Prosecutorial Reform and Local Crime Rates*

Amanda Agan
Cornell University

Jennifer L. Doleac
Arnold Ventures

Anna Harvey
New York University

Anna Kyriazis
Social Science Research Council

Lauren Schechter
University of Notre Dame

March 27, 2026

Abstract

Many communities across the United States have elected reform-minded prosecutors who seek to safely reduce the reach and burden of the criminal justice system. In this paper, we use variation in the timing of when these prosecutors took office across jurisdictions both to empirically characterize their effects on case outcomes and to estimate downstream effects on local reported crime rates and drug mortality rates. We find that after a reform prosecutor takes office there are large and statistically significant decreases in charging and conviction rates, with larger decreases for misdemeanor offenses than felony offenses and for drug offenses than other offense types. We find few to no downstream effects on local crime rates or drug mortality rates. These findings suggest that the types of policies being implemented by reform prosecutors appear to be decreasing the footprint of the criminal justice system without large adverse effects on public safety.

*Agan is Associate Professor in the Brooks School of Public Policy at Cornell University, amanda.agan@cornell.edu. Doleac is Executive Vice President of Criminal Justice at Arnold Ventures, jdoleac@arnoldventures.org. Harvey is Professor of Politics at New York University, anna.harvey@nyu.edu. Kyriazis is Postdoctoral Fellow at the Social Science Research Council and the New York University Public Safety Lab, anna.kyriazis@nyu.edu. Schechter is Assistant Research Professor of Economics in the Wilson Sheehan Lab for Economic Opportunities at the University of Notre Dame, lschecht@nd.edu. We thank Amber Andrews, Meghna Baskar, Jessica Goode, Tessie Krishna, Ananya Prakash, Rebecca Regan, and Mitchell Zufelt for excellent research assistance. The opinions expressed here are the authors' alone and do not represent the official position of any organization with which they are currently affiliated. The authors would like to thank the editor and reviewers, as well as the participants in the George Mason University Law & Economics Center's June 2021 Research Roundtable on Capitalism and the Rule of Law, for their helpful comments.

1 Introduction

Across the United States, many communities are electing reform-minded prosecutors to handle criminal cases in their jurisdictions. The policy changes implemented by these prosecutors are wide-ranging, but typically include practices that aim to reduce the severity of sanctions for low-level offenders (Mitchell and Petersen, 2025). The reformers' argument is that excessively punitive intervention by the criminal justice system for less serious offenses is unnecessarily disruptive to people's lives and may actually increase future criminal activity rather than reduce it. Opponents worry that these reforms may increase crime by reducing deterrence and incapacitation effects. The net impact of reform prosecutors' policy changes is an empirical question on which we have limited evidence.

Prosecutors often have substantial discretion in the enforcement of criminal law in their jurisdictions, creating a variety of pathways through which changes in prosecutorial leadership may influence case outcomes and public safety (Hessick and Morse, 2019; Bowman et al., 2024). After an arrest is made, prosecutors must decide whether and at what level to file charges, whether to request pretrial detention, whether to offer diversion and/or a plea bargain, and whether to drop charges at any point before trial. If and when a conviction is obtained, prosecutors typically also make sentencing recommendations to judges (Bushway and Piehl, 2001). Although there is scope for most case outcomes to also be affected by the decision-making of other actors— including police discretion in making arrests, defendants' choices of whether to take up diversion programming or accept a plea bargain, and judges' decisions about pretrial detention and sentencing —prosecutors nonetheless influence outcomes at several different stages in the life of a case (Bushway and Forst, 2013; Pfaff, 2017; Spohn, 2018). In addition to the establishment of specific policy directives, local chief prosecutors may also influence case outcomes through personnel and day-to-day management decisions over the line prosecutors they supervise.

To understand how reform prosecutors impact criminal justice and public safety in their jurisdictions, we use variation in the timing of the inauguration of reform-minded chief prosecutors in jurisdictions across the country as a natural experiment. We employ a staggered difference-in-differences design to empirically characterize the resulting changes in criminal case outcomes in these jurisdictions. We then estimate the average effect of a reform prosecutor's inauguration on local reported crime rates and drug overdose mortality rates. We combine information on prosecutors' inauguration dates with charging and conviction data from the Criminal Justice Administrative Records System (CJARS), incident-level reported crime data collected from local police departments and the National Incident-Based Reporting System (NIBRS), and drug mortality data from CDC Wonder.

We build on prior work considering the effects of prosecutor-driven reforms on case outcomes, typically studying specific reforms in individual jurisdictions or states. For example, Suffolk County, Massachusetts District Attorney Rachael Rollins implemented a presumption of non-prosecution for a set of 15 low-level offenses. Agan et al. (2023) find a 15-20% increase in the declination rate for nonviolent misdemeanors after her inauguration. A few studies consider directives by Philadelphia District Attorney Larry Krasner's office, including eliminating cash bail requests for 25 low-level offenses, a presumptive declination policy for a few select misdemeanor offenses, and a sentencing recommendation reform for graded felony offenses. These policies reduced the cost of bail to defendants, reduced charging rates for the marijuana, sex work, and retail theft offenses covered by the declination policy, and reduced the average duration of incarceration and parole supervision among felony convictions (Ouss and Stevenson, 2023; Amaral et al., 2025). Likewise, Gottlieb et al. (2025) find that, after the election of State's Attorney Kim Foxx in Chicago on a reform platform, per capita prison incarceration rates in Cook County declined by about 10 percent in her first year and about 20 percent in her second year relative to a synthetic control composed of other counties in the state. In a broader consideration of reform prosecutors in general rather than the effects of a specific prosecutor or type of reform, Mitchell et al. (2022) compare case outcomes across jurisdictions in Florida in 2017 and find that cases in jurisdictions with a reform prosecutor were less likely to end in a felony conviction.

A few of the above studies also estimate the effects of reform prosecutors or their specific policy changes on public safety: Agan et al. (2023) show that their estimated increase in the declination rate for low-level misdemeanors in Boston did not coincide with an increase in local reported crime rates, and Ouss and Stevenson (2023) show that the reduced use of cash bail in Philadelphia had no effect on pretrial misconduct in affected cases.[1] In perhaps the most similar study to ours, Petersen et al. (2024) implement a difference-in-differences design to estimate the effect of electing a reform prosecutor on reported index crime rates in the 100 largest counties in the United States. They find a modest but statistically significant 7% increase in property crime rates. Our primary contribution to this growing body of empirical work on the causal effects of reform prosecutors is to estimate the average effect of a reform prosecutor's inauguration on both case outcomes and local crime

[1] An additional study attempts to use a synthetic control design to estimate the effect of a purported "de-prosecution" (also known as declination) policy on the local homicide rate in Philadelphia (Hogan, 2022). The author states that the policy began in 2015 under DA Williams and continued after reform DA Krasner's inauguration in 2018, and estimates that it substantially increased the homicide rate. The study has been widely criticized, both for failing to clearly define the stated "de-prosecution" policy or present sufficient empirical evidence that it began in 2015 (Kajeepeta, 2023) and for incorrect application of the synthetic control method (Kaplan et al., 2026). An additional report notes that the spike in homicide rates during the pandemic was not correlated with leadership by progressive prosecutors (Foglesong et al., 2022).

rates in a broad, overlapping set of jurisdictions that eventually elected reform prosecutors, with no restrictions on jurisdiction size or inauguration date except where affected by data availability. More generally, we also contribute to a growing literature estimating causal relationships between prosecutorial politics and various criminal justice and public safety outcomes. Prior work finds that narrowly elected Republican district attorneys typically increase incarceration (Arora, 2018; Krumholz, 2019), and recent work finds that they may also reduce external-cause mortality among young men, likely by reducing the proliferation of firearms among those at high risk for violence or suicide (Bencsik and Giles, 2026).

The rest of the paper proceeds as follows. Section 2 provides background information on how the reform prosecutors in our sample were identified and potential channels through which they may affect case outcomes, local reported crime rates, and drug mortality rates. Section 3 describes the data sources and sample construction for each portion of the analysis. Section 4 provides more detail about our staggered difference-in-differences design and estimation strategy, and the assumptions required for causal identification. Section 5 presents our main findings: we find that reform prosecutors reduce per capita charging and conviction rates in their jurisdictions, with the largest decreases occurring among misdemeanor offenses and drug offenses (5.1). However, we do not find robust evidence of large or statistically significant effects on local reported crime rates (5.2) or drug mortality rates (5.3). Lastly, Section 6 discusses the policy implications of our findings.

2 Background on Reform Prosecutors

2.1 Classification of Reform Prosecutors

A nonprofit organization that coordinates a network of reform prosecutors provided us with a list of prosecutors whom it had identified as reform-minded and who had chosen to join its network based on shared policy goals. The organization, which has requested to remain anonymous, promotes reducing the punitiveness of the criminal justice system through practices like increasing declination rates for low-level offenses, increasing the use of pretrial diversion programs, reducing the use of cash bail and/or pretrial detention, and prioritizing treatment over criminalization for drug offenses where possible and applicable. We view inclusion on its list as reflective of a two-way selection process: the organization views these prosecutors as aligning with its values, and these prosecutors chose to join the organization's network in order to connect with like-minded peers. The vast majority of the prosecutors on the list are local district attorneys and three are state attorneys general; we focus our analysis on the local district attorneys, who typically oversee routine prosecutions of most

crimes in their jurisdictions. Most jurisdictions are single counties, but a few are cities or groups of counties, as defined by state and local laws.

Prior work in this literature has classified district attorneys as "reform" or "progressive" prosecutors based on stated campaign policy platforms (Mitchell et al., 2022; Petersen et al., 2024; Mitchell and Petersen, 2025). We instead rely on prosecutors' self-selection into a reform-based network rather than categorizing them based on our own criteria, and include in our analysis sample (see section 3 below) all local prosecutors on the list for whom incident-level crime data were available from at least one city in their jurisdiction. Despite this different approach, our resulting list of reform prosecutors largely aligns with those classified as progressive by Petersen et al. (2024), with most differences stemming from the fact that their sample was limited to the 100 largest counties in the United States and to prosecutors inaugurated prior to 2020, and our sample includes some prosecutors from smaller counties and who were inaugurated after 2020, as shown in Table 1. We view this agreement as evidence that our hands-off approach to classification leads to a list sufficiently representative of the prosecutorial reform movement to be informative about its effects. Like the organization that provided us with the list, Petersen et al. (2024) also include in their criteria for what constitutes a "progressive" prosecutor elements like self-identification, increased use of diversion programming, decreased use of cash bail or pretrial detention, and increased use of declinations for certain case types. Further, we view our analyses of the effects of the reform prosecutors on our list on per capita charge and conviction rates in their jurisdictions as additional empirical validation that the prosecutors we study are likely pursuing meaningful reforms to reduce the punitive nature of the justice system.

2.2 Measuring Justice System Reach through Case Outcomes

Quantifying the extent to which reform prosecutors have successfully implemented the specific reforms on which they campaigned is challenging, partially due to data limitations and partially due to the causal identification challenges presented by multiple simultaneous and overlapping policy directives. We instead use charges per 100,000 residents per year, share of charges resulting in a conviction, and convictions per 100,000 residents per year as proxy measures of the ways in which reform prosecutors, on average, might change the footprint of the criminal justice system in their jurisdictions.

We first consider how the inauguration of a reform prosecutor might affect per capita charge rates. The most direct channels are through increased declination and/or reduction to non-criminal charges for low-level offenses, either through categorical policies or more informal directives to line prosecutors to redirect resources toward more serious cases. How-

ever, because an analysis of changes to arrest rates is beyond the scope of this paper, we cannot rule out the possibility that police responses to a reform prosecutor's inauguration may also affect charge rates by changing the number and composition of arrests presented to the prosecutor's office, or similarly that reporting behavior by civilians may be impacted.

Reform prosecutors may also affect the share of initial charges ending in a conviction through a variety of channels. Many such prosecutors campaigned on increasing the use of diversion, which typically offers programming and/or imposes conditions on defendants after charges are filed, and offers a dismissal of the case if programming is completed or conditions are met (Wright and Levine, 2021). In fact, a recent examination of pretrial diversion programs implemented by three of the reform prosecutors in our sample found that such programs are associated with much lower conviction rates for defendants who take them up (Davis et al., 2021). There are, however, additional indirect channels through which reform prosecutors may affect the share of charges convicted. For example, reform prosecutors often campaign on reducing requests for cash bail and/or pretrial detention, and existing evidence indicates that pretrial detention in a case increases the probability it ends in a conviction (Leslie and Pope, 2017; Dobbie et al., 2018). Unfortunately, we cannot directly measure this channel or other intermediate case events that may occur between initial charging and final disposition.[2] Nonetheless, it is possible that reduced detention or other unobserved leniency measures after initial charging may provide additional indirect mechanisms through which reform prosecutors could affect the share of initial charges ending in a conviction.

Lastly, we consider effects on per capita conviction rates. Any of the above channels affecting either the number of cases charged or the share of charges ending in a conviction may also have a downstream effect on convictions per 100,000 residents per year. Although different reform prosecutors may have different preferred vehicles for change, they generally all campaign on reducing the punitiveness of the system in one way or another. Per capita conviction rates therefore provide a convenient proxy measure for the extent to which average exposure to legal sanctions decreases after a reformer takes office.

2.3 Potential Mechanisms for Effects on Local Crime Rates

One potential mechanism through which we might expect prosecutor-driven reforms to affect crime rates is by reducing the incapacitation of offenders. The reformers' policy changes

[2]We lack jail incarceration data, and the extent to which any prosecutor-driven reductions in pretrial detention have come to fruition is uncertain. For example, Ouss and Stevenson (2023) show that in Philadelphia, a prosecutor-driven directive to reduce requests for cash bail did not reduce detention, instead just reducing financial costs to defendants who would have otherwise posted bail. However, implementation of such reforms may vary across jurisdictions, so some reform prosecutors may have reduced detention elsewhere.

could reduce incapacitation in two ways: either by limiting the use of pretrial detention or by reducing the number of convictions that lead to incarceration. Crime could increase if either of these channels allows would-be offenders to return to their neighborhoods and commit additional offenses. However, some proposed policy changes around declination and diversion may not necessarily reduce incarceration if the resulting leniency is concentrated among low-level offenses that typically do not lead to either pretrial detention or post-conviction incarceration.

There are two other potential mechanisms through which reform prosecutors might impact crime rates: deterrence and avoidance of collateral consequences. Reducing the likelihood of prosecution, pretrial detention, and/or conviction for low-level offenses could lead to increases in these offenses among those who had previously been deterred by the threat of sanctions. On the other hand, if the previous consequences for these offenses were criminogenic — for instance, if prosecution, pretrial detention, and/or conviction incentivize additional criminal activity due to the employment consequences of criminal records or other collateral consequences of criminal justice contact — then reducing those consequences could reduce crime. Agan et al. (2023) and Humphries et al. (2024) consider the individual-level effects of nonviolent misdemeanor prosecution and noncarceral felony conviction, respectively. Both find that the criminogenic collateral effects of legal sanctions appear to outweigh any specific deterrence effects, leading to increases in recidivism for the marginal defendant prosecuted for a nonviolent misdemeanor offense or convicted of a noncarceral felony offense. Although Humphries et al. (2024) do not evaluate the net effects of noncarceral felony convictions on general deterrence (i.e., effects on local crime rates), Agan et al. (2023) find no effects of a reduction in nonviolent misdemeanor prosecutions on local crime rates.

Lastly, we note that local reported crime rates have an important limitation as an outcome variable: we cannot disentangle effects on underlying rates of criminal behavior from changes in recording of incidents by police or reporting behavior by civilians. Many low-level reported offenses result from proactive police enforcement actions taken in response to observed criminal activity. After the inauguration of reform prosecutors, police officers may have strategically reallocated their effort (Gonçalves and Mello, 2023; Soliman, 2022), reducing certain types of stops or arrests that had become less likely to result in charges or convictions. In theory, this type of police response could result in reduced recording of low-level offenses. To mitigate this concern, we supplement the reported local crime rates analysis with an analysis of the effects of reform prosecutors on county-level drug overdose mortality rates. This approach provides a supplementary measure of likely criminal drug use that should be unaffected by changes in police officers' propensity to record drug-related crime incidents.

3 Data

As previously mentioned in section 2, we identify reform prosecutors using a list provided by a nonprofit that coordinates a network of such prosecutors and promotes policy changes intended to reduce the punitiveness of the justice system. Because the inauguration dates of prosecutors on our list range from 2007 to 2021, we attempted to collect outcome data for as many relevant jurisdictions as possible covering the period from 2005 to 2023. However, data availability for each outcome varies substantially across jurisdictions and over time. As a result, we limit all analyses to jurisdictions corresponding to prosecutors on the list for which local crime data are available in at least one police department in the jurisdiction for at least some portion of the sample period prior to the prosecutor's inauguration. We study each of the other outcomes for the subset of jurisdictions in the local crime sample for which at least some pre-inauguration data are available for that outcome. We discuss in more detail below the data sources for each outcome we study and potential limitations of the analysis due to data availability.

To understand how case disposition outcomes change under reform prosecutors' leadership, relative to their predecessors, we use data from the Criminal Justice Administrative Records System (CJARS). CJARS is a large data infrastructure project hosted at the University of Michigan that executes individual data sharing agreements with criminal justice agencies throughout the United States and links their data with other restricted-use Census data. The Justice Outcomes Explorer (CJARS JOE) aggregates these restricted-use individual-level data to produce publicly available, county-level measures of a variety of criminal justice outcomes (Finlay et al., 2025). The availability of each outcome varies across counties and over time depending on the specific data contributions from each participating agency. The primary measures we use from the CJARS JOE are county-year rates of misdemeanor and felony charges per 100,000 residents, and the share of initial misdemeanor and felony charges in a given county and year that eventually ended in a conviction. We also use these measures to construct downstream measures of convictions per 100,000 residents among cases charged in a given year. The analysis samples for misdemeanor and felony case outcomes include all counties and years for which the relevant charge and conviction data are available in the CJARS JOE data, that see the inauguration of a reform prosecutor in our time frame, and for which we were also able to obtain data on local crime outcomes (see below). We ensure that each jurisdiction in each analysis sample is covered over a continuous window that includes at least one year prior to the reform prosecutor's inauguration. Incidentally, we observe sufficient coverage for only one county per jurisdiction in the case outcomes analysis. The window of coverage for each jurisdiction in each analysis sample is

reported in Table 2.[3] In the Appendix, we similarly use the CJARS JOE data to estimate the effects of reform prosecutors on county-year prison incarceration rates per 100,000 residents where possible.[4]

To understand impacts on reported local crime rates, we attempted to obtain data on reported crime incidents from police departments in each reform prosecutor's jurisdiction. We keep in the sample all jurisdictions for which we were able to obtain data from at least one local police department with at least some coverage prior to the reform prosecutor's inauguration. Where possible, we collected data directly from a local police department via an open records request or an online open data portal (twenty-six jurisdictions). For these jurisdictions, we constructed rates using city-year or county-year (as defined by the coverage area of the police department in question) intercensal population estimates from the U.S. Census bureau for that year. We then supplemented these hand-collected, incident-level crime data with incident-level data from the FBI's National Incident-Based Reporting System (NIBRS), adding an additional thirteen prosecutorial jurisdictions. For jurisdictions where we rely on NIBRS data, we rely on the internally reported variable indicating the population served by the originating agency to construct per capita rates. Although NIBRS is a federally-administered program, participation for police departments is optional. Likewise, although it is becoming more commonplace for police departments to have internal IT systems or online open data portals from which they can easily provide incident-level data to researchers, coverage varies substantially across departments. We combine both types of incident-level data and aggregate up to the month for each offense type included in the analysis so as to maximize the number of relevant jurisdictions we are able to include and improve statistical power. Due to temporally and geographically inconsistent data availability, we define a "coverage window" for each jurisdiction throughout which local crime data are consistently available. In jurisdictions for which we rely on NIBRS data, we include any police departments whose boundaries lie mostly or entirely within the relevant jurisdiction and that consistently report offenses in every month of the coverage window. Where multiple agencies are included, we standardize the coverage window across agencies within the jurisdiction to allow for aggregation, creating a measure of total crime incidents reported by

[3]There are some jurisdictions for which we observe case outcomes in the CJARS JOE data only in periods prior to the prosecutor's inauguration, with no post-treatment periods. We include these jurisdictions in the comparison group.

[4]Ideally, we would like to also include an analysis of jail incarceration rates, since some reform prosecutors campaign on reducing the use of cash bail and/or pretrial detention, but we lack sufficient jail incarceration data. Other work on prosecutorial politics and incarceration uses jail data consisting of intermittent snapshots, which is suitable in close-election regression discontinuity designs that do not rely heavily on pre-inauguration outcomes, but is less suitable in our difference-in-differences design (Arora, 2018; Bencsik and Giles, 2026).

participating agencies in that jurisdiction-month per 10,000 residents served by those same agencies. This aggregation procedure ensures that the same police agencies are represented throughout the coverage window for each prosecutorial jurisdiction included in the analysis.

We aggregate the number of crimes reported in each jurisdiction to the month level, for each of the following offenses: homicide, assault, robbery, burglary, theft, vandalism, and drug offenses. We present results for broad, aggregated crime categories to reduce noise in our estimates, improve statistical power, and allow for comparison with the drug, property, and violent crime categories available for the case outcomes in the CJARS JOE data. We then divide by the local population and multiply by 10,000 to generate reported crimes per 10,000 residents for each category or offense type.[5] Outcomes are the number of offenses per 10,000 people per month. The final reported local crime analysis dataset is an unbalanced panel that includes 39 jurisdictions (cities, counties, or groups of counties, depending on the jurisdiction of the prosecutor and the available data) from January 2005 through December 2023.

