

Prosecutor Elections and Police Accountability *

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Abstract

Prosecutors play an important role in holding police accountable by determining whether or not an officer has broken the law. At the same time, prosecutors and police officers work together closely, raising concern over conflicts of interest. We study the effect of prosecutor turnover on the number of deaths caused by police officers. Using data from 2,315 district attorney elections in the United States, we find that the election of a new district attorney leads to a 17% reduction in the number of deaths caused by police officers. For close elections, deaths decrease by 40% (0.3 fewer deaths per year) after a new district attorney ousts an incumbent. We observe no corresponding changes in crime, arrests, assaults on police officers, or deaths of police officers. The effects are significant regardless of the political party of the newly elected district attorney. We find suggestive evidence in favor of increased police accountability and uncertainty about the district attorney's type as mechanisms.

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Introduction

Police misconduct in the United States is in the limelight, with approximately 1,000 deaths caused by police each year nationwide (Peeples 2020, Edwards, Lee, and Esposito, 2019). It is important to investigate what factors contribute to an environment conducive to police misconduct and to lethal use of force. Scholars and pundits have argued that the lack of accountability facing police officers likely contributes to the high incidence of deaths caused by police (Katz, 2015, Simmons, 2015). In particular, police officers rarely face criminal charges. In the fifteen-year period from 2005 to 2019, 104 non-federal police officers were arrested for manslaughter or murder and 35 were convicted (Stinson and Wentzlof, 2019).

The decision of whether or not to charge a police officer with a crime often falls on the local prosecutor. However, prosecutors and police officers work together closely to enforce the law, creating potential conflicts of interest. If police officers anticipate a low likelihood of being charged with a crime, then they may be more willing to engage in misconduct or risky behavior. The introduction of a new prosecutor may shift these expectations and cause police to be more hesitant to use lethal force.

In this paper, we investigate the extent to which prosecutors affect police behavior. In particular, we examine how the number of deaths caused by police changes after a new prosecutor takes control of a local jurisdiction. To do so, we focus on district attorney elections in the United States. District attorneys (DAs) are the highest ranking prosecutors in state court districts and are elected officials in most states (this includes 45 states, covering 2,315 districts).¹ The election of

¹District attorneys have different names depending on the state and district they serve: state attorney, commonwealth attorney, prosecuting attorney, or county attorney are alternative names that are used to label district attorneys in certain states. In this paper, we will henceforth refer to

a new DA is of interest because it marks the end to the relationship between the incumbent prosecutor and the police department and any conflicts of interest that may have arisen over time. Moreover, from a police officer's perspective, a new district attorney introduces greater uncertainty over prosecutorial decisions. This uncertainty alone may lead to less risky police behavior and less frequent use of force. Last, a new DA may introduce reforms explicitly aimed at changing police behavior. These mechanisms have been discussed in the context of police reform but have not, to our knowledge, been investigated systematically.

In the aftermath of the recent killings of George Floyd and Breonna Taylor, and the prosecutorial decisions that followed, more voices have expressed concern over the potential conflicts of interest between DAs and law enforcement. Prosecutors and police officers work together routinely to enforce the law. They coordinate to investigate crimes, subpoena witnesses, and issue warrants, among other activities. The DA may also serve as legal counsel to the local police agency. While a strong working relationship between prosecutors and police officers can improve law enforcement in many areas, it can also introduce conflicts of interest when police officers are under investigation. Due to these concerns, scholars, attorneys, and activists have lobbied for policies that limit the role of DAs in cases involving local police officers.² After no officers were indicted for the death of Eric Garner in 2014, then New York State Attorney General Schneiderman wrote to Governor Cuomo in support of a reform to limit the role of local prosecutors

all of them as district attorneys and will also use the acronym DA to refer to them.

²See Levine (2016), Katz (2015), Simmons (2015), and Stephen Montemayor. 2020. "Minnesota AG Keith Ellison to take over case in Floyd killing." *Star Tribune*, June 1. <https://www.startribune.com/ag-keith-ellison-to-take-over-case-in-floyd-killing/570911922/>, Costello, Darcy, Tessa Duvall, and Andrew Wolfson. 2020. "Breonna Taylor's mother asks for a special prosecutor to review Louisville cops' conduct." *Louisville Courier Journal*, October 28. <https://www.courier-journal.com/story/news/local/breonna-taylor/2020/10/28/breonna-taylors-mother-requests-special-prosecutor-look-case/6053661002/>.

in officer-related deaths. He explains, “A common thread in many of these cases is the belief of the victim’s family and others that the investigation of the death, and the decision whether to prosecute, have been improperly and unfairly influenced by the close working relationship between the county District Attorney and the police officers he or she works with and depends on every day. It is understandable that many New Yorkers feel that it is unfair to charge a local District Attorney with the task of investigating and prosecuting those officers when they are accused of a serious crime committed in the course of their duties.”³ New York later joined a small set of states that require outside investigators or prosecutors to intervene in cases of officer-involved deaths.⁴ In most districts, there are no laws to prevent a DA from handling cases involving a member of local law enforcement.

Conflicts of interest may worsen in cases where a DA depends on the support of police unions to further their career.⁵ There is no regulation banning police union contributions to district attorney campaigns, and information on campaign contributions to district attorneys is not always readily available.⁶ In Alameda

³New York State Office of the Attorney General. 2014. “A.G. Schneiderman Requests Executive Order To Restore Public Confidence In Criminal Justice System.” *Press Release Archives*, December 8. <https://ag.ny.gov/press-release/2014/ag-schneiderman-requests-executive-order-restore-public-confidence-criminal>.

⁴California, Connecticut, Florida, Illinois, Indiana, New Jersey, New York, Utah, and Wisconsin have passed legislation requiring either a state agency or outside investigator to intervene in cases of officer-related deaths. More states have begun to consider similar measures in 2021 (see the National Conference of State Legislatures’ Legislative Responses for Policing - State Bill Tracking Database, accessed March 2021 at <https://www.ncsl.org/research/civil-and-criminal-justice/legislative-responses-for-policing.aspx>).

⁵Levine (2016) outlines additional sources of conflicts of interest that are inherent to the position of the local prosecutor in cases of police misconduct. For instance, a police officer defendant is unlikely to object to a prosecutor’s (advantageous) conflict of interest, and the prosecutor, representing the state, has no client that could object. This feature of the prosecutor’s position and others, including protections from a Law Enforcement Officer Bill of Rights, do not change with the election of a new prosecutor and so they are not the focus of our discussion.

⁶Most states have a central, state-run website that contains information on campaign contributions received by district attorneys. However, 11 states do not make this information available in a central, state-run website (as of 2018).

County, California, District Attorney Nancy O'Malley accepted a \$10,000 contribution from the Fremont police union toward her re-election, while her office was investigating the president of the union and two other officers. None of the officers were ultimately charged.⁷ In response to incidents like these, DAs themselves have voiced support for campaign finance regulation. Diana Becton, the District Attorney of Northern California's Contra Costa County, has stated that conflicts of interest between DAs and police unions have damaged trust in law enforcement and contributed to the widespread belief that the system is ill-suited to prosecute police officers for misconduct.⁸

Separately from disrupting conflicts of interest, a newly elected DA may affect police behavior through policy reform. For instance, a DA can improve transparency over police misconduct allegations and has the discretion to bring in outside prosecutors when a member of local law enforcement is under investigation. These policies are more likely to be implemented by so-called progressive DAs. Progressive DAs have gained traction in recent elections, especially in cities with high-profile incidents of police misconduct.⁹ In Philadelphia, for example, a career defense attorney named Larry Krasner won the district attorney election in 2017. Krasner had sued the police department more than 75 times before taking office and went on to prosecute an officer-involved shooting case in Philadelphia for the first time in two decades.¹⁰

⁷Carissa Bryne Hessick and Alison Rossi. 2018. "Op-Ed: California voters deserve to know who is bankrolling our district attorneys." *Los Angeles Times*, March 29. <https://www.latimes.com/opinion/op-ed/la-oe-hessick-rossi-district-attorney-campaign-contributions-20180329-story.html>.

⁸Eric Westervelt. 2020. "Are prosecutors too cozy with police? Some DAs say campaign contributions need to end." *National Public Radio*, June 18. <https://www.npr.org/2020/06/18/879562369/are-prosecutors-too-cozy-with-police-some-das-say-campaign-contributions-need-to>.

⁹See Sam Reisman. 2019. "The Rise of the Progressive Prosecutor." *Law360*, April 7. <https://www.law360.com/articles/1145615/the-rise-of-the-progressive-prosecutor>

¹⁰Steve Volk. 2018. "Former Philly Cop Who Shot Two Fleeing Black Suspects to be Indicted." *Philadelphia Magazine*, November 3. <https://www.phillymag.com/news/2018/09/03/ryan-pownall-indictment/>

Whether through policy change, disrupting conflicts of interest, or simply by introducing uncertainty, the election of a new DA might incentivize more cautious police behavior and decrease deaths caused by police. Alternatively, DAs at the beginning of their terms in office might be less likely to hold police officers accountable than their predecessors, since they need to establish a good working relationship with police. In this case, the election of a new DA could give rise to less cautious police behavior and an increase in deaths caused by police. It is, thus, unclear ex-ante whether and how the election of a new DA impacts power relations between the DA office and police departments and the number of deaths caused by police. This paper constitutes a first attempt to shed light on this important question through a systematic analysis of data.

We investigate the relationship between prosecutor turnover and police behavior using data from 2,315 district attorney elections in the 2013-2017 period. We combine these data with information about the number of people killed by police each year. Using a differences-in-differences approach, we find that police killings decrease by 17% after a district elects a new DA. However, DA turnover is a relatively rare event. In most districts, incumbent DAs face no challengers. A concern is that districts that tend to have competitive DA elections differ systematically from districts that do not in ways that impact law enforcement. For this reason, we separately examine districts that experienced a close election during the period of analysis. We compare districts where a challenger narrowly defeats an incumbent to districts where an incumbent narrowly defeats a challenger. This specification selects comparable districts with no evidence of differential trends in police killings prior to the election date. The results show that the number of deaths caused by police significantly decrease in districts that elect a challenger DA. The effects are sizable (0.3 fewer deaths per year, a 40% reduction) and last

for several years.

The decrease in police killings experienced by districts that dismissed incumbent DAs and elected new DAs is unlikely to be due to decreased policing. We do not observe any changes in property crimes, violent crimes, or arrests in the years following the election of a new DA. Taken together, the empirical evidence suggests that police officers respond to a newly elected district attorney by de-escalating potentially violent situations, rather than ‘de-policing’. At the same time, we find no evidence of changes in the numbers of officers assaulted or killed. This result suggests that officers under a new DA manage to reduce use of force without increased risk of violence towards themselves.

To understand the mechanisms behind these results, we consider several characteristics of the DA candidates. We find suggestive evidence that the effect size is largest when a Democrat wins compared to when a Republican, independent, or non-partisan wins. However, the effect of a new DA on police killings is negative and statistically significant regardless of political party affiliation. It is unlikely that agenda alone explains the observed effects. Uncertainty and conflicts of interest likely also play a role. In line with both of these mechanisms, effects are largest when the new DA wins in a close election and negligible when the new DA wins an uncontested election. To assess the importance of conflicts of interest, we also compare districts based on tenure of the incumbent, but find no significant differences. A limitation of the study, however, is that we have a relatively small number of districts that can be used to tease out different mechanisms.

To the best of our knowledge, this is the first study that quantifies how prosecutor turnover impacts police behavior. Prior studies of prosecutors focus on the accountability of prosecutors themselves and the effects of electoral incentives on the types of cases selected for trial (Bandyopadhyay and McCannon, 2014a,

2014b), sentencing (Arora, 2019, Berdejó and Yuchtman, 2012, Boylan, 2005), and mistakes (McCannon 2013). Glaeser, Kessler and Piehl (2000) and Boylan (2005) find that long-term career concerns are also likely to affect case selection and sentencing. Silveira (2017) studies the incentives of a prosecutor to accept a plea bargain or go to trial and the effects of judicial reforms on incarceration.

