sheriff's deputies is not absolute: it depends on office size and other factors.

Because state constitutions typically vest broad autonomous authority in the office of the sheriff, and because sheriffs in several jurisdictions face direct, personal civil liability for their deputies' operational conduct, the judiciary has consistently held that sheriffs may dismiss deputies for political reasons. In *Terry v. Cook* (866 F.2d 373, 377 (11th Cir. 1989)), for instance, the court held that state constitutional design and a sheriff's personal legal exposure justify requiring political loyalty from deputies.[2] Consequently, even where formal

[1]Medina County Gazette 1972, Humboldt County Grand Jury 1975, Schlichtman 1995, Cherrie 2002, Perez-Trevino 2003, Smith 2017, Finlon 2018, *Jenkins v. Medford*, 119 F.3d 1156 (4th Cir. 1997), and *Ezell v. Darr*, #13-15851, 2015 U.S. App. Lexis 16851 (11th Cir. 2015).

[2]Additional cases articulating this doctrine include *Silva v. Bieluch*, 351 F.3d 1045 (11th Cir. 2003); *Ezell*

civil service protections or collective bargaining agreements exist for county deputies, they frequently provide avenues only to appeal procedural elements of employment actions, rather than eliminating the fundamental legal expectation of political loyalty to the sitting sheriff.[3]

Importantly, judicial doctrine dictates that politically motivated dismissals are more permissible in smaller sheriff's offices. For example, in *Upton v. Thompson* (930 F.2d 1209 (7th Cir. 1991)), the court recognized that effective law enforcement in small departments depends heavily on close interpersonal trust and mutual political alignment between the sheriff and frontline personnel. By contrast, in larger agencies, sheriffs function primarily as administrative executives, delegating day-to-day operational responsibilities to career undersheriffs and entrenched supervisors (Falcone and Wells 1995).

While case law has not established a bright-line numerical threshold for an agency's size, the empirical divide in jurisprudence is distinct. Courts have explicitly highlighted small organizational scales to permit political firings in agencies with ten and eight deputies.[4] Conversely, political firings have been barred in larger agencies spanning 29 and 72 deputies, where the courts ruled that line-level personnel do not occupy policy-making positions.[5] Accordingly, the empirical analysis implements two alternative thresholds for a small office: 20 deputies (the median size in the crime sample) and 24 deputies (the median size in the personnel turnover sample). This size-based legal variation generates the cross-sectional prediction tested in Section 5: the crime increase following an incumbent's defeat should be concentrated in smaller offices, where deputies face the greatest institutional exposure to patronage-driven dismissals.

Law enforcement turnover involves substantial institutional frictions. Deputies who transition between agencies must often forfeit department-specific human capital, sacrifice accumulated pension benefits, or accept rank demotions under state civil service rules. These structural penalties bind a deputy's human capital stock (h_t) closely to their current jurisdiction, making the long-term returns on their ongoing investments (i_t) uniquely vulnerable to political instability. Because these frictions deter voluntary resignations, observed spikes in deputy turnover following an incumbent's defeat are more likely driven by patronage-based dismissals than by personal choice.

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v. Darr, No. 13-15851 (11th Cir. 2015); *Burns v. Cole*, No. 20-2961 (8th Cir. 2021); *Jenkins v. Medford* (119 F.3d 1156 (4th Cir. 1997)); and *Whited v. Fields*, 581 F. Supp. 1444, 1456 (W.D. Va. 1984). The historical framework governing a sheriff's liability for deputies' conduct is discussed in *Ramsey v. Strobach*, 52 Ala. 513 (1875); *State, to Use of Russell v. Moore*, 19 Mo. 369, 371–372 (1854); *Horne v. Allen*, 27 N.C. 36 (1844); and *Miller v. Jones*, 50 Va. (9 Gratt.) 584 (1853).

[3] *Ezell v. Darr*, No. 13-15851 (11th Cir. 2015).

[4] See *Upton*, 1991; *McBee v. Jim Hogg County*, 730 F.2d 1009 (5th Cir. 1984).

[5] See *Hall v. Tollett*, 128 F.3d 418 (6th Cir. 1997); *DiRuzza v. County of Tehama*, 206 F.3d 1304 (9th Cir. 2000).

The jurisdiction-specific capital at risk is substantive. A deputy's non-transferable expertise includes close familiarity with regional neighborhood networks, local geography, and community offender profiles — assets that accumulate over years of service and cannot be quickly replicated by incoming replacements (Quick and Wolff 2025; Hoogesteyn et al. 2025). When leadership transitions prompt sudden personnel departures, the loss of these experienced deputies degrades institutional memory and compromises organizational capability (Mastrobuoni 2019; Quick and Wolff 2025; Hoogesteyn et al. 2025). Because personnel experience is a critical determinant of public safety (Ba et al. 2021), this politically induced human capital depletion may explain why an incumbent's defeat raises crime.

3.2 Data sources

Determining how sheriff defeats affect crime necessitates data on crime, elections, and deputy sheriff employment. The numbers of crimes and deputies in each sheriff's jurisdiction are obtained from the Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest and the Uniform Crime Reporting Program Data: Law Enforcement Officers Killed and Assaulted, 1960–2020.

The sheriff election information is obtained from Zoorob 2022, county election offices, NewsBank, NewspaperArchive, and Community History Archives. I obtained information on the employment histories of current and former peace officers from various sources, including the Ohio Attorney General's Office, the California Commission on Peace Officer Standards and Training, and the National Police Index (`national.cpdp.co`).

The estimation uses demographics as control variables. County population by age and race for 1969-2020 is obtained from the Surveillance, Epidemiology, and End Results, U.S. State and County Population Data. County income per capita is obtained from the 1970, 1980, 1990, 2000, 2010, and 2020 Decennial Census and the 2006–2010 and 2016–2020 American Community Survey (5-year estimates); I interpolated per capita income for the intervening years. Finally, historical consumer price index data used to compute real income per capita were obtained from `https://www.bls.gov/cpi/tables/historical-cpi-u-201710.pdf`.

3.3 Sample

I exclude several states and counties from the analysis. First, states and counties that do not have sheriffs; e.g., Alaska and Riley County (Kansas). Second, states and counties where the sheriff is appointed rather than elected; e.g., Rhode Island and Dade County (Florida). Third, states and counties where sheriffs do not patrol or respond to calls for service, but

instead manage jails and provide courthouse security; e.g., Massachusetts and Glynn County (Georgia). Fourth, states and counties for which I had difficulty obtaining sheriff election information. Panel (a) of Figure 1 displays the geographic distribution of the regression discontinuity sample used to estimate the impact of sheriff defeat on crime.

To ensure comparability of the crime data, the sample is initially restricted to the years 1974–2020.[6] Because the ability to dismiss deputy sheriffs may have changed following the Supreme Court's 1976 decision in *Elrod v. Burns*, the sample is further restricted to 1976–2020.[7]

Information on sheriff deputies' employment is available for a much more limited set of states and years. Panel (b) of Figure 1 displays the geographic distribution of the regression discontinuity sample used to estimate the impact of sheriff defeat on deputy sheriff turnover.

4 Empirical specification

I apply a close-election sharp regression discontinuity framework to identify whether an incumbent sheriff's defeat is associated with an increase in crime. The key identifying assumption is that counties where incumbent sheriffs barely lost are similar in all other ways to those where they barely won. In particular, absent the incumbent sheriff's removal, crime in counties where the incumbent barely lost would have evolved similarly to crime in counties where the sheriff barely won.

If the key identifying assumption holds, the impact of an incumbent defeat on crime can be measured by the change in crime in counties where the incumbent sheriff barely lost, minus the change in crime in counties where the incumbent won by a narrow margin. Specifically let $n^I_{i,t}$ denote the votes received by the incumbent in county i and year t, $n^C_{i,t}$ the highest vote total received among all challengers, and $R_{i,t} = n^I_{i,t}/(n^I_{i,t} + n^C_{i,t})$ the incumbent's vote share over the primary opponent (the "running variable"). Thus, the analysis examines close elections, $|R_{i,t} - 0.5| < b$, and compares county-years where incumbents were barely defeated ($R_{i,t} \in (0.5 - b, 0.5)$) to county-years where incumbents were narrowly reelected ($R_{i,t} \in (0.5, 0.5+b)$). Which elections are "close" is determined by a "bandwidth" b computed from the data using the procedure discussed in Calonico et al. 2014.