The thirty-nine jurisdictions, date ranges, and data sources included in the reported local crime rates analysis are listed in Table 3. The prosecutors' inauguration dates in this sample range from January 1, 2007, through January 12, 2021. Our main specifications limit the analysis to the twenty-nine jurisdictions for which all crime types are reported, but we also report estimates using the full available sample for each individual offense type.

Lastly, we collected data on drug mortality from CDC Wonder, as measured by county-year drug mortality deaths per 100,000 residents. These data are reported only for counties with at least ten drug mortality deaths per year, resulting in a sample of twenty-nine counties. The years covered for each jurisdiction in the CDC Wonder data are reported in Table 4.

4 Empirical Strategy

We use elected reform prosecutors' inauguration dates as shocks to local prosecution policies. If those prosecutors' reforms affect charges per capita, case outcomes, and reported local crime rates, then we should see a change in trends relative to trends in other jurisdictions beginning soon after the reform prosecutors took office. Because the types of places that elect reform-minded prosecutors may have unobservable differences from those that do not, we focus on the sample of counties that elect a reform-minded prosecutor at some point during

[5] In jurisdictions where data were collected directly from a county police department, we used the county population from the U.S. Census. Where data were collected from a single city police department, we use the city population. Where NIBRS data were used, we used the population served by each included agency indicated in NIBRS and aggregated to the total population served by the included agencies.

the sample period.[6] The few jurisdictions for which we have data only for time periods before a reform-minded prosecutor was inaugurated are included in the comparison group.

In recent years, there has been substantial econometric innovation in staggered difference-in-differences estimation, given that traditional two-way-fixed-effect estimators (simply using place and time fixed effects to isolate the effect of policy changes) can generate misleading estimates (Goodman-Bacon, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021). Intuitively, this problem arises because such estimates are a weighted average of comparisons of treated places with places that were treated in the past, places that are treated in the future, and places that are never treated. Comparing treated places with places that were treated in the past can be particularly problematic.

We use the procedure developed by Callaway and Sant'Anna (2021) to compare groups of treated places with other places that are *not yet* treated. Specifically, we use the *csdid* package in Stata to generate group-time average treatment effect on the treated (ATT) estimates for each set of places where a reform-minded prosecutor took office in the same month. We then aggregate those estimates using a simple average to estimate the overall average treatment effect on the treated (ATT).

This procedure also estimates the following event study equations, for each group of treated places:

$$y_{j,t} = \sum_{i=-n}^{i=n} \beta_t I(t - t_m^* = i) + \gamma_t + \delta_j + \epsilon_{j,t} \tag{1}$$

where j is a jurisdiction, t is month for reported local crime rates or year for other outcomes, and t_m^* is the month or year the prosecutor in question took office. The outcome measure, $y_{j,t}$, is the number of reported crime incidents per 10,000 residents per month for the local crime rates analysis, share of charges resulting in a conviction for the convictions analysis, and rate per 100,000 residents per year for charging rate, incarceration, and drug overdose mortality outcomes. γ_t are month/year fixed effects and δ_j are place fixed effects. These absorb baseline variation in outcomes across places and shocks that are common across places over time. In the charging and conviction rates analyses, we observe only one county in each treated jurisdiction and perform county-level analyses. For the reported local crime rates and drug mortality rates analyses, we aggregate to the jurisdiction level and perform jurisdiction-level analyses.

Our staggered difference-in-differences estimation procedure compares newly-treated jurisdictions only to not-yet-treated jurisdictions, excluding problematic comparisons between newly-treated and earlier-treated jurisdictions. This also means that the final cohort of

[6] A related paper uses the same list of prosecutors and the full set of jurisdictions for which the CJARS JOE data are available, and finds qualitatively similar reductions in the footprint of the criminal justice system after a reform prosecutor takes office (Agan et al., 2026).

treated jurisdictions only ever contributes to the comparison group, since there are no remaining not-yet-treated jurisdictions from which to estimate their counterfactual after they are treated. However, if jurisdictions that elect reform prosecutors differ from those that never elect reform prosecutors on dimensions that may be correlated with our outcomes, this approach mitigates that selection by limiting the sample to eventually-treated jurisdictions and relying on variation in timing that may be more idiosyncratic. The use of the Callaway and Sant'Anna (2021) estimator also allows for the use of all available time periods for each jurisdiction, making full use of the available data in our unbalanced panel, which is particularly useful given the varying time horizon over which data are available across jurisdictions. It computes standard errors using a wild cluster bootstrap procedure that is robust to the small number of jurisdictions in some of our analyses. We weight by the population covered in each jurisdiction in each analysis.

The identifying assumption underlying our staggered difference-in-differences approach is that trends in charges, convictions, and reported crime rates in treated places and not-yet treated places would have evolved similarly in the absence of the reform prosecutors' inaugurations. If these prosecutors' inauguration dates (and their policy changes) systematically coincided with other local events that independently affected criminal behavior (such as a change in police activity), this would confound our estimates. Importantly, this parallel trends assumption does allow for level differences in crime rates across earlier-treated and later-treated jurisdictions. Differences in crime rates prior to the prosecutors' inaugurations— including single-month deviations from the overall trend —do not necessarily indicate violations of the equal trends assumption and may reflect noise. Likewise, the lack of data on the entirety of a prosecutor's jurisdiction should not necessarily produce a violation of the parallel trends assumption as long as the same portion of the jurisdiction is represented throughout the window of time the jurisdiction is included in the analysis.

Our main specification uses all time periods available for each jurisdiction within its "coverage window" (defined above in section 3) for which data are available, with no additional balancing procedures. The standardization of the coverage windows within a jurisdiction in each analysis should guard against bias from compositional change, but our estimates may not be fully representative of the portions of the jurisdiction for which outcome data are not available. To further demonstrate that our ATT estimates are unlikely to be driven by compositional change, for the reported local crime rates analysis we also present event studies that are balanced in event time using the stacked difference-in-differences estimator developed in Wing et al. (2024). Further, to account for endogenous selection into the drug mortality data since only county-year observations with at least ten overdose deaths are represented, we also present estimates using only the jurisdictions that are represented in

the CDC Wonder data in every year of our analysis from 2005-2023.[7]

5 Results

Overall, we find evidence that the inauguration of reform prosecutors reduces average per capita charge rates, the average share of initial charges resulting in a conviction, and average downstream per capita conviction rates in their jurisdictions. The largest estimated decreases are for misdemeanor offenses and drug offenses. These findings are consistent with the types of policy changes these prosecutors typically promote during their campaigns. We find no evidence that reform prosecutors have large or statistically significant effects on average reported local crime rates or drug mortality rates in the jurisdictions they lead, which is consistent with existing evidence that marginal nonviolent misdemeanor prosecutions (Agan et al., 2023) and noncarceral convictions (Humphries et al., 2024) tend to lead to higher recidivism rates for the defendants in question and therefore do not appear to generate meaningful public safety benefits.

5.1 ATT Effects on Case Outcomes

Table 5 presents our main estimates of the average treatment effect of a reform prosecutor's inauguration on misdemeanor case outcomes in a treated county. The sample includes all jurisdictions for which both misdemeanor charging and misdemeanor conviction data are available through CJARS JOE and for which we were able to obtain data on local reported crime rates. Results in column 1 of Panel A suggest that the inauguration of a reform prosecutor reduces overall misdemeanor charge rates by an estimated 622 charges per 100,000 residents per year, or about 19 percent of the pre-inauguration mean ($p < 0.1$). While point estimates in columns 2-4 of Panel A are suggestive of decreases in charge rates across offense types, only the decrease in drug charges is statistically significant ($p < 0.5$) at about 243 charges per 100,000 residents per year, a reduction of about 42 percent from the pre-inauguration average drug charge rate. Results in Panel B indicate that reform prosecutors reduce the share of charges convicted in their jurisdictions by about 9-11 percentage points (17-24 percent), and effects are statistically significant across all offense types. Lastly, Panel C shows the overall combined effect through both channels on per capita conviction rates. Column 1 shows that reform prosecutors reduce overall misdemeanor conviction rates by about 333 cases per 100,000 residents per year (23 percent, $p < 0.5$). Estimates are again

[7]The nine jurisdictions not represented over the full sample period in the drug mortality data have a balanced coverage window spanning the relevant prosecutor's inauguration and running until the end of the sample period in 2023.

suggestive of reductions in overall convictions across offense types, with the largest reduction for misdemeanor drug offenses at 86 cases per 100,000 residents per year ($p < 0.01$), a 39 percent effect relative to the baseline mean, and a relatively smaller but still meaningful (29 cases per 100,000 residents or 26 percent) and statistically significant ($p < 0.001$) effect for violent misdemeanor offenses, which appears mainly driven by the share of charges convicted and not charge rates.

Figure 1 presents event-study estimates of the effects of reform prosecutors on misdemeanor case outcomes. Note that causal identification in our staggered difference-in-differences design does not require that there be no level differences in average outcomes between the treated and not-yet-treated jurisdictions in the absence of the reform prosecutor's inauguration, only that the counterfactual trends in outcomes would have been parallel. While this is fundamentally untestable, our event-study estimates show no evidence of any clear pre-trend in per capita charge rates (1a), share of charges convicted (1b), or convictions per 100,000 residents (1c) in the two years prior to inauguration, which supports the assumption that trends would have remained similar in the absence of the new prosecutor. Consistent with the main results in Table 1, misdemeanor charges per capita, the share of misdemeanor charges convicted, and misdemeanor convictions per capita fall sharply in the reform prosecutor's inauguration year and the two years after.

Table 6 presents estimates of the effects of reform prosecutors on felony case outcomes. The sample includes all counties included in the local crime rates analysis for which both felony charging and felony conviction data are available in CJARS JOE. While there is substantial overlap between the misdemeanor and felony case outcomes samples, there are a few counties for which either misdemeanor or felony outcomes are available but not both, so the two samples differ slightly. Panel A presents estimated effects on per capita felony charge rates, with column 1 indicating that the inauguration of a reform prosecutor results in 335 fewer felony charges per 100,000 residents per year, a statistically significant 22 percent decrease ($p < 0.05$). While estimated effects are not statistically significant for any individual offense type, point estimates in columns 2-4 suggest decreases across all offense types with the largest decrease for drug offenses, consistent with the misdemeanor results. Panel B shows a similar pattern for the share of felony charges resulting in a conviction, with a 2.5 percentage point (5 percent) overall decrease in column 1 that appears to be driven primarily by drug offenses (column 2). Lastly, in Panel C, we show that the combination of these two channels produces 157 fewer felony convictions per 100,000 residents per year (-25 percent, $p < 0.001$) after the inauguration of a reform prosecutor. As was the case with misdemeanor offenses, columns 2-4 suggest that this decrease is driven primarily by drug offenses, with 112 fewer drug felony convictions per 100,000 residents per year (-57 percent, $p < 0.01$).

However, similar to the misdemeanor conviction results in Table 5, columns 2-4 suggest decreases in convictions across all offense types and we do find a statistically significant ($p < 0.01$) 25 percent decrease in violent felony conviction rates, a reduction of 24 violent felony convictions per 100,000 residents per year. Event-study estimates are presented in Figure 2; consistent with the main results in Table 6, we see similar trends in felony case outcomes in the two years leading up to inauguration and reductions in felony charges per capita (2a), share of felony charges convicted, especially for drug charges (2b), and felony convictions per capita (2c) in the year the reform prosecutor takes office and the two years after.

Our main estimates limit the sample to counties where both charging and conviction information are available in the CJARS JOE data. In Appendix Table A.1, we show that estimated effects on per capita charge rates are qualitatively similar when expanding the sample to include counties where conviction information is not available. Similarly, we note above in Section 2 that Petersen et al. (2024) drop from their analysis progressive prosecutors who had progressive predecessors. In our main analysis, we keep all reform prosecutors on our list regardless of their predecessors' political leanings, because they still campaigned on platforms of reform relative to the status quo in their jurisdictions. In Appendix Table A.2, we show that our main estimates of effects on case outcomes are similar to those obtained when we drop from the sample prosecutors classified by Petersen et al. (2024) as having progressive predecessors (see Table 1 for a list of these prosecutors). A similar analysis in a related paper using the same list of reform prosecutors and the same case outcomes from the CJARS JOE data, but without limiting the sample of treated jurisdictions based on the availability of crime data or limiting the comparison group to eventually-treated jurisdictions, produces qualitatively similar results (Agan et al., 2026). Lastly, in Appendix Table C.1 and Figure C.1, we estimate effects on prison incarceration rates in counties for which the CJARS JOE data include incarceration information. The point estimate suggests a 5 percent decrease in the per capita prison incarceration rate, but the effect is not statistically significant.

Overall, our findings are consistent with key elements of reform prosecutors' policy platforms identified both by Petersen et al. (2024) in prior work and by the nonprofit that coordinates the reform prosecutors we study. Our finding of reductions in per capita charge rates, with the largest estimated decreases occurring among drug offenses, is consistent with declination policies for low-level offenses. While our estimated ATT effects on per capita drug charge rates are quite large— 42 percent relative to the baseline mean for misdemeanor drug charges, and 66 percent for felony drug charges— they are plausible in light of policy directives implemented or promised by some reform prosecutors. Such policies include a

broad presumption of non-prosecution for marijuana offenses (Amaral et al., 2025) or for drug possession in general (Agan et al., 2023) in addition to other potential reforms promoting leniency for nonviolent offenses and/or treatment over criminalization in cases where substance use disorders are present. Further, the finding of decreases in the share of initial charges convicted, with larger decreases among misdemeanor offenses, is consistent with increased use of pretrial diversion programming that can result in the dismissal of initial charges if conditions are met. However, because we rely on summary measures of case outcomes that are available across jurisdictions, we cannot rule out more diffuse effects through other channels such as behavioral responses by line prosecutors or police leading to lower charge rates or unobserved decreases in pretrial detention leading to lower conviction rates (Leslie and Pope, 2017; Dobbie et al., 2018). Although we are unable to definitively attribute our findings to any one particular mechanism, the downstream decrease in per capita convictions indicates that, on average, the overall footprint of the criminal justice system in a given county is reduced after a reformer takes office.

5.2 ATT Effects on Local Reported Crime Rates

Table 7 presents our main estimates of the effect of reform-minded prosecutors on local reported crime rates, limiting the sample to jurisdictions where all the offense types are reported and aggregating up to broad categories to improve statistical power. The property offenses category (column 3) includes theft, burglary and vandalism, the violent offenses category (column 4) includes homicide, assault, and robbery, and the all-offense crime rate (column 1) includes all of the above plus drug offenses (column 2). We also disaggregate by whether an offense is typically financially motivated, showing results for income-generating offenses (robbery, burglary, theft) and non-income-generating offenses (homicide, assault, and vandalism).[8] We find no statistically significant change in reported offenses per 100,000 residents per month for any broad category. For all categories except drug offenses, our 95% confidence intervals—with standard errors computed using a wild cluster bootstrap due to the small number of clusters—can rule out increases larger than 10 percent over the baseline mean. For drug offenses, our confidence interval rules out increases larger than 19 percent over the baseline mean. We cannot rule out modest effects, such as the 7% increase in property offenses estimated by Petersen et al. (2024), but we can rule out large increases in reported offense rates for most crime types.

Figure 3 presents analogous event-study figures for these outcomes in the year prior to

[8]We exclude drug offenses from both categories, as police data often describe drug incidents by indicating possession of a substance without specifying the exact amount and whether assumed to be for distribution or personal use.

a reform prosecutor's inauguration and the year after. We see some pre-period differences in crime rates between the treated and not-yet-treated jurisdictions, but none indicating a steady pre-existing trend upward or downward for any outcome. Consistent with the aggregated estimates in Table 7, we see no evidence of large reductions or increases for any broad crime type after a reform prosecutor's inauguration.

In the Appendix we present a number of robustness checks for this finding. First, we present estimates that are not restricted to a consistent sample for all crime categories (Table B.1), since there are jurisdictions that consistently report some offenses but not others. We also present estimates using event-balanced samples, limiting the sample to jurisdictions with data available for six months (Table B.2, Panel A) or twelve months (Panel B) before and after the reform prosecutor's inauguration. These estimates employ the stacked difference-in-differences estimator described in Wing et al. (2024). In both cases, results are similar to the main estimates, with no statistically significant change in reported incidents per 100,000 residents per month for any offense category. For each of these alternate estimation strategies and samples, our 95% confidence intervals again rule out large increases for most offense types and indicate the most uncertainty for drug offenses. The event studies and trends presented in Appendix Figures B.1–B.6 (sub-figures a, b, c) indicate that the earlier-treated jurisdictions often see greater volatility in crime rates both before and after treatment than the not-yet-treated jurisdictions that form their comparison group, despite overall trends being similar. To account for this, we also estimate a synthetic difference-in-differences specification using the stacked panel that selects from the not-yet-treated comparison donor pool a weighted average of jurisdictions whose trends are most similar to the treated jurisdictions in each time period (Arkhangelsky et al., 2021). Trends presented in Figures B.1–B.6 (sub-figures d, e) appear roughly parallel both before and after treatment. Likewise, the corresponding ATT estimates presented in Table B.3, like our main estimates, indicate no evidence that the inaugurations of the reform prosecutors in our sample had significant effects on local reported crime rates.

Lastly, we present estimates dropping reform prosecutors in our sample that were classified by Petersen et al. (2024) as having progressive predecessors (Table B.4). For most crime categories, results are similar but confidence intervals are wider due to the smaller sample. However, we do find an increase of 0.35 drug offenses per 100,000 residents per month or 10 percent of the baseline mean ($p < 0.05$). This effect size is modest and well within the confidence intervals produced by the other specifications, but is statistically significant only in this specification. Based on the totality of our results, we are unable to draw strong conclusions as to whether the reform prosecutors in our sample had an effect on drug crime rates.

Table 8 presents our main estimates of the effects of reform prosecutors on monthly reported incident rates for individual offense types in our sample. This analysis of course trades off statistical power for specificity, as each offense type will be more rare on its own than when pooled with other similar offense types in a category (with the exception of drug offenses, which are already their own category). We again limit the main sample to jurisdictions where all offense types are consistently reported for comparability across crime types. For most offenses, we find no statistically significant effects of reform prosecutors on reported incidents per 100,000 residents per month, although the estimation is quite noisy and confidence intervals are wide for some offenses. Corresponding event studies for individual offense types are reported in Figure B.7 in the Appendix.

We do find a decrease in reported robberies of 0.7 incidents per 100,000 residents per month (50 percent, $p < 0.01$), but this finding is rather sensitive to specification checks. In the Appendix, we show results for samples that include all jurisdictions for which each individual offense type is reported, which increases the available sample for each offense type (Table B.5). We continue to find no statistically significant effects for other offense types, and the estimated effect on robberies decreases in magnitude and significance to an estimated decrease of 0.5 incidents per 100,000 residents per month (36 percent, $p < 0.1$). In the event-balanced samples using the stacked difference-in-differences estimator (Wing et al., 2024), the decrease in robberies becomes statistically insignificant in the six-month balanced sample (Table B.6, Panel A) and disappears entirely in the twelve-month balanced sample (Panel B). We continue to find no statistically significant effects for other offense types. We likewise find no evidence of statistically significant effects in the synthetic difference-in-differences specification (Table B.7). Our 95% confidence intervals from most specifications reject large increases in most offenses.

Critics of reform prosecutors have hypothesized that their more lenient approach to prosecution may lead to increases in crime via reduced deterrence, as their leadership may reduce the expected penalty for some forms of criminal activity. However, despite the large reductions in charges and convictions under reform prosecutors estimated above in section 5.1, our estimates generally rule out proportionally large increases in reported crime rates. Even for drug offenses, for which some specifications suggest a modest increase, our confidence intervals rule out relative magnitudes proportional to roughly half the estimated decreases in charges and convictions.

5.3 ATT Effects on Drug Overdose Mortality Rates

One limitation of the local reported crime rates analysis is that we are largely unable to disentangle any effects on underlying crime rates from effects on recording behavior by police or reporting behavior by civilians. This is a common concern in empirical work using crime rates as an outcome. To mitigate this concern, we estimate effects on local drug mortality rates as an additional measure of drug use that does not rely on police recording.

These results are presented in Table 9 with corresponding event studies in Figure 4. Panel A presents estimates for the rate of overdose deaths per 100,000 residents per year. Because counties must report at least 10 overdoses per year to be included in the data, we included jurisdictions in the analysis that have a continuous period of data with at least one year of data prior to a reform prosecutor's inauguration and one year after. Estimates for this main sample are reported in column 1. In light of this feature of the data, we also present results for a balanced sample — with data available in all years of the analysis (column 2)— and for log mortality rates for both the main sample and balanced sample (Panel B). Our main estimate suggests an increase of about of 3 overdose deaths per 100,000 residents per year (25 percent, $p < 0.1$), but this estimated effect is not statistically significant in other specifications. While a moderate increase in drug overdose mortality rates would be consistent with the robustness check suggesting a possible increase in drug incidents in the local reported crime rates analysis, the estimates presented here should be interpreted with caution. Event studies presented in Figure 4 suggest that the increase in drug mortality rates may begin in the period prior to a reform prosecutor's inauguration, calling the equal trends assumption into question.

Overall, we do not find robust and convincing evidence of an effect on drug mortality rates, but results suggest, if anything, a modest increase. This pattern is consistent with the results for drug offenses presented in section 5.2 above. We are therefore unable to draw any strong conclusions about effects of reform prosecutors on drug possession and drug use, but we find no evidence that police recording behavior is a substantial source of bias in our estimates, given that the patterns of results across local reported drug offenses and local drug mortality rates are quite consistent with each other.