This paper is more closely related to studies of police accountability and use of force.¹¹ Importantly, we identify a new determinant of use of force: turnover in the DA's office. This finding is consistent with police officers benefiting from their relationships with the incumbent DA and behaving more conservatively when facing a new DA. Similarly, union contracts can provide officers with protection from litigation. Dharmapala, McAdams and Rappaport (2019) find that sheriffs' offices in Florida saw higher rates of violent police misconduct after adopting collective bargaining agreements. Other factors that have been found to increase police use of force include the on-duty injury of a peer officer (Holz, Rivera, and Ba, 2020), and the militarization of police equipment (Masera, 2020).¹² Jennings and Rubado (2020) find that increased reporting requirements for use of force incidents are associated with fewer officer involved shootings. So far, there is mixed evidence about the effect of body-worn cameras on use of force, despite the large amount of attention and funding devoted to it (Lum et al. 2019). Last, individual police officers differ in use of force, and growing evidence shows that

¹¹Recent studies have investigated mechanisms for improving police accountability, without a focus on use of force. Ornaghi (2019) investigates how police reform impacts performance by focusing on the implementation of merit systems in municipal police departments in the U.S.. Rozema and Schanzenbach (2019) find that civilian allegations and civil rights litigation have strong predictive power over future police misconduct. Last, Nowacki and Thompson (2020) find that elections can increase police accountability. They leverage an intervention in the UK after which police commissioners started to be directly elected instead of appointed. The results suggest that direct elections induced a decrease in drug arrests in left-leaning districts in an effort to align policies with public preferences.

¹²Other studies find no effect of police militarization on deaths caused by police (Bove and Gavrilova, 2017, Harris et al., 2017).

male and white officers are more likely to use force than their peers (Ba et al., 2020, Hoekstra and Sloan, 2020).

Other papers have investigated whether police officers de-police in response to outside pressures or controversies. Rivera and Ba (2019) find evidence of de-policing after community oversight commissions are formed, but not after self-monitoring through police union memos. Sloan (2020) finds that police make fewer arrests after incidents in which officers are ambushed while on duty. In contrast, we find no evidence of de-policing in response to a new prosecutor taking office, perhaps because there is also no observable change in the rates at which officers are attacked.

The findings in this paper suggest that prosecutors can play an important role in reducing violence in interactions between police and the public. The empirical results show that police officers apply lethal force less frequently in the years following the election of a new DA. These results suggest that electoral competition in DA elections can improve police accountability and public safety by increasing prosecutor turnover. Currently, however, district attorneys stay in office for many years without ever facing a challenger, especially in rural areas (Hessick and Morse, 2020). At the same time, legal controls over prosecutors are weak compared to the controls facing police departments and judges. Thus, district attorney accountability relies to a great extent on voters' decisions in elections (Wright, 2009). As public attention over police misconduct has grown in the past decade (Ouss and Rappaport, 2020), so too might attention over prosecutor elections.

Data

We combine data on district attorney elections and deaths caused by police. We focus on deaths caused by police as the main outcome of interest for two reasons. First, understanding what factors contribute to the high incidence of violence in interactions between police and civilians in the United States, relative to other developed countries, is pressing. Second, deaths caused by police are documented in news reports and collected by several national datasets. Other types of police behavior or measures of police misconduct are difficult to document in a systematic way across many jurisdictions and are more prone to misreporting and measurement error.

Data on deaths caused by police come from the Mapping Police Violence project. They define a police killing as an incident in which a person dies as a result of an interaction with an on-duty or off-duty police officer. In this context, the term “police killing” (also used in Bor, 2018, Masera, 2020, and Streeter, 2019) is a better representation of these incidents than more narrowly defined terms such as police shootings, officer-involved shootings, or arrest-related deaths.

Mapping Police Violence covers police killings from 2013 through 2020. They combine information from three other datasets (FatalEncounters.org, the U.S. Police Shootings Database, and KilledbyPolice.net). We also validate our main findings using data on officer-involved shootings collected by the Washington Post from 2015 through 2020 (see Table A.5 in the Appendix). These independently managed datasets largely rely on local news reports. There are no official government statistics that can be used instead. The Bureau of Justice Statistics estimates that only half of fatal police shootings were counted by official sources over the

2003-2011 period. In contrast, they estimate that over 92% of arrest-related deaths are covered by media reports (BJS, 2016).

Data on DA elections comes from the Prosecutors and Politics Project (Hessick, 2020). DA elections are held by local authorities and election years and term lengths vary from district to district. The dataset includes candidate-level information about the most recent general and primary election in each district in the 2012 to 2017 period.¹³

Using the two main datasets—namely the police killing and district attorney election datasets—we construct a district-level panel for the years 2013 to 2020.¹⁴ We observe one election per district and the annual number of police killings in each of the 2,315 districts in the sample. To this panel, we add other outcomes of interest (offenses, arrests, and police officers assaulted or killed from the FBI’s Uniform Crime Reporting and Law Enforcement Officers Killed and Assaulted programs, available for years 2013-2019), time-varying controls (lagged population and per capita personal income from the Bureau of Economic Analysis, available for years 2013-2019) and baseline district characteristics (demographics, education, employment, and income from the 5-year 2012 American Community Survey). We aggregate all outcomes, covariates, and baseline characteristics to the district-year level.¹⁵

In the study period, a death caused by police occurred at least once in 52%

¹³The following states do not elect local prosecutors and are therefore not included in the sample: Alaska, Connecticut, Delaware, the District of Columbia, New Jersey, and Rhode Island. The following counties do not elect prosecutors and are not included in the sample: Maui, Hawaii; Carter, Montana; Petroleum, Montana; Golden Valley, North Dakota; and Steele, North Dakota.

¹⁴Note that for a small number of districts that had elections in 2012 (2% of districts in the sample), we only have police killing data in the post-election period.

¹⁵Districts typically coincide with counties or groups of counties. There are only four counties that are split across two districts. There is no need to disaggregate police killings data for these counties, since the address and responding police agency is known for each incident. For other variables, we assume that the parts of the counties split across two districts are identical. Covariates are unavailable for 38 districts in Virginia.

of districts. In total, there were roughly 1,000 deaths caused by police each year from 2013 to 2020 (Figure 1). In 14% of incidents the civilian killed was reportedly unarmed (occurring at least once in 19% of districts). The indictment of an officer involved in the death occurs in 2% of incidents. Only 5% of districts have a known indictment of an officer for an incident during the study period.

DA turnover is a relatively infrequent event. In the elections we observe, a new DA took office in 27% of districts (630 districts in the sample). A DA candidate ran unopposed in the general election in 70% of districts (1,612 of districts in the sample). A full set of summary statistics for districts in which a new DA won office and for districts in which an incumbent DA remained in office are reported in Table A.1 in the Appendix.

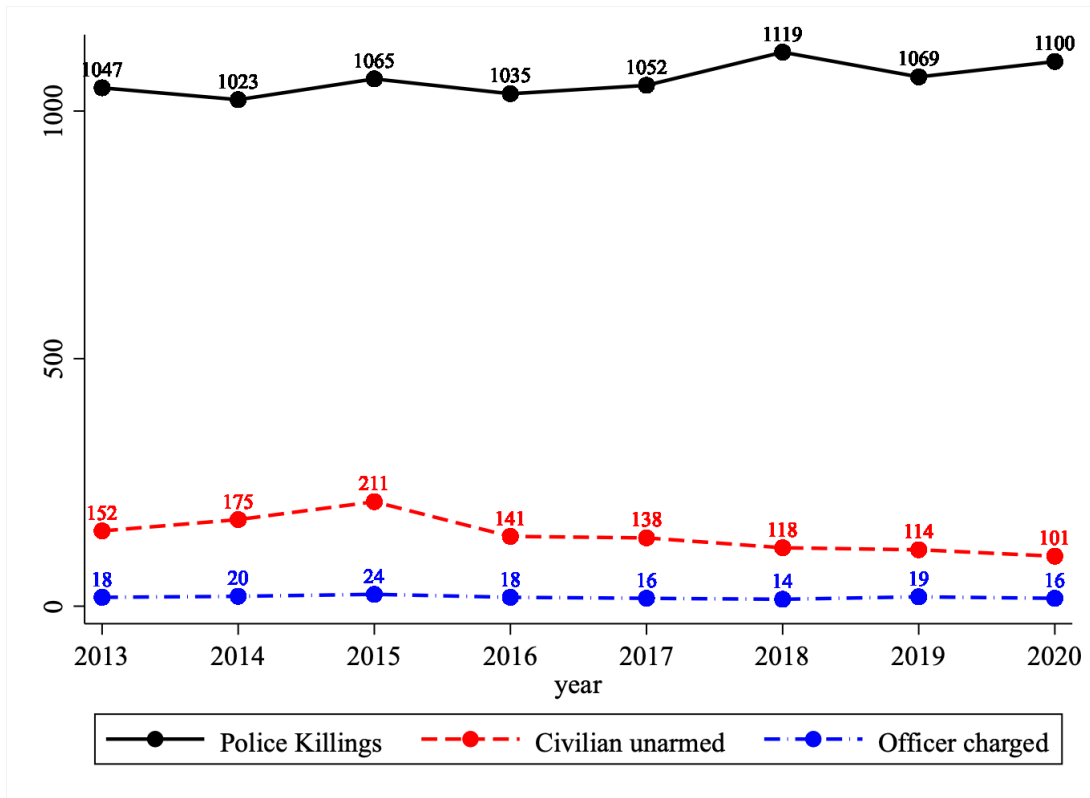
Empirical Strategy

We start by investigating the effects of district attorneys on police killings by using a differences-in-differences specification. We test whether districts in which a new district attorney wins the election experience any differential change in the number of police killings in the years after the election. To this end, we first estimate the following specification:

$$y_{it} = \alpha + \beta \text{new DA}_i \times \text{after}_{it} + \delta_i + \tau_t + X_{it}\gamma + \epsilon_{it} \quad (1)$$

where new DA_{it} is an indicator variable that takes value 1 if a new district attorney wins the election in district i , after_{it} takes value 1 if year t is after the election year in district i , δ_i denotes district fixed effects, τ_t denotes year fixed effects, and X_{it} denotes lagged population and per capita personal income of the district in year

Figure 1: Deaths caused by police per year



Note: This figure reports the annual numbers of police killings in the sample, the number of police killings in which the civilian was reportedly unarmed, and the number of police killings in which at least one officer was charged with a crime.

t . The standard errors are clustered at the district level.

Next, we investigate dynamic effects and possible pre-trends between treatment and control districts by estimating an event-study specification (see Borusyak and Jaravel, 2018 for further details on event studies):

$$y_{it} = \alpha + \sum_{k=\underline{T}}^{\bar{T}} \beta_k \text{new } DA_{it}^k + \delta_i + \tau_t + X_{it}\gamma + \epsilon_{it} \quad (2)$$

where $\text{new } DA_{it}^k$ are indicator variables that take value 1 for districts that elect new DAs in year k before/after the election. The variable $\text{new } DA_{it}^1$ takes value 1 if a new DA was elected 1 year ago in district i , $\text{new } DA_{it}^0$ takes value 1 if a new DA was elected in year t in district i , $\text{new } DA_{it}^{-1}$ takes value 1 if a new DA will be elected 1 year from year t in district i , et cetera. The variable $\text{new } DA_{it}^T$ takes value 1 for treatment units 3 or more years before the election, and $\text{new } DA_{it}^{\bar{T}}$ takes value 1 for treatment units 4 or more years after the election, δ_i denotes district fixed effects and τ_t denotes year fixed effects. Standard errors are again clustered at the district level. The year before the election is taken as the base year for the event study.

Results

Table 1 reports the results of estimating equation 1. Column 1 shows that the number of police killings decreases by 0.08—which is equivalent to a 17% reduction—after a new DA wins an election. Adding time-varying controls in Column 2 does not significantly change the estimated coefficient.

