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Prosecutorial Reform and Local Crime Rates

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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 to empirically characterize their policy changes and estimate downstream effects on prison incarceration rates, local reported crime rates, and drug mortality rates. We find that after a reform prosecutor takes office there are consistent and often statistically significant decreases in charging and conviction rates for nonviolent misdemeanor offenses, particularly misdemeanor drug offenses, but not for violent or felony offenses. We find little to no downstream effects on prison incarceration rates and no effects on local reported 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 adverse effects on public safety.

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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.

In this paper, we use variation in the timing of the inauguration of reform-minded prosecutors in jurisdictions across the country as a natural experiment to empirically characterize the resulting changes in charges, convictions, prison incarceration rates, local reported crime rates, and drug overdose mortality rates. We combine information on prosecutors’ inauguration dates with charging, conviction, and prison incarceration 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, and drug mortality data from CDC Wonder. We consistently find that the reform prosecutors in our sample charge fewer nonviolent misdemeanor cases, particularly misdemeanor drug cases, and have lower conviction rates among the misdemeanor cases that are charged. We find less consistent results for felony charging and conviction rates, with suggestive decreases in charging rates for felony drug offenses. We also find little to no downstream effects on prison incarceration rates and no effects on local reported crime rates or drug overdose mortality rates. On the margin at which these prosecutors are implementing policy changes, they appear to be able to reduce criminal justice system interaction without adverse consequences on public safety.

We build on prior work considering the effects of specific policy changes by individual reform-minded prosecutors. [Agan et al. \(2023\)](#) evaluate the effects of the implementation of a presumption of non-prosecution for a set of 15 low-level offenses by Suffolk County, Massachusetts District Attorney Rachael Rollins, finding a 15-20% increase in the declination rate for nonviolent misdemeanors after Rollins’ inauguration with no corresponding change in the local reported crime rate. [Ouss and Stevenson \(2023\)](#) consider Philadelphia District Attorney Larry Krasner’s directive to no longer request cash bail for 25 low-level offenses, finding no changes in pretrial detention or pretrial misconduct for the offenses in question. As a supplement to this analysis, they empirically confirm a decline in charging rates for

marijuana, sex work, and retail theft offenses — which Krasner indicated that his office would typically decline to prosecute — but do not analyze downstream effects of these changes on local reported crime rates. In our data, which include both Rollins and Krasner, we estimate an average 7-23% reduction in the overall rate of misdemeanor charges per capita after a reform prosecutor takes office with no changes in violent misdemeanor charging rates and inconsistent changes in felony charging rates, indicating that a broader cross-section of reform prosecutors implemented a more lenient approach to low-level cases after taking office. Consistent with [Agan et al. \(2023\)](#), we find no corresponding changes in local reported crime rates after reform prosecutors are inaugurated.

A few other studies use variation across jurisdictions in the election of reform prosecutors to estimate the effects of reform prosecutors on case outcomes and/or local reported crime rates. [Mitchell et al. \(2022\)](#) compare case outcomes across jurisdictions in Florida in 2017 and find that cases in jurisdictions with a reform prosecutor are less likely to end in a felony conviction and have smaller racial disparities in case outcomes. We build on this finding by demonstrating differences in charging rates between reform and traditional prosecutors in addition to differences in conviction outcomes among cases charged. [Petersen et al. \(2024\)](#) implement a difference-in-differences design to estimate the effect of electing a reform prosecutor on reported index crime rates from the Uniform Crime Reports in the 100 largest counties in the United States. They find that electing a reform prosecutor is associated with a 7% increase in reported property crime and no change in reported violent crime. In our main reported crime analysis, we take a similar staggered difference-in-differences approach but compare only across jurisdictions that eventually elect a reform prosecutor to allow for the possibility that such jurisdictions may be unobservably different from those that never elect a reform prosecutor. We also use incident-level data (either hand-collected from individual police departments or from the National Incident-Based Reporting System) so that we can include a wider range of offense types. Lastly, we make no restrictions on the size of jurisdictions that are eligible to be included in the sample. We find no statistically significant effects of the inauguration of a reform prosecutor on reported local crime rates for any offense type. However, we cannot rule out a 7% increase in reported property offenses as found in [Petersen et al. \(2024\)](#). Our results confirm that their finding of no effects on violent reported crime rates is robust to using only eventually-treated jurisdictions and to including smaller jurisdictions in the analysis.