6 Discussion and Policy Implications

The results in this paper show that reform-minded prosecutors typically reduce per capita charge and conviction rates shortly after they take office, especially for lower-level offenses. While we are unable to rule out other mechanisms, our findings are consistent with some of

the reforms on which these prosecutors typically campaign, such as presumptive declination policies for certain minor offenses (Agan et al., 2023; Amaral et al., 2025) and increased use of pretrial diversion programming (Wright and Levine, 2021; Davis et al., 2021). We find that the inauguration of a reform prosecutor in our sample reduces convictions per capita by about 23-25 percent on average in the jurisdiction they represent.

These substantial reductions in legal sanctions do not appear to translate to proportionately large changes in reported crime rates, and we do not find robust evidence of an effect on drug mortality rates. However, some caution is warranted given that we cannot fully disentangle any changes in civilian or police reporting behavior from changes in underlying criminal activity nor guarantee the external validity of our estimates in jurisdictions or parts of jurisdictions excluded due to data availability. Despite these limitations, our findings provide suggestive evidence that reform prosecutors can successfully reduce the imposition of legal sanctions on the margin, especially for minor offenses, without large adverse effects on public safety. It is an open research question, however, at what level of leniency and for what crimes there might be adverse consequences for public safety via reduced deterrence.

Although we do not find that the marginal charges and convictions averted under reform prosecutors led to statistically significant downstream reductions in prison incarceration rates, prior findings in this literature give us reason to believe these foregone sanctions may have nonetheless been costly to defendants without generating substantial public safety benefits.

Existing work finds that the marginal misdemeanor charge (Agan et al., 2023) or conviction (Kamat et al., 2024) tends to increase the likelihood the defendant is re-arrested, especially for defendants without an existing criminal record. Likewise, for felony defendants, the marginal non-carceral felony conviction increases recidivism relative to non-conviction Humphries et al. (2024). Further, prior charges and convictions may result in harsher sanctions for future offenses in the event that the defendant is re-arrested, including increases in incarceration (Harrington et al., 2025).

In addition to increases in recidivism and future sanctions, a growing literature documents substantial collateral consequences of criminal charges and convictions through channels outside of the criminal justice system. For example, evidence from audit experiments indicates that employers are less likely to return calls from applicants who report a past charge or conviction (Uggen et al., 2014; Agan and Starr, 2017). Further, convictions can lead to substantial long-term earnings losses (Mueller-Smith and Schnepel, 2021) that are likely driven primarily by channels other than incarceration (Garin et al., 2025) and often persist even after a conviction has been expunged due to labor market scarring (Agan et al., 2025). Lastly, criminal justice sanctions can also lead to other economic losses for affected individuals and

their families, such as exclusion from social safety net programs (Mueller-Smith et al., 2023), increased debt due to fines (Mello, 2025), difficulty securing rental housing (Clark, 2007; Evans and Porter, 2015; Evans, 2016), or reduced access to higher education (Stewart and Uggen, 2020). Forgoing the marginal charge or conviction is likely to shield at least some defendants from these costs.

Further, charging fewer cases may have additional benefits for the efficiency and functioning of the justice system. Some have theorized that charging fewer cases may reduce strain on indigent defense systems by reducing the caseloads of court-appointed attorneys (Hashimoto, 2007), who often face caseload pressures that are associated with worse outcomes for defendants (Caspi, 2023). Others have highlighted the frequency with which various actors fail to appear in court, often resulting in continuances that delay proceedings or logistically-driven case dismissals after some initial effort and resources have already been expended (Graef et al., 2023). It has also been hypothesized that court caseload pressures and resource constraints can influence case outcomes in ways that may not necessarily be optimal (Ulmer and Johnson, 2004; Yang, 2016). While it is beyond the scope of our paper to estimate effects on congestion-related costs across the jurisdictions in our sample, it stands to reason that reduced caseloads in criminal courts may alleviate some of them.

If, as our findings suggest, reform prosecutors are successfully reducing legal sanctions without substantial corresponding increases in serious crime, their less punitive approach may generate cost savings both for the affected defendants and for the justice system in general. We leave for future work more granular explorations of the effects of specific programs or practices and analyses of the long-term effects of prosecutor-driven reforms.

References

Agan, A., Doleac, J. L., and Harvey, A. (2023). Misdemeanor prosecution. *The Quarterly Journal of Economics*, 138(3):1453–1505.

Agan, A., Doleac, J. L., Harvey, A., Kyriazis, A., and Schechter, L. (2026). Reform prosecutors and the footprint of the criminal justice system. *AEA Papers and Proceedings*, Forthcoming.

Agan, A., Garin, A., Koustas, D., Mas, A., and Yang, C. S. (2025). Can you erase the mark of a criminal record? labor market impacts of criminal record remediation. *Forthcoming, American Economic Journal: Economic Policy*.

Agan, A. and Starr, S. (2017). The effect of criminal records on access to employment. *American Economic Review*, 107(5):560–64.

Amaral, F. A., Ouss, A., and Ozier, D. I. (2025). Prosecutor-driven reform and racial disparities. *Criminology & Public Policy*, 24(4):527–555.

Arkhangelsky, D., Athey, S., Hirshberg, D. A., Imbens, G. W., and Wager, S. (2021). Synthetic difference-in-differences. *American Economic Review*, 111(12):4088–4118.

Arora, A. (2018). Too tough on crime? the impact of prosecutor politics on incarceration. *American Economic Association (Conference Paper)*.

Bencsik, P. and Giles, T. (2026). Local prosecutors and public health. *Working Paper*.

Bowman, R., Lowrey-Kinberg, B., and Gould, J. (2024). An integrated model of prosecutor decision-making. *Law & Society Review*, 58(3):452–480.

Bushway, S. D. and Forst, B. (2013). Studying discretion in the processes that generate criminal justice sanctions. *Justice Quarterly*, 30(2):199–222.

Bushway, S. D. and Piehl, A. M. (2001). Judging judicial discretion: Legal factors and racial discrimination in sentencing. *Law & Society Review*, 35(4):733–764.

Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230.

Caspi, A. (2023). Overworking public defenders. *Available at SSRN 4401227*.

Clark, L. M. (2007). Landlord attitudes toward renting to released offenders. *Fed. Probation*, 71:20.

Davis, R. C., Reich, W. A., Rempel, M., and Labriola, M. (2021). A multisite evaluation of prosecutor-led pretrial diversion: Effects on conviction, incarceration, and recidivism. *Criminal Justice Policy Review*, 32(8):890–909.

De Chaisemartin, C. and d'Haultfoeuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, 110(9):2964–2996.

Dobbie, W., Goldin, J., and Yang, C. S. (2018). The effects of pretrial detention on conviction, future crime, and employment: Evidence from randomly assigned judges. *American Economic Review*, 108(2):201–40.

Evans, D. N. (2016). The effect of criminal convictions on real estate agent decisions in new york city. *Journal of Crime and Justice*, 39(3):363–379.

Evans, D. N. and Porter, J. R. (2015). Criminal history and landlord rental decisions: A new york quasi-experimental study. *Journal of Experimental Criminology*, 11(1):21–42.

Finlay, K., Mueller-Smith, M., and the CJARS team (2025). Justice outcomes explorer (joe) [dataset]. https://joe.cjars.org.

Foglesong, T., Levi, R., Rosenfeld, R., Schoenfeld, H., Stemen, D., Wood, J., and Rengifo, A. (2022). Violent crime and public prosecution: A review of recent data on homicide, robbery, and progressive prosecution in the united states. Policy report.

Garin, A., Koustas, D., McPherson, C., Norris, S., Pecenco, M., Rose, E. K., Shem-Tov, Y., and Weaver, J. (2025). The impact of incarceration on employment, earnings, and tax filing. *Econometrica*, 93(2):503–538.

Gonçalves, F. M. and Mello, S. (2023). Police discretion and public safety. *Forthcoming, Journal of Political Economy Microeconomics*.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2):254–277.

Gottlieb, A., Epperson, M., Giugno, L., and Jung, H.-M. (2025). What happens to imprisonment rates when a progressive prosecutor is elected: Quasi-experimental evidence from cook county. *Punishment & Society*, 27(2):315–337.

Graef, L., Mayson, S. G., Ouss, A., and Stevenson, M. T. (2023). Systemic failure to appear in court. *U. Pa. L. Rev.*, 172:1.

Harrington, E., Murdock, W. I., and Shaffer, H. (2025). Prediction errors, incarceration, & violent crime: Evidence from linking prosecutor surveys to court records. *Forthcoming, American Economic Journal: Economic Policy*.

Hashimoto, E. J. (2007). The price of misdemeanor representation. *Wm. & Mary L. Rev.*, 49:461.

Hessick, C. B. and Morse, M. (2019). Picking prosecutors. *Iowa L. Rev.*, 105:1537.

Hogan, T. P. (2022). De-prosecution and death: A synthetic control analysis of the impact of de-prosecution on homicides. *Criminology & Public Policy*, 21(3):489–534.

Humphries, J. E., Ouss, A., Stavreva, K., Stevenson, M. T., and van Dijk, W. (2024). Conviction, incarceration, and recidivism: Understanding the revolving door. *NBER Working Paper No. 32894*.

Kajeepeta, S. (2023). Comment on hogan (2022): Fundamental problems with a test of "de-prosecution". *Criminology & Public Policy*, 22:83.

Kamat, V., Norris, S., and Pecenco, M. (2024). Conviction, incarceration, and policy effects in the criminal justice system. *Available at SSRN 4777635*.

Kaplan, J., Naddeo, J., and Scott, T. (2026). De-prosecution and death: A comment on hogan (2022). *Economic Inquiry*.

Krumholz, S. (2019). The effect of district attorneys on local criminal justice outcomes. *Available at SSRN 3243162*.

Leslie, E. and Pope, N. G. (2017). The unintended impact of pretrial detention on case outcomes: Evidence from new york city arraignments. *Journal of Law and Economics*, 60(3).

Mello, S. (2025). Fines and financial wellbeing. *Review of Economic Studies*, 92(5):3340–3374.

Mitchell, O., Mora, D. O., Sticco, T. L., and Boggess, L. N. (2022). Are progressive chief prosecutors effective in reducing prison use and cumulative racial/ethnic disadvantage? evidence from florida. *Criminology & Public Policy*, 21(3):535–565.

Mitchell, O. and Petersen, N. (2025). The rise of progressive prosecutors in the united states: Politics, prospects, and perils. *Annual Review of Criminology*, 8(2025):459–484.

Mueller-Smith, M. and Schnepel, K. (2021). Diversion in the criminal justice system. *The Review of Economic Studies*, 88(2):883–936.

Mueller-Smith, M. G., Reeves, J. M., Schnepel, K., and Walker, C. (2023). The direct and intergenerational effects of criminal history-based safety net bans in the us. *NBER Working Paper No. 31983*.

Ouss, A. and Stevenson, M. (2023). Does cash bail deter misconduct? *American Economic Journal: Applied Economics*, 15(3):150–182.

Petersen, N., Mitchell, O., and Yan, S. (2024). Do progressive prosecutors increase crime? a quasi-experimental analysis of crime rates in the 100 largest counties, 2000–2020. *Criminology & Public Policy*, 23(2):459–490.

Pfaff, J. (2017). *Locked in: The true causes of mass incarceration-and how to achieve real reform*. Basic Books.

Soliman, A. (2022). Crime and (a preference for) punishment: The effects of drug policy reform on policing activity. *The Journal of Law and Economics*, 65(4):791–810.

Spohn, C. (2018). Reflections on the exercise of prosecutorial discretion 50 years after publication of the challenge of crime in a free society. *Criminology & Public Policy*, 17(2):321–340.

Stewart, R. and Uggen, C. (2020). Criminal records and college admissions: A modified experimental audit. *Criminology*, 58(1):156–188.

Uggen, C., Vuolo, M., Lageson, S., Ruhland, E., and K. WHITHAM, H. (2014). The edge of stigma: An experimental audit of the effects of low-level criminal records on employment. *Criminology*, 52(4):627–654.

Ulmer, J. T. and Johnson, B. (2004). Sentencing in context: A multilevel analysis. *Criminology*, 42(1):137–178.

Wing, C., Freedman, S. M., and Hollingsworth, A. (2024). Stacked difference-in-differences. *NBER Working Paper No. 32054*.

Wright, R. F. and Levine, K. L. (2021). Models of prosecutor-led diversion programs in the united states and beyond. *Annual Review of Criminology*, 4(1):331–351.

Yang, C. S. (2016). Resource constraints and the criminal justice system: Evidence from judicial vacancies. *American Economic Journal: Economic Policy*, 8(4):289–332.

7 Figures and Tables

7.1 Sample Description and Classification of Prosecutors

Table 1: Classification of reform prosecutors relative to Petersen et al. (2024)

Name	Title	Jurisdiction	State	Reason(s)
Panel A: Overlap With Petersen, Mitchell and Yan (2024) Sample				
Wesley Bell	Prosecuting Attorney	St Louis County	Missouri	
Aisha Braveboy	State's Attorney	Prince George's County	Maryland	
John Chisholm	District Attorney	Milwaukee County	Wisconsin	
John Creuzot	District Attorney	Dallas County	Texas	
Kim Foxx	State's Attorney	Cook County	Illinois	
Larry Krasner	District Attorney	Philadelphia	Pennsylvania	
Beth McCann	District Attorney	Second Judicial District	Colorado	
Ryan Mears	District Attorney	Marion County	Indiana	
Brian Middleton	District Attorney	Fort Bend County	Texas	
Rachael Rollins	District Attorney	Suffolk County	Massachusetts	
Panel B: Prosecutors Ineligible for Petersen, Mitchell, and Yan (2024) Sample				
Buta Biberaj	Commonwealth's Attorney	Loudoun County	Virginia	Jurisdiction Size
Dave Clegg	District Attorney	Ulster County	New York	Jurisdiction Size
Shalena Cook Jones	District Attorney	Chatham County	Georgia	Size, Sampling Period
Parisa Dehghani-Tafti	Commonwealth's Attorney	Arlington County/City of Falls Church	Virginia	Jurisdiction Size
Michael Dougherty	District Attorney	20th Judicial District	Colorado	Jurisdiction Size
Mark Dupree	District Attorney	Wyandotte County	Kansas	Jurisdiction Size
Matthew Ellis	District Attorney	Wasco County	Oregon	Size, Sampling Period
Kim Gardner	Circuit Attorney	City of St Louis	Missouri	Jurisdiction Type
José Garza	District Attorney	Travis County	Texas	Sampling Period
Sarah George	State Attorney	Chittenden County	Vermont	Jurisdiction Size
Mark Gonzalez	District Attorney	Nueces County	Texas	Jurisdiction Size
Andrea Harrington	District Attorney	Berkshire County	Massachusetts	Jurisdiction Size
Jim Hingeley	Commonwealth's Attorney	Albemarle County	Virginia	Jurisdiction Size
Alexis King	District Attorney	First Judicial District	Colorado	Size, Sampling Period
Karen McDonald	Prosecuting Attorney	Oakland County	Michigan	Sampling Period
Marilyn Mosby	State's Attorney	Baltimore City	Maryland	Jurisdiction Type
Alonzo Payne	District Attorney	12th Judicial District	Colorado	Size, Sampling Period
Harold Pryor	State Attorney	17th Judicial Circuit	Florida	Sampling Period
Dalia Racine	District Attorney	Douglas County	Georgia	Size, Sampling Period
Eli Savit	Prosecuting Attorney	Washtenaw County	Michigan	Size, Sampling Period
Carol Siemon	Prosecuting Attorney	Ingham County	Michigan	Jurisdiction Size
Jared Williams	District Attorney	Augusta Judicial Circuit	Georgia	Size, Sampling Period
Panel C: Differences from Petersen, Mitchell, and Yan (2024) Sample				
Sherry Boston	District Attorney	DeKalb County	Georgia	Classified Traditional
Chesa Boudin	District Attorney	City and County of San Francisco	California	Progressive Predecessor
Steve Descano	Commonwealth's Attorney	Fairfax County	Virginia	Progressive Predecessor
Mimi Rocah	District Attorney	Westchester County	New York	Progressive Predecessor
Mike Schmidt	District Attorney	Multnomah County	Oregon	Progressive Predecessor
Monique Worrell	State Attorney	9th Judicial Circuit	Florida	Progressive Predecessor

Table 2: CJARS JOE data coverage, case outcomes analysis

					Misdemeanor Samples		Felony Samples	
Name	Title	Jurisdiction	State	Inaug.	Main	Expanded	Main	Expanded
Buta Biberaj	Commonwealth's Attorney	Loudoun County	Virginia	2019	2009-2019	2009-2019	2009-2019	2009-2019
Aisha Braveboy	State's Attorney	Prince George's County	Maryland	2019	-	2005-2017	-	2005-2017
John Chisholm	District Attorney	Milwaukee County	Wisconsin	2007	2005-2018	2005-2018	-	-
Laura Conover	County Attorney	Pima County	Arizona	2020	2005-2020	2005-2020	-	-
John Creuzot	District Attorney	Dallas County	Texas	2019	2005-2012	2005-2012	2005-2012	2005-2012
Parisa Dehghani-Tafti	Commonwealth's Attorney	Arlington County, Falls Church	Virginia	2019	2009-2019	2009-2019	2009-2019	2009-2019
Steve Descano	Commonwealth's Attorney	Fairfax County	Virginia	2019	2009-2019	2009-2019	-	-
Matthew Ellis	District Attorney	Wasco County	Oregon	2021	2018-2020	2005-2020	2005-2020	2005-2020
José Garza	District Attorney	Travis County	Texas	2021	2005-2013	2005-2013	2005-2017	2005-2017
Mark Gonzalez	District Attorney	Nueces County	Texas	2017	2005-2012	2005-2012	2005-2012	2005-2012
Jim Hingeley	Commonwealth's Attorney	Albemarle County	Virginia	2019	2009-2019	2009-2019	2009-2019	2009-2019
Larry Krasner	District Attorney	Philadelphia	Pennsylvania	2018	2009-2018	2009-2018	2009-2018	2009-2018
Karen McDonald	Prosecuting Attorney	Oakland County	Michigan	2021	2005-2020	2005-2020	2005-2020	2005-2020
Marilyn Mosby	State's Attorney	Baltimore City	Maryland	2015	2005-2017	2005-2017	2005-2017	2005-2017
Harold Pryor	State Attorney	17th Judicial Circuit	Florida	2021	-	2005-2020	2005-2020	2005-2020
Eli Savit	Prosecuting Attorney	Washtenaw County	Michigan	2021	2007-2020	2007-2020	2015-2020	2005-2020
Mike Schmidt	District Attorney	Multnomah County	Oregon	2021	2005-2020	2005-2020	2005-2020	2005-2020
Carol Siemon	Prosecuting Attorney	Ingham County	Michigan	2017	2005-2020	2005-2020	2005-2020	2005-2020
Monique Worrell	State Attorney	9th Judicial Circuit	Florida	2021	2005-2020	2005-2020	2005-2020	2005-2020

Table 3: Analysis sample and crime data sources

Name	Title	Jurisdiction	State	Inaug.	Data Source	Begin	End
Wesley Bell	Prosecuting Attorney	St Louis County	Missouri	1/1/19	St. Louis Cty. PD	12/17	11/20
Buta Biberaj	Commonwealth's Attorney	Loudoun County	Virginia	12/20/19	NIBRS (Leesburg/Loudoun)	1/05	12/23
Sherry Boston	District Attorney	DeKalb County	Georgia	12/19/16	DeKalb County PD	6/16	6/21
Chesa Boudin	District Attorney	City and County of San Francisco	California	1/8/20	San Francisco PD	1/18	6/21
Aisha Braveboy	State's Attorney	Prince George's County	Maryland	7/1/19	Prince George's Cty. PD	2/17	3/21
John Chisholm	District Attorney	Milwaukee County	Wisconsin	1/1/07	NIBRS (Milwaukee)	1/05	12/23
Dave Clegg	District Attorney	Ulster County	New York	1/1/20	Ulster Cty. Sheriff	1/19	7/22
Laura Conover	County Attorney	Pima County	Arizona	1/2/20	Tucson PD	1/18	3/21
Shalena Cook Jones	District Attorney	Chatham County	Georgia	1/4/21	Chatham Cty. PD	2/18	6/21
John Creuzot	District Attorney	Dallas County	Texas	1/1/19	Dallas PD	6/14	4/21
Parisa Dehghani-Tafti	Commonwealth's Attorney	Arlington County, Falls Church	Virginia	12/16/19	Arlington County PD	1/15	6/21
Steve Descano	Commonwealth's Attorney	Fairfax County	Virginia	12/16/19	NIBRS (Fairfax/Herndon)	1/05	12/23
Michael Dougherty	District Attorney	20th Judicial District	Colorado	3/1/18	NIBRS (6 agencies)	1/15	7/22
Mark Dupree	District Attorney	Wyandotte County	Kansas	1/9/17	Kansas City PD	1/15	7/21
Matthew Ellis	District Attorney	Wasco County	Oregon	1/4/21	NIBRS (The Dalles)	8/17	12/23
Kim Foxx	State's Attorney	Cook County	Illinois	12/1/16	Chicago PD	2/14	12/23
Kim Gardner	Circuit Attorney	City of St Louis	Missouri	1/6/17	St. Louis Metropolitan PD	1/15	12/20
José Garza	District Attorney	Travis County	Texas	1/1/21	NIBRS (9 agencies)	9/19	12/23
Sarah George	State Attorney	Chittenden County	Vermont	1/20/17	Burlington PD	12/11	3/21
Mark Gonzalez	District Attorney	Nueces County	Texas	1/12/17	Corpus Christi PD	10/14	7/21
Andrea Harrington	District Attorney	Berkshire County	Massachusetts	1/2/19	Pittsfield PD	1/17	4/21
Jim Hingeley	Commonwealth's Attorney	Albemarle County	Virginia	12/17/19	NIBRS (Albemarle Cty.)	1/05	12/16
Alexis King	District Attorney	First Judicial District	Colorado	1/12/21	Golden PD	10/19	5/21
Larry Krasner	District Attorney	Philadelphia	Pennsylvania	1/2/18	Philadelphia PD	1/16	12/20
Beth McCann	District Attorney	Second Judicial District	Colorado	1/10/17	Denver PD	2/16	3/21
Karen McDonald	Prosecuting Attorney	Oakland County	Michigan	1/1/21	NIBRS (33 agencies)	1/18	12/23
Ryan Mears	District Attorney	Marion County	Indiana	9/23/19	NIBRS (Indianapolis)	7/19	12/23
Brian Middleton	District Attorney	Fort Bend County	Texas	1/1/19	NIBRS (Stafford)	2/18	12/23
Marilyn Mosby	State's Attorney	Baltimore City	Maryland	1/8/15	Baltimore PD	1/14	4/21
Alonzo Payne	District Attorney	12th Judicial District	Colorado	1/12/21	NIBRS (5 agencies)	12/19	10/22
Harold Pryor	State Attorney	17th Judicial Circuit	Florida	1/5/21	Fort Lauderdale PD	1/15	2/21
Dalia Racine	District Attorney	Douglas County	Georgia	12/29/20	NIBRS (Douglasville)	1/20	12/23
Mimi Rocah	District Attorney	Westchester County	New York	1/4/21	Mt. Pleasant PD	1/19	8/21
Rachael Rollins	District Attorney	Suffolk County	Massachusetts	1/2/19	Boston PD	7/15	10/20
Eli Savit	Prosecuting Attorney	Washtenaw County	Michigan	1/2/21	NIBRS (7 agencies)	1/05	12/23
Mike Schmidt	District Attorney	Multnomah County	Oregon	1/4/21	Portland PD	5/15	12/20
Carol Siemon	Prosecuting Attorney	Ingham County	Michigan	1/1/17	NIBRS (5 agencies)	1/08	12/23
Jared Williams	District Attorney	Augusta Judicial Circuit	Georgia	12/28/20	Richmond Cty. Sheriff	1/19	7/21
Monique Worrell	State Attorney	9th Judicial Circuit	Florida	1/5/21	Orlando PD	1/19	9/21