Figure 2 reports the results of estimating equation 2. There is no evidence of differential pre-treatment trends in police killings between districts in which

Table 1: Differences-in-differences estimates

	Police Killings	
	(1)	(2)
New DA $\times$ after election year	-0.081*** (0.029)	-0.084*** (0.029)
District FE	Yes	Yes
Year FE	Yes	Yes
Controls	No	Yes
N	18520	17976
Outcome mean	0.460	0.466
Adjusted R^2	0.831	0.832

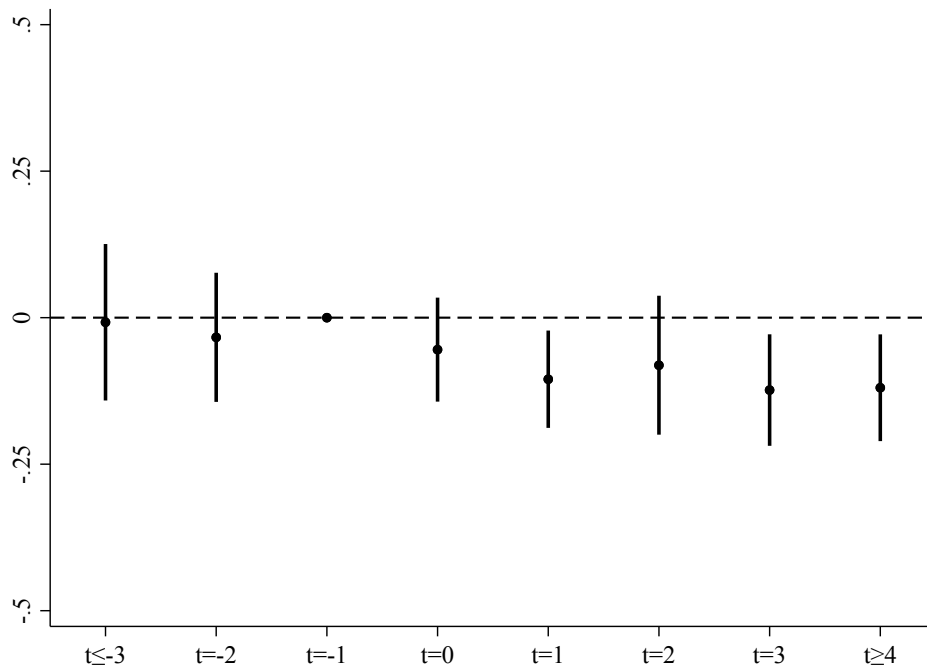
Note: The table reports the results of estimating equation 1. The dependent variable is the number of police killings in the district-year. Control variables include the lagged log of district population and the lagged log of district personal income per capita. Standard errors are clustered at the district level (in parentheses). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

the incumbent DA was reelected and districts in which a new DA won. There is also no evidence that the effect of a new DA on police killings changes over time after the election year. Rather, the effect of a new DA on police killings is sustained for a number of years after the election. The mean point estimate in the post-treatment period is -0.11. We return to a discussion of the dynamic effects of a new DA when we investigate mechanisms.

The results in Table 1 suggest that district attorney turnover can affect change, improve police accountability, and decrease the number of police killings. The estimates in Table 1 are unbiased under the assumption of parallel trends in police killings in areas that do and do not elect new DAs. Although there is no strong evidence of a violation to the parallel trends assumption in Figure 2, it is important to consider the identifying assumption more carefully.

Summary statistics in Table A.1 in the Appendix indicate that the districts that

Figure 2: Event study estimates



Note: This figure reports the results of estimating equation 2 for the full sample of districts. The dependent variable is the number of police killings in the district-year. The base year for the event study is the year before the DA election.

elect new DAs are in fact similar to districts that do not elect new DAs in many observable dimensions, including law enforcement outcomes and demographics. However, the parallel trends assumption may be unreasonable given large differences in the political environment for DAs across districts. To start, the election of a new DA occurs in relatively few districts (27%), and most incumbents (74%) face no challengers. Incumbents who won reelection were also in office longer than incumbents who lost (Table A.1 in the Appendix). This is consistent with the fact there are many districts, especially in rural areas, where incumbents tend to stay in office for a long tenure without facing political competition (Hessick and Morse 2019). Comparing districts in which incumbents rarely face challengers to districts in which a new DA is elected may be misleading. Importantly, the study period coincides with an increase in public demand for police accountability after the protests starting in Ferguson, Missouri in 2014. It is possible that the demand for greater police accountability caused both a decline in deadly use of force and induced new candidates to run for DA.¹⁶ Then, politically competitive districts would have experienced a decline in police killings prior to the election year, relative to districts where the incumbent remained likely to win. Estimates in Table 1 would then overstate the true effect of a new DA on police killings. Alternatively, the estimates in Table 1 may underestimate the true effect if districts that experienced an increase in police killings prior to the election year were more likely to see the incumbent resign or lose to a challenger.

Due to the concerns about the validity of the parallel trends assumption, we next limit attention to a subset of districts with close elections in which the in-

¹⁶During this period, deaths caused by police declined slightly in large population districts and not elsewhere. Districts where new DAs are elected are slightly larger in population than those where the incumbent remains in office, though the difference is not statistically significant (Table A.1 in the Appendix). Estimates in Table 1 may then reflect different trends in policing in urban areas compared to rural areas.

cumbent DA runs for reelection. By comparing districts in which a challenger narrowly defeats an incumbent to those in which an incumbent narrowly defeats a challenger, we expect to hold constant the unobservable political factors that could lead to differential trends in police killings prior to the election. To confirm, we test the null hypothesis that there is a meaningful violation of the parallel trends assumption in both the full sample and the close election sample, following Bilinski and Hatfield (2019). We find strong evidence against a violation of parallel trends in the sample of close elections, versus only weak evidence against a violation of parallel trends in the full sample (see Table A.4 in the Appendix).¹⁷

Close Elections

We focus on close elections in which the incumbent DA is running for reelection. We define a close election as one in which the margin of victory is less than 10 percentage points.¹⁸ We do not include close open elections because the dynamics of these elections and policing in these districts might be conditioned by the fact that incumbents are not running for reelection.¹⁹ These restrictions result in a sample of 157 districts, spread across 39 states.

In Table A.2 in the Appendix, we show that within the sample of close elections, districts in which the incumbent narrowly won and districts in which the incumbent narrowly lost are comparable to each other based on observable dis-

¹⁷See section A.3 in the Appendix for further discussion of the tests for parallel trend violations. We also show in this section that estimates are robust to the inclusion of state-specific time trends.

¹⁸Fisman, Schulz and Vig (2014) analyze whether winning candidates in Indian state elections experience higher rates of asset accumulation relative to runners-up. They also focus on close elections to compare politicians with similar observable characteristics that are subject to "treatment" (winning the election) plausibly randomly.

¹⁹If we also include close general elections in which no incumbent ran in the sample (an additional 77 districts), we find estimates similar to those reported in this section (see Table A.9 in the Appendix).

trict characteristics. DA characteristics are also comparable, although Republican DAs are slightly over-represented among newly elected DAs.²⁰ Importantly, there is no evidence of incumbency advantage in these close elections – incumbents win 49% of close races. Thus, within the close elections sample, the election of new DAs is plausibly quasi-random.

Table 2 reports the results of estimating equation equation 1. The results in columns 1 and 2 show that police killings significantly decrease in districts in which new DAs narrowly defeat incumbent DAs in the election. The magnitude of the estimated effects is sizable compared to the mean number of police killings per district-year. The -0.29 coefficient in column 2 constitutes a 40% decrease in police killings. Columns 3-4 and 5-6 present analogous estimates for the sample of close general elections and close primary elections in which incumbents run for reelection, respectively. The estimates are slightly larger for primary elections, although the differences in estimated effects across samples are small.

As before, we estimate equation 2 using the close election sample. The results of the event study in Figure 3 show no evidence against parallel pre-treatment trends in police killings between districts that had close elections in which the incumbent DA was reelected and districts that had close elections in which the incumbent DA was ousted. The negative and borderline significant coefficient for $t = 0$ suggests that the effects of electing a new DA on police killings start in the very election year. This is plausible since the results of the election are known before the end of the year. The effects are larger for the year after the election and subsequent years. The mean of the point estimates for the years after the election

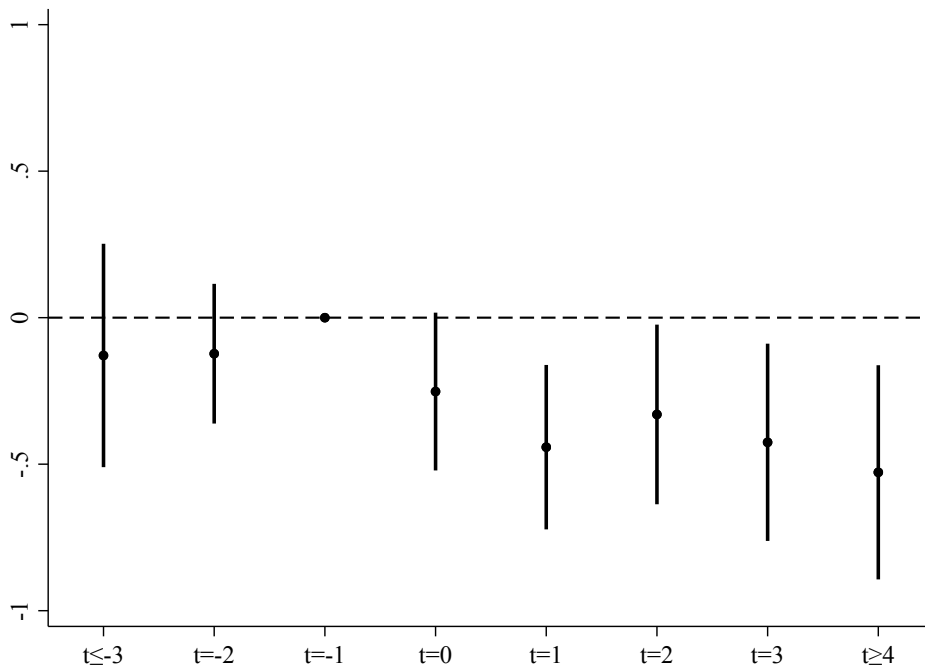
²⁰This imbalance arises in a context in which competitive districts were more likely to feature a Democrat incumbent and Republicans achieved big electoral wins in their 2014 and 2016 "red wave" years.

Table 2: Differences-in-differences estimates: Close elections only

	Close Elections		Close General Elections		Close Primary Elections	
	(1)	(2)	(3)	(4)	(5)	(6)
New DA $\times$ after election year	-0.29*** (0.09)	-0.29*** (0.09)	-0.26** (0.11)	-0.23** (0.11)	-0.41** (0.16)	-0.41** (0.16)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	1256	1200	768	720	512	504
Outcome mean	0.69	0.72	0.80	0.85	0.71	0.72
Adjusted R^2	0.84	0.84	0.86	0.86	0.80	0.80

Note: The table reports the results of estimating equation 1. The sample includes only close elections in Columns 1 and 2, close general elections in Columns 3 and 4, and close primary elections in Columns 5 and 6. The dependent variable is the number of police killings in the district-year. Control variables include the lagged log of district population and the lagged log of district personal income per capita. Standard errors are clustered at the district level (in parentheses). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 3: Police Killings

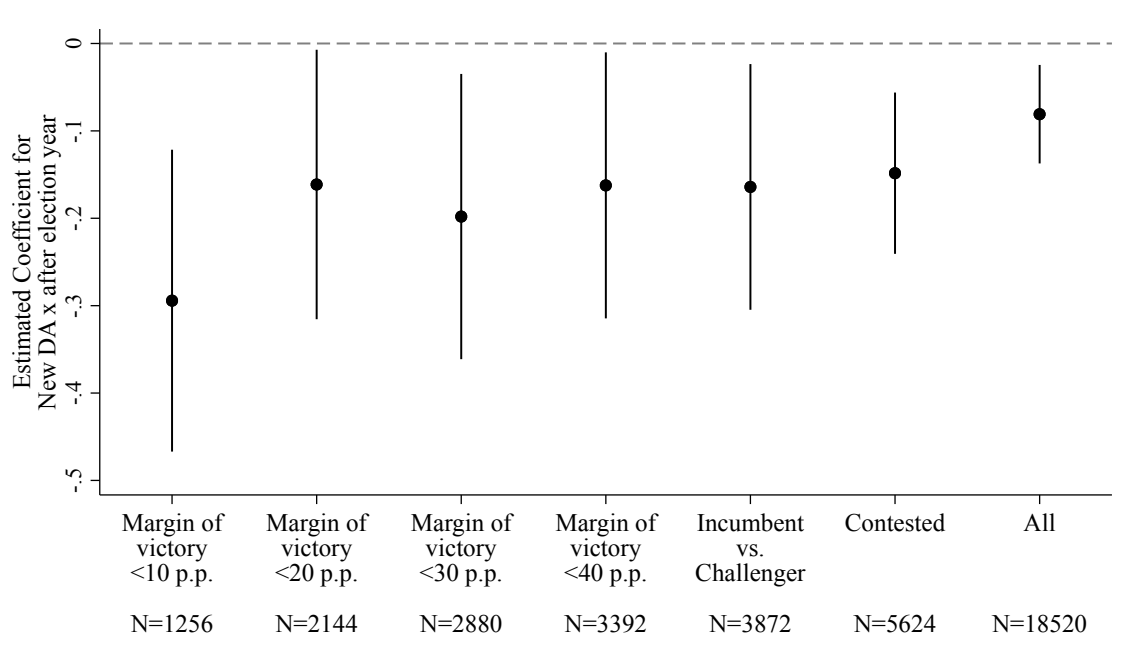


Note: This figure reports the results of estimating equation 2 for the sample of close primary and close general elections in which the incumbent DA faces a challenger. The base year for the event study is the year before the DA election.

is 0.43.²¹

Because our definition of a close election is arbitrary, we vary the margin of victory cutoff and estimate analogous effects. Figure 4 shows that, regardless of the definition of a close election, police killings significantly decrease after a new DA defeats the incumbent. As we narrow the definition of a close election, the point estimates tend to grow larger in magnitude.