Both [Mitchell et al. \(2022\)](#) and [Petersen et al. \(2024\)](#) characterize prosecutors as either “progressive” or “traditional” based on whether their campaign platforms include elements like data-driven or evidence-based decision-making, declination or diversion of minor offenses, reducing financial costs for defendants, and establishing post-conviction review units. We

rely on a list from a non-profit organization that coordinates a network of reform-minded prosecutors, and find that their categorization of reform prosecutors largely overlaps with [Petersen et al. \(2024\)](#)’s list of “progressive” prosecutors, with differences between our sample and theirs stemming primarily from differences in the time periods considered and jurisdiction sizes included, not from differential characterization of prosecutors’ policies among those who meet the inclusion criteria for both samples.¹

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 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, reform prosecutors’ policy changes may not necessarily reduce incarceration, as these reforms may be targeted at low-level offenses which typically do not lead to either pretrial detention or post-conviction incarceration ([Ouss and Stevenson, 2023](#)). Although we do not quantify the impact of the reform-minded prosecutors in our sample on jail incarceration, we do find that their policies led to small or no changes in prison incarceration rates, likely because the largest and most robust reductions in charging and conviction rates were for misdemeanor drug offenses that typically do not lead to prison incarceration sentences.

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 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

¹See Table 16 for a characterization of how our treated sample for the local reported crime rates analysis differs from that of [Petersen et al. \(2024\)](#).

no effects of a reduction in nonviolent misdemeanor prosecutions on local crime rates. We also find no changes in reported local crime rates immediately following the inauguration of reform-minded prosecutors who reduced charging and conviction rates for low-level offenses, suggesting that if potential offenders are responding to either reduced incapacitation and deterrence or reduced collateral consequences, these effects roughly offset each other.

Lastly, one limitation of the reported local crime rates analysis is that it is challenging to disentangle changes in underlying crime rates from changes in police behavior. Many low-level reported offenses result from proactive police enforcement actions taken in response to observed criminal activity. Existing studies suggest that police officers may adjust their enforcement activity in response to expected sanctions (Gonçalves and Mello, 2023; Soliman, 2022). If some low-level offenses are enforced and thus reported at lower rates because police officers anticipate lower probabilities of charges or convictions, our estimates would be biased downward. 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 constructs a measure of drug use that should be unaffected by police officers' propensity to record an incident. We do not find evidence of significant changes in drug mortality rates despite large reductions in charging and conviction rates for misdemeanor drug offenses, suggesting that our finding of no changes in local reported crime rates after the inauguration of reform prosecutors is not being driven by changes in police behavior.

Overall, we find that reform prosecutors did in fact change the way that criminal cases were handled in their jurisdictions, primarily through reductions in charging and conviction rates for nonviolent misdemeanor offenses that led to few or no changes in prison incarceration rates. We find that these reforms did not result in substantial effects on reported local crime or drug mortality, adding to the existing evidence indicating that it is possible to reduce legal sanctions for low-level offenses without compromising public safety.

2 Data

A non-profit organization that coordinates a network of reform prosecutors provided us with a list of sixty prosecutors it had identified as reform-minded. Fifty-seven were local district attorneys and three were 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 counties, but a few are cities or groups of counties.

To understand what these prosecutors may be doing differently than their predecessors, we use data from the Criminal Justice Administrative Records System's Justice Outcomes

Explorer (CJARS JOE) (Finlay et al., 2025), which aggregates administrative data from many jurisdictions to form county-year rates of misdemeanor and felony charges per 100,000 residents and convictions per felony or misdemeanor charge. The analysis sample for these charging and case outcomes includes all counties and years for which the CJARS JOE data are available and that see the inauguration of a reform prosecutor in our time frame, resulting in an unbalanced county-year panel representing twenty-six of the fifty-seven local prosecutors on the original list (included jurisdictions listed in Table 1). We similarly use the CJARS JOE data to estimate the effects of these prosecutors on county-year prison incarceration rates per 100,000 residents. Prison incarceration data are available for at least one county in the jurisdiction of twenty-four of the original fifty-seven prosecutors (see Table 2 for a list of jurisdictions included in this analysis).

To understand impacts on reported local crime rates, we obtained data on reported crime incidents from at least one police department in each reform prosecutor’s jurisdiction for the years before and after the prosecutor’s inauguration, for thirty-nine jurisdictions out of the fifty-seven on the original list. 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). We then supplemented these hand-collected, incident-level data with incident-level data from the FBI’s National Incident-Based Reporting System (NIBRS), adding an additional thirteen jurisdictions. In jurisdictions for which we rely on NIBRS data, we include 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. The thirty-nine jurisdictions, date ranges, and data sources included in the reported local crime rates analysis are listed in Table 10. The prosecutors’ inauguration dates in this sample range from January 1, 2007, through January 12, 2021.

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 also present results for broad, aggregated crime categories to reduce noise in our estimates and improve statistical power. 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.² 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

²In 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 available data) from January 2005 through December 2023. Observations are at the jurisdiction-month level, and our analysis sample contains approximately 3,000 observations for most crime types; some jurisdictions did not provide data for all crime categories in all years.³

Lastly, we collected data on drug mortality from CDC Wonder, as measured by county-year drug mortality rates per 100,000 residents. These data are reported only for counties with at least ten drug mortality deaths per year, resulting in a sample representing forty-one of the fifty-seven prosecutors on the original list.