In states with partisan primaries, an incumbent's vote share is defined as their lowest

[6]The FBI transitioned to the National Incident-Based Reporting System on January 1, 2021, while the type of assaults recorded changed in 1974.

[7]Before the 1970s, employees fired for political reasons could not obtain legal redress (Marshall 1977). In *Elrod v. Burns* (427 U.S. 347 (1976)), the Supreme Court first limited politically motivated terminations, holding that four sheriff's office employees who were fired for refusing to switch party affiliation had viable First and Fourteenth Amendment claims.

vote share across the primary and general elections. For example, if an incumbent sheriff receives 55% of the vote in the primary and 58% in the general election, the incumbent's vote share is 55%.

For various reasons, the number of crimes reported by each sheriff can be quite noisy. First, there are clear errors in the reported figures. The procedure for identifying these errors and replacing them with missing values is described in the appendix. For example, the sheriff's office in St. Charles County, Missouri, reported only one crime in both 2016 and 2017; these observations are therefore treated as missing, as illustrated in Figure 2a.

Second, in counties served by multiple law enforcement agencies, the number of crimes reported by the sheriff may reflect incidents both within and outside the sheriff's jurisdiction for several reasons. For example, many city police departments outsource dispatching to sheriff's offices, which can inflate the number of crimes attributed to the sheriff; this figure may subsequently decline if cities later bring dispatching in-house. Similarly, some cities contract with the sheriff for law enforcement services, a practice shown to increase the number of crimes reported by the sheriff (Boylan 2022). Consistent with this concern, the number of crimes reported by the Pottawattamie County, Iowa, sheriff's office appears to be on a different scale in 1982–1991 than in 1992–2001, as shown in Figure 2b.

Thus, if the number of crimes within sheriff i's jurisdiction in year t is $c_{i,t}$, the sheriff's office may report $\tilde{c}_{i,t} = \rho_{i,t}c_{i,t}$. I account for this measurement error by examining the percentage change in reported crimes $\ln(\tilde{c}_{i,t+1}) - \ln(\tilde{c}_{i,t})$, which is approximately equal to the percentage change in crimes in the sheriff jurisdiction $(\ln(c_{i,t+1}) - \ln(c_{i,t}))$ when reporting bias does not change drastically from year to year $(\rho_{i,t+1} \approx \rho_{i,t})$.

Consequently, I estimate the impact of an incumbent's reelection on changes in crime as the coefficient ϕ_1 from a local regression of changes in reported crime on the incumbent's vote share over the primary opponent (R_{it}):

$$\ln(\tilde{c}_{i,t+1}) - \ln(\tilde{c}_{i,t}) = \begin{cases} \phi_0 + \phi_1 + \gamma R_{i,t} + \epsilon_{i,t} & \text{if } R_{i,t} \in (0.5, 0.5 + b) \\ \beta_0 + \delta R_{i,t} + \epsilon_{i,t} & \text{if } R_{i,t} \in (0.5 - b, 0.5). \end{cases} \tag{2}$$

Because reelection prevents the disruption that defeat causes, $\phi_1 < 0$ corresponds to the finding that defeat increases crime.

To complement the regression discontinuity design, I also exploit structural variation in sheriff term-limit laws to instrument for leadership turnover. This instrumental variables approach helps rule out alternative explanations specific to close elections, such as heightened

local political instability. Specifically, I estimate the following two-stage least squares system:

$$\text{Turnover}_{i,t} = \pi_0 + \pi_1 \text{Term-Limited}_{i,t} + X'_{i,t}\Pi + \mu_t + \nu_{i,t} \tag{3}$$

$$\ln(\tilde{c}_{i,t+1}) - \ln(\tilde{c}_{i,t}) = \phi_0 + \phi_1 \widehat{\text{Turnover}}_{i,t} + X'_{i,t}\Theta + \mu_t + \xi_{i,t} \tag{4}$$

where $\text{Turnover}_{i,t}$ is an indicator for whether the incumbent sheriff is replaced (either by losing reelection or leaving office), and $\text{Term-Limited}_{i,t}$ is an exogenous indicator equal to one if the incumbent is barred by law from seeking reelection. The vector $X_{i,t}$ contains the county-level demographic controls (population, racial composition, and real income per capita), μ_t represents year-fixed effects to absorb macroeconomic or national crime trends, and $\xi_{i,t}$ is the error term. The parameter of interest is ϕ_1, which captures the effect of sheriff turnover on crime growth.

5 Main Result

First, I examine the main result: the impact of an incumbent candidate's defeat on crime. Second, I present evidence supporting the validity of the regression discontinuity design in sheriff elections.

5.1 Impact of the sheriff's defeat on crime

I examine the relation between the incumbent's share of the vote and crime graphically and then through local regression.

Graphically, I divide the incumbent's vote share into 40 equal-sized bins. I then calculate the percentage change in crime for all observations within a bin. In panel (a), Figure 3, each dot plots the average change in crime against the incumbent's share of the vote. Moreover, fourth-order polynomials that best approximate the dots are estimated, separately, for incumbent sheriffs who are reelected versus those who are defeated. Overall, crime tends to increase when incumbent sheriffs are defeated and tends to decrease when they are reelected. Additionally, the increase in crime after a sheriff's defeat holds for close elections.

When I use a local linear regression to estimate the relation between the incumbent vote share over the primary opponent and the change in crime (Equation 2), I find that a sheriff's defeat increases crime by nine percent in close elections (see column (1) in Table 2).

Regression discontinuity estimates are sensitive to the bandwidth choice: a smaller bandwidth reduces bias by restricting the sample to nearly identical observations, but raises standard errors due to the smaller sample size. Table 2 reports results for the bandwidth

that minimizes the sum of mean square error and variance Calonico et al. 2014, namely a bandwidth of 12% points. However, the results remain qualitatively unchanged if different bandwidths are used (see Table 3).

5.2 Validity of the regression discontinuity framework

The identification condition of the impact of incumbent defeat assumes that elections won by the incumbent are ex ante comparable to elections won by the challenger. Thus, to ensure the validity of the regression discontinuity results, I test for continuity at the reelection threshold across five variables: the density of the incumbent's vote share, three control variables, and the crime rate in neighboring counties (a "placebo outcome").

Specifically, the regression discontinuity framework would be invalid if some incumbents manipulated election results. Under such manipulations, any observed effects of turnovers on performance could be driven by unobservable differences between elections in which the incumbent is reelected through manipulations and those in which the incumbent is not.

Figure 4 plots a histogram approximating the density of the incumbent's vote share over the primary opponent. The figure also overlays a local polynomial estimate of the density, computed separately on either side of the 0.5 cutoff, along with its confidence band (Cattaneo et al. 2020b). If incumbent sheriffs manipulated elections, they would be more likely to narrowly win than narrowly lose, leading to a discontinuous upward jump in the density at the cutoff. The absence of such a jump in Figure 4, therefore, provides evidence against electoral manipulation.

A more formal test based on local polynomial estimation of the density function fails to reject the null of no discontinuity, with a p-value of 0.75 (Cattaneo et al. 2020b). An approximate sign test likewise finds no evidence of discontinuity, yielding a p-value of 0.61 (Bugni and Canay 2021).

Another concern is that the characteristics of counties where sheriffs are defeated differ from those in counties where sheriffs are reelected. This hypothesis can be tested by estimating the local regression (2) with the log of the county population, the share of the population that is Black, and the log of real income per capita as the dependent variables. I find no demographic differences between counties where the incumbent sheriff is reelected and those where the incumbent is defeated (see Table 4).

Alternatively, the estimated impact of sheriff defeat on crime can incorporate the log of county population, the share of the population that is Black, and the log of real income per capita, using the estimator proposed by Calonico et al. 2019. The main result remains robust, as the estimate is unchanged (see column (2) in Table 2).

Additionally, crime trends could affect a sheriff's reelection, so that the expectation of rising crime drives the incumbent's defeat rather than the reverse. This hypothesis can be tested by assuming that crime trends are correlated across neighboring counties and examining whether a sheriff's defeat influences crime in those neighboring jurisdictions.