Figure 4: District attorney election subsamples



Note: This figure reports the results of estimating equation 1 for the full sample and for sub-samples defined by margin of victory between an incumbent and challenger, presence of an incumbent and challenger, and presence of more than one candidate (contested). For each sub-sample, the figure shows the point estimate and 95% confidence interval (with standard errors clustered at the district-level) for the treatment effect of a newly elected DA on the number of police killings.

²¹Borusyak and Jaravel (2018) recommend to estimate the treatment effect more efficiently with a semi-dynamic specification, which restricts all pre-treatment coefficients to zero. With this semi-dynamic specification, we find similar results to those in Figure 2 (using the full sample) and Figure 3 (using the close elections sample), with the mean coefficients of -0.08 and -0.30, respectively.

The effects estimated in the close election sample are substantially larger than those estimated using the full sample. It could be that estimates from the full sample are biased for reasons discussed above. Alternatively, effects might be larger in districts with more political competition for the DA position for a number of reasons. First, the outcome of a close election is more likely to be unanticipated by police officers, requiring them to make larger adjustments to their behavior post-election. Second, incumbent DAs in competitive districts are more likely to seek endorsements or campaign contributions from law enforcement organizations, worsening conflicts of interest that may either be reversed or reinforced through the election. Third, the election of a new DA in a competitive district may reflect a change in political preferences of the constituents, which may then result in prosecutor-initiated police reform. Last, districts in which a new DA is elected in the full sample include districts where a long-serving incumbent DA decides to retire and assigns an heir apparent (likely the assistant DA) for the position, who is in turn elected in an uncontested or barely competitive election. We should expect the election of a new DA to have small or non-existent effects on police killings in these districts if successor DAs closely follow incumbent DAs' policies. Indeed, we find no significant negative effect of a new DA on the number of police killings in districts where the incumbent does not run and the new DA faces no contest (Table A.3 in the Appendix).

We cannot determine the extent to which the difference in estimates from the full and close elections samples are driven by heterogeneous effects versus bias. However, we proceed with the sample of close elections for further analyses because the parallel trends assumption is more plausibly satisfied. We note, however, that it is difficult to extrapolate results from the close election sample to historically uncompetitive districts.

Robustness Checks

We present several checks to complement the baseline analyses. First, we run a permutation test for the difference-in-differences specification in which we randomly assign treatment to districts. The goal is to assess the sampling variability of the differences-in-differences estimates under the sharp null hypothesis of no effects for all districts. The results in Figure A.1 in the Appendix show that, if the null hypothesis of no effects were true, it would be exceedingly rare to obtain such extreme coefficients as the ones in Tables 1 and 2 (See Appendix Section A.4 for further details about the permutation tests).

The results are not sensitive to outliers. We show that the estimated effects in Tables 1 and 2 are not due to a single district or state in a set of leave-one-out exercises (Figure A.3 in the Appendix). Similarly, results are not sensitive to the election year (Table A.7 in the Appendix). We also test if the election of a new DA causes a decrease in the likelihood of a police killing, rather than the number of police killings. The election of a new DA decreases the likelihood of any police killing occurring in a district-year by 9 percentage points, equivalent to a 35% reduction (Table A.6 in the Appendix).

We follow the approach of de Chaisemartin and D’Haultfoeuille (2020) to assess robustness to heterogeneous treatment effects. In our setting, the main concern is that heterogeneous effects over time could lead to biased estimates, given the staggered timing of the elections. However, we find that treatment effect heterogeneity would need to be substantial for the sign of the differences-in-differences estimate to be incorrect.²² Moreover, the estimator of de Chaisemartin

²²The differences in differences estimator is the weighted average of the treatment on the treated effects (ATT) across groups, where some weights may be negative (see de Chaisemartin and D’Haultfoeuille, 2020). If a large share of weights are negative, the average sign of the ATT could

and D’Haultfoeuille (2020), which is robust to heterogeneity in treatment effects in our context, yields similar, though slightly smaller, estimates compared to those in Tables 1 and 2 (the point estimate is -0.04 for the full sample and -0.23 for the sample of close elections).

Last, we address the possibility that the election of other elected officials, rather than DAs, is driving the results. For example, if the election of challenger DAs in a district happens around the same time as the election of challenger sheriffs, turnover in the sheriff’s office might explain the decrease in police killings. In this case, we would be falsely attributing the effects to prosecutors. As we lack information about sheriff election returns in the sample of districts with close DA elections, we instead leverage information about the timing of sheriff elections to explore this hypothesis.²³ Approximately 60% of districts hold DA and sheriff elections in the same year, both in the full sample and in the close election sample. For the remaining districts, sheriffs are appointed or sheriff elections are held in different years from DA elections. We test the hypothesis that the election of a challenger DA who ousts an incumbent has a larger effect on police killings in districts in which there is a concurrent sheriff election, relative to all other districts. We find no evidence of such an interaction effect (Table A.8 in the Appendix).

be different from the sign of the differences-in-differences estimator. In our setting, only 1% of districts in the close election sample receive a negative weight. Given the distribution of weights, for the true average ATT to be equal to zero, treatment effects would have to be vastly different across units, some with large positive effects and some with large negative effects (ninety-five percent of treatment effects would fall within the range of -1.01 to 1.01 , if we assume treatment effects are normally distributed).

²³Information about the timing of sheriff elections comes from The Appeal’s “Political Report” (2020) (accessed March, 2021 at <https://theappeal.org/political-report/when-are-elections-for-prosecutor-and-sheriff/>). Studies of election outcomes for local elections—such as Thompson (2020) on sheriff elections, de Benedictis-Kessner (2018) on mayoral elections and de Benedictis-Kessner and Warshaw (2020) on county council elections—tend to focus on larger counties or districts. In our setting, the paucity of data precludes a thorough analysis of the interplay between these local elections and DA elections. In particular, there is only one *known* district in the sample of close DA elections where a mayoral election took place in the very same year in which a challenger won the DA election (using data from de Benedictis-Kessner, 2018).

Other Law Enforcement Outcomes

In the previous section we document that the number of police killings falls after a new DA ousts an incumbent. One explanation for this result is that, after a new DA is elected, police officers find ways to de-escalate situations that would otherwise turn violent. Another explanation is that police officers avoid situations that would otherwise turn violent, known as de-policing. If de-escalation explains the reduction in deaths, we would not expect to see changes in the number of arrests or criminal offenses in response to the election of a new DA. Instead, if de-policing explains the results, we would expect fewer arrests (a direct effect of decreased policing) and perhaps a higher number of criminal offenses (an indirect effect of decreased policing). Figure 5 shows estimates for the event-study specification (equation 2) using the sample of close elections, with offenses and arrests for property and violent crimes as the dependent variables. We find no evidence that the outcome of a close DA election has any impact on the number of property crimes, violent crimes, nor arrests. These null results are consistent with the de-escalation hypothesis rather than the de-policing hypothesis.

Next, we explore whether de-escalation puts police officers at greater risk of harm. To test this, we repeat the event-study estimation strategy using the number of officers killed or assaulted as the dependent variables. During this time period there were, on average, 53 officers killed and 39,000 officers assaulted each year. Figure 6 shows estimates for the event-study specification. There is no evidence that the election of a new DA over an incumbent DA increases the number of officers killed or assaulted while on duty.

We also replicate the analysis of crime, arrests, and officer outcomes with the differences-in-differences specification (equation 1). This analysis corroborates

that the election of a new DA in a close election does not significantly impact the number of criminal offenses, arrests, or the number of officers assaulted or killed (Tables A.10, A.11, and A.12 in the Appendix, respectively). Figures 5 and 6 together suggest that officers are able to de-escalate potentially violent situations without undermining their capacity to arrest suspects and without putting themselves in greater risk of harm.

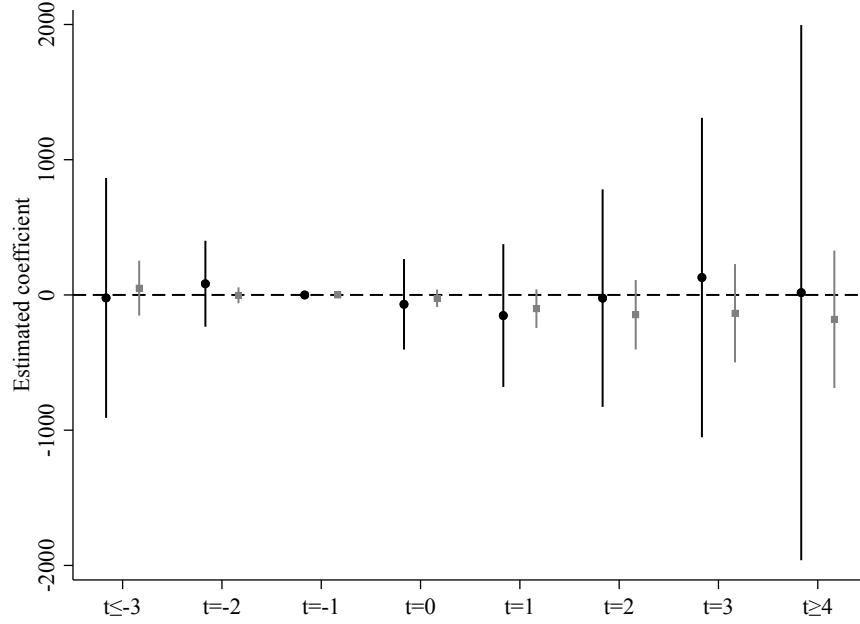
We find no evidence that the election of a new DA significantly changes the number of police officers indicted (Table A.13 in the Appendix). However, a known charge occurs in only 2% of incidents in the study period and we are under-powered to detect small effects on indictments. Note that the main result – that the election of a new DA causes a reduction in the number of police killings – does not require that the election of a new DA causes an increase in the number of officers indicted. If police respond to changes in the perceived risk of being indicted, then we can observe a decline in use of deadly force without an increase in the number or rate of indictments. Moreover, there are many costs associated with an investigation that officers may try to avoid, even if no charges are ultimately brought against the officer.