Table 4: CDC Wonder data coverage, drug mortality analysis

Name	Title	Jurisdiction	State	Inauguration	Years
Wesley Bell	Prosecuting Attorney	St Louis County	Missouri	2019	2005-2023
Buta Biberaj	Commonwealth's Attorney	Loudoun County	Virginia	2019	2013-2023
Sherry Boston	District Attorney	DeKalb County	Georgia	2016	2005-2023
Chesa Boudin	District Attorney	City and County of San Francisco	California	2020	2005-2023
John Chisholm	District Attorney	Milwaukee County	Wisconsin	2007	2005-2023
Laura Conover	County Attorney	Pima County	Arizona	2020	2005-2023
Shalena Cook Jones	District Attorney	Chatham County	Georgia	2021	2005-2023
John Creuzot	District Attorney	Dallas County	Texas	2019	2005-2023
Parisa Dehghani-Tafti	Commonwealth's Attorney	Arlington County, Falls Church	Virginia	2019	2015-2023
Steve Descano	Commonwealth's Attorney	Fairfax County	Virginia	2019	2005-2023
Michael Dougherty	District Attorney	20th Judicial District	Colorado	2018	2005-2023
Kim Foxx	State's Attorney	Cook County	Illinois	2016	2005-2023
Kim Gardner	Circuit Attorney	City of St Louis	Missouri	2017	2005-2023
José Garza	District Attorney	Travis County	Texas	2021	2005-2023
Mark Gonzalez	District Attorney	Nueces County	Texas	2017	2005-2023
Alexis King	District Attorney	First Judicial District	Colorado	2021	2005-2023
Larry Krasner	District Attorney	Philadelphia	Pennsylvania	2018	2005-2023
Beth McCann	District Attorney	Second Judicial District	Colorado	2017	2005-2023
Karen McDonald	Prosecuting Attorney	Oakland County	Michigan	2021	2005-2023
Ryan Mears	District Attorney	Marion County	Indiana	2019	2005-2023
Brian Middleton	District Attorney	Fort Bend County	Texas	2019	2005-2023
Harold Pryor	State Attorney	17th Judicial Circuit	Florida	2021	2005-2023
Mimi Rocah	District Attorney	Westchester County	New York	2021	2006-2023
Rachael Rollins	District Attorney	Suffolk County	Massachusetts	2019	2005-2023
Eli Savit	Prosecuting Attorney	Washtenaw County	Michigan	2021	2008-2023
Mike Schmidt	District Attorney	Multnomah County	Oregon	2021	2005-2023
Carol Siemon	Prosecuting Attorney	Ingham County	Michigan	2017	2005-2023
Jared Williams	District Attorney	Augusta Judicial Circuit	Georgia	2020	2005-2023
Monique Worrell	State Attorney	9th Judicial Circuit	Florida	2021	2005-2023

7.2 ATT Effects on Case Outcomes

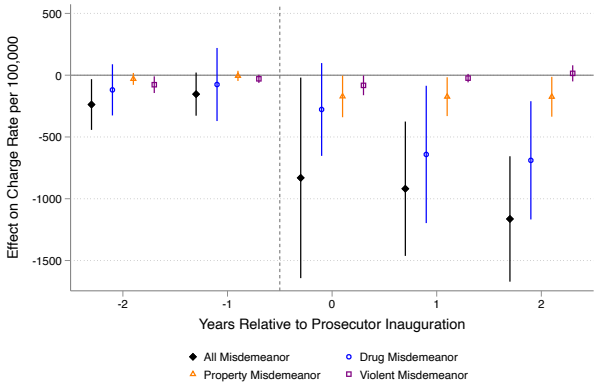
Table 5: ATT effect of reform prosecutors on misdemeanor case outcomes

	(1) All	(2) Drug	(3) Property	(4) Violent
Panel A: Charges per 100,000 residents				
ATT	-622.3^{+}	-243.2*	-126.0	-16.24
	(367.5)	(112.3)	(88.74)	(35.32)
Pre-Treatment Mean	3197.7	574.8	567.9	449.2
Percentage Effect	-19.46	-42.31	-22.19	-3.616
Jurisdictions	17	17	17	17
Observations	203	203	203	203
Panel B: Share of charges convicted				
ATT	-0.0976***	-0.107***	-0.0931**	-0.118**
	(0.0271)	(0.0291)	(0.0319)	(0.0411)
Pre-Treatment Mean	0.553	0.558	0.555	0.484
Percentage Effect	-17.66	-19.23	-16.78	-24.32
Jurisdictions	17	17	17	17
Observations	203	203	203	203
Panel C: Convictions per 100,000 residents				
ATT	-333.0*	-85.99***	-69.07*	-29.28***
	(140.8)	(24.88)	(34.52)	(5.852)
Pre-Treatment Mean	1409.9	220.3	232.6	112.0
Percentage Effect	-23.62	-39.03	-29.70	-26.14
Jurisdictions	17	17	17	17
Observations	203	203	203	203

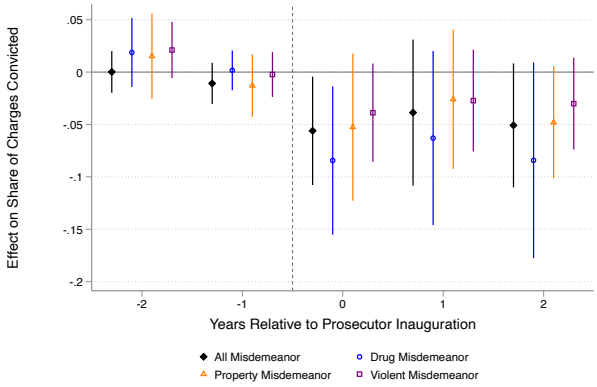
Notes. The dependent variable in Panel A is the misdemeanor charging rate per 100,000 residents; in Panel B, it is the share of misdemeanor charges resulting in a conviction, measured as of the year the case was initially charged; and in Panel C, it is the misdemeanor conviction rate per 100,000 residents. Charging and conviction data are available for only one county per jurisdiction. The sample includes all jurisdictions for which both misdemeanor charging and misdemeanor conviction data are available through CJARS JOE and for which we were able to obtain data on local reported crime rates. The analysis is at the county-year level, and average treatment on the treated (ATT) effects are estimated using the Callaway and Sant'Anna (2021) estimator. Standard errors, clustered at the county level, are reported in parentheses. +10%, *5%, **1%, and ***0.1% significance levels.

Figure 1: Event-study ATT effects of reform prosecutors on misdemeanor case outcomes

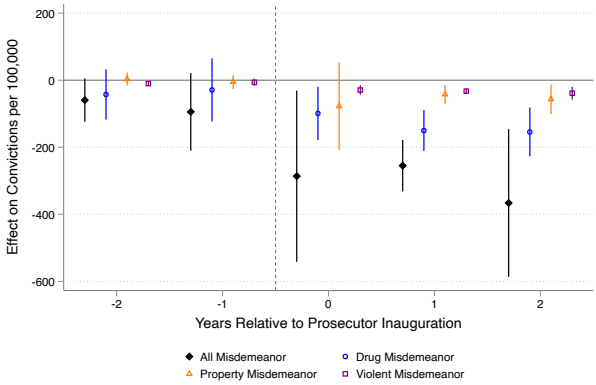
(a) Charges per 100,000 residents



(b) Share of charges convicted



(c) Convictions per 100,000 residents



Notes. Event studies of average treatment on the treated (ATT) effects of prosecutors' inaugurations on misdemeanor case outcomes by year. Panel (a) shows misdemeanor charges per 100,000 residents; panel (b) shows the proportion of misdemeanor charges convicted; and panel (c) shows the misdemeanor conviction rate per 100,000 residents. Time 0 is the inauguration year. Conviction outcomes are among cases initially charged in a given year. Figures generated by the csdid Stata package (Callaway and Sant'Anna, 2021), using the event option and weighting by population.

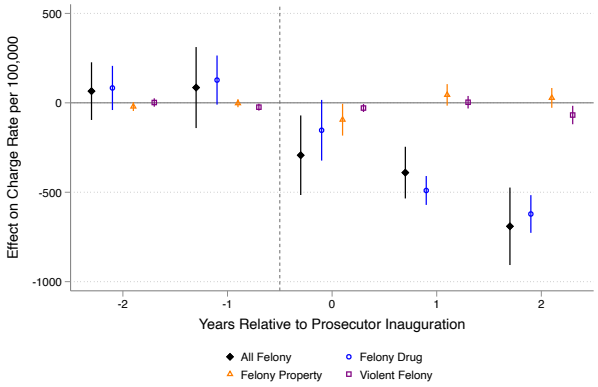
Table 6: ATT effect of reform prosecutors on felony case outcomes

	(1) All	(2) Drug	(3) Property	(4) Violent
Panel A: Charges per 100,000 residents				
ATT	-334.9*	-281.2	-38.69	-16.97
	(142.7)	(251.0)	(41.16)	(19.21)
Pre-Treatment Mean	1538.9	422.3	472.9	326.8
Percentage Effect	-21.76	-66.59	-8.180	-5.193
Jurisdictions	15	15	15	15
Observations	187	187	187	187
Panel B: Share of charges convicted				
ATT	-0.0254*	-0.0438	-0.00790	-0.00670
	(0.00996)	(0.0379)	(0.00940)	(0.0133)
Pre-Treatment Mean	0.482	0.505	0.501	0.411
Percentage Effect	-5.262	-8.681	-1.576	-1.630
Jurisdictions	15	15	15	15
Observations	187	187	187	187
Panel C: Convictions per 100,000 residents				
ATT	-157.4***	-112.1+	-10.18	-24.67**
	(34.72)	(57.45)	(13.66)	(8.326)
Pre-Treatment Mean	630.9	194.7	196.9	99.82
Percentage Effect	-24.95	-57.57	-5.169	-24.71
Jurisdictions	15	15	15	15
Observations	187	187	187	187

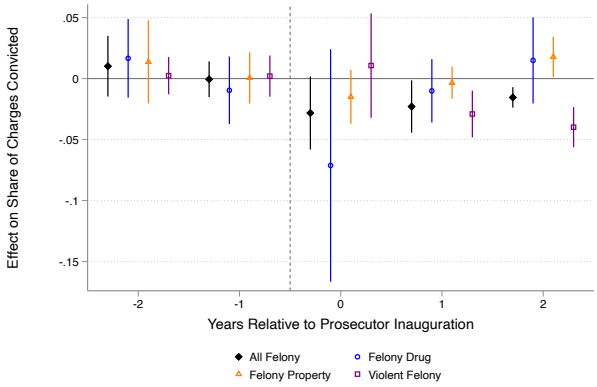
Notes. The dependent variable in Panel A is the felony charging rate per 100,000 residents; in Panel B, it is the share of felony charges resulting in a conviction, measured as of the year the case was initially charged; and in Panel C, it is the felony conviction rate per 100,000 residents. Charging and conviction data are available for only one county per jurisdiction. The sample includes all jurisdictions for which both felony charging and felony conviction data are available through CJARS JOE and for which we were able to obtain data on local reported crime rates. The analysis is at the county-year level, and average treatment on the treated (ATT) effects are estimated using the Callaway and Sant'Anna (2021) estimator. Standard errors, clustered at the county level, are reported in parentheses. +10%, *5%, **1%, and ***0.1% significance levels.

Figure 2: Event-study ATT effects of reform prosecutors on felony case outcomes

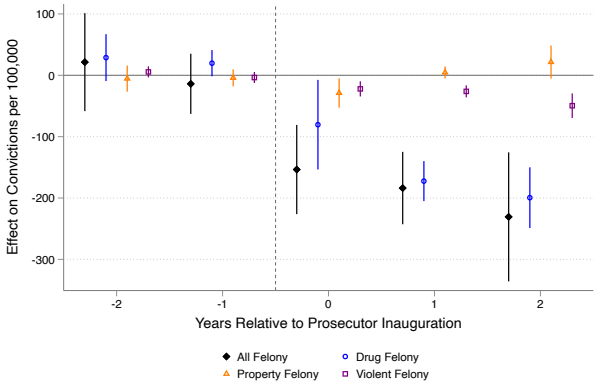
(a) Charges per 100,000 residents



(b) Share of charges convicted



(c) Convictions per 100,000 residents



Notes. Event studies of average treatment on the treated (ATT) effects of prosecutors' inaugurations on felony case outcomes by year. Panel (a) shows felony charges per 100,000 residents; panel (b) shows the proportion of felony charges convicted; and panel (c) shows the felony conviction rate per 100,000 residents. Time 0 is the inauguration year. Conviction outcomes are among cases initially charged in a given year. Figures generated by the csdid Stata package (Callaway and Sant'Anna, 2021), using the event option and weighting by population.

7.3 Effects on Local Crime Rates

Table 7: ATT effects of reform prosecutors on local reported crime rates, by broad offense category

	(1) All Offenses	(2) Drug	(3) Property	(4) Violent	(5) Income-Generating	(6) Non-Income
ATT	-4.304	0.0965	-3.852	-0.548	-3.649	-0.751
	(3.984)	(0.254)	(3.799)	(0.392)	(3.595)	(0.859)
Jurisdictions	29	29	29	29	29	29
Pre-Inauguration Mean	37.08	3.312	23.79	9.976	18.84	14.93
Percentage Effect	-11.61	2.914	-16.19	-5.495	-19.37	-5.032
95% CI (Percentage Effect)	[-29.36, 6.15]	[-12.77, 18.60]	[-41.04, 8.65]	[-13.35, 2.36]	[-46.97, 8.22]	[-14.93, 4.86]
Observations	2249	2249	2249	2249	2249	2249

Notes. The dependent variables in columns 1 to 6 are the overall reported crime rate (homicide, assault, robbery, burglary, theft, vandalism, and drug offenses), followed by the crime rate of drug offenses, violent (homicide, assault and robbery), property (burglary, theft, and vandalism), income-generating (robbery, theft, and burglary), and non-income generating (homicide, assault, and vandalism) offenses, respectively. The sample includes only jurisdictions where all offense types are reported. The analysis is at the jurisdiction-month level, and average treatment on the treated (ATT) effects are estimated using the Callaway and Sant'Anna (2021) estimator. Standard errors, clustered at the jurisdiction level, are reported in parentheses. +10%, *5%, **1%, and ***0.1% significance levels.

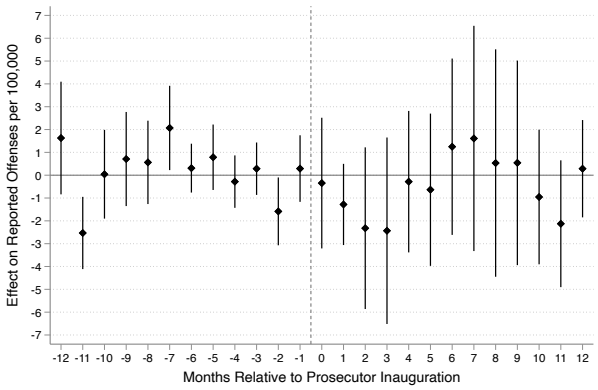
Table 8: ATT effects of reform prosecutors on local reported crime rates, by specific offense type

	(1) Homicide	(2) Assault	(3) Robbery	(4) Burglary	(5) Theft	(6) Vandalism	(7) Drug
ATT	0.00312	0.137	-0.688**	-0.184	-2.777	-0.891	0.0965
	(0.0310)	(0.301)	(0.250)	(0.558)	(2.628)	(0.675)	(0.254)
Jurisdictions	29	29	29	29	29	29	29
Pre-Inauguration Mean	0.101	8.515	1.359	3.458	14.02	6.313	3.312
Percentage Effect	3.084	1.608	-50.63	-5.317	-19.81	-14.12	2.914
95% CI (Percentage Effect)	[-69.07, 75.24]	[-5.32, 8.53]	[-82.41, -18.85]	[-36.92, 26.29]	[-49.03, 9.41]	[-32.32, 4.08]	[-12.77, 18.60]
Observations	2249	2249	2249	2249	2249	2249	2249

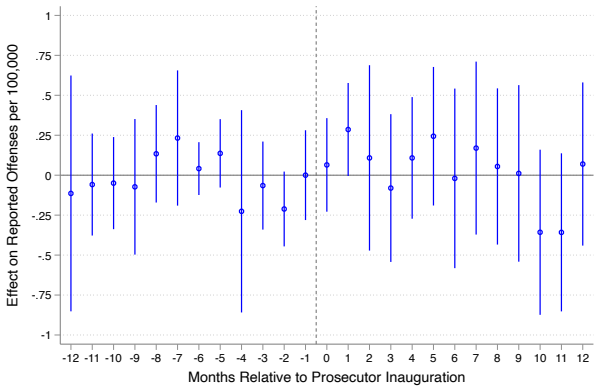
Notes. The dependent variables in columns 1 to 7 are the reported crime rate for homicide, assault, robbery, burglary, theft, vandalism, and drug offenses, respectively. The sample includes only jurisdictions where all offense types are reported. The analysis is at the jurisdiction-month level, and average treatment on the treated (ATT) effects are estimated using the Callaway and Sant'Anna (2021) estimator. Standard errors, clustered at the jurisdiction level, are reported in parentheses. +10%, *5%, **1%, and ***0.1% significance levels.

Figure 3: Event-study ATT effects of reform prosecutors on local reported crime rates, by broad offense category

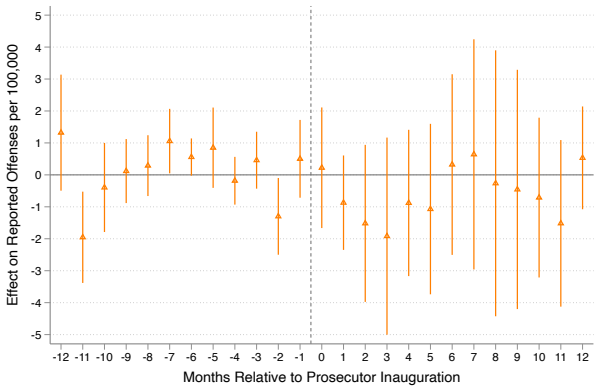
(a) All Offenses



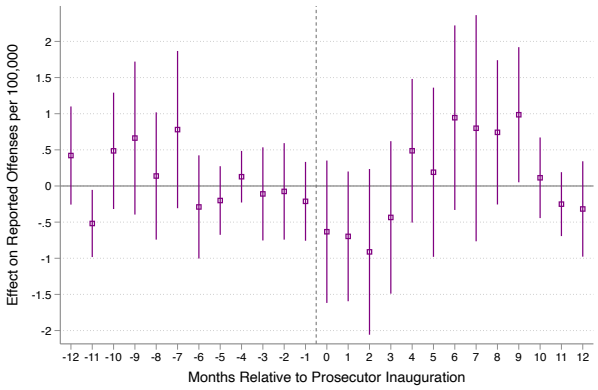
(b) Drug Offenses



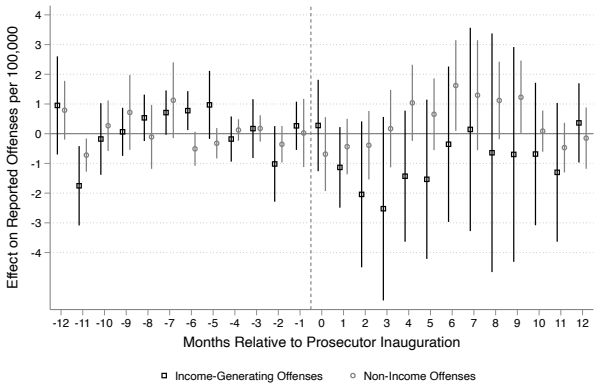
(c) Property Offenses



(d) Violent Offenses



(e) Income-Generating vs. Non-Income Offenses



Notes. Event-study estimates of average treatment on the treated (ATT) effects of prosecutors' inaugurations on local reported crime rates by offense category. Outcomes are monthly reported incidents per 10,000 residents at the jurisdiction level. Time 0 corresponds to the inauguration month. Figures generated by the csdid Stata package (Callaway and Sant'Anna, 2021), using the event option and weighting by population.