Specifically, I use a local linear regression to estimate the relationship between the incumbent's vote share over their primary opponent and changes in crime in neighboring counties, and I find no statistically significant effect (see column (5) in Table 2). Thus, the impact of incumbent defeat on crime does not appear to be driven by factors that are correlated with crime trends in neighboring counties.

Finally, I estimate the effect of the incumbent's vote share on crime using placebo cutoffs – for example, assuming that an incumbent sheriff is reelected with 49 percent of the vote. The motivation for this test is that evidence of a discontinuity away from the true cutoff would cast doubt on the validity of the regression discontinuity design (Cattaneo et al. 2020a). To prevent real treatment effects from biasing the placebo tests, the sample is restricted to elections in which the incumbent won when using cutoffs above the true threshold, and to elections in which the incumbent lost when using cutoffs below the true threshold. This sample restriction eliminates contamination from the actual discontinuity.

The estimated coefficients measuring the effect of reelection on crime at the artificial cutoffs, along with their 95 percent confidence intervals, are presented in Figure 5. The true cutoff at 0.5 is included as a benchmark for comparison with the placebo cutoffs. None of the artificial cutoffs produce coefficients that are statistically significant at conventional levels, providing empirical support for the conclusion that sheriff defeats increase crime.

6 Mechanisms

This section investigates the channels driving the post-defeat increase in crime. First, I isolate the roles of political partisanship and institutional memory by comparing incumbent defeats with open-seat party transitions and by evaluating incoming sheriffs' managerial experience. Second, I examine whether the disruption is an artifact of electoral selection or a structural consequence of leadership change, exploiting term limits as an independent source of variation. Finally, I test the core theoretical mechanism directly, examining whether political transitions trigger deputy turnover and whether this effect varies with an agency's exposure to patronage under judicial doctrine.

6.1 Political Channels vs. Managerial Experience

I first examine whether partisanship explains the increase in crime after a sheriff's defeat. Specifically, I use a local linear regression to estimate the relationship between the incumbent sheriff's party vote share and changes in crime in elections in which the incumbent did not seek reelection. I find that the defeat of the incumbent party candidate does not affect crime (see column (3) in Table 2). Moreover, using the procedure in Calonico et al. 2025, I can show that the effect of an incumbent's defeat on crime is statistically different from the effect of an incumbent party's defeat at the five percent significance level (i.e., the coefficients in Table 2 columns (1) and (3) are statistically different from one another).

Thus, the increase in crime does not appear to be caused by costly policy changes resulting from a change in the sheriff's party. Moreover, the increase in crime does not appear to be caused by mismatches in priorities between a sheriff from one party and deputies from another party.

The absence of an effect of incumbent party defeat on crime is consistent with the findings in Ferreira and Gyourko 2009 and de Benedictis-Kessner et al. 2025. Specifically, these papers employ a regression discontinuity framework to demonstrate that a mayor's party affiliation does not impact policies or priorities regarding law enforcement. The results in this paper are also consistent with the finding that the sheriff's party affiliation does not affect compliance with federal requests to detain unauthorized immigrants (Thompson 2020). Thus, in open-seat elections, an incumbent party's defeat should not affect crime.

I also examine whether the increase in crime following an incumbent sheriff's defeat is driven by the inexperience of the newly elected sheriff. In practice, most incoming sheriffs possess substantial law enforcement and managerial experience. For example, Ohio law requires sheriff candidates to have "at least five years of full-time law enforcement experience" and "at least two years of supervisory experience" (Baldwin's Ohio Revised Code Annotated, 311.01). Likewise, the data used in this study indicate that sheriffs elected in California between 1982 and 2022 averaged nearly 24 years of law enforcement experience.

Nevertheless, some skills necessary for effective leadership may only be acquired through prior service as sheriff. To evaluate this possibility, I examine whether counties that elect a former sheriff experience smaller increases in crime than counties that elect a candidate without prior experience as sheriff. Specifically, I regress changes in crime on an indicator for whether the winning candidate previously served as sheriff, restricting the sample to elections in which the incumbent is not reelected.

The results provide no evidence that prior experience as sheriff affects crime. As reported in Regression (4) of Table 5, the election of a former sheriff has no statistically significant

effect on crime. This finding suggests that the increase in crime following an incumbent sheriff's defeat is unlikely to be driven by the newly elected sheriff's lack of experience.

6.2 Structural Disruption vs. Electoral Resentment

I examine next whether the effect of an incumbent's defeat on crime stems from resentment generated by a narrow loss. To test this hypothesis, I analyze turnover induced by term limits. Specifically, I regress the change in crime on whether the sheriff is reelected, instrumenting the likelihood of reelection with an indicator for whether the incumbent is term-limited. I find that a sheriff's defeat increases crime by 10 percent (Regression (1) in Table 5), an effect indistinguishable from the regression discontinuity estimate.[8]

The primary threat to the instrumental variable's exclusion restriction is the "lame duck" effect documented in the political economy literature (e.g., Besley and Case 1995). If term-limited sheriffs shirk their duties or alter their enforcement priorities because they no longer face electoral accountability, one would expect crime to decrease once they are replaced by a new, motivated sheriff. I test this "lame duck" hypothesis directly by examining crime dynamics in the periods leading up to a transition.

Furthermore, the regression discontinuity and instrumental variable designs identify Local Average Treatment Effects for distinct sub-populations: the former isolates highly competitive electoral environments, while the latter exploits legally mandated transitions. The fact that both empirical strategies yield a nearly identical 9 to 10 percent increase in crime indicates that the post-turnover decline in public safety is a consequence of leadership disruption, rather than an artifact of political divisiveness, electoral instability, or diminished incumbent motivation.

6.3 Personnel Turnover and Patronage Heterogeneity

To establish the causal chain linking political transitions to public safety, I examine three empirical questions in turn: whether an incumbent's defeat triggers an immediate wave of deputy departures; whether this disruption is concentrated in smaller jurisdictions where deputies face greater institutional exposure to patronage; and whether the observed crime increases reflect a genuine deterioration in public safety rather than altered reporting practices.

I begin by testing whether incumbent defeats generate personnel shocks in practice. Figure 3b plots average deputy turnover rates against the incumbent's vote share, restricting the data to a three-month post-election window to isolate immediate, involuntary departures

[8]The first stage F-statistic vastly exceeds the 104.7 threshold recommended by Lee et al. 2022.

from normal career attrition. The figure reveals a sharp, discontinuous jump in deputy turnover at the 0.5 reelection threshold.

The local linear regression estimates in Table 6 confirm this pattern. An incumbent's narrow defeat raises deputy turnover by approximately six percentage points within the first quarter of the new administration (Column 1). This provides direct empirical support for the model's firing mechanism. In open-seat contests where no incumbent is running, the narrow defeat of the candidate from the incumbent's party has no corresponding effect on turnover (Column 2). This contrast indicates that the personnel shock is tied to the defeat of the individual sheriff, and the political allegiances that defeat disrupts, rather than to partisan ideology or institutional mismatch.

Turning to the second question, I exploit the size-based variation in judicial doctrine described in Section 3.1. Because courts are more likely to permit patronage-driven dismissals in smaller departments, an incumbent's defeat should raise crime primarily in small offices. I partition the sample using the median agency size in each dataset: offices with 20 or fewer deputies in the crime sample, and 24 or fewer deputies in the personnel sample.

The regression discontinuity estimates support this prediction.Table 7 shows that an incumbent's narrow defeat raises crime by 14 percent in offices with 20 or fewer deputies, while the effect in larger offices is small and only statistically significant at the 10 percent confidence level or not significant at all. Crucially, this size-based variation is not merely an artifact of close elections; the instrumental variable estimates using term limits yield nearly identical patterns. As shown in Table , sheriff turnover induced by term limits increases crime by 12 to 15 percent in smaller offices, whereas the effects in larger agencies are substantially smaller or statistically insignificant. Together, these complementary empirical strategies confirm that the public safety decline is concentrated precisely where deputies are most exposed to patronage-based dismissal.

The personnel data reinforce the mechanism: an incumbent's defeat raises immediate deputy turnover by 7 percentage points in small offices, compared to 4 percentage points in larger ones (the last four columns of Table 6). Together, these patterns confirm that the public safety decline is concentrated precisely where deputies are most exposed to patronage-based dismissal.