Last, we test if the reduction in police killings after a new DA defeats an incumbent is concentrated among certain types of incidents, depending on characteristics of the civilian involved.²⁴ The likelihood of being killed by police varies widely across the population. African American men and women, Hispanic men, and American Indian and Alaskan Native men and women face a substantially higher risk of being killed by police than their white peers (Edwards et al., 2019). In the Table A.14 in the Appendix, we consider police-involved deaths of white,

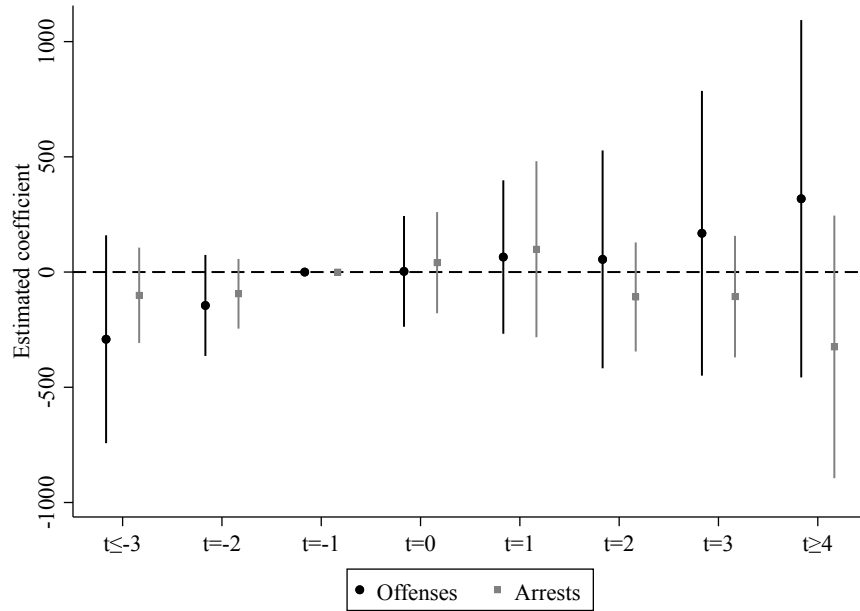
²⁴While Mapping Police Violence data includes information about the incident and the individual killed by police, we generally do not have information about the police officers involved.

Figure 5: Criminal Offenses and Arrests

A. Property Crime

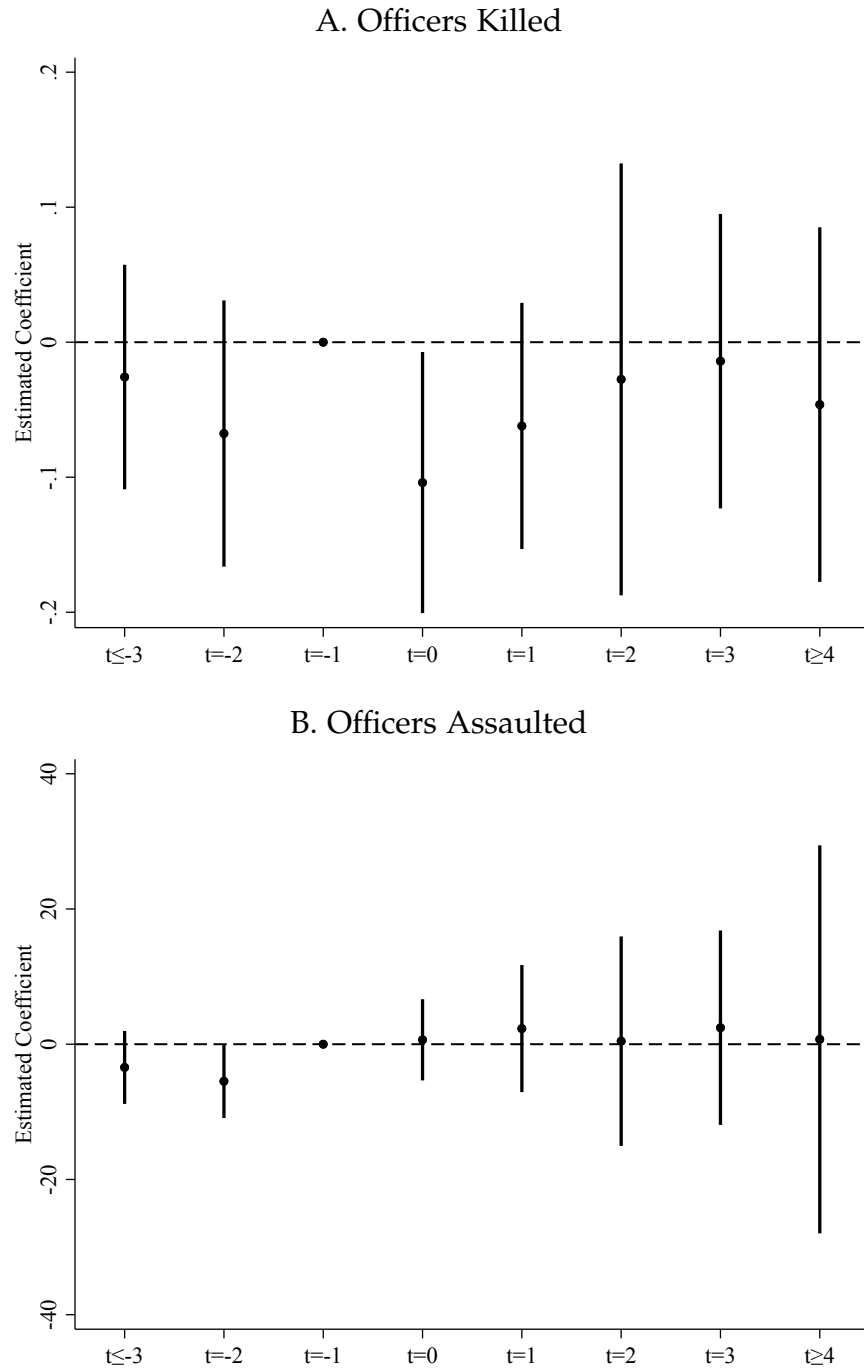


B. Violent Crime



Note: This figure reports the results of estimating equation 2 for the sample of close primary and general elections in which the incumbent DA faces a challenger. The outcomes are the numbers of criminal offenses and arrests for property crime (A) and violent crime (B). The base year for the event study is the year before the DA election.

Figure 6: Law Enforcement Officers Killed or Assaulted



Note: This figure reports the results of estimating equation 2 for the sample of close primary and general elections in which the incumbent DA faces a challenger. The outcomes are the numbers of officers killed (A) or assaulted (B) while on duty. The base year for the event study is the year before the DA election.

Black, Hispanic, American Indian/Alaskan Native, and Asian individuals as separate outcomes. We find no significant differences in the percent reduction in police killings across these racial and ethnic groups.²⁵ Note that this analysis is not a test for racial or ethnic discrimination in police use of force. Instead, the results suggest that the distribution of police killings across racial and ethnic groups does not change following the election of a new DA. Similarly, we find that there is an equal percent reduction in deaths of reportedly armed versus reportedly unarmed individuals following the election of a new DA.

Mechanisms

In the previous section, we show evidence that suggests that the decrease in the number of police killings in response to the election of a new DA might be due to police de-escalation. In this section, we explore several potential political mechanisms that can help us link the election of a new DA to the subsequent increases in police de-escalation efforts. First, officers face greater uncertainty over the DAs type when a new DA takes office. Unlike incumbent DAs, new DAs do not typically have a record of handling cases involving police officers in the district. Second, police officers may lose the benefits of an advantageous or cozy relationship with the incumbent DA. Third, the new DA may introduce policies or reforms that aim to increase police transparency, reduce use of force, and foster police de-escalation efforts. Each of these mechanisms could result in more conserva-

²⁵Specifically, point estimates imply a 39% reduction in the number of deaths of white non-Hispanic individuals, compared to a 40% reduction in the number of deaths overall. Other race or ethnicity-specific outcomes occur relatively infrequently, so we are underpowered to estimate statistically significant effects. However, point estimates suggest a 38% reduction in the number of deaths of Black non-Hispanic individuals, and a 50% reduction in the deaths of Hispanic individuals. The point estimates for deaths of American Indian and Alaskan Native and Asian individuals suggest even larger percent changes— but these events are especially infrequent, with a mean of 0.01 deaths per district-year, so estimates are difficult to interpret.

tive policing behavior, which would be consistent with the empirical evidence presented so far.

To test the hypothesis that a progressive agenda leads to a decrease in police killings, we first use affiliation with the Democratic party as a proxy for progressiveness. In most districts (81%), DA candidates are affiliated with either the Democratic or Republican party. Policy positions have become increasingly important in DA elections over the past decade (Sklansky 2017). In line with national party positions, Republican DAs tend to be tougher on crime (Arora 2019) whereas Democrat DAs are more likely to initiate reforms aimed at reducing police misconduct. Among the 80 new DAs that beat incumbents in our sample of close elections, 23 are Democrats (29%), 49 are Republicans (61%), and 8 (10%) are independent or non-partisan. In Table 3, we show the differences-in-differences estimate, interacted with party affiliation indicators.²⁶ There is some suggestive evidence that the effects are larger when Democrats, Independents, or non-partisans win, relative to when Republicans win (interaction effects are negative but not statistically significant). However, police killings decrease when a new DA beats an incumbent regardless of political affiliation. It is thus unlikely that the agenda of the new DA *fully* explains the negative effect on police killings.

A concern with using party affiliation to test the progressive agenda hypothesis is that not all Democrat DAs are progressive and some reform-minded DAs may not be Democrats. We construct an alternative indicator for a progressive DA by reviewing local news reports of elections that involve two candidates from the same party or races in which at least one of the top two candidates is independent or non-partisan. We consider candidates as progressive if they promote po-

²⁶We find similar results if we instead test for heterogeneous effects depending on transitions between parties (See Table A.16 in the Appendix).

lice accountability, diversion programs, reduced reliance on bail, reduced prison sentences for non-violent crimes, or opposition to the death penalty (Sklansky 2017). We identify 13 progressive DAs that defeat incumbents in the close election sample, of which 10 are Democrats and 3 are independents or non-partisans. Similar to results in Table 3, we find a reduction in police killings after a new DA wins even if the new DA does not have a progressive platform. There is again only suggestive evidence that the effect size is larger in those districts in which a progressive DA wins (Table A.17 in the Appendix).

Given that the platform of the DA can not fully explain the results, we investigate if there is any evidence consistent with the other two hypothesized mechanisms: changes in conflicts of interest and uncertainty. We have already shown that the decrease in the number of killings by police associated with DA turnover is smaller when the challenger DA defeats the incumbent by a large margin (Figure 4) and is negligible when the new DA runs unopposed (Table A.3 in the Appendix). These findings suggest that effects are smaller where officers face greater uncertainty over the outcome or are more likely to already have familiarity with an heir apparent.

We additionally test if effects are larger where the incumbent DA was in office for longer, since the incumbent would be more likely to have both a long track record of relevant cases and a strong relationship with the police department. Table 4 reports differences-in-differences estimates for the close election sample with the addition of an interaction effect between the treatment effect and an indicator for whether or not the incumbent DA had above-median tenure in office (8 years or longer).²⁷ We can reject the hypothesis that effects are larger in districts in

²⁷There is significant variation in tenure of the incumbent across districts, with a standard deviation of 8 years and the longest tenure equal to 47 years (the modal term length is 4 years). The tenure information comes from the Prosecutors and Politics Project data and was supplemented

Table 3: Differences-in-differences estimates: Heterogeneous effects by political party

	Close Elections		Close General Elections		Close Primary Elections	
	(1)	(2)	(3)	(4)	(5)	(6)
New DA $\times$ after election year	-0.24*** (0.08)	-0.22*** (0.08)	-0.27** (0.12)	-0.24** (0.11)	-0.27* (0.14)	-0.28** (0.13)
New DA $\times$ after $\times$ Democrat	-0.13 (0.18)	-0.13 (0.19)	0.01 (0.16)	-0.01 (0.17)	-0.42 (0.41)	-0.37 (0.40)
New DA $\times$ after $\times$ Independent	-0.20 (0.33)	-0.27 (0.34)	0.12 (0.11)	0.06 (0.12)	-0.52 (0.64)	-0.62 (0.66)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	1256	1200	768	720	512	504
Outcome mean	0.69	0.72	0.80	0.85	0.71	0.72
Adjusted R^2	0.84	0.84	0.86	0.86	0.80	0.81

Note: The table reports the results of estimating equation 1. The dependent variable is the number of police killings in the district. The sample includes only close elections in Columns 1 and 2, close general elections in Columns 3 and 4, and close primary elections in Columns 5 and 6. Control variables are the lagged log of district population and the lagged log of district personal income per capita. Standard errors are clustered at the district level (in parentheses). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Differences-in-differences estimates: Heterogeneous effects by tenure of incumbent

	Close Elections		Close General Elections		Close Primary Elections	
	(1)	(2)	(3)	(4)	(5)	(6)
New DA $\times$ after election year	-0.44*** (0.14)	-0.41*** (0.15)	-0.36** (0.17)	-0.28 (0.20)	-0.58** (0.23)	-0.56** (0.23)
New DA $\times$ after $\times$ high tenure	0.25* (0.14)	0.21 (0.16)	0.16 (0.16)	0.08 (0.21)	0.33 (0.24)	0.28 (0.25)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	1256	1200	768	720	512	504
Outcome mean	0.69	0.72	0.80	0.85	0.71	0.72
Adjusted R^2	0.84	0.84	0.86	0.86	0.80	0.80

Note: The table reports the results of estimating equation 1. The dependent variable is the number of police killings in the district. The variable High Tenure is equal to one if the incumbent's tenure in office is greater than or equal to the median value of 8 years. The sample includes only close elections in Columns 1 and 2, close general elections in Columns 3 and 4, and close primary elections in Columns 5 and 6. Control variables include the log of district population and the log of district personal income per capita. Standard errors are clustered at the district level (in parentheses). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

which the incumbent was in office for longer. In fact, there is suggestive evidence that the effect is smaller in magnitude where the incumbent had a long tenure. A caveat is that the incumbent's tenure may be a poor measurement for changes in uncertainty and conflicts of interest. For example, incumbents with short tenures might still have a long track-record in the district if they were previously assistant DAs. Incumbent DAs with long tenure are also more likely to lose to Republicans in the close-election sample. Given this correlation and the suggestive evidence that Republican challengers bring about a smaller reduction in police killings, the results in Table 4 may reflect heterogeneous effects by party affiliation.