7.4 ATT Effects on Drug Mortality Rates

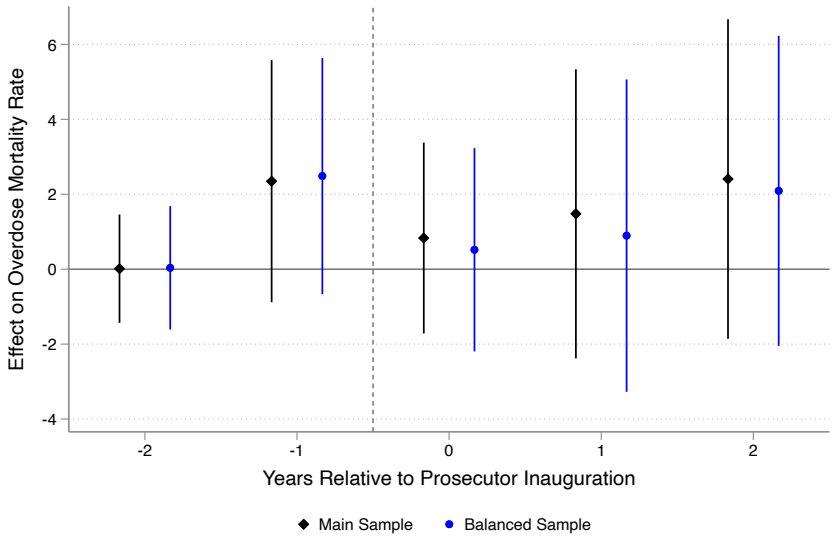
Table 9: ATT effect of reform prosecutors on drug mortality rates

	(1)	(2)
	Main Sample	Balanced Sample
Panel A: Drug overdose mortality rate per 100,000 residents		
ATT	3.223^{+}	2.812
	(1.946)	(1.931)
Jurisdictions	29	25
Pre-Treatment Mean	12.82	13.03
Percentage Effect	25.14	21.59
95% CI(% Effect)	[-2.77, 53.04]	[-6.88, 50.06]
Panel B: Log drug overdose mortality rate		
ATT	0.120	0.0976
	(0.124)	(0.129)
Jurisdictions	29	25
95% CI	[-0.088, 0.329]	[-0.115, 0.310]

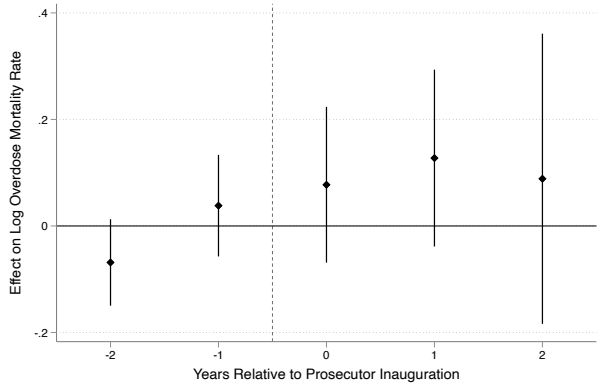
Notes. The dependent variable in Panel A is the drug overdose mortality rate per 100,000 residents; in Panel B, it is the logarithm of the drug overdose mortality rate. Column 1 includes counties with a continuous data series (requiring at least 10 deaths per year for inclusion) spanning at least one year before and one year after the reform prosecutor's inauguration. Column 2 uses a balanced sample including counties with data available in all years of the analysis. The analysis is at the county-year level, and average treatment on the treated (ATT) effects are estimated using the Callaway and Sant'Anna (2021) estimator. Standard errors, clustered at the jurisdiction level, are reported in parentheses. +10%, *5%, **1%, and ***0.1% significance levels.

Figure 4: Event-study ATT effects of reform prosecutors on drug mortality rates

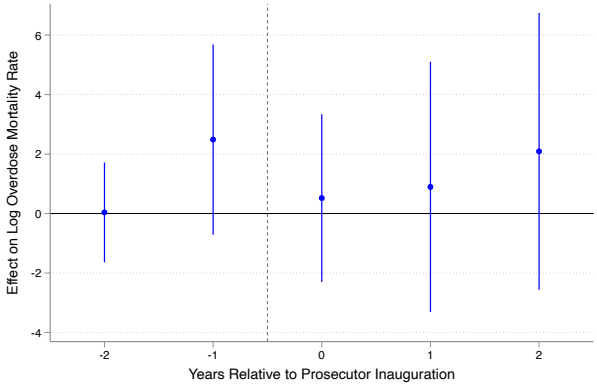
(a) Mortality rate per 100,000 residents



(b) Log mortality rate



(c) Log mortality rate (balanced sample)



Notes. Event studies of average treatment on the treated (ATT) effects of prosecutors' inaugurations on drug mortality rates per 100,000 residents. Time 0 is the inauguration year, and conviction outcomes are among cases initially charged in a given year. Figures generated by the csdid Stata package, using the event option and weighting by population.

Appendix

A Robustness: ATT Effects on Case Outcomes

In this section, we present two additional analyses to support the robustness of our estimated ATT effects of reform prosecutors on average charge and conviction rates in their jurisdictions, described in detail in section 5.1.

In our main analysis of misdemeanor (Table 5) and felony (Table 6) case outcomes, we limit the sample to county-year observations for which both charge and conviction data are available. Table A.1 presents ATT effects on per capita charge rates, expanding the main sample to include county-year observations for which charges per capita are reported in the CJARS JOE data but the share of charges convicted is unavailable.

Recall that, in the main analysis, we include all reform prosecutors regardless of their predecessors' affiliations. Table A.2 compares the main estimates (columns 1 and 3) to samples dropping prosecutors classified by Petersen et al. (2024) as having progressive predecessors (columns 2 and 4). The prosecutors whose jurisdictions are dropped in this specification are Steve Descano (Fairfax County, VA), Mike Schmidt (Multnomah County, OR), and Monique Worrell (Florida 9th Judicial Circuit). Prosecutors Chesa Boudin (San Francisco, CA) and Mimi Rocah (Westchester County, NY) were also classified by Petersen et al. (2024) as having progressive predecessors, but case outcomes for their jurisdictions are unavailable in the CJARS JOE data, so they are already excluded from the main case outcome analyses (columns 1 and 3) due to missing data.

Table A.1: ATT effect of reform prosecutors on charges per 100,000 residents per year, maximum available sample

	(1) All	(2) Drug	(3) Property	(4) Violent
Panel A: Misdemeanor charges per 100,000 residents				
ATT	-737.3*	-242.5**	-141.1+	-31.92
	(341.9)	(75.55)	(72.62)	(27.49)
Pre-Treatment Mean	3153.0	526.4	550.8	426.1
Percentage Effect	-23.38	-46.06	-25.61	-7.491
Jurisdictions	19	19	19	19
Observations	245	245	245	245
Panel B: Felony charges per 100,000 residents				
ATT	-338.6**	-280.6	-40.57	-18.25
	(128.9)	(175.0)	(40.13)	(16.34)
Pre-Treatment Mean	1480.1	405.1	459.9	321.5
Percentage Effect	-22.88	-69.27	-8.822	-5.675
Jurisdictions	16	16	16	16
Observations	210	210	210	210

Notes. The dependent variables in Panels A and B are misdemeanor and felony charging rates per 100,000 residents, respectively. Charging data are available for only one county per jurisdiction. The sample expands the main sample to include counties for which conviction information is not available. The analysis is at the county-year level, and average treatment on the treated (ATT) effects are estimated using the Callaway and Sant'Anna (2021) estimator. Standard errors, clustered at the jurisdiction level, are reported in parentheses. +10%, *5%, **1%, and ***0.1% significance levels.

Table A.2: ATT effect of reform prosecutors on case outcomes, comparison with Petersen et al sample

	(1) Misdemeanor Main	(2) Misdemeanor Drop Pred.	(3) Felony Main	(4) Felony Drop Pred.
Panel A: Charges per 100,000 residents				
ATT	-622.3^{+}	-898.7***	-334.9*	-383.7**
	(367.5)	(221.8)	(142.7)	(131.5)
Pre-Treatment Mean	3197.7	3659.2	1538.9	1451.8
Percentage Effect	-19.46	-24.56	-21.76	-26.43
Jurisdictions	17	14	15	13
Observations	203	160	187	155
Panel B: Share of charges convicted				
ATT	-0.0976***	-0.120***	-0.0254*	-0.0248**
	(0.0271)	(0.0282)	(0.00996)	(0.00834)
Pre-Treatment Mean	0.553	0.594	0.482	0.508
Percentage Effect	-17.66	-20.15	-5.262	-4.883
Jurisdictions	17	14	15	13
Observations	203	160	187	155
Panel C: Convictions per 100,000 residents				
ATT	-333.0*	-448.3***	-157.4***	-176.3***
	(140.8)	(112.3)	(34.72)	(26.43)
Pre-Treatment Mean	1409.9	1615.1	630.9	611.5
Percentage Effect	-23.62	-27.76	-24.95	-28.84
Jurisdictions	17	14	15	13
Observations	203	160	187	155

Notes. The dependent variables in Panel A, Panel B, and Panel C are the misdemeanor charging rate per 100,000 residents, the felony charging rate per 100,000 residents, and the conviction rate per 100,000 residents, respectively. This table restricts the sample to prosecutors that overlap with the Petersen et al. (2024) classification of progressive prosecutors. Charging and conviction data are available for only one county per jurisdiction. The analysis is at the county-year level, and average treatment on the treated (ATT) effects are estimated using the Callaway and Sant'Anna (2021) estimator. Standard errors, clustered at the county level, are reported in parentheses. +10%, *5%, **1%, and ***0.1% significance levels.

B Robustness: ATT Effects on Local Crime Rates

In this section, we provide a variety of supplemental analyses and specification checks to support the estimated null ATT effects of reform prosecutors on local crime rates presented in section 5.2. We show that our main findings are fairly robust to alternate sample construction and estimation strategies.

We first consider the robustness of our estimates for our broad, aggregated offense categories. Our main ATT estimates in Table 7 include only the jurisdictions for which all offense types are consistently represented in the local crime data to ensure comparability across categories. However, because police departments vary in what they include in their data, this approach necessarily results in smaller samples than are actually available for some offense categories. Table B.1 presents results including all police departments for which the offense types in each category are reported. For example, if a jurisdiction consistently reports burglary, theft, and vandalism offenses, but drug offenses are missing, it would be included in the property offenses analysis in column 3 of Table B.1 but not in Table 7 in the main text. We find that point estimates and confidence intervals for this expanded sample are similar to those in the main sample in Table 7.

We also implement the stacked difference-in-differences estimation strategy proposed in Wing et al. (2024). In this procedure, we create a cohort-level dataset for each inauguration timing group consisting of all the jurisdictions treated in a given month m, and all the jurisdictions that remain untreated for six months before month m and six months after month m for which data are available for a given offense category. These datasets are then appended together and weighted such that a classic difference-in-differences equation estimates an event-balanced, population-weighted ATT. We do the same for a longer event window consisting of the twelve months before and after treatment. The shorter window allows us to include more jurisdictions, but the longer window of course allows more time periods, so we show both. The resulting ATT estimates, presented in Table B.2, are similar to our main estimates in Table 7.

Using the same stacked, event-balanced panel, we also implement a synthetic difference-in-differences estimation (Arkhangelsky et al., 2021). This estimation selects from the not-yet-treated donor pool a weighted average of comparison units whose trends are most nearly parallel to the treated group, providing additional supporting evidence that our main ATT estimates are unlikely to be driven by violations of the parallel trends assumption if we are willing to assume that, had the reform prosecutor not been elected, crime trends that were parallel prior to the inauguration would have remained parallel after the inauguration. We implement this estimation using the *sdid* stata package, which is designed for

use with a calendar-balanced panel, but which is not available to us due to inconsistent data coverage. We perform the estimation using the event variable as the time variable, a cohort-by-jurisdiction id variable (since repeated time values of the same id are not allowed), and including cohort dummies and population as covariates. We would like to cluster our standard errors at the jurisdiction level as recommended in Wing et al. (2024) for stacked panels, but the estimation procedure does not allow for clustering at a level other than the id variable. Because of this limitation, we compute jurisdiction-cohort placebo standard errors v_{jc} and scale them up numerically to account for the reduced number of clusters. We report our confidence intervals using these adjusted standard errors $v_j = v_{jc} * \sqrt{\frac{N_c*}{N_c}}$, where N_c* is the number of unique values of the jurisdiction-cohort variable in the comparison group and N_c is the actual number of unique jurisdictions in the comparison group. This is an approximation, and the resulting confidence intervals should be interpreted with caution. However, we perform this exercise to illustrate that the event-balanced synthetic estimates using comparison units with better-matched pre-trends, reported in Table B.3, are similar to those using the unadjusted event-balanced units from the regular stacked difference-in-differences estimator in Table B.2.

For each offense category, we present a figure showing event-study estimates from the two stacked, event-balanced specifications (sub-figure a), corresponding trends for the six-month (b) and 12-month (c) event windows, and synthetically-adjusted trends for the six-month (d) and 12-month (e) event windows. We show this set of graphs for all offenses (Figure B.1), drug offenses (Figure B.2), property offenses (Figure B.3), violent offenses (Figure B.4), income-generating offenses (Figure B.5), and non-income-generating offenses (Figure B.6). These figures allow for more transparent exploration of parallel trends, since the composition of units in the treatment and comparison groups does not change throughout the event window. In each case, the trends in the unadjusted stacked specification (b and c) exhibit more volatility in the treatment group than the comparison group both before and after the inauguration date, and the trends in the synthetic specification (d and e) are more parallel. Despite this difference, both estimators produce similar aggregated ATT estimates in Tables B.2 and B.3.

Lastly, we present estimates using the main Callaway and Sant'Anna (2021) estimation strategy and the main sample inclusion criteria (reporting all offense types) but dropping from the sample the jurisdictions of any prosecutors classified by Petersen et al. (2024) as having progressive predecessors. These prosecutors include Chesa Boudin (San Francisco, CA), Steve Descano (Fairfax County, VA), Mimi Rocah (Westchester County, NY), Mike Schmidt (Multnomah County, OR), and Monique Worrell (FL 9th Judicial Circuit).[9] Results

[9] Petersen et al. (2024) classify one other prosecutor in our sample, Sherry Boston (DeKalb County, GA)

of this estimation, presented in Table B.4, suggest a modest but statistically significant increase in drug offenses after a reform prosecutor takes office, resulting in approximately 0.35 additional drug offenses per 10,000 residents per month (+10 percent of the baseline mean, $p < 0.05$). As we note in the main text, the confidence interval on this estimate is similar to the other specifications, and the estimated ATT effect on drug offenses is statistically significant only in this specification. We therefore hesitate to draw strong conclusions about whether reform prosecutors had an effect on drug offense rates. Our estimates for other offense types are similar to the main results, but the confidence intervals are a bit wider, as expected due to the smaller sample size.

We next consider the robustness of estimates broken out by specific offense type. In figure B.7 we show event studies for the individual offenses for which aggregated ATT effects are reported in Table 8.[10] As in the event studies reported for broad, aggregated crime categories in Figure 3, we do not see evidence of a clear upward or downward trend, either before or after a reform prosecutor's inauguration.

As we did for the broader offense categories, we present estimated ATT effects on specific offense types using the maximum available sample for each offense type (Table B.5). As before, we also present event-balanced stacked difference-in-differences estimates (Table B.6) and synthetic difference-in-differences estimates (Table B.7). Lastly, for each individual offense type, we present a figure showing stacked event-studies (sub-figure a), event-balanced trends (b and c), and trends adjusted for better pre-treatment fit using the synthetic difference-in-differences estimator (d and e). These trends and event studies are presented in Figures B.8 (homicide), B.9 (assault), B.10 (robbery), B.11 (burglary), B.12 (theft), B.13 (vandalism), and B.2 (drugs). As in the robustness analysis above for broad offense categories, results for specific offense types are largely similar to the main results across the various specification checks.

as traditional rather than progressive. However, her jurisdiction's data is missing some offense types and is therefore only used in the expanded samples in Tables B.1 and B.5. Because the resulting estimates are similar to the main estimates in Tables 7 and 8, we assume our main findings are not sensitive to her classification.

[10] With the exception of drug offenses, for which the event study is included in Figure 3 in the main text because they are reported both as a broad crime category in Table 7 and as a specific offense type in Table 8.

Table B.1: ATT effects of reform prosecutors on local reported crime rates, maximum available sample by broad offense category

	(1) All Offenses	(2) Drug	(3) Property	(4) Violent	(5) Income-Generating	(6) Non-Income
ATT	-4.304	0.0889	-3.751	-0.139	-2.937	-0.735
	(3.371)	(0.240)	(3.684)	(0.532)	(2.649)	(0.779)
Jurisdictions	29	32	31	36	36	31
Pre-Inauguration Mean	37.08	3.289	23.46	9.769	18.75	14.55
Percentage Effect	-11.61	2.702	-15.99	-1.419	-15.66	-5.049
95% CI (Percentage Effect)	[-29.43, 6.21]	[-11.34, 16.74]	[-41.55, 9.58]	[-12.26, 9.43]	[-37.99, 6.67]	[-15.37, 5.27]
Observations	2249	2375	2344	2555	2579	2332

Notes. The dependent variables in columns 1 to 6 are the overall reported crime rate and rates by broad offense category (drug, property, violent, income-generating, and non-income), using the maximum available sample by offense category. The analysis is at the jurisdiction-month level, and average treatment on the treated (ATT) effects are estimated using the Callaway and Sant'Anna (2021) estimator. Standard errors, clustered at the jurisdiction level, are reported in parentheses. +10%, *5%, **1%, and ***0.1% significance levels.

Table B.2: ATT effects of reform prosecutors on local reported crime rates by broad crime category, event-balanced samples

	(1) All Offenses	(2) Drug	(3) Property	(4) Violent	(5) Income-Generating	(6) Non-Income
Panel A: Offenses per 10,000 residents per month, Event-Balanced [-6,6]						
ATT	-1.833	0.0686	-2.331	-0.317	-1.653	-0.431
	(1.468)	(0.258)	(1.390)	(0.346)	(1.277)	(0.443)
Jurisdictions	24	28	27	31	32	26
Pre-Inauguration Mean	35.58	3.171	24.38	8.453	19.56	13.02
Percentage Effect	-5.152	2.162	-9.562	-3.754	-8.450	-3.313
95% CI (%Effect)	[-13.68, 3.38]	[-14.52, 18.84]	[-21.28, 2.16]	[-12.10, 4.60]	[-21.77, 4.87]	[-10.33, 3.70]
Observations	897	988	975	1144	1170	949
Panel B: Offenses per 10,000 residents per month, Event-Balanced [-12,12]						
ATT	-0.522	-0.0331	-1.849	-0.273	-1.392	-0.663
	(1.474)	(0.306)	(1.927)	(0.409)	(1.791)	(0.603)
Jurisdictions	20	24	23	25	27	21
Pre-Inauguration Mean	35.42	3.453	23.78	8.307	19.36	12.82
Percentage Effect	-1.475	-0.959	-7.778	-3.283	-7.187	-5.170
95% CI (%Effect)	[-10.18, 7.23]	[-19.32, 17.40]	[-24.58, 9.03]	[-13.45, 6.89]	[-26.20, 11.83]	[-14.98, 4.64]
Observations	1475	1600	1575	1800	1850	1525

Notes. The dependent variables in columns 1 to 6 are the overall reported crime rate and rates by broad offense category (drug, property, violent, income-generating, and non-income), using event-balanced samples by offense category. Panel A covers the six months before and after a prosecutor's inauguration; Panel B covers 12 months. The analysis is at the jurisdiction-month level, and average treatment on the treated (ATT) are estimated using the stacked difference-in-differences estimator in Wing et al. (2024). Standard errors, clustered at the jurisdiction level, are reported in parentheses. +10%, *5%, **1%, and ***0.1% significance levels.