One remaining concern is that the post-election crime increase is a statistical artifact of altered administrative reporting rather than a genuine deterioration in public safety. If outgoing sheriffs systematically underreport crimes to inflate their performance record before an election, the transition to a new administration would appear to raise crime purely through more complete reporting by incoming officials.

Following Levitt 1998, I exploit the fact that crime categories differ in how much their

reporting depends on police discretion. Offenses like murder are highly visible and difficult to conceal, making them insensitive to reporting practices. Offenses such as burglary, rape, and assault are more susceptible to variation in police effort and administrative discretion. If reporting changes – rather than enforcement changes – drove the estimated effects, the share of highly discretionary offense categories would rise systematically after an incumbent's defeat.

Table 8 reports regression discontinuity estimates of year-over-year changes in each offense category's share of total reported crime, restricting the sample to offices with 20 or fewer deputies where the primary treatment effects reside. Across all categories, the estimated coefficients are small and statistically insignificant, with no evidence of systematic drift toward discretionary offenses. These findings reject the reporting bias hypothesis and confirm that the post-defeat increase in crime reflects a genuine decline in public safety.

7 Economic significance of sheriff turnover

Although the baseline estimates establish that crime rises following incumbent turnover, the aggregate social costs depend on two additional factors: how frequently defeats occur and how long the resulting crime increase persists. On the first dimension, the aggregate social cost is partially mitigated by the relative rarity of incumbent turnover, which occur in roughly 37 percent of elections. On the second dimension, there are strong reasons to expect the effects of politically motivated dismissals and retaliatory personnel changes to be long-lasting.

Because rebuilding jurisdiction-specific human capital takes time – newly installed deputies must relearn neighborhood networks, offender profiles, and local institutional relationships from scratch – the organizational damage from a patronage-driven transition is not quickly undone. Disruptions of this kind are therefore likely to depress agency effectiveness for an extended period rather than producing a brief, transitory spike in crime.

To examine the duration of these effects, Figure 6 presents dynamic treatment effects over an eight-year horizon, spanning two years before the election and six years after. Each coefficient measures the change in crime relative to the election year. The analysis is restricted to offices with 20 or fewer deputies, the settings where politically motivated firings are most likely.

The parallel-trends identifying assumptions underlying both frameworks hold. Prior to the election, crime rates in counties where incumbents narrowly lost evolved identically to those in counties where incumbents narrowly won reelection. Likewise, crime trends in counties where sheriffs were term-limited tracked those in counties where they were not.

These pre-election patterns validate the parallel-trends assumption in both the regression discontinuity and instrumental variable designs.

After the election, in counties where the incumbent sheriff is replaced, crime does not simply spike and revert. The regression discontinuity design reveals a sustained 7 to 16 percent increase in crime lasting several years before finally returning to the original baseline in year six. The instrumental variable estimator – using term limits as an instrument – also shows a 10 to 24 percent increase in crime persisting for four years before reverting to baseline.

These persistent effects suggest that sheriff turnover generates substantial organizational frictions; in smaller offices, where effective policing depends on interpersonal trust and local knowledge, rebuilding capacity may require several years. This pattern is consistent with Proposition 2, which explains why the human capital gap is persistent. Because fired workers are replaced by total novices at baseline capital (h_0), and surviving workers adopt more pessimistic beliefs, rebuilding the department's aggregate human capital stock takes several periods. The results parallel the findings of Akhtari et al. 2022, who show that political turnover in Brazil reduced student test scores for five years, despite personnel changes being concentrated immediately post-election.

Consequently, even though incumbent sheriffs are defeated infrequently, a persistent increase in crime lasting four to five years imposes sizable social costs on affected communities. More broadly, the findings suggest that political turnover can generate prolonged declines in the performance of government organizations that rely heavily on experienced personnel, specialized human capital, and stable organizational relationships.

Finally, it is also notable that the 95 percent confidence intervals in Figure 6 widen at longer time horizons. The growing uncertainty likely reflects increasing variation in how crime is reported over time, as illustrated in Figure 2b, since crime statistics measured in periods close to one another tend to be recorded more consistently than those measured several years apart.

8 Robustness tests

I conduct several robustness exercises to assess whether the baseline findings depend on particular measurement choices, sample restrictions, or definitions of the running variable.

The baseline specification measures crime in logarithms to account for intertemporal and cross-sectional differences in reporting methodologies (see Figure 2b). Nevertheless, the main conclusions remain unchanged when crime is measured per capita. Under this alternative specification, sheriff turnover continues to produce statistically significant increases in crime.

In the regression discontinuity framework, the estimated effect is statistically significant at the ten percent level for the full sample and at the five percent level when the analysis is restricted to small sheriff's offices. Using the instrumental variables specification, the effect remains statistically significant at the one percent level in both the full sample and the small-office subsample.

I also examine whether the results are sensitive to the treatment of anomalous crime observations. The baseline analysis excludes outliers identified using the procedure described in the appendix. Re-estimating the models without removing any observations yields nearly identical conclusions: sheriff turnover continues to increase crime. The estimated effects remain statistically significant at the five percent level in the regression discontinuity framework and at the one percent level in the instrumental variables specification.

Finally, I test whether the findings depend on the construction of the running variable. In the baseline analysis, the incumbent's vote share is defined using vote shares from both the primary and general elections. As an alternative, I define the running variable in terms of the incumbent's vote share in the general election. The results remain qualitatively and quantitatively similar under this alternative definition. An incumbent sheriff's defeat continues to be associated with a statistically significant increase in crime, with significance at the five percent level in the regression discontinuity analysis and at the one percent level in the instrumental variables specification.

Taken together, these robustness checks indicate that specific modeling choices and data-cleaning procedures do not drive the estimated increase in crime following sheriff turnover.

9 Conclusion

Despite the perceived job security of police officers, I find that electoral defeats of incumbent sheriffs lead to substantial turnover among deputy sheriffs.[9] This pattern is consistent with anecdotes of deputies being pressured to campaign for incumbents and facing retaliation following incumbents' losses (Medina County Gazette 1972; Humboldt County Grand Jury 1975; Schlichtman 1995; Cherrie 2002; Perez-Trevino 2003; Smith 2017; Finlon 2018). Such turnover is legally enabled by judicial doctrine, which affords sheriffs considerably greater discretion to dismiss deputies on political grounds than for municipal police officers.[10]

[9]The difficulty of firing police officers is discussed in Fisk and Richardson 2017 and Rushin 2019.

[10]For police officers, *Stephens v. Kerrigan*, 122 F.3d 171 (3d Cir. 1997), states that "It is not disputed that ... positions of sergeant and lieutenant do not require a political affiliation"; similarly, in *Saullo v. Burough of Nesquehoning*, Civil No. 3:16-CV-00811 (M.D. Pa. Sep. 21, 2020) "by alleging he was employed as a police officer, Saullo has shown he was employed at a public agency in a position that does not require political affiliation." In contrast, in many circumstances, deputy sheriffs can be subject to politically-based firings (Berry 2001).

Politically motivated deputy sheriff departures are consequential since they are associated with a 9 to 10 percent increase in crime, a finding consistent with the model's prediction that patronage-based dismissals erode the jurisdiction-specific human capital stock that effective policing requires. Moreover, departing deputy sheriffs face substantial obstacles when seeking new employment and may forfeit accrued pension benefits. Consequently, the prospects of politically motivated dismissals make employment as a sheriff's deputy less attractive and may contribute to ongoing difficulties in recruiting and retaining deputies (Police Executive Research Forum 2019; Chapman 2025). Dal Bó et al. 2013 show that the compensation and security offered by a government post shape both the quantity and quality of applicants: higher-quality candidates are more responsive to improvements in job attractiveness. Applied here, the threat of patronage-based dismissal functions as a non-pecuniary reduction in job quality, which should reduce the pool of high-ability candidates willing to serve as deputies and compound the human capital losses documented above. Moreover, consistent with Proposition 3, the damage is not quickly undone.

The increase in crime and staffing instability following an incumbent sheriff's defeat is socially costly because sheriffs provide the primary law enforcement for 83% of suburban and 92% of rural U.S. counties (National Sheriffs' Association 1978). To mitigate the volatility of these electoral transitions, standard police employment protections could be extended to sheriff's deputies. Nonetheless, any increase in job security may reduce deputies' accountability (Dharmapala et al. 2022).