Concluding Remarks

Police reform, particularly as it relates to use of force, is receiving extensive attention and is heavily debated. We contribute to this debate by studying how the election of new prosecutors impacts police killings. The results indicate that prosecutors can play an important role in ensuring police employ de-escalation methods, which could benefit both communities and the police departments themselves (Ang, 2021, Bor, 2018, Mullinix, Bolsen, and Norris, 2020).

Our paper is a first step in quantitatively exploring how district attorneys impact police behavior. Using evidence from 2,315 DA elections, we find that the number of deaths caused by police falls by 17% after a new DA is elected. We find larger effects in a sample of comparable districts with close DA elections. After a challenger DA narrowly defeats an incumbent, the number of deaths caused by police decreases by 40%. We find that these effects are sustained for a number of years after the election. Importantly, DA turnover has a significant effect on

by our own investigation of local news reports of district attorney elections.

police killings regardless of the party affiliation of the new DA. However, there is suggestive evidence that effects are larger where the newly elected DA is a Democrat or has a progressive agenda.

The magnitude of the effect of a new DA on deaths caused by police is large but not implausible. If we use estimates from the full sample and from the close elections sample as lower and upper bounds, respectively, the election of a new DA causes between 0.08 and 0.29 fewer deaths per district-year. The estimates imply that in the year after the 2016 elections alone, the election of new DAs led to between 21 and 76 fewer deaths caused by police.

In comparing our findings to other studies of police use of force, the effects of electing new DAs on police behavior are most similar to the effects of collective bargaining rights. Dharmapala, McAdams and Rappaport (2019) find that violent police misconduct increases by 40% after agencies gain collective bargaining rights. In contrast, Masera (2020) finds that the militarization of police equipment increases police killings by 8%. Holz, Rivera, and Ba (2020) find that after a peer officer is injured while on-duty, the probability that an officer injures a civilian increases by 10%. The comparability of the magnitudes of the effects of DA turnover and collective bargaining rights on police killings—although, crucially, the effects go in the opposite direction—might be explained by the similarity of the mechanisms. Both collective bargaining rights and entrenched DAs increase legal protections for police officers, which disincentivize de-escalation efforts.

The results in the paper indicate that increased electoral competitiveness in district attorney elections can increase police accountability. However, as Hessick and Morse (2019) point out, many district attorney elections are uncontested, especially in rural areas where the supply of lawyers and potential district attorneys is limited. Implementing term limits could alleviate conflicts of interest between

district attorney offices and police departments, but at the expense of exacerbating existing supply problems. Requiring outside investigators or prosecutors to intervene in cases in which a police officer uses deadly force is yet another policy reform that aims at alleviating conflicts of interest between the district attorney's office and local law enforcement. A small number states and local jurisdictions have implemented such policies in recent years, but more data-driven research is needed to assess the effects of these policies.

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Appendix to:
Prosecutor Elections and Police Accountability

A Supporting Information Appendix

A.1 Summary Statistics

Table A.1: Summary statistics – Full Sample

	New DA		Incumbent		
	Mean	SD	Mean	SD	Difference
<i>Law Enforcement Outcomes</i>					
Police killings	0.51	2.03	0.44	1.72	0.06
Crimes (per 1M pop.)	342.57	231.76	351.98	418.78	-9.41
Arrests (per 1M pop.)	127.23	100.12	129.65	144.24	-2.42
Officers assaulted	17.13	72.37	16.99	101.12	0.14
Officers killed	0.03	0.20	0.02	0.23	0.01
<i>Winning DA Characteristics</i>					
Democrat	0.23	0.42	0.28	0.45	-0.05**
Republican	0.59	0.49	0.55	0.50	0.04*
Male	0.73	0.44	0.83	0.37	-0.10***
<i>Incumbent DA Characteristics</i>					
Incumbent tenure (years)	8.53	7.38	10.05	8.21	-1.52**
Democrat	0.39	0.49	0.28	0.45	0.11***
Republican	0.49	0.50	0.55	0.50	-0.05
Number of candidates	1.97	0.92	1.19	0.46	0.78***
<i>District Covariates</i>					
Population (10k)	14.11	40.27	12.70	36.92	1.42
Personal income p.c. (1k USD)	41.07	10.48	41.33	11.58	-0.26
<i>Baseline District Characteristics</i>					
% White	0.87	0.14	0.86	0.14	0.00
% Black	0.06	0.11	0.07	0.12	-0.01
% Hispanic	0.08	0.13	0.07	0.12	0.01
% with Bachelors degree or higher	0.20	0.08	0.20	0.08	-0.00
% Unemployed	0.04	0.01	0.04	0.01	0.00
% Below poverty line	0.15	0.05	0.15	0.06	-0.00
Number of Observations	6930		18535		
Number of Districts	630		1685		

Note: The New DA columns reports the mean and standard deviation for districts where a new DA takes office. The Incumbent column reports the mean and standard deviation for districts where the incumbent remains in office. The Difference column reports the differences in means: *** indicates that the difference is statistically significant with $p < 0.01$, ** indicates $p < 0.05$ and * indicates $p < 0.10$.

Table A.2: Summary Statistics – Close Election Sample

	New DA		Incumbent		
	Mean	SD	Mean	SD	Difference
<i>Law Enforcement Outcomes</i>					
Police killings	0.78	2.49	0.59	1.44	0.19
Crimes (per 1M pop.)	396.49	223.00	345.74	191.78	50.75
Arrests (per 1M pop.)	138.44	87.24	119.30	84.48	19.13
Officers assaulted	37.24	129.96	35.44	196.19	1.80
Officers killed	0.06	0.32	0.03	0.24	0.02
<i>Winning DA Characteristics</i>					
Democrat	0.29	0.45	0.52	0.50	-0.23***
Republican	0.61	0.49	0.39	0.49	0.22***
Male	0.76	0.43	0.81	0.40	-0.04
<i>Incumbent DA Characteristics</i>					
Incumbent tenure (years)	9.89	7.16	10.29	8.83	-0.40
Democrat	0.39	0.49	0.52	0.50	-0.13*
Republican	0.51	0.50	0.39	0.49	0.12
Number of candidates	2.40	0.58	2.21	0.41	0.19**
<i>District Covariates</i>					
Population (10k)	21.83	62.42	25.76	57.00	-3.92
Personal income p.c. (1k USD)	40.73	11.62	41.30	11.61	-0.57
<i>Baseline District Characteristics</i>					
% White	0.86	0.14	0.86	0.15	-0.00
% Black	0.07	0.12	0.06	0.08	0.01
% Hispanic	0.12	0.19	0.09	0.15	0.03
% with Bachelors degree or higher	0.21	0.08	0.21	0.09	-0.00
% Unemployed	0.04	0.01	0.04	0.01	-0.00
% Below poverty line	0.15	0.06	0.15	0.06	0.00
Number of Observations	880		847		
Number of Districts	80		77		

Note: The New DA columns reports the mean and standard deviation for districts where a challenger beat an incumbent in either the primary or general election by a margin of victory less than 10 percentage points. The Incumbent column reports the mean and standard deviation for districts where the incumbent beat a challenger in either the primary or general election by a margin of victory less than 10 percentage points. The Difference column reports the difference in means: *** indicates that the difference is statistically significant with $p < 0.01$, ** indicates $p < 0.05$ and * indicates $p < 0.10$.

A.2 Heterogeneous Effects in Districts with Uncontested, Open Elections

Table A.3: Differences-in-differences estimates – Heterogeneous effects for uncontested and open elections

	(1)	(2)
New DA $\times$ after election year	-0.05* (0.03)	-0.05* (0.03)
New DA $\times$ after $\times$ contested	-0.03 (0.04)	-0.03 (0.05)
New DA $\times$ after $\times$ incumbent ran	-0.03 (0.07)	-0.03 (0.08)
District FE	Yes	Yes
Year FE	Yes	Yes
Controls	No	Yes
N	18520	17976
Outcome mean	0.46	0.47
Adjusted R^2	0.831	0.832

Note: The table reports the results of estimating equation 1 with an additional interaction term between *New DA* and *contested*, an indicator equal to one if there were two or more candidates in the general election, and an interaction term between *New DA* and *incumbent ran*, an indicator equal to one if the incumbent ran in the election. The baseline group then are newly elected DAs who won uncontested, open elections. Control variables include the log of district population and the log of district personal income per capita. Standard errors are clustered at the district level (in parentheses). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.3 Assessment of Parallel Trends Assumption

In this section we assess the plausibility of the identifying assumption that trends in police killings are parallel between districts that do and do not elect a new DA. First, we estimate the fixed effects model (Equation 1) with the addition of a linear trend for treated districts:

$$y_{it} = \alpha + \beta \text{new DA}_i \times \text{after}_{it} + \theta \text{new DA}_i \times t + \delta_i + \tau_t + X_{it}\gamma + \epsilon_{it}.$$

If θ were statistically significantly different from zero, that would be strong evidence against the parallel trends assumption. Second, we allow for state-specific time trends. In Table A.4, Columns 1 and 4 repeat the estimates from the main specification for the full sample (Table 1) and for the sample of close elections (Table 2). Columns 2 and 5 include a linear trend for treated districts in the full sample and close election sample, respectively. Estimates show that there are no statistically significant pre-trends. Columns 3 and 6 allow for state-specific time trends, again in the full and close election samples respectively. Estimates are similar to those in columns 1 and 3. The evidence in Table A.4 is also in line with evidence against a violation of the parallel trends assumption in the event study specification (Figure 2).

However, failing to reject the presence of pre-trends may result from lack of statistical power. Following Bilinski and Hatfield (2019), we additionally test the null hypothesis that there is a parallel trends violation of a meaningful size ($H_0 : \theta \geq |\delta|$, where we consider different thresholds δ). In the close election sample, we can reject that a violation that would reverse the sign of the treatment effect ($\delta = 0.29$, $p = 9 * 10^{-6}$). Stronger than that, we can reject a violation that would

reverse the minimum detectable effect ($\delta = 0.15$, $p = 0.004$), as well as a violation that would explain as small as one-fourth of the treatment effect ($\delta = 0.07$, $p = 0.049$; note that this violation is similar to the magnitude of the effect in the full sample). In the full sample, we can reject a violation that would reverse the treatment effect ($\delta = 0.08$, $p = 0.004$), but we fail to reject a violation that would reverse the minimal detectable effect ($\delta = 0.05$, $p = 0.064$). We conclude that there is stronger evidence against a violation to the parallel trends assumption in the sample of close elections.