Table B.3: ATT effects of reform prosecutors on local reported crime rates, by broad crime category, event-balanced synthetic difference-in-differences

	(1) All Offenses	(2) Drug	(3) Property	(4) Violent	(5) Income-Generating	(6) Non-Income
Panel A: Offenses per 10,000 residents per month, Event-Balanced [-6,6], Synthetic DiD						
ATT	-1.616	0.236	-1.606	-0.280	-0.819	-0.0917
Unadj. Placebo SE	*(0.799)*	*(0.270)*	*(0.741)*	*(0.130)*	*(0.630)*	*(0.211)*
Adj. SE	(1.502)	(0.499)	(1.331)	(0.233)	(1.116)	(0.383)
Pre-Inauguration Mean	36.46	3.190	24.92	8.513	20.05	13.22
Percentage Effect	-4.431	7.388	-6.444	-3.289	-4.083	-0.694
Adj. 95% CI (%Effect)	[-12.51, 3.64]	[-23.26, 38.04]	[-16.91, 4.02]	[-8.65, 2.07]	[-14.99, 6.83]	[-6.38, 4.99]
Observations	897	988	975	1144	1170	949
Panel B: Offenses per 10,000 residents per month, Event-Balanced [-12,12], Synthetic DiD						
ATT	-2.098	-0.239	-1.629	-0.295	-1.020	-0.0949
Unadj. Placebo SE	*(0.984)*	*(0.301)*	*(0.930)*	*(0.148)*	*(0.716)*	(0.301)
Adj. SE	(1.783)	(0.516)	(1.579)	(0.259)	(1.209)	(0.532)
Pre-Inauguration Mean	36.27	3.439	24.50	8.432	19.45	13.25
Percentage Effect	-5.784	-6.959	-6.650	-3.500	-5.243	-0.716
Adj. 95% CI (%Effect)	[-15.42, 3.85]	[-36.35, 22.43]	[-19.28, 5.98]	[-9.52, 2.52]	[-17.43, 6.94]	[-8.59, 7.16]
Observations	1475	1600	1575	1800	1850	1525

Notes. The dependent variables in columns 1 to 6 are the overall reported crime rate and rates by broad offense category (drug, property, violent, income-generating, and non-income), using event-balanced samples by offense category. Panel A covers the six months before and after a prosecutor's inauguration; Panel B covers 12 months. The analysis is at the jurisdiction-cohort-event month level, and average treatment on the treated (ATT) effects are estimated using the synthetic difference-in-differences estimator in Arkhangelsky et al. (2021) on a stacked, event-balanced panel using the sdid stata package. We scale up the *sdid* placebo standard errors by $\sqrt{\frac{N_c*}{N_c}}$, where N_c* is the number of unique jurisdiction-cohort observations in the comparison donor pool and N_c is the number of unique jurisdictions in the comparison donor pool in accordance with the actual number of independent clusters used to form the synthetic comparison group. However, this correction is a numerical approximation, and the resulting confidence intervals should be interpreted with caution.

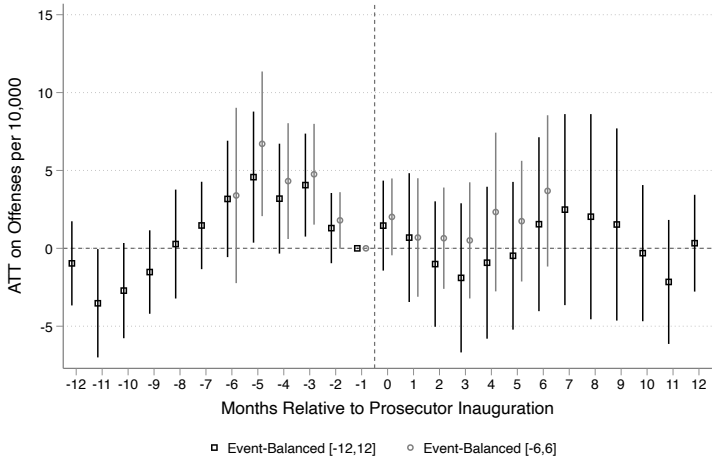
Table B.4: ATT effects of reform prosecutors on local reported crime rates by broad offense category, comparison with Petersen et al. (2024) sample

	(1) All Offenses	(2) Drug	(3) Property	(4) Violent	(5) Income-Generating	(6) Non-Income
ATT	-2.528	0.353*	-2.458	-0.423	-2.313	-0.569
	(4.125)	(0.176)	(4.046)	(0.521)	(2.889)	(1.208)
Jurisdictions	24	24	24	24	24	24
Pre-Inauguration Mean	40.26	3.466	25.27	11.52	20.41	16.38
Percentage Effect	-6.279	10.19	-9.728	-3.669	-11.33	-3.471
95% CI (Percentage Effect)	[-24.58, 12.02]	[0.57, 19.81]	[-36.48, 17.02]	[-12.45, 5.12]	[-37.80, 15.14]	[-15.99, 9.05]
Observations	1916	1916	1916	1916	1916	1916

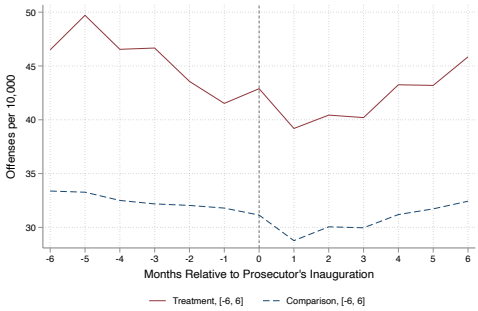
Notes. The dependent variables in columns 1 to 6 are the overall reported crime rate and rates by broad offense category (drug, property, violent, income-generating, and non-income). This sample is restricted to prosecutors that overlap with the Petersen et al. (2024) classification. The analysis is at the jurisdiction-month level, and average treatment on the treated (ATT) effects are estimated using the Callaway and Sant'Anna (2021) estimator. Standard errors, clustered at the jurisdiction level, are reported in parentheses. +10%, *5%, **1%, and ***0.1% significance levels.

Figure B.1: Trends and event studies for event-balanced samples, all offense rate per 10,000 residents per month

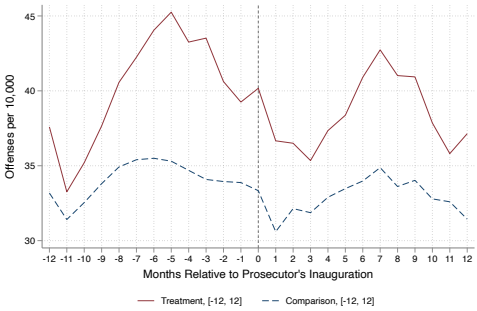
(a) OLS stacked event-studies



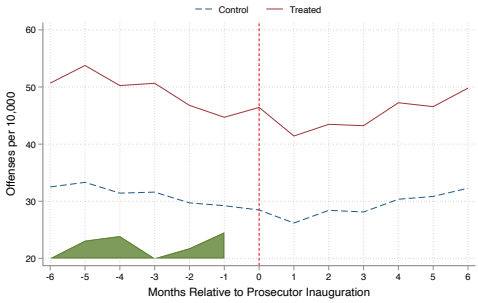
(b) 6-month trends, OLS stacked



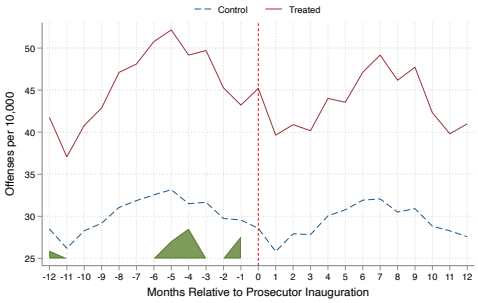
(c) 12-month trends, OLS stacked



(d) 6-month trends, synthetic DiD



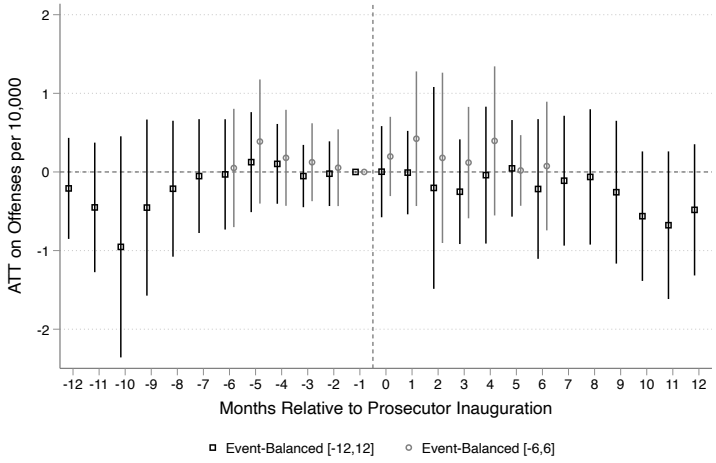
(e) 12-month trends, synthetic DiD



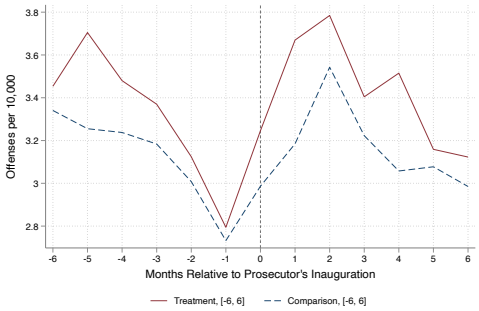
Notes. Event studies of the average treatment on the treated (ATT) effect of prosecutors' inauguration on offense rates per 10,000 residents per month and corresponding outcome trends using event-balanced samples. Time 0 corresponds to the inauguration month. Panel (a) shows OLS stacked event studies for the [6, 6] and [12, 12] event-time windows. Panels (b) and (c) show OLS stacked outcome trends for the [6, 6] and [12, 12] event-balanced samples, respectively. Panels (d) and (e) show synthetic DiD outcome trends for the same event-time windows. Panels (a)–(c) are estimated using the stacked difference-in-differences estimator in Wing et al. (2024), while panels (d)–(e) are estimated using the synthetic difference-in-differences estimator in Arkhangelsky et al. (2021).

Figure B.2: Trends and event studies for event-balanced samples, drug offense rate per 10,000 residents per month

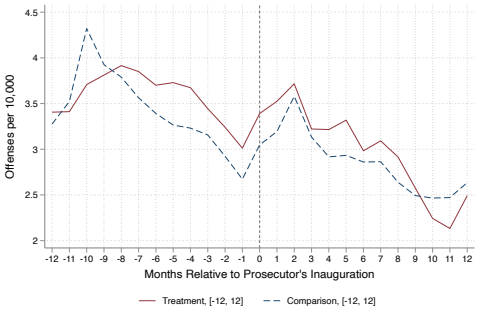
(a) OLS stacked event-studies



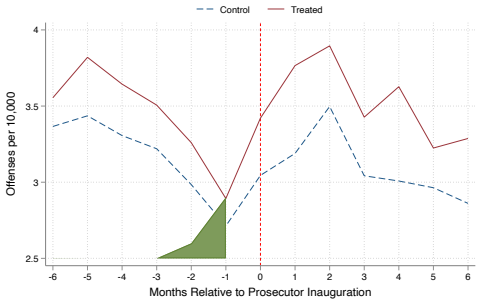
(b) 6-month trends, OLS stacked



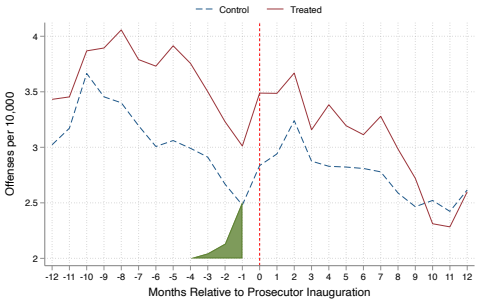
(c) 12-month trends, OLS stacked



(d) 6-month trends, synthetic DiD



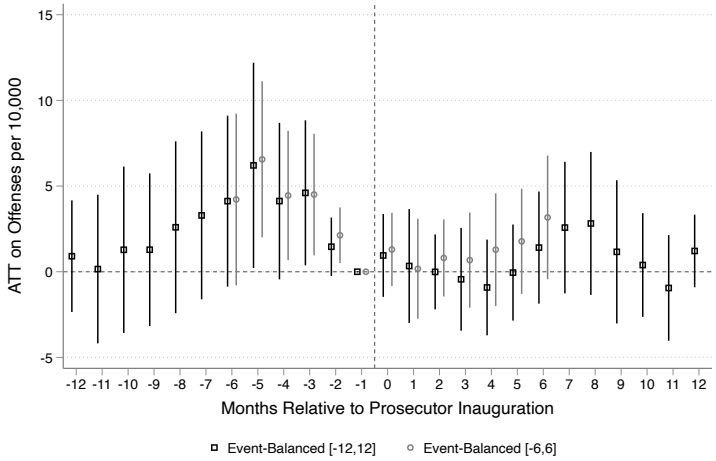
(e) 12-month trends, synthetic DiD



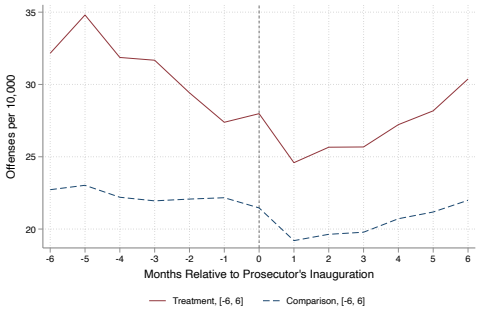
Notes. Event studies of the average treatment on the treated (ATT) effect of prosecutors' inauguration on offense rates per 10,000 residents per month and corresponding outcome trends using event-balanced samples. Time 0 corresponds to the inauguration month. Panel (a) shows OLS stacked event studies for the [6, 6] and [12, 12] event-time windows. Panels (b) and (c) show OLS stacked outcome trends for the [6, 6] and [12, 12] event-balanced samples, respectively. Panels (d) and (e) show synthetic DiD outcome trends for the same event-time windows. Panels (a)–(c) are estimated using the stacked difference-in-differences estimator in Wing et al. (2024), while panels (d)–(e) are estimated using the synthetic difference-in-differences estimator in Arkhangelsky et al. (2021).

Figure B.3: Trends and event studies for event-balanced samples, property offense rate per 10,000 residents per month

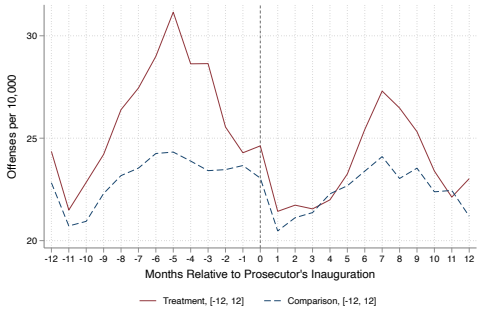
(a) OLS stacked event-studies



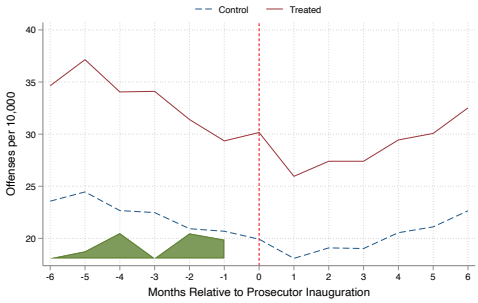
(b) 6-month trends, OLS stacked



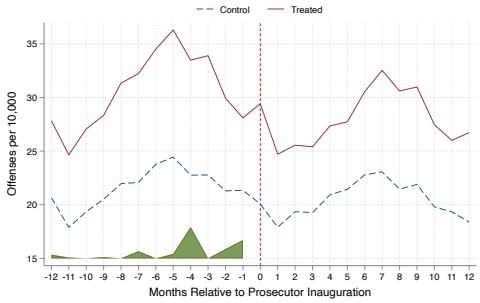
(c) 12-month trends, OLS stacked



(d) 6-month trends, synthetic DiD



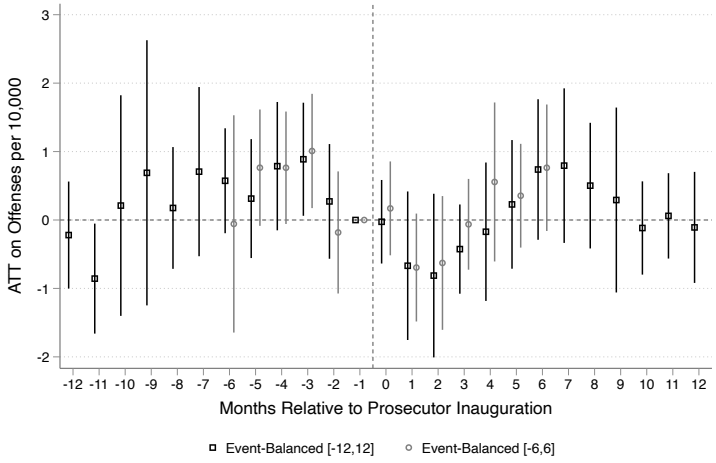
(e) 12-month trends, synthetic DiD



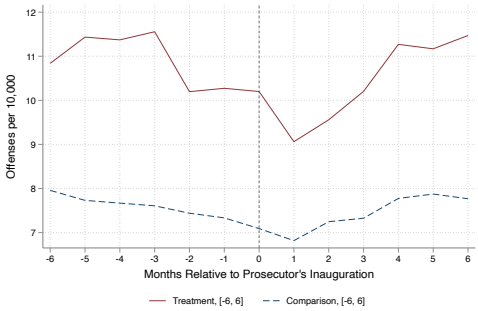
Notes. Event studies of the average treatment on the treated (ATT) effect of prosecutors' inauguration on offense rates per 10,000 residents per month and corresponding outcome trends using event-balanced samples. Time 0 corresponds to the inauguration month. Panel (a) shows OLS stacked event studies for the [6, 6] and [12, 12] event-time windows. Panels (b) and (c) show OLS stacked outcome trends for the [6, 6] and [12, 12] event-balanced samples, respectively. Panels (d) and (e) show synthetic DiD outcome trends for the same event-time windows. Panels (a)–(c) are estimated using the stacked difference-in-differences estimator in Wing et al. (2024), while panels (d)–(e) are estimated using the synthetic difference-in-differences estimator in Arkhangelsky et al. (2021).

Figure B.4: Trends and event studies for event-balanced samples, violent offense rate per 10,000 residents per month

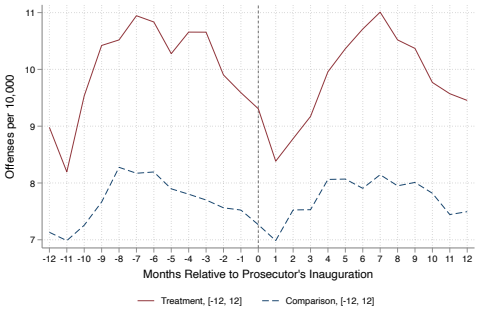
(a) OLS stacked event-studies



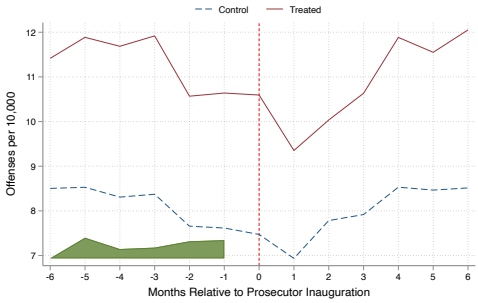
(b) 6-month trends, OLS stacked



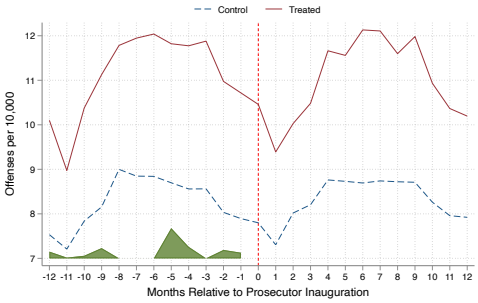
(c) 12-month trends, OLS stacked



(d) 6-month trends, synthetic DiD



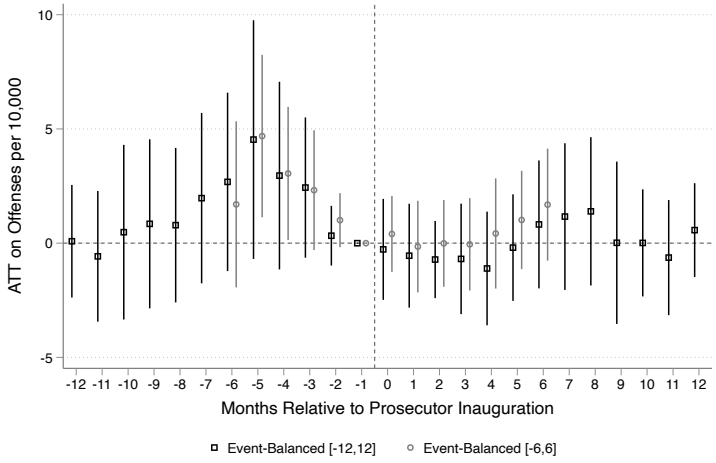
(e) 12-month trends, synthetic DiD



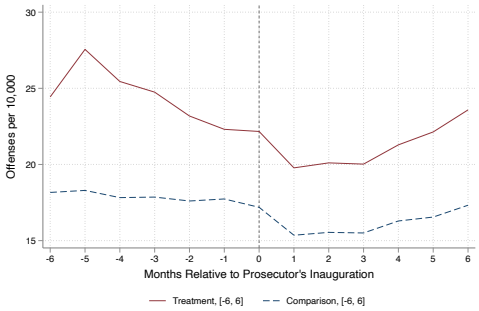
Notes. Event studies of the average treatment on the treated (ATT) effect of prosecutors' inauguration on offense rates per 10,000 residents per month and corresponding outcome trends using event-balanced samples. Time 0 corresponds to the inauguration month. Panel (a) shows OLS stacked event studies for the [6, 6] and [12, 12] event-time windows. Panels (b) and (c) show OLS stacked outcome trends for the [6, 6] and [12, 12] event-balanced samples, respectively. Panels (d) and (e) show synthetic DiD outcome trends for the same event-time windows. Panels (a)–(c) are estimated using the stacked difference-in-differences estimator in Wing et al. (2024), while panels (d)–(e) are estimated using the synthetic difference-in-differences estimator in Arkhangelsky et al. (2021).