Moreover, the sheriffs' ability to dismiss deputies contributes to higher crime rates following an incumbent's defeat, adding to the body of evidence that constraining politicians' firing authority can improve bureaucratic performance (Xu 2018; Ornaghi 2019; Aneja and Xu 2024). This paper is most directly comparable to Aneja and Xu 2024, who find that civil service protections reduced postal delivery errors. Both papers document that politically motivated firing reduces government performance, but the present paper adds a dynamic learning mechanism absent from their framework, explaining why organizational damage persists beyond the initial personnel disruption.

These findings closely complement the insights of Iyer and Mani 2012. While they demonstrate that restricting a politician's ability to shuffle bureaucrats can improve baseline incentives to build competence, this paper underscores an even more fundamental prerequisite for administrative capacity: basic civil service protection against politically motivated dismissals. Insulating frontline implementers from outright termination is a critical institutional guardrail required to prevent severe, multi-year disruptions to public goods provision, such as public safety.

This study contributes to the broader literature on the consequences of incumbent elec-

toral defeat. Building on existing empirical research, the findings demonstrate that electoral turnover can either diminish or enhance government performance (Coviello and Gagliarducci 2017; Akhtari et al. 2022; Marx et al. 2025; Bazzi et al. 2025). Specifically, performance tends to decline in tasks delegated to career civil servants – such as teachers and sheriff's deputies – while improving in areas under the direct purview of elected officials, including tax policy, contract allocation, and hiring. These results suggest that when assigning policy tasks to elected versus appointed officials, policymakers must account for how these roles affect performance during political transitions, alongside the trade-offs identified by Besley and Coate 2003; Maskin and Tirole 2004; Alesina and Tabellini 2007, and Iaryczower et al. 2013.

These findings carry direct implications for how policymakers should think about building and preserving state capacity (Evans and Rauch 1999; Aneja and Xu 2024). The results show that the absence of civil service protection does not merely select lower-quality personnel into government positions; it actively destroys the incentive of otherwise capable frontline agents to invest in the jurisdiction-specific human capital that effective public goods provision requires. From this perspective, extending merit-based employment protections to sheriff's deputies is not merely a labor relations question but a state capacity investment. More generally, the findings suggest that any analysis of institutional reform that focuses solely on the quality of elected leadership, while neglecting the employment security of the career workforce beneath it, will systematically underestimate the organizational costs of political transitions.

Finally, this study addresses the ongoing debate surrounding term limits for criminal justice officials. While a significant majority of surveyed voters favor enacting or shortening term limits for district attorneys and sheriffs (Petrow-Cohen 2024), empirical research has been lacking. To my knowledge, this paper provides the first empirical estimates of the effect of term limits for sheriffs on crime rates.

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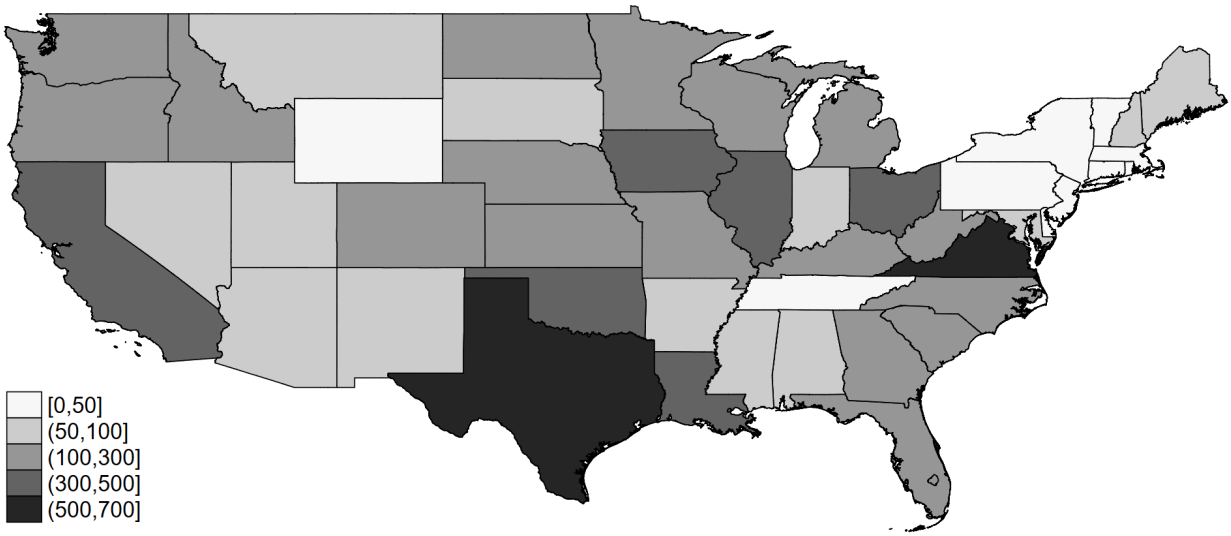
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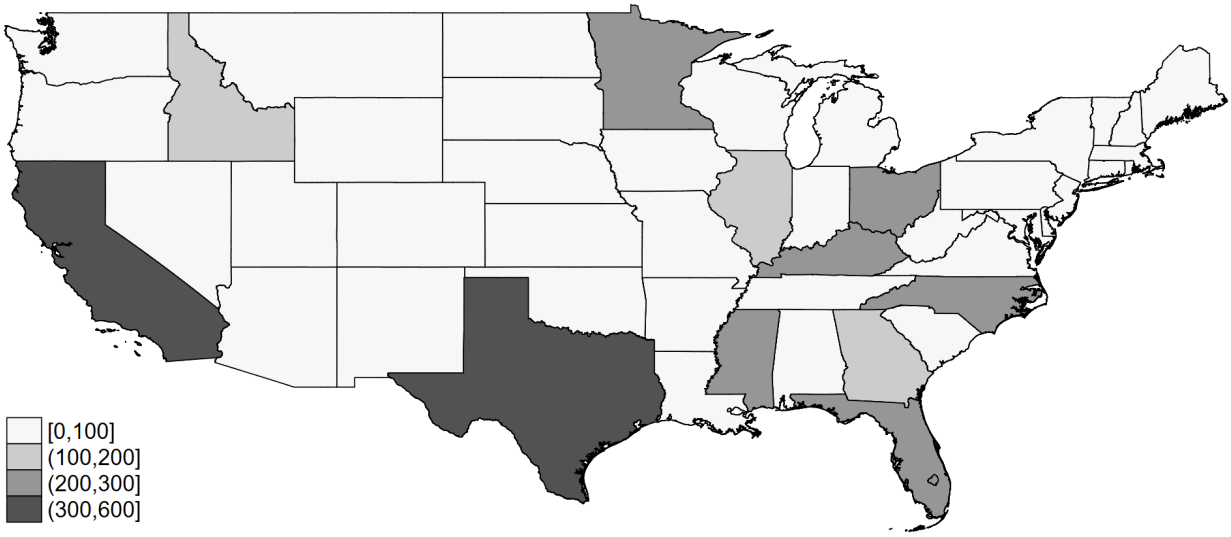
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Figure 1: Number of observations for the regression discontinuity analysis of the impact of sheriff defeat on crime and deputy sheriff turnover

(a) Impact of sheriff turnover on crime



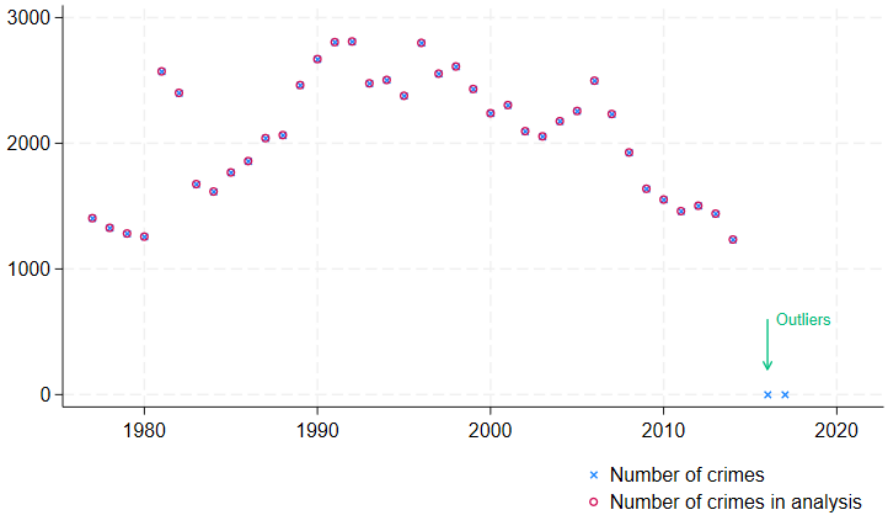
(b) Impact of sheriff turnover on deputy sheriff turnover