Table A.4: Tests for violations of the parallel trends assumption

	All Elections			Close Elections		
	(1)	(2)	(3)	(4)	(5)	(6)
New DA $\times$ after election year	-0.08*** (0.03)	-0.07* (0.04)	-0.09*** (0.03)	-0.29*** (0.09)	-0.22** (0.10)	-0.24*** (0.07)
New DA $\times$ year		-0.00 (0.01)			-0.02 (0.03)	
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State Trends	No	No	Yes	No	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	17976	17976	17976	1200	1200	1200
Outcome mean	0.47	0.47	0.47	0.72	0.72	0.72
Adjusted R^2	0.832	0.832	0.834	0.837	0.837	0.844

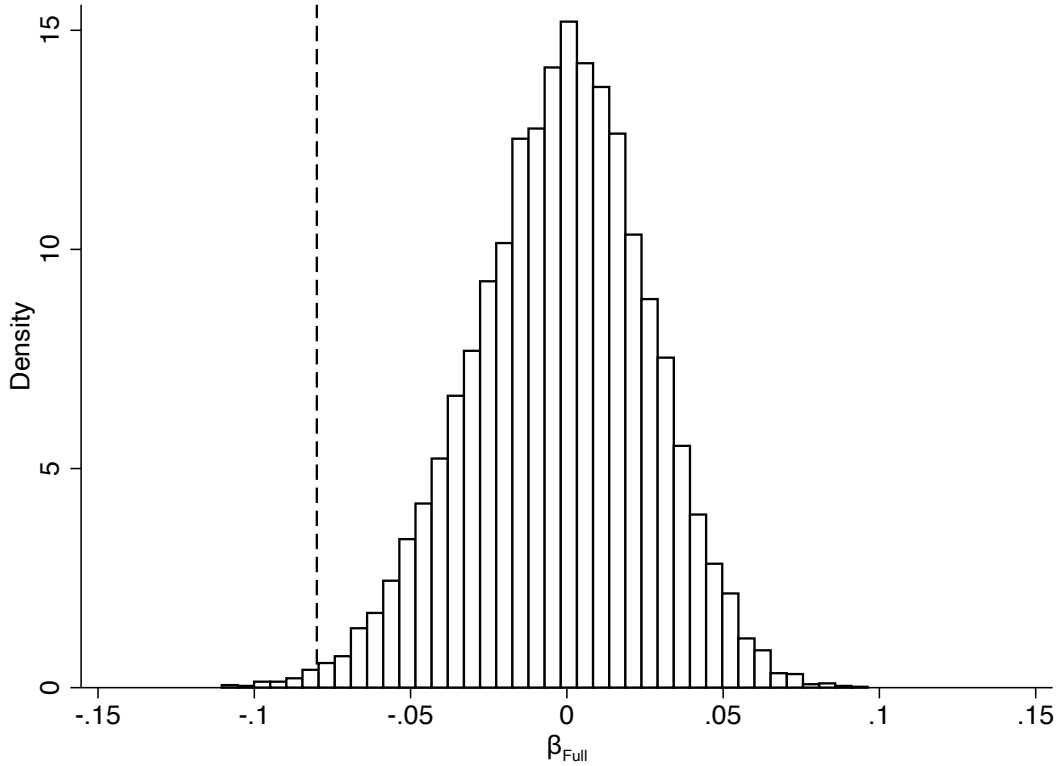
Note: The dependent variable is the number of police killings in the district. Control variables are the lagged log of district population and the lagged log of district personal income per capita. Standard errors are clustered at the district level (in parentheses). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.4 Permutation Test

In this section, we conduct a permutation test for the effects of electing a challenger district attorney on officer killings. The objective is to compute the sampling variability of the main point estimate imposing the sharp null hypothesis of no effects for all districts. To this end, we create 10,000 surrogate data sets, in which we randomly flip whether a district elected a new DA or not. As the fraction of districts in which a new DA was elected is equal to 27 percent in our sample, we randomly assume each district is treated (i.e. that the district got a new district attorney) with probability 0.27. In all cases, we do not alter the election years of each district. Using the regression model in equation 1, we then compute a placebo effect of electing a new DA on the number of police killings for each newly created instance of the data. By construction, under the sharp null, the distribution of placebo estimates coincides with the sampling distribution of the estimand.

Figure A.1 shows the results from this test for the sample that contains all districts. The dashed line corresponds to the estimate reported in the main text. We see that the estimate is an outlier compared to the distribution of placebo estimates. More specifically, under the sharp null hypothesis, we would expect to obtain an estimate that is larger (in absolute value) than the one obtained ($\beta_{Full} = -0.08$) with probability 0.59%. Hence, the results from this permutation test imply that we can reject the claim that the election of a new district attorney did not decrease officer killings. In addition, we note that, based on the regression specification in equation 1, we reject the null $H_0 : \beta_{Full} = 0$ in about 3.94% of the realizations. This number is close to the theoretical size of 5 percent, which suggests that clustering by district is appropriate.

Figure A.1: Permutation test: Full Sample

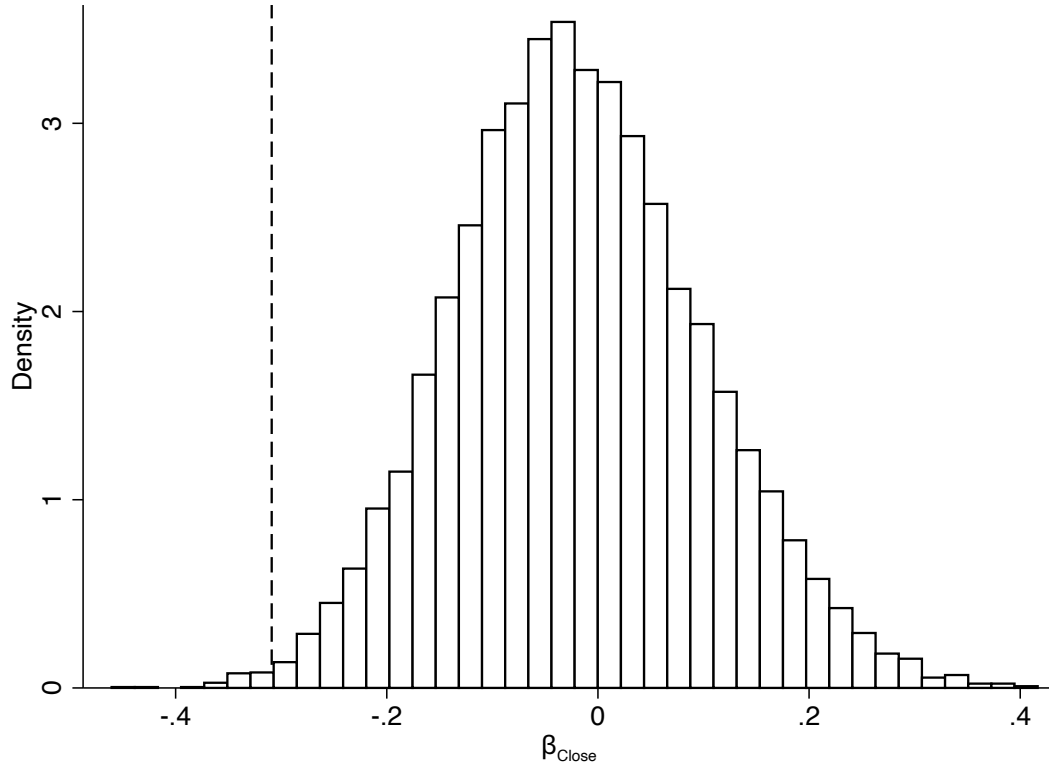


Note: The figure shows a histogram of estimated placebo effects of electing a new district attorney on police killings, based on 10,000 randomly generated permutations, as explained in Appendix A.4. The vertical dashed line indicates the point estimate obtained in the main text with the original, unaltered data.

Figure A.2 shows analogous results for the sample that contains all close primary and general elections between an incumbent DA and challenger DAs. The fraction of districts in which an incumbent DA lost the election in this subsample is equal to 51%. Thus, we randomly assume each district in the close elections sample is treated with probability 51%. Again, we observe in Figure A.2 that the dashed line corresponding to the estimate reported in the main text is a clear outlier. Only 0.79% of simulations yield a point estimate that is larger in absolute value than the point estimate obtained in Table 2 (which is equal to $\beta_{Close} = -0.29$). Likewise, we reject the null $H_0 : \beta_{Close} = 0$ in about 4.68% of the

realizations.

Figure A.2: Permutation test: Close Elections Sample

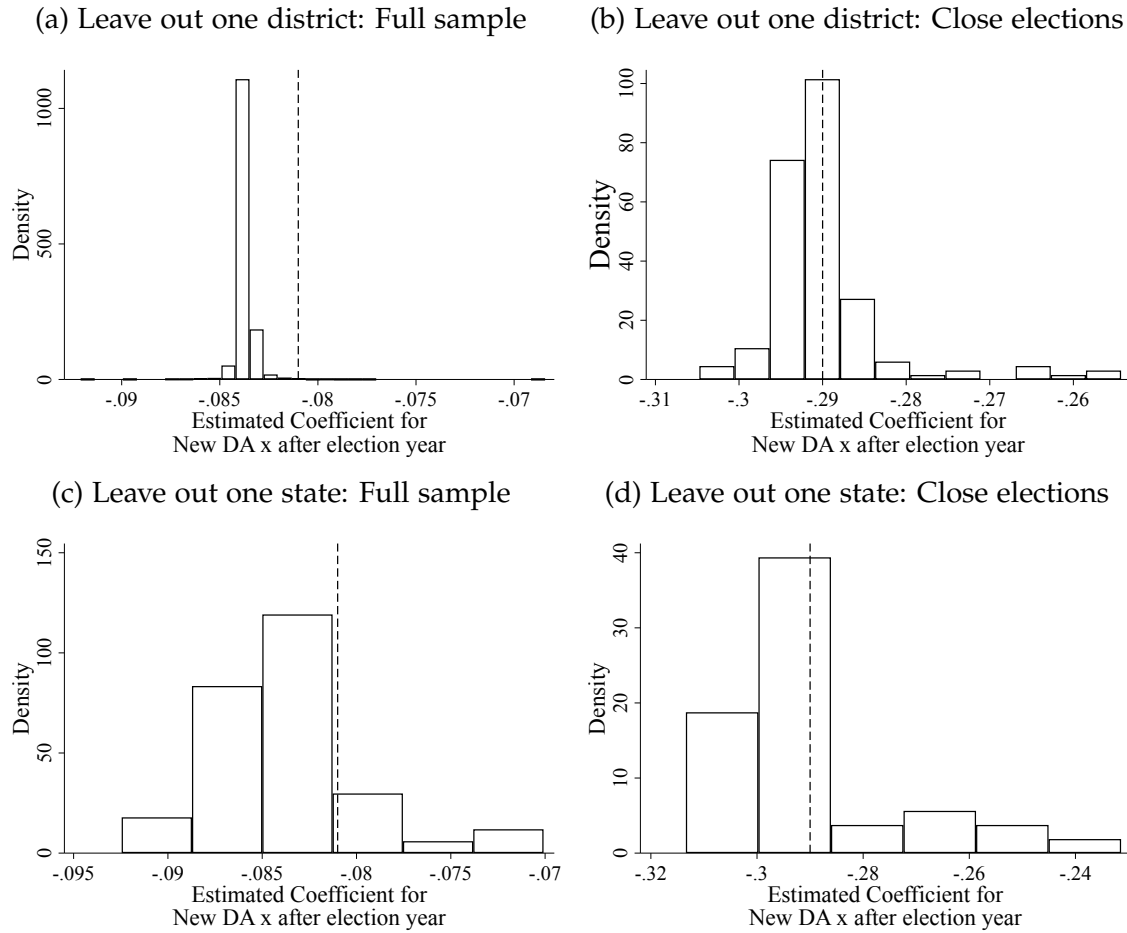


Note: The figure shows a histogram of estimated placebo effects on police killings after electing a new district attorney facing the incumbent in close primary or general elections. It is based on 10,000 randomly generated permutations, as explained in Appendix A.4. The vertical dashed line indicates the point estimate obtained in the main text with the original, unaltered data.

A.5 Leave-one-out Validation Exercise

In this section we report the results of a leave-one-out validation exercise to rule out the case that the negative treatment effect is due to an outlier district or state. In each regression, we estimate equation 1, leaving out one district (one cluster). Figure A.3, Panels (a) and (b) show the distributions of estimated differences-in-differences coefficients in the full and close elections samples, respectively. All coefficients are negative and with a p -value less than 0.01. Next, we repeat this exercise leaving out one state at a time. Figure A.3, panels (c) and (d) show the distributions of estimated coefficients for $new DA_i \times after_{it}$. Again, all coefficients are negative with a p -value is less than 0.01.