Figure B.5: Trends and event studies for event-balanced samples, income-generating offense rate per 10,000 residents per month

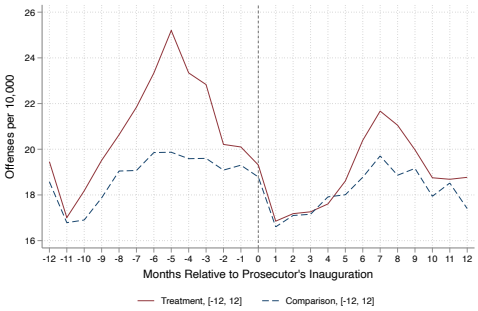
(a) OLS stacked event-studies



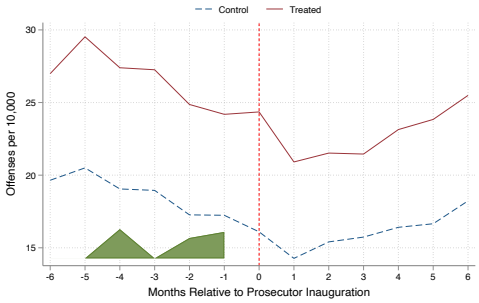
(b) 6-month trends, OLS stacked



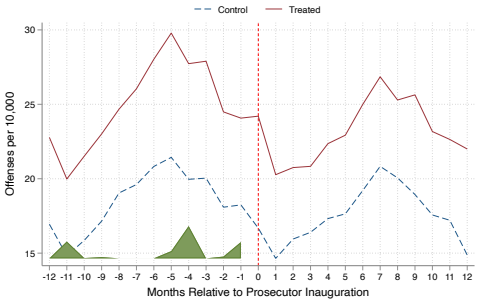
(c) 12-month trends, OLS stacked



(d) 6-month trends, synthetic DiD



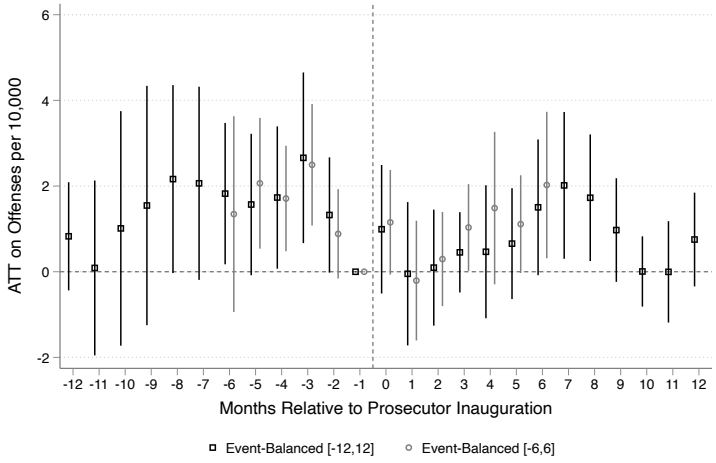
(e) 12-month trends, synthetic DiD



Notes. Event studies of the average treatment on the treated (ATT) effect of prosecutors' inauguration on offense rates per 10,000 residents per month and corresponding outcome trends using event-balanced samples. Time 0 corresponds to the inauguration month. Panel (a) shows OLS stacked event studies for the [6, 6] and [12, 12] event-time windows. Panels (b) and (c) show OLS stacked outcome trends for the [6, 6] and [12, 12] event-balanced samples, respectively. Panels (d) and (e) show synthetic DiD outcome trends for the same event-time windows. Panels (a)–(c) are estimated using the stacked difference-in-differences estimator in Wing et al. (2024), while panels (d)–(e) are estimated using the synthetic difference-in-differences estimator in Arkhangelsky et al. (2021).

Figure B.6: Trends and event studies for event-balanced samples, non-income-generating offense rate per 10,000 residents per month

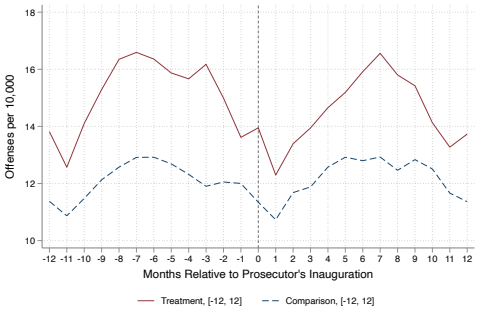
(a) OLS stacked event-studies



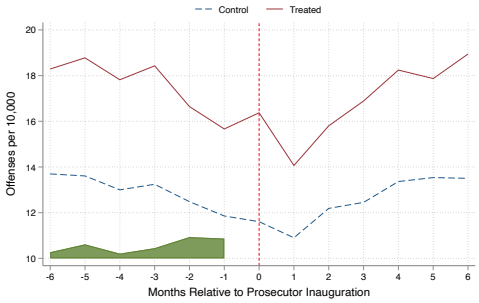
(b) 6-month trends, OLS stacked



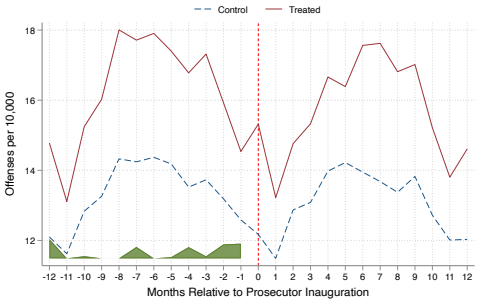
(c) 12-month trends, OLS stacked



(d) 6-month trends, synthetic DiD



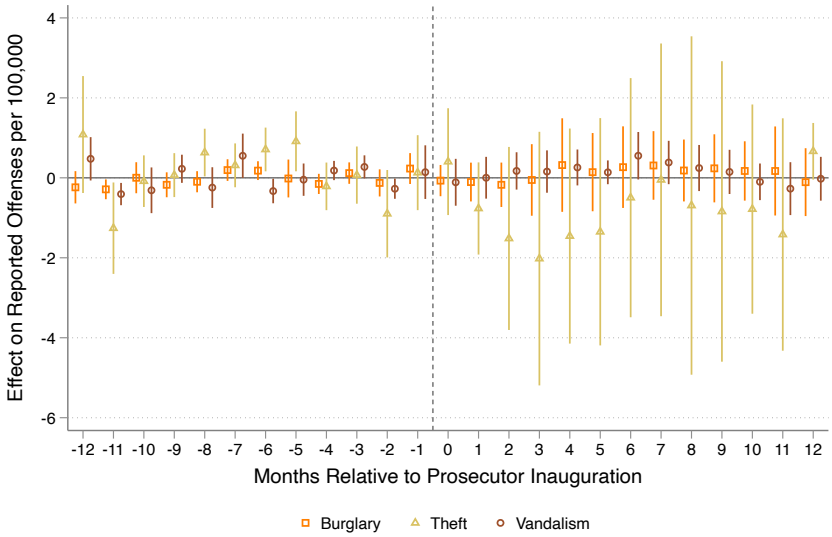
(e) 12-month trends, synthetic DiD



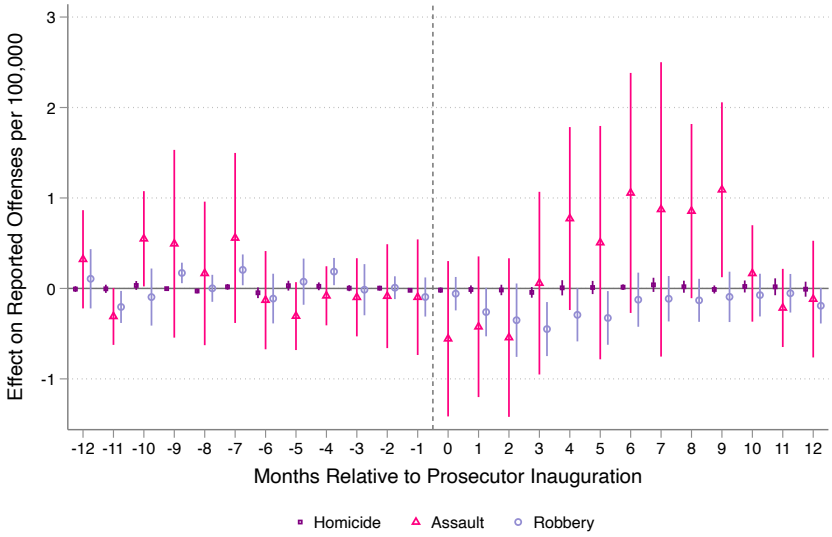
Notes. Event studies of the average treatment on the treated (ATT) effect of prosecutors' inauguration on offense rates per 10,000 residents per month and corresponding outcome trends using event-balanced samples. Time 0 corresponds to the inauguration month. Panel (a) shows OLS stacked event studies for the [6, 6] and [12, 12] event-time windows. Panels (b) and (c) show OLS stacked outcome trends for the [6, 6] and [12, 12] event-balanced samples, respectively. Panels (d) and (e) show synthetic DiD outcome trends for the same event-time windows. Panels (a)–(c) are estimated using the stacked difference-in-differences estimator in Wing et al. (2024), while panels (d)–(e) are estimated using the synthetic difference-in-differences estimator in Arkhangelsky et al. (2021).

Figure B.7: Event-study ATT effects of reform prosecutors on local property and violent crime rates, by specific offense type

(a) Property Offenses



(b) Violent Offenses



Notes. Event-study average treatment on the treated (ATT) effects of reform prosecutors on local property and violent crime rates, by specific offense type. Panel (a) shows property offenses (burglary, theft, vandalism) and panel (b) shows violent offenses (homicide, assault, robbery). Time 0 is the inauguration month. Rates are reported incidents per 100,000 residents per month. Figure generated by the csdid Stata package (Callaway and Sant'Anna, 2021), using the event option and weighting by population.

Table B.5: ATT effects of reform prosecutors on local reported crime rates, maximum available sample by specific offense type

	(1) Homicide	(2) Assault	(3) Robbery	(4) Burglary	(5) Theft	(6) Vandalism	(7) Drug
ATT	0.0131	0.367	-0.496^{+}	-0.234	-2.163	-0.870	0.0889
	(0.0338)	(0.385)	(0.273)	(0.500)	(2.384)	(0.658)	(0.240)
Jurisdictions	38	39	37	38	39	31	32
Pre-Inauguration Mean	0.0983	8.011	1.360	3.468	14.03	6.164	3.289
Percentage Effect	13.36	4.577	-36.45	-6.741	-15.41	-14.11	2.702
95% CI (Percentage Effect)	[-50.70, 77.41]	[-3.85, 13.00]	[-74.48, 1.58]	[-34.49, 21.01]	[-39.59, 8.76]	[-31.91, 3.69]	[-11.34, 16.74]
Observations	2650	2666	2594	2662	2677	2344	2375

Notes. The dependent variables in columns 1 to 7 are the reported crime rate for homicide, assault, robbery, burglary, theft, vandalism, and drug offenses, using the maximum available sample by specific offense type. The analysis is at the jurisdiction-month level, and average treatment on the treated (ATT) effects are estimated using the Callaway and Sant'Anna (2021) estimator. Standard errors, clustered at the jurisdiction level, are reported in parentheses. +10%, *5%, **1%, and ***0.1% significance levels.

Table B.6: ATT effects of reform prosecutors on local reported crime rates by specific offense type, event-balanced samples

	(1) Homicide	(2) Assault	(3) Robbery	(4) Burglary	(5) Theft	(6) Vandalism	(7) Drug
Panel A: Offenses per 10,000 residents per month, Event-Balanced [-6,6]							
ATT	0.00190	-0.184	-0.127	-0.441	-1.535	-0.148	0.0686
	(0.0123)	(0.259)	(0.0999)	(0.460)	(0.987)	(0.219)	(0.258)
Jurisdictions	33	34	32	34	34	27	28
Pre-Inauguration Mean	0.0818	6.819	1.254	4.570	14.20	5.603	3.171
Percentage Effect	2.325	-2.704	-10.10	-9.651	-10.81	-2.644	2.162
95% CI (%Effect)	[-28.29, 32.94]	[-10.43, 5.03]	[-26.35, 6.15]	[-30.14, 10.84]	[-24.95, 3.32]	[-10.69, 5.40]	[-14.52, 18.84]
Observations	1352	1391	1170	1391	1391	975	988
Panel B: Offenses per 10,000 residents per month, Event-Balanced [-12,12]							
ATT	0.0108	-0.261	0.0526	-0.189	-1.405	-0.311	-0.0331
	(0.0129)	(0.289)	(0.0989)	(0.655)	(1.131)	(0.328)	(0.306)
Jurisdictions	27	29	27	29	29	23	24
Pre-Inauguration Mean	0.0747	6.768	1.152	4.595	14.27	5.367	3.453
Percentage Effect	14.41	-3.853	4.569	-4.112	-9.848	-5.787	-0.959
95% CI (%Effect)	[-21.11, 49.94]	[-12.61, 4.90]	[-13.08, 22.21]	[-33.33, 25.10]	[-26.08, 6.39]	[-18.46, 6.89]	[-19.32, 17.40]
Observations	2025	2075	1850	2075	2075	1575	1600

Notes. The dependent variables in columns 1 to 7 are the reported crime rate for homicide, assault, robbery, burglary, theft, vandalism, and drug offenses, using event-balanced samples by specific offense type. Panel A covers the six months before and after a prosecutor's inauguration; Panel B covers 12 months. The analysis is at the jurisdiction-month level, and average treatment on the treated (ATT) effects are estimated using the stacked difference-in-differences estimator in Wing et al. (2024). Standard errors, clustered at the jurisdiction level, are reported in parentheses. +10%, *5%, **1%, and ***0.1% significance levels.

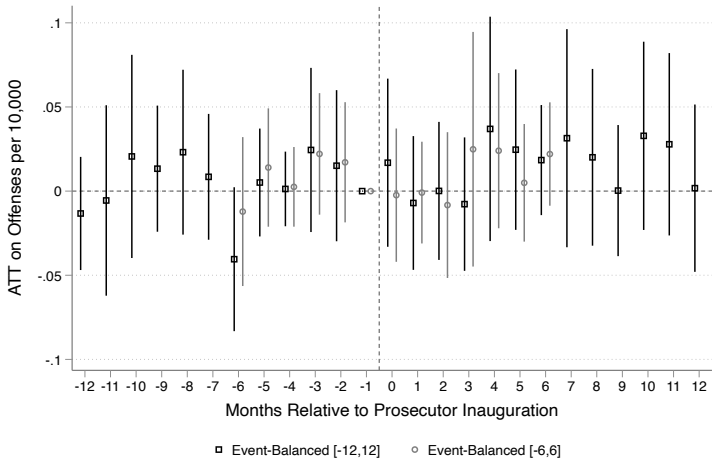
Table B.7: ATT effects of reform prosecutors on local reported crime rates by specific offense type, event-balanced synthetic difference-in-differences

	(1) Homicide	(2) Assault	(3) Robbery	(4) Burglary	(5) Theft	(6) Vandalism	(7) Drug
Panel A: Offenses per 10,000 residents per month, Event-Balanced [-6,6], Synthetic DiD							
ATT	-0.000942	-0.117	-0.112	-0.190	-0.505	0.128	0.236
Unadj. Placebo SE	*(0.00663)*	*(0.106)*	*(0.0541)*	*(0.226)*	*(0.430)*	*(0.184)*	*(0.270)*
Adj. SE	(0.012)	(0.194)	(0.096)	(0.414)	(0.788)	(0.330)	(0.499)
Pre-Inauguration Mean	0.0804	6.727	1.252	4.569	14.52	5.688	3.190
Percentage Effect	-1.172	-1.745	-8.951	-4.164	-3.478	2.247	7.388
Adj. 95% CI (%Effect)	[-30.87, 28.52]	[-7.40, 3.91]	[-23.95, 6.05]	[-21.91, 13.59]	[-14.12, 7.17]	[-9.12, 13.62]	[-23.26, 38.04]
Observations	1352	1391	1170	1391	1391	975	988
Panel B: Offenses per 10,000 residents per month, Event-Balanced [-12,12], Synthetic DiD							
ATT	0.0115	-0.194	-0.0331	-0.153	-0.997	0.166	-0.239
Unadj. Placebo SE	*(0.00502)*	*(0.124)*	*(0.0661)*	*(0.220)*	*(0.477)*	*(0.243)*	*(0.301)*
Adj. SE	(0.009)	(0.217)	(0.112)	(0.383)	(0.832)	(0.412)	(0.516)
Pre-Inauguration Mean	0.0760	6.893	1.162	4.440	14.06	5.583	3.439
Percentage Effect	15.07	-2.813	-2.844	-3.449	-7.090	2.976	-6.959
Adj. 95% CI (%Effect)	[-8.32, 38.45]	[-8.99, 3.36]	[-21.66, 15.97]	[-20.37, 13.48]	[-18.69, 4.51]	[-11.49, 17.44]	[-36.35, 22.43]
Observations	2025	2075	1850	2075	2075	1575	1600

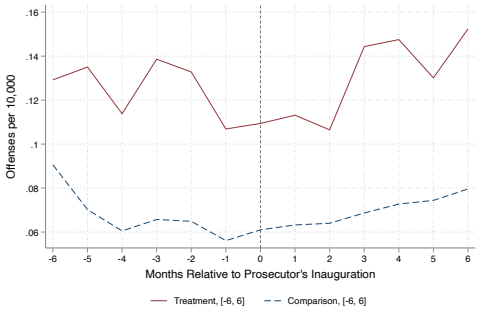
Notes. The dependent variables in columns 1 to 7 are the reported crime rate for homicide, assault, robbery, burglary, theft, vandalism, and drug offenses, using event-balanced samples by specific offense type. Panel A covers the six months before and after a prosecutor's inauguration; Panel B covers 12 months. The analysis is at the jurisdiction-cohort-event month level, and average treatment on the treated (ATT) effects are estimated using the synthetic difference-in-differences estimator in Arkhangelsky et al. (2021) on a stacked, event-balanced panel using the *sdid* stata package. We scale up the *sdid* placebo standard errors by $\sqrt{\frac{N_c*}{N_c}}$, where N_c* is the number of unique jurisdiction-cohort observations in the comparison donor pool and N_c is the number of unique jurisdictions in the comparison donor pool in accordance with the actual number of independent clusters used to form the synthetic comparison group. However, this correction is a numerical approximation, and the resulting confidence intervals should be interpreted with caution.

Figure B.8: Trends and event studies for event-balanced samples, homicide rate per 10,000 residents per month

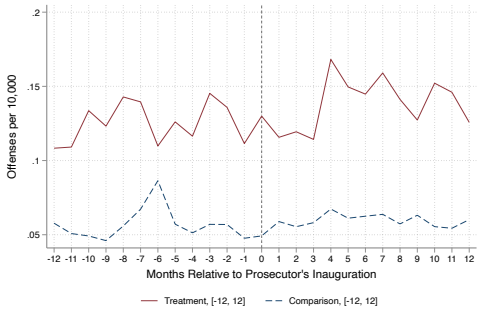
(a) OLS stacked event-studies



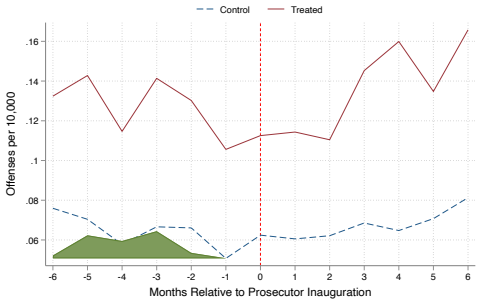
(b) 6-month trends, OLS stacked



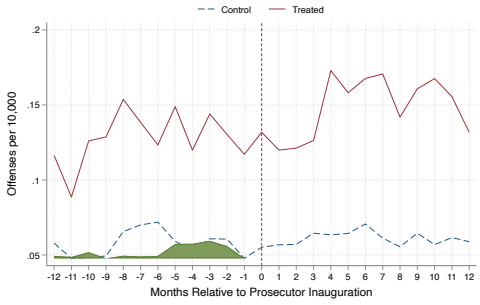
(c) 12-month trends, OLS stacked



(d) 6-month trends, synthetic DiD



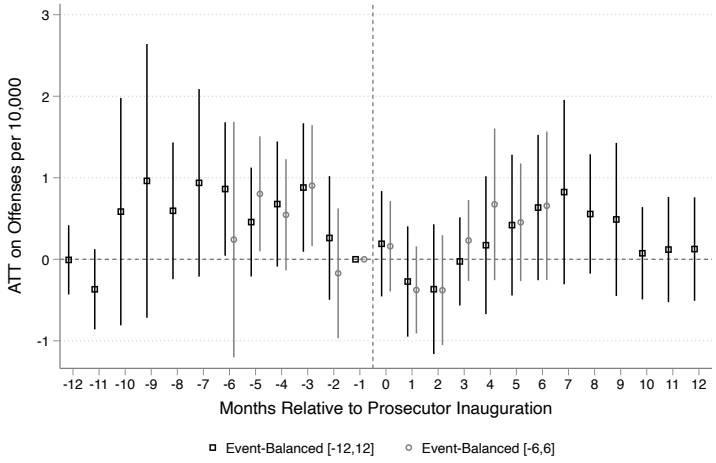
(e) 12-month trends, synthetic DiD



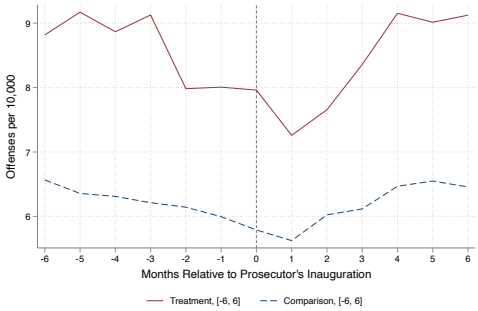
Notes. Event studies of the average treatment on the treated (ATT) effect of prosecutors' inauguration on offense rates per 10,000 residents per month and corresponding outcome trends using event-balanced samples. Time 0 corresponds to the inauguration month. Panel (a) shows OLS stacked event studies for the [6, 6] and [12, 12] event-time windows. Panels (b) and (c) show OLS stacked outcome trends for the [6, 6] and [12, 12] event-balanced samples, respectively. Panels (d) and (e) show synthetic DiD outcome trends for the same event-time windows. Panels (a)–(c) are estimated using the stacked difference-in-differences estimator in Wing et al. (2024), while panels (d)–(e) are estimated using the synthetic difference-in-differences estimator in Arkhangelsky et al. (2021).