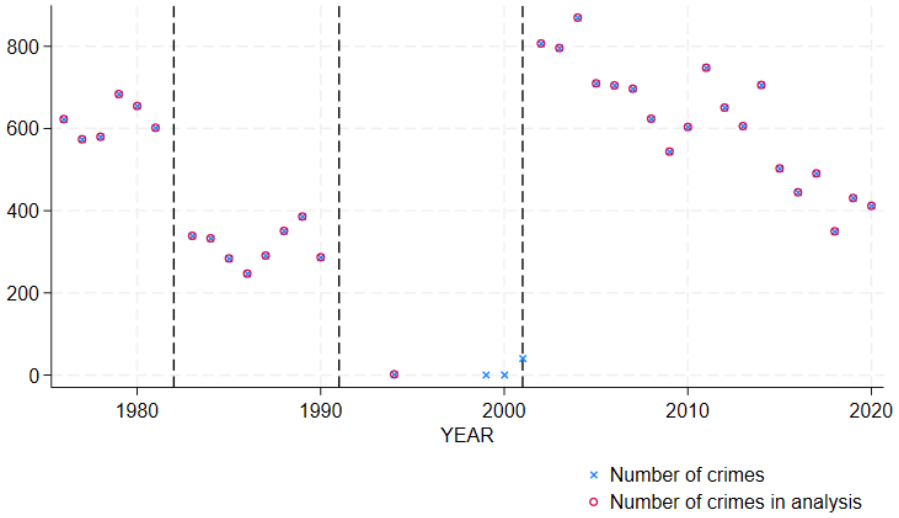
Data sources are listed in Section 3.

Figure 2: Number of reported crimes and number of crimes used in this paper for two sheriff agencies

(a) St. Charles County, Missouri



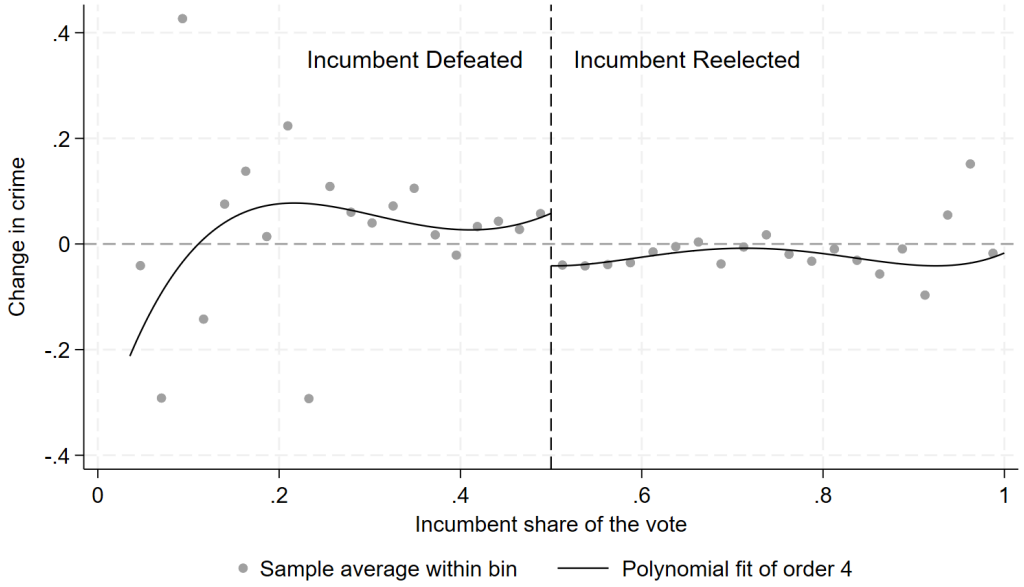
(b) Pottawattamie County, Iowa



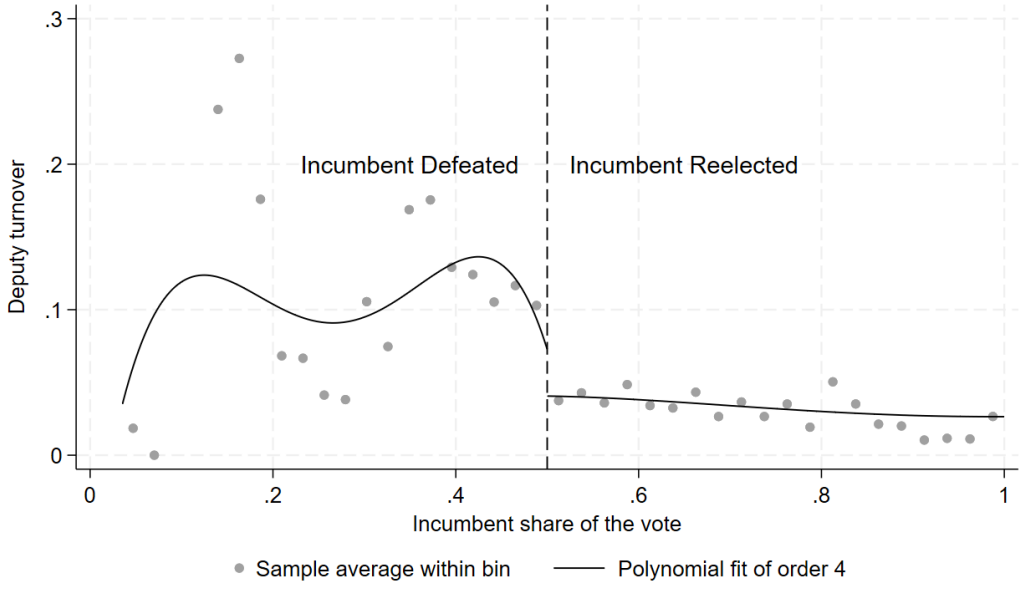
Pottawattamie County's sheriff office appears to use a similar methodology in reporting crimes for the years within the vertical dashed lines in panel (b).

Figure 3: Crime and deputy sheriff turnover as a function of the incumbent's vote share over primary opponent