Figure A.3: Leave-one-out Regression estimates



Note: The figure shows a histogram of estimated effects of a New DA on the number of police killings in a district-year. In Panels (a) and (b), each regression leaves out one districts. In Panels (c) and (d), each regression leaves out one state. The vertical dashed lines indicates the point estimates obtained in the main text with the original, unaltered data.

A.6 Washington Post Data

Table A.5: Washington Post Police Shootings Data

	All Elections		All Close Elections	
	(1)	(2)	(3)	(4)
New DA $\times$ after election year	-0.03 (0.04)	-0.05 (0.04)	-0.22*** (0.08)	-0.23*** (0.08)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
N	13890	13482	942	900
Outcome mean	0.38	0.38	0.72	0.75
Adjusted R^2	0.767	0.770	0.826	0.826

Note: The table reports the results of estimating equation 1. The dependent variable is the number of police shootings in the district (from the Washington Post). The dependent variable is available for 2015 to 2020. The sample includes all elections in Columns 1 and 2, and only close elections in Columns 3 and 4. Control variables are the lagged log of district population and the lagged log of district personal income per capita. Standard errors are clustered at the district level (in parentheses). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.7 The likelihood of any police killing

Table A.6: The likelihood of any police killing

	All Elections		All Close Elections	
	(1)	(2)	(3)	(4)
New DA $\times$ after election year	-0.01 (0.01)	-0.02 (0.01)	-0.10*** (0.04)	-0.09** (0.04)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
N	18520	17976	1256	1200
Outcome mean	0.20	0.20	0.25	0.26
Adjusted R^2	0.408	0.411	0.444	0.444

Note: The table reports the results of estimating equation 1. The dependent variable is equal to one if there are any police killings in the district-year and is equal to zero otherwise. The sample includes only close elections in Columns 1 and 2, close general elections in Columns 3 and 4, and close primary elections in Columns 5 and 6. Control variables are the lagged log of district population and the lagged log of district personal income per capita. Standard errors are clustered at the district level (in parentheses). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.8 Election year sub-samples

In the sample of districts that have close elections, 57 districts had an election in 2014, 17 districts had an election in 2015, 76 districts had an election in 2016, and 5 districts had an election in 2017.

Table A.7: Differences-in-differences estimates for election year sub-samples

	2014 or 2015		2016 or 2017	
	(1)	(2)	(3)	(4)
New DA $\times$ after election year	-0.39*** (0.13)	-0.35** (0.14)	-0.30** (0.13)	-0.31** (0.13)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
N	592	552	648	632
Outcome mean	0.61	0.65	0.76	0.78
Adjusted R^2	0.758	0.761	0.877	0.877

Note: The table reports the results of estimating equation 1. The dependent variable is equal to one if there are any police killings in the district-year and is equal to zero otherwise. The sample includes only close elections that occur in 2014 or 2015 in Columns 1 and 2, and only close elections that occur in 2016 or 2017 in Columns 3 and 4. Control variables are the lagged log of district population and the lagged log of district personal income per capita. Standard errors are clustered at the district level (in parentheses). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.9 Concurrent sheriff elections

Table A.8: Differences-in-differences estimates – Heterogeneous effects by existence of concurrent sheriff election

	Close Elections		Close General Elections		Close Primary Elections	
	(1)	(2)	(3)	(4)	(5)	(6)
New DA $\times$ after election year	-0.26** (0.10)	-0.20** (0.10)	-0.23* (0.12)	-0.12 (0.10)	-0.41* (0.21)	-0.39* (0.20)
New DA $\times$ after $\times$ Concurrent sheriff election	-0.05 (0.12)	-0.14 (0.13)	-0.04 (0.11)	-0.19 (0.15)	-0.00 (0.22)	-0.03 (0.22)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	1256	1200	768	720	512	504
Outcome mean	0.69	0.72	0.80	0.85	0.71	0.72
Adjusted R^2	0.84	0.84	0.86	0.86	0.80	0.80

Note: The table reports the results of estimating equation 1. The dependent variable is the number of police killings in the district-year. The sample includes only close elections in Columns 1 and 2, close general elections in Columns 3 and 4, and close primary elections in Columns 5 and 6. Control variables are the lagged log of district population and the lagged log of district personal income per capita. Standard errors are clustered at the district level (in parentheses). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.10 Close Election Sub-Sample, Inclusive of Open Elections

Table A.9: Differences-in-differences estimates using sample of close elections, including open elections

	Close Elections		Close General Elections		Close Primary Elections	
	(1)	(2)	(3)	(4)	(5)	(6)
New DA $\times$ after election year	-0.29*** (0.09)	-0.29*** (0.09)	-0.19** (0.09)	-0.16** (0.08)	-0.41** (0.16)	-0.41** (0.16)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	1256	1200	1144	1096	512	504
Outcome mean	0.69	0.72	0.64	0.66	0.71	0.72
Adjusted R^2	0.836	0.837	0.835	0.837	0.803	0.804

Note: The table reports the results of estimating equation 1. The dependent variable is the number of police killings in the district-year. The sample includes only close elections in Columns 1 and 2, close general elections in Columns 3 and 4, and close primary elections in Columns 5 and 6. Control variables are the lagged log of district population and the lagged log of district personal income per capita. Standard errors are clustered at the district level (in parentheses). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.11 Other Law Enforcement Outcomes

Table A.10: Offenses

	(1) All	(2) Violent	(3) Property
New DA $\times$ after election year	357.163 (510.620)	193.718 (239.750)	25.985 (460.174)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	1570	1570	1570
Outcome mean	12686.58	3375.60	6759.37
Adjusted R^2	0.994	0.987	0.987

Note: The table reports the results of estimating equation 1 in the close elections sample. The dependent variables are the total number of offenses (Column 1), violent crime offenses (Column 2), and property crime offenses (Column 3). Standard errors are clustered at the district level (in parentheses). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: Arrests

	(1) All	(2) Violent	(3) Property
New DA $\times$ after election year	-136.846 (282.370)	-23.522 (113.551)	-133.900 (133.007)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	1570	1570	1570
Outcome mean	3783.84	1541.22	1053.46
Adjusted R^2	0.975	0.973	0.954

Note: The table reports the results of estimating equation 1 in the close elections sample. The dependent variables are the total number of arrests (Column 1), arrests for violent crimes (Column 2), and arrests for property crimes (Column 3). Standard errors are clustered at the district level (in parentheses). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12: Police officers assaulted or killed

	(1) Total	(2) Assaulted	(3) Killed
New DA $\times$ after election year	0.721 (6.731)	0.729 (6.734)	-0.008 (0.031)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	1570	1570	1570
Outcome mean	36.40	36.36	0.04
Adjusted R^2	0.955	0.955	0.264

Note: The table reports the results of estimating equation 1 in the close elections sample. The dependent variables are the total number of officers assaulted or killed (Column 1), total number of officers assaulted (Column 2), and total number of officers killed (Column 3) while on duty. Standard errors are clustered at the district level (in parentheses). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.13: Police officers indicted

	All Elections		All Close Elections	
	(1)	(2)	(3)	(4)
New DA $\times$ after election year	-0.0033 (0.0036)	-0.0045 (0.0037)	0.0093 (0.0184)	0.0106 (0.0208)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
N	18520	17976	1256	1200
Outcome mean	0.01	0.01	0.01	0.01
Adjusted R^2	0.066	0.066	-0.008	-0.009

Note: The table reports the results of estimating equation 1. The dependent variable is the number of known indictments of police officers in the district-year. The sample includes all districts in Columns 1 and 2 and only districts with a close general or primary election (< 10 p.p. margin of victory) close elections in Columns 3 and 4. Control variables are the lagged log of district population and the lagged log of district personal income per capita. Standard errors are clustered at the district level (in parentheses). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.14: Police Killings by race and ethnicity of civilian

	All	White	Black	Hispanic	AI/AN	Asian
	(1)	(2)	(3)	(4)	(5)	(6)
New DA $\times$ after election year	-0.29*** (0.09)	-0.11** (0.05)	-0.08 (0.05)	-0.06 (0.04)	-0.01 (0.01)	-0.02 (0.02)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	No	Yes	Yes	Yes
N	1200	1200	1200	1200	1200	1200
Outcome mean	0.72	0.28	0.21	0.12	0.01	0.01
Adjusted R^2	0.837	0.485	0.814	0.702	0.021	0.200

Note: The table reports the results of estimating equation 1. The dependent variable in each column is the number of police killings of individuals belonging to the racial or ethnic group in the heading. Mapping Police Violence identifies race and ethnicity of the civilians in each incident from media reports. They determine whether an individual belongs to one of the following exclusive groups: white (non-Hispanic), black (non-Hispanic), Hispanic, AI/AN (American Indian/Alaska Native), or Asian. Very few incidents (0.5%) fall outside of one of these categories in the sample. The regression samples includes all district-years in the close election sample. Control variables are the lagged log of district population and the lagged log of district personal income per capita. Standard errors are clustered at the district level (in parentheses). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.15: Police Killings by armed vs. unarmed status of civilian

	All	Armed	Unarmed
	(1)	(2)	(3)
New DA $\times$ after election year	-0.29*** (0.09)	-0.22*** (0.07)	-0.03 (0.04)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	1256	1256	1256
y variable mean	0.69	0.54	0.09
Adjusted R^2	0.836	0.821	0.432

Note: The table reports the results of estimating equation 1. The dependent variables are the number of police killings of a reportedly armed individuals (Column 1) and the number of police killings of reportedly unarmed individuals (Column 2). The sample includes all district-years in the close election sample. Control variables are the lagged log of district population and the lagged log of district personal income per capita. Standard errors are clustered at the district level (in parentheses). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.12 Additional Heterogeneous Effects Analysis

Table A.16: Differences-in-differences estimates: Heterogeneous effects by party transitions

	Close Elections		Close General Elections		Close Primary Elections	
	(1)	(2)	(3)	(4)	(5)	(6)
New DA $\times$ after election year	-0.32*** (0.11)	-0.31*** (0.11)	-0.11 (0.13)	-0.05 (0.14)	-0.41** (0.17)	-0.41** (0.16)
New DA $\times$ after $\times$ Republican defeats Democrat	0.09 (0.12)	0.09 (0.14)	-0.16 (0.13)	-0.19 (0.16)	-0.09 (0.12)	-0.09 (0.12)
New DA $\times$ after $\times$ Democrat defeats Republican	0.03 (0.18)	-0.02 (0.21)	-0.25 (0.18)	-0.33 (0.24)	0.28* (0.14)	-0.08 (0.26)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	1256	1200	768	720	512	504
Outcome mean	0.69	0.72	0.80	0.85	0.71	0.72
Adjusted R^2	0.84	0.84	0.86	0.86	0.80	0.80

Note: The table reports the results of estimating equation 1. The dependent variable is the number of police killings in the district-year. The sample includes only close elections in Columns 1 and 2, close general elections in Columns 3 and 4, and close primary elections in Columns 5 and 6. Control variables are the lagged log of district population and the lagged log of district personal income per capita. Standard errors are clustered at the district level (in parentheses). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.17: Differences-in-differences estimates: Heterogeneous effects by progressive platform

	Close Elections		Close General Elections		Close Primary Elections	
	(1)	(2)	(3)	(4)	(5)	(6)
New DA $\times$ after election year	-0.28*** (0.09)	-0.26*** (0.09)	-0.27** (0.11)	-0.21** (0.10)	-0.36** (0.16)	-0.36** (0.16)
New DA $\times$ after $\times$ Progressive	-0.11 (0.29)	-0.20 (0.30)	0.04 (0.22)	-0.08 (0.21)	-0.55 (0.75)	-0.53 (0.72)
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	1256	1200	768	720	512	504
Outcome mean	0.69	0.72	0.80	0.85	0.71	0.72
Adjusted R^2	0.84	0.84	0.86	0.86	0.80	0.81

Note: The table reports the results of estimating equation 1. The dependent variable is the number of police killings in the district-year. The sample includes only close elections in Columns 1 and 2, close general elections in Columns 3 and 4, and close primary elections in Columns 5 and 6. Control variables are the lagged log of district population and the lagged log of district personal income per capita. Standard errors are clustered at the district level (in parentheses). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.