Figure B.9: Trends and event studies for event-balanced samples, assault rate per 10,000 residents per month

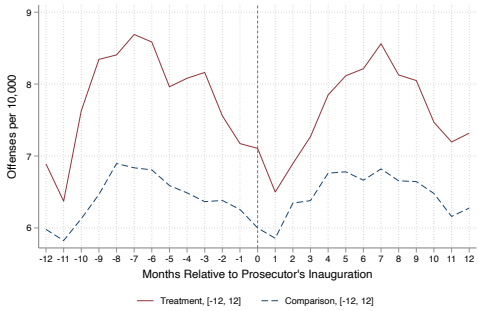
(a) OLS stacked event-studies



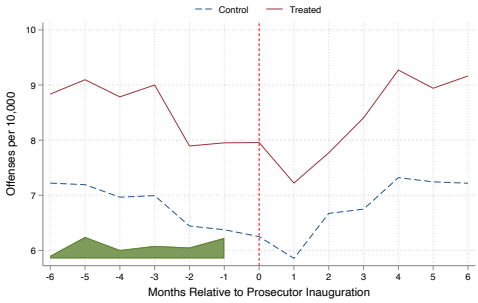
(b) 6-month trends, OLS stacked



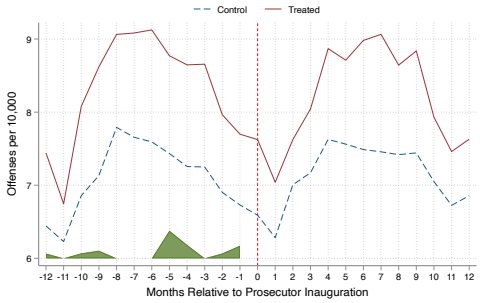
(c) 12-month trends, OLS stacked



(d) 6-month trends, synthetic DiD



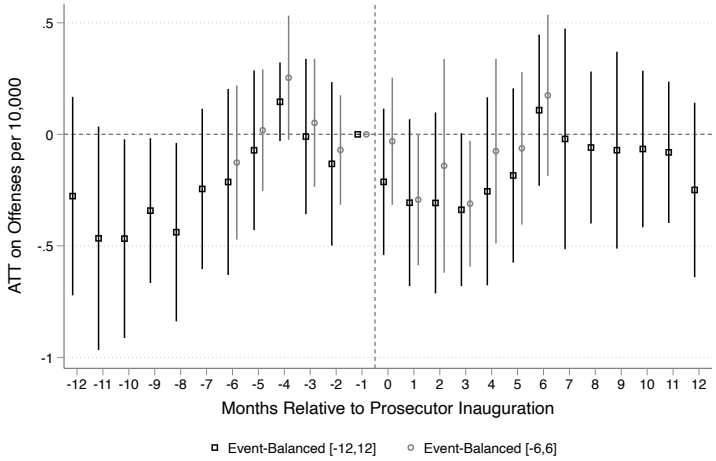
(e) 12-month trends, synthetic DiD



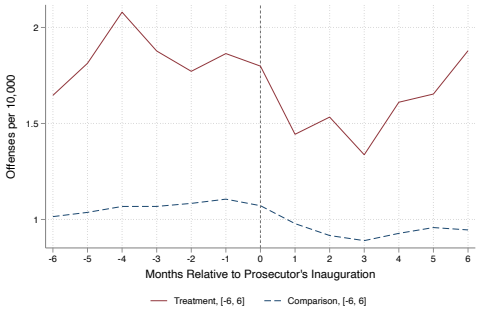
Notes. Event studies of the average treatment on the treated (ATT) effect of prosecutors' inauguration on offense rates per 10,000 residents per month and corresponding outcome trends using event-balanced samples. Time 0 corresponds to the inauguration month. Panel (a) shows OLS stacked event studies for the [6, 6] and [12, 12] event-time windows. Panels (b) and (c) show OLS stacked outcome trends for the [6, 6] and [12, 12] event-balanced samples, respectively. Panels (d) and (e) show synthetic DiD outcome trends for the same event-time windows. Panels (a)–(c) are estimated using the stacked difference-in-differences estimator in Wing et al. (2024), while panels (d)–(e) are estimated using the synthetic difference-in-differences estimator in Arkhangelsky et al. (2021).

Figure B.10: Trends and event studies for event-balanced samples, robbery rate per 10,000 residents per month

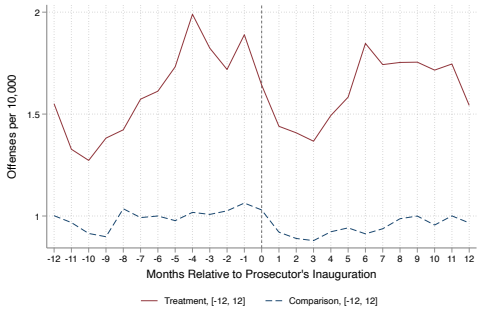
(a) OLS stacked event-studies



(b) 6-month trends, OLS stacked



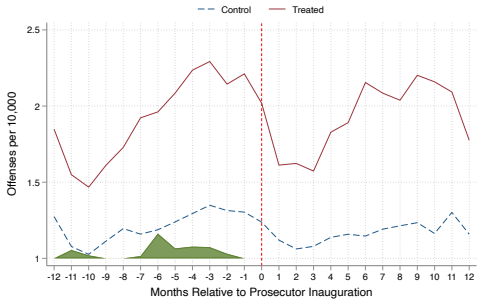
(c) 12-month trends, OLS stacked



(d) 6-month trends, synthetic DiD



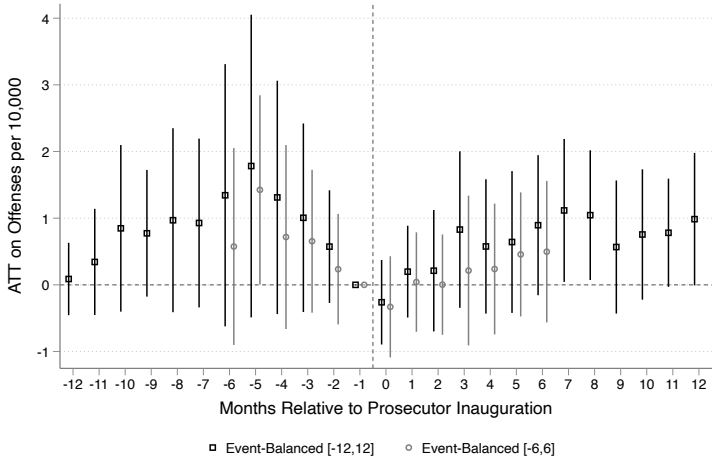
(e) 12-month trends, synthetic DiD



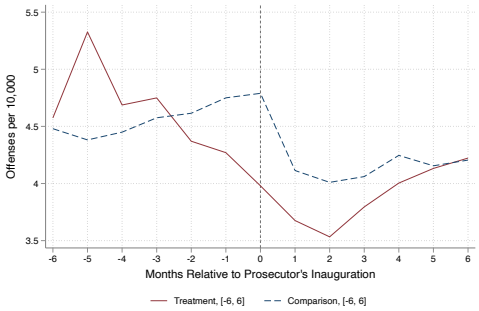
Notes. Event studies of the average treatment on the treated (ATT) effect of prosecutors' inauguration on offense rates per 10,000 residents per month and corresponding outcome trends using event-balanced samples. Time 0 corresponds to the inauguration month. Panel (a) shows OLS stacked event studies for the [6, 6] and [12, 12] event-time windows. Panels (b) and (c) show OLS stacked outcome trends for the [6, 6] and [12, 12] event-balanced samples, respectively. Panels (d) and (e) show synthetic DiD outcome trends for the same event-time windows. Panels (a)–(c) are estimated using the stacked difference-in-differences estimator in Wing et al. (2024), while panels (d)–(e) are estimated using the synthetic difference-in-differences estimator in Arkhangelsky et al. (2021).

Figure B.11: Trends and event studies for event-balanced samples, burglary rate per 10,000 residents per month

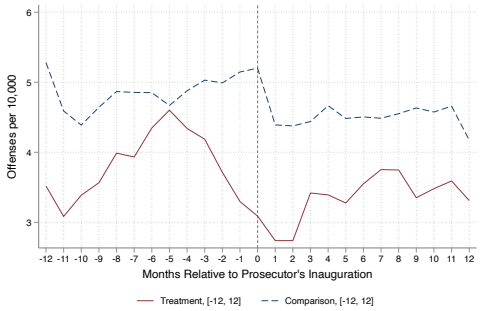
(a) OLS stacked event-studies



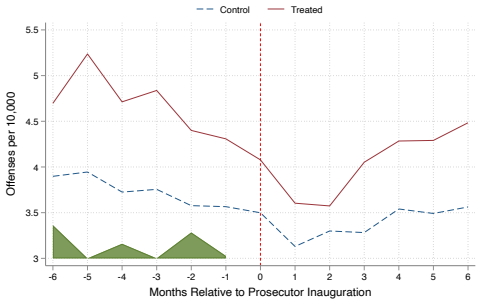
(b) 6-month trends, OLS stacked



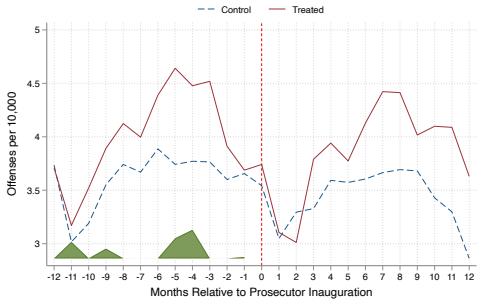
(c) 12-month trends, OLS stacked



(d) 6-month trends, synthetic DiD



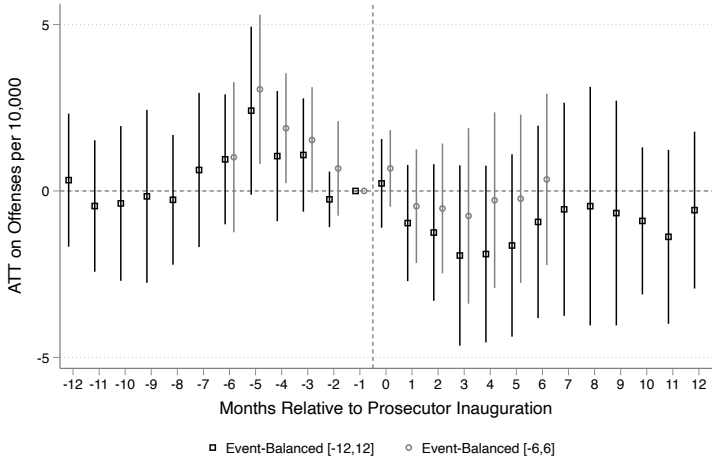
(e) 12-month trends, synthetic DiD



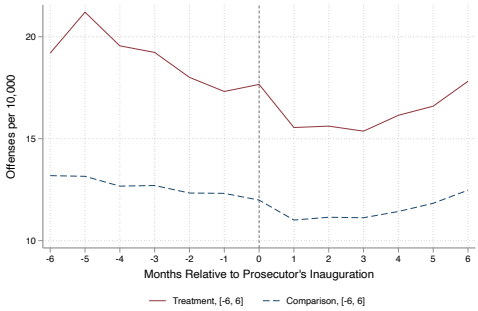
Notes. Event studies of the average treatment on the treated (ATT) effect of prosecutors' inauguration on offense rates per 10,000 residents per month and corresponding outcome trends using event-balanced samples. Time 0 corresponds to the inauguration month. Panel (a) shows OLS stacked event studies for the [6, 6] and [12, 12] event-time windows. Panels (b) and (c) show OLS stacked outcome trends for the [6, 6] and [12, 12] event-balanced samples, respectively. Panels (d) and (e) show synthetic DiD outcome trends for the same event-time windows. Panels (a)–(c) are estimated using the stacked difference-in-differences estimator in Wing et al. (2024), while panels (d)–(e) are estimated using the synthetic difference-in-differences estimator in Arkhangelsky et al. (2021).

Figure B.12: Trends and event studies for event-balanced samples, theft rate per 10,000 residents per month

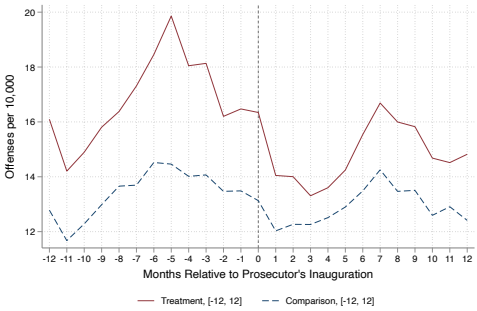
(a) OLS stacked event-studies



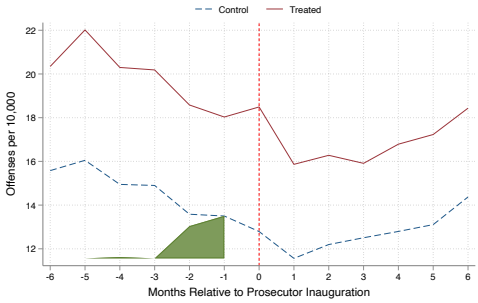
(b) 6-month trends, OLS stacked



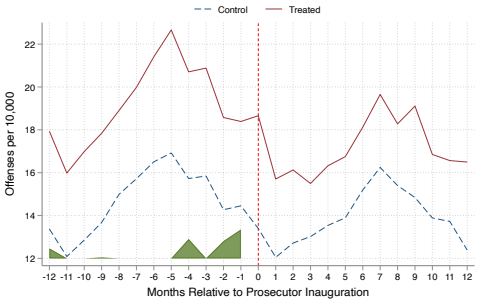
(c) 12-month trends, OLS stacked



(d) 6-month trends, synthetic DiD



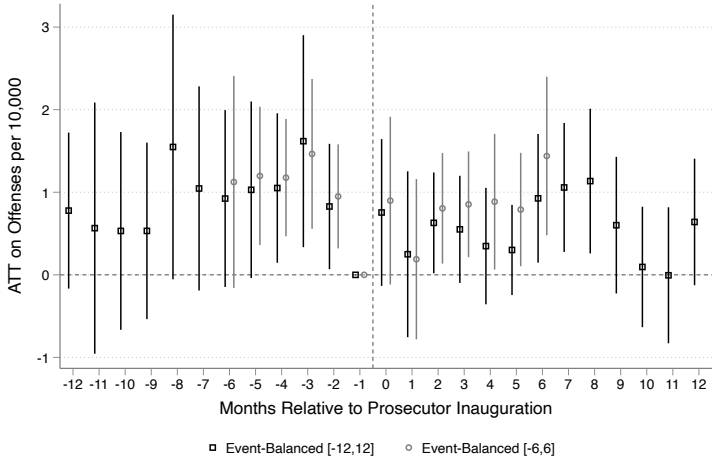
(e) 12-month trends, synthetic DiD



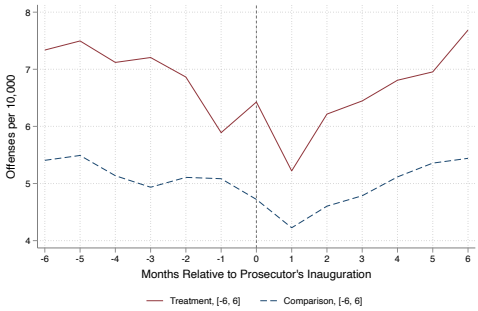
Notes. Event studies of the average treatment on the treated (ATT) effect of prosecutors' inauguration on offense rates per 10,000 residents per month and corresponding outcome trends using event-balanced samples. Time 0 corresponds to the inauguration month. Panel (a) shows OLS stacked event studies for the [6, 6] and [12, 12] event-time windows. Panels (b) and (c) show OLS stacked outcome trends for the [6, 6] and [12, 12] event-balanced samples, respectively. Panels (d) and (e) show synthetic DiD outcome trends for the same event-time windows. Panels (a)–(c) are estimated using the stacked difference-in-differences estimator in Wing et al. (2024), while panels (d)–(e) are estimated using the synthetic difference-in-differences estimator in Arkhangelsky et al. (2021).

Figure B.13: Trends and event studies for event-balanced samples, vandalism rate per 10,000 residents per month

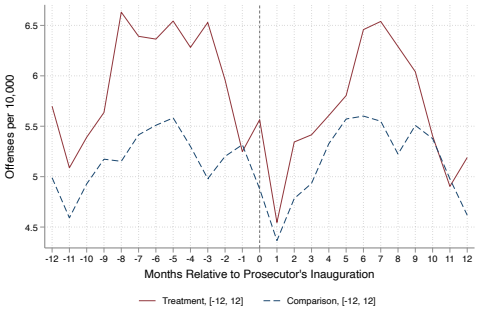
(a) OLS stacked event-studies



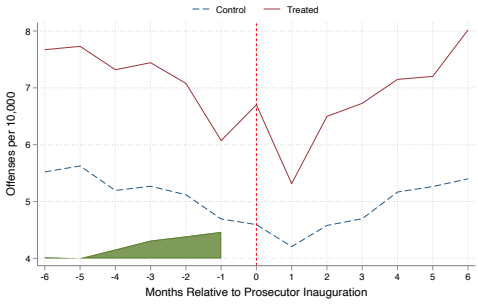
(b) 6-month trends, OLS stacked



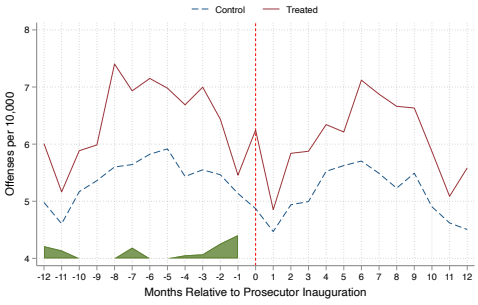
(c) 12-month trends, OLS stacked



(d) 6-month trends, synthetic DiD



(e) 12-month trends, synthetic DiD



Notes. Event studies of the average treatment on the treated (ATT) effect of prosecutors' inauguration on offense rates per 10,000 residents per month and corresponding outcome trends using event-balanced samples. Time 0 corresponds to the inauguration month. Panel (a) shows OLS stacked event studies for the [6, 6] and [12, 12] event-time windows. Panels (b) and (c) show OLS stacked outcome trends for the [6, 6] and [12, 12] event-balanced samples, respectively. Panels (d) and (e) show synthetic DiD outcome trends for the same event-time windows. Panels (a)–(c) are estimated using the stacked difference-in-differences estimator in Wing et al. (2024), while panels (d)–(e) are estimated using the synthetic difference-in-differences estimator in Arkhangelsky et al. (2021).

C ATT Effect on Prison Incarceration Rates

Analogous to the case outcomes analysis presented in the main text, we use the same staggered difference-in-differences strategy to estimate ATT effects of a reform prosecutor's inauguration on county-year prison incarceration rates.[11] For this analysis, we sometimes observe multiple counties for the same reform prosecutor with a multi-county jurisdiction, so we perform a county-level analysis and cluster standard errors at the jurisdiction level. Our ATT estimates of the effect of reform prosecutors on county-year prison incarceration rates in their jurisdictions are presented in Table C.1, with corresponding event studies in Figure C.1. Our main point estimate would suggest a decrease of approximately 28 people incarcerated per 100,000 county residents per year, equivalent to 5 percent of the baseline mean, but the effect is not statistically significant. Note that the sample for this analysis differs from the samples used in the case outcomes analysis due to differential coverage of incarceration outcomes in the CJARS JOE data.

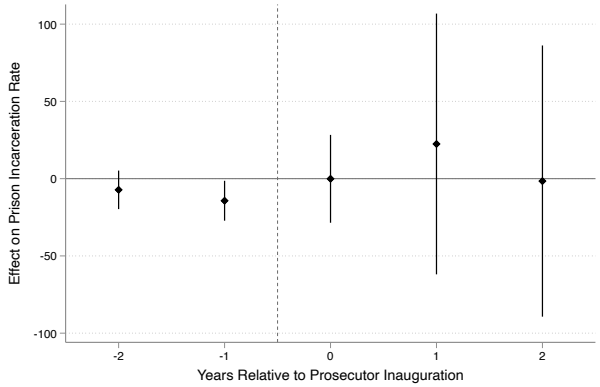
Table C.1: ATT effect of reform prosecutors on prison incarceration rate per 100,000 residents per year

	(1) Prison Incarceration Rate
ATT	-27.62
	(25.96)
Pre-Treatment Mean	545.8
Percentage Effect	-5.059
Jurisdictions	17
Counties	24
Observations	349

Notes. The dependent variable is the prison incarceration rate per 100,000 residents per year. Prison incarceration data are available for multiple counties within prosecutors' multi-county jurisdictions. The analysis is at the county-year level, and the average treatment on the treated (ATT) effect is estimated using the Callaway and Sant'Anna (2021) estimator. Standard errors, clustered at the jurisdiction level, are reported in parentheses. +10%, *5%, **1%, and ***0.1% significance levels.

[11] While we would ideally like to include jail incarceration as well, since some reform prosecutors campaign on reducing requests for cash bail and/or pretrial detention, we lack sufficient jail incarceration data to do so.

Figure C.1: Event-study ATT effects of reform prosecutors on prison incarceration rates



Notes. Event study of average treatment on the treated (ATT) effect of reform prosecutors' inaugurations on prison incarceration rates per 100,000 residents. Time 0 is the inauguration year. Figure generated by the csdid Stata package, using the event option and weighting by population.