(a) Change in crime rate

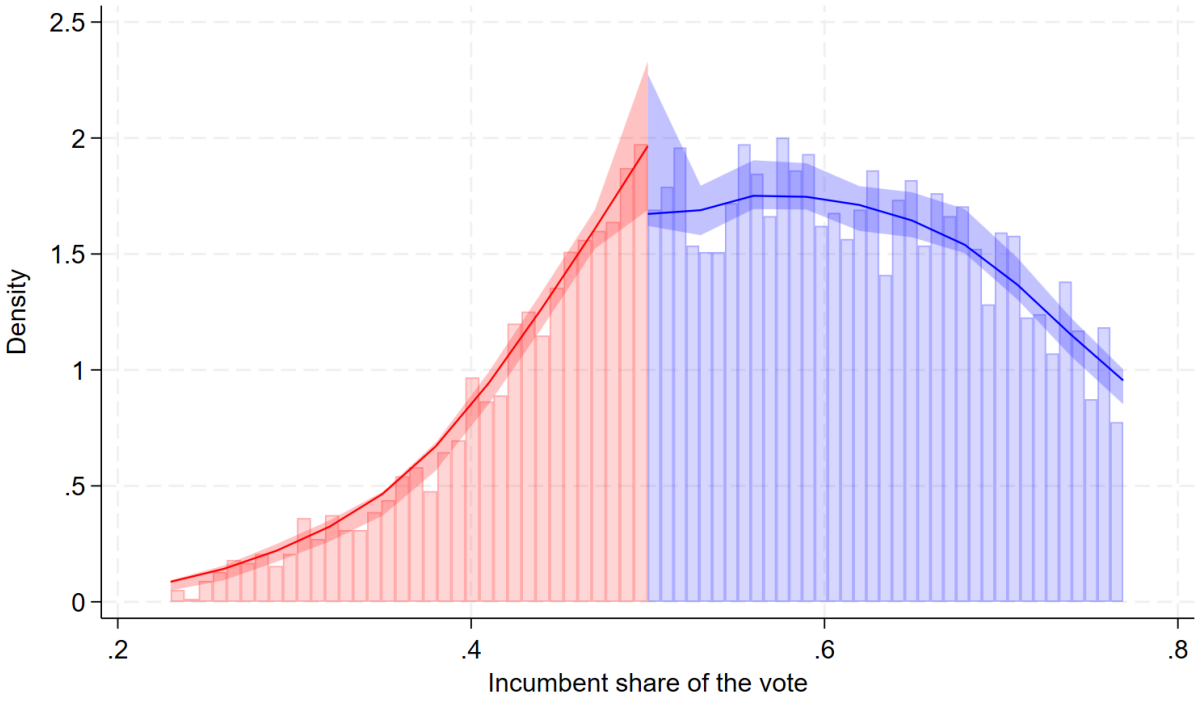


(b) Deputy sheriff turnover



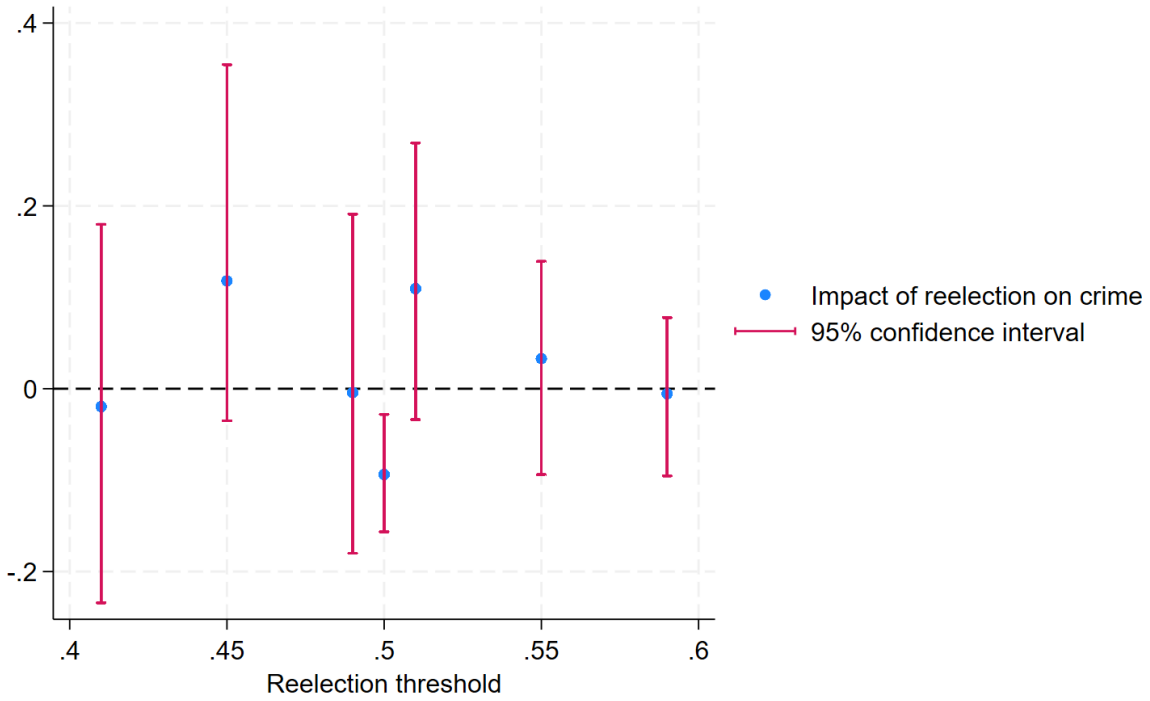
Values are split into 40 bins of equal size. Each dot plots the average outcome against the incumbent's vote share (within a specific bin), adding a fourth-order polynomial fit estimated separately for incumbent sheriffs who win and lose reelection. The data are discussed in Section 3.

Figure 4: Density of incumbent margin of victory over primary opponent



The horizontal axis denotes the incumbent's margins of victory, where values below 0.5 indicate an incumbent sheriff defeat, while values above 0.5 indicate an incumbent victory. I graph a histogram for the data, a local polynomial approximation to the histogram, and a confidence band. The data are discussed in Section 3. The graph is generated by Matias D. Cattaneo & Michael Jansson & Xinwei Ma, 2022. "RDDENSITY: Stata module to perform Manipulation Testing Using Local Polynomial Density Estimation," Statistical Software Components S459094, Boston College Department of Economics. The documentation for the Stata module explains why the confidence band is not centered on the point estimate.

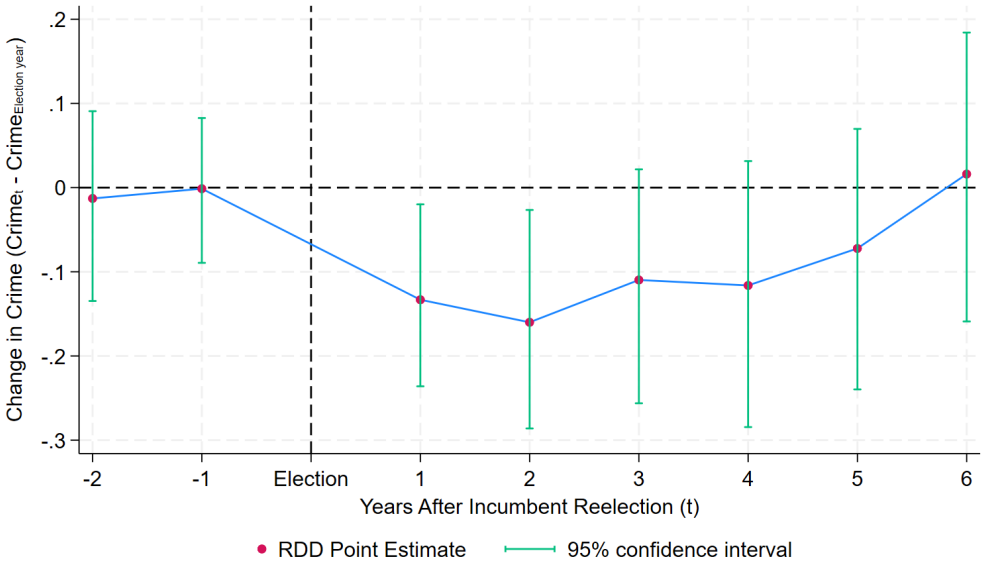
Figure 5: Impact of reelection on crime using true and placebo minimum vote share for victory



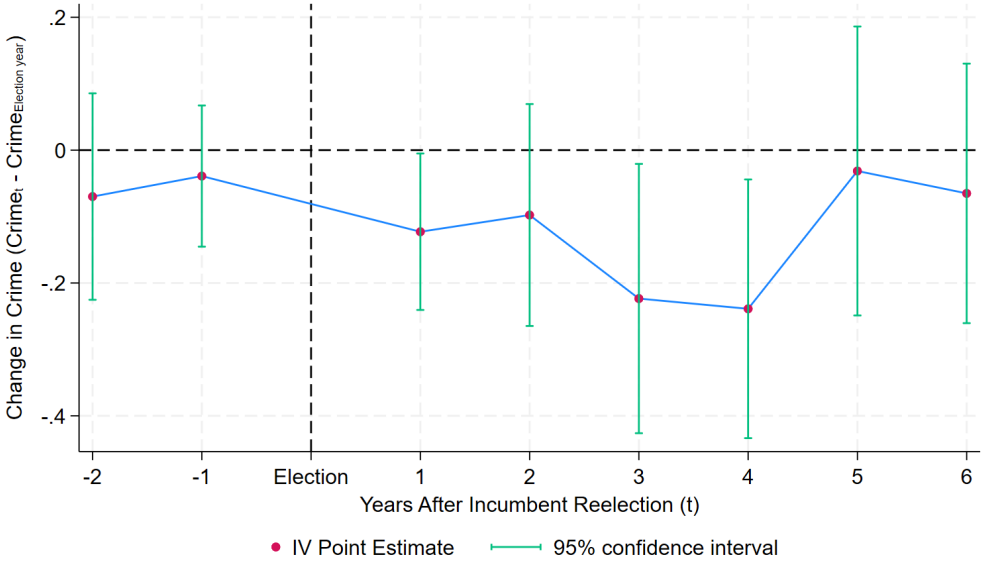
The horizontal axis denotes the minimum incumbent's vote share necessary for reelection (true and placebo). The vertical axis denotes the estimated impact of reelection on crime (true reelection when the threshold for victory is 0.5, and placebo reelection when the threshold for victory is different than 0.5). The bandwidth is selected using the procedure in Calonico et al. 2014. The data are discussed in Section 3.

Figure 6: Dynamic Impact of Incumbent Reelection on Crime Rates

(a) Regression discontinuity



(b) Instrumental variable



Both panels plot estimates of the impact of an incumbent sheriff's reelection on local crime rates over an eight-year horizon ($n = -2, -1, 1, 2, 3, 4, 5, 6$). The sample is restricted to offices with 20 or fewer deputies. Each point estimate represents the change in crime between the election year and n years post-election ($\text{Crime}_n - \text{Crime}_{\text{Election year}}$). Vertical bars indicate 95% confidence intervals calculated using robust standard errors clustered at the county level.

Table 2: Change in crime rate after incumbent and incumbent-party reelection

	Sheriff jurisdiction				Neigh. sher. juris.			
	Incumbent		Inc. party		Incumbent		Inc. party	
	1	2	3	4	5	6	7	8
Reelection	-0.094**	-0.092**	0.024	0.017	0.006	0.008	-0.014	-0.015
	(0.028)	(0.028)	(0.048)	(0.047)	(0.019)	(0.019)	(0.033)	(0.033)
Controls	N	Y	N	Y	N	Y	N	Y
Number of Observations	7,377	7,377	2,335	2,335	9,428	9,428	3,209	3,209

*** p<.001; ** p<.01; * p<.05; + p<.1.
Robust standard errors in parentheses

Regression discontinuity of change in crime on incumbent's vote share over primary opponent and incumbent party's vote share (for races without an incumbent). "Change in crime" is measured over the sheriff's jurisdiction and in the jurisdictions of neighboring county sheriffs (placebo outcome). Finally, controls consist of the log of the population and the share of the population that is Black. The bandwidth is selected using the procedure in Calonico et al. 2014. Standard errors are clustered at the county level.

Table 3: Change in crime rate after incumbent reelection: different bandwidths

	(1)	(2)	(3)	(4)	(5)
Incumbent Reelection	-0.077*	-0.084**	-0.094***	-0.098***	-0.089***
Standard Error	(0.036)	(0.031)	(0.028)	(0.025)	(0.021)
Bandwidth	.06	.09	.12	.15	.24
N (in bandwidth)	1414	2034	2564	3041	4168

*** p<.001; ** p<.01; * p<.05; + p<.1.

Regression discontinuity of change in crime on incumbent's vote share over primary opponent. Standard errors are clustered at the county level.

Table 4: Population, share Black, and real income per capita after incumbent reelection

	Percent black	Population	Income pc
Incumbent Reelection	0.011	-0.015	0.009
	(0.011)	(0.112)	(0.018)
Number of Observations	10,117	10,117	10,117

*** p<.001; ** p<.01; * p<.05; + p<.1.
Robust standard errors in parentheses

Regression discontinuity of county demographics on the incumbent's vote share over primary opponent. The bandwidth is selected using the procedure in Calonico et al. 2014. Standard errors are clustered at the county level.

Table 5: Change in crime as a function of incumbent reelection and prior sheriff election

	(1)	(2)	(3)	(4)
Incumbent wins	-0.105**			
	(0.035)			
(Incumbent wins) (# Deputies $\leq$ 20)		-0.123**		
		(0.041)		
(Incumbent wins) (# Deputies $>$ 20)		-0.087*		
		(0.041)		
# Deputies $\leq$ 20		0.031		
		(0.038)		
(Incumbent wins) (# Deputies $\leq$ 24)			-0.149***	
			(0.038)	
(Incumbent wins) (# Deputies $>$ 24)			-0.043	
			(0.045)	
# Deputies $\leq$ 24			0.077*	
			(0.039)	
Former sheriff wins				0.021
				(0.043)
Observations	17,639	17,639	17,639	6,194

In regressions (1)–(3), the change in crime is regressed on an indicator for reelection (interacted with the size of the sheriff office), instrumented by whether the incumbent sheriff is term-limited (interacted with the size of the sheriff office). In regression (4), the change in crime is regressed on an indicator for a former sheriff's electoral victory, restricting the sample to elections in which the incumbent is not reelected. The regressions include year fixed effects and standard errors account for clustering at the county level. *** $p < .001$; ** $p < .01$; * $p < .05$;

Table 6: Deputy sheriff turnover after incumbent and incumbent-party reelection

	All	All	Small	Large	Small	Large
Reelection	-0.057***	-0.008	-0.072*	-0.039**	-0.069*	-0.039**
Standard Error	(0.015)	(0.041)	(0.029)	(0.012)	(0.027)	(0.012)
Reelection	Incumbent	Inc. party	Incumbent	Incumbent	Incumbent	Incumbent
Deputy threshold	.	.	20	20	24	24
Observations	2,946	864	1,147	1,704	1,308	1,577

*** p<.001; ** p<.01; * p<.05; + p<.1.
Robust standard errors in parentheses

Regression discontinuity of deputy sheriff turnover on the incumbent and incumbent-party vote share. "Turnover" is the fraction of deputy sheriffs who depart during the first three months of the new sheriff's term. The bandwidth is selected using the procedure in Calonico et al. 2014. Standard errors are clustered at the county level.

Table 7: Change in crime rate after incumbent reelection in small versus large offices

	Small	Large	Small	Large
Inc. Reelection	-0.137**	-0.045+	-0.142***	-0.025
Standard Error	(0.046)	(0.023)	(0.042)	(0.026)
Deputy threshold	20	20	24	24
Observations	3,627	3,693	3,991	3,423

*** p<.001; ** p<.01; * p<.05; + p<.1.
Robust standard errors in parentheses

Regression discontinuity of change in crime on incumbent's vote share over primary opponent in small (20 or fewer deputies, 24 or fewer deputies) versus large sheriff offices. The bandwidth is selected using the procedure in Calonico et al. 2014. Standard errors are clustered at the county level.

Table 8: Change in share of crime that are of each offense category

	Murders	Vehicle thefts	Robberies	Larcenies	Assaults	Burglaries	Rapes
Inc. Reelection	-0.050	-0.300	0.075	1.540	-1.155	-0.323	0.346
	(0.481)	(0.424)	(0.109)	(1.459)	(1.195)	(1.183)	(0.424)
Observations	3,684	3,684	3,684	3,684	3,684	3,684	3,684

*** p<.001; ** p<.01; * p<.05; + p<.1.
Robust standard errors in parentheses

Regression discontinuity of change in the share of crimes in each offense category on incumbent's vote share over primary opponent. The sample is restricted to small (20 or fewer deputies) offices. Offenses are ordered by estimates of how strongly the reporting and recording are affected by the number of police officers, where offenses on the left are least affected and offenses on the right are most affected. The bandwidth is selected using the procedure in Calonico et al. 2014. Standard errors are clustered at the county level.

Appendix: identification of outliers in crime data

Let $c_{i,t}$ be the number of crimes reported by the sheriff in county i and year t. Reported crime, $c_{i,t}$, is an outlier and dropped from the data when one of the following conditions holds:

$$
\begin{aligned}
c_{i,t} &> 8 \max\{c_{i,t+1}, c_{i,t+2}, c_{i,t-1}\} \\
c_{i,t} &> 8 \max\{c_{i,t+1}, c_{i,t-1}, c_{i,t-2}\} \\
c_{i,t} &< (1/4) \min\{c_{i,t+1}, c_{i,t+2}, c_{i,t+3}\} \\
c_{i,t} &< (1/4) \min\{c_{i,t+2}, c_{i,t+3}, c_{i,t+4}\} \\
c_{i,t} &< (1/8) \min\{c_{i,t-1}, c_{i,t-2}, c_{i,t-3}\}.
\end{aligned}
$$