3 Empirical Strategy

We use elected reform prosecutors’ inauguration dates as shocks to local prosecution policies. If those prosecutors’ reforms affect charging, 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 are likely different from those that do not, we focus primarily on the sample of counties that elect a reform-minded prosecutor at some point during the sample period.⁴ However, there is limited representation of jurisdictions led by reform prosecutors in the CJARS JOE data. To balance these concerns, we present estimates both restricting the sample to eventually-treated counties and including all available counties for outcomes relying on these data. We use samples including never-treated counties only for these CJARS JOE outcome categories that are available for fewer than thirty treated jurisdictions.

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 estimates for each set of

³All crime category-calendar year-jurisdiction cells with no reported offenses are treated as missing.

⁴The few jurisdictions for which we have data only for time periods before a reform-minded prosecutor was inaugurated are included in the comparison group.

places where a reform-minded prosecutor took office in the same month. We then aggregate those estimates using a simple average to estimate the Average Treatment effect on the Treated (ATT).

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

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

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 analysis, we observe only one county in each treated jurisdiction and perform county-level analyses. For the prison incarceration rates 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 treated-jurisdiction level to allow for the inclusion of untreated counties for whom we do not observe prosecutor jurisdiction boundaries. Lastly, for the reported local crime rates and drug mortality rates analyses, we aggregate to the jurisdiction level and perform jurisdiction-level analyses.

The [Callaway and Sant’Anna \(2021\)](#) estimation procedure compares newly-treated jurisdictions only to not-yet-treated jurisdictions, excluding problematic comparisons between newly-treated and earlier-treated jurisdictions. It also allows for the use of all available time periods for each jurisdiction, making full use of the available data in our unbalanced panel. It computes standard errors using a wild cluster bootstrap procedure that is robust to the small number of jurisdictions in some of our analyses.

The identifying assumption underlying our staggered difference-in-differences approach is that trends in charges, convictions, prison incarceration, and crime rates in treated places and not-yet treated places would have evolved similarly in the absence of the reform prosecutor’s inauguration. 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.

Our main specification uses all time periods available for each jurisdiction, with no additional balancing procedures. However, 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. \(2024\)](#) to test the sensitivity of our estimates to compositional changes in the group of treated jurisdictions we are able to observe in each event-month following a prosecutor’s inauguration. Additionally, for comparison across outcome categories, we estimate the charging, conviction, prison incarceration, and drug mortality effects both for the full set of available treated jurisdictions and for the subset that are covered in the reported local crime rates analysis. 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 representing only the jurisdictions that are represented in the CDC Wonder data in every year of our analysis from 2005-2023.⁵

4 Results

Overall, we consistently find that the reform prosecutors in our sample charge fewer non-violent misdemeanor cases, particularly misdemeanor drug cases, and have lower conviction rates among the misdemeanor cases that are charged. We find less consistent results for felony charging and conviction rates, with the largest suggestive decreases observed in charging rates for felony drug offenses. We find small to no effects on prison incarceration rates, likely because observed reductions in charges and convictions are larger for the nonviolent misdemeanor offenses that are unlikely to result in prison incarceration sentences. We also find no effects of reform prosecutors on reported local crime rates or drug mortality rates, which is consistent with existing evidence that marginal nonviolent misdemeanor prosecutions ([Agan et al., 2023](#)) and noncarceral convictions ([Humphries et al., 2024](#)) do not increase public safety.

4.1 Charging, Conviction, and Prison Incarceration

Table 3 presents estimates of the effect of a reform prosecutor’s inauguration on county-year misdemeanor charging rates per 100,000 residents (Panel A) and the share of misdemeanor charges resulting in a conviction (Panel B), measured as of the year the case was initially charged. Charging and conviction data are available for only one county per jurisdiction, so each treated county represents a different prosecutor. The sample in column 1 compares earlier-treated counties only to later-treated counties, and includes only jurisdictions that are represented in the sample used in the reported local crime rates analysis in Section

⁵The 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.

4.2. In this sample, we find that after a reform prosecutor takes office, misdemeanor charges decrease by approximately 737 charges per 100,000 residents per year, a 23% decrease relative to the baseline mean ($p < .05$). In column 2, we expand the sample to all eventually-treated jurisdictions for which charging and conviction data are available. In this sample, the estimated effect on misdemeanor charging rates is directionally consistent but smaller in magnitude, indicating a statistically insignificant decrease of approximately 258 charges per 100,000 residents per year (7% relative to the mean). However, both of these samples are relatively small, containing only 19 counties in column 1 and 26 counties in column 2. In columns 3 and 4 we include all counties with available charge data in the comparison group. Results are directionally consistent and statistically significant in the largest sample (column 4), providing strong evidence that reform prosecutors reduced the number of misdemeanors charged in their jurisdictions after taking office.

Among misdemeanor cases charged, in column 1 conviction rates decrease by about 10 percentage points, an 18% decrease relative to the baseline mean ($p < .01$). In column 2, the effect on the conviction rate among cases charged is similar in magnitude to that in the reported local crime sample, indicating a decrease of about 8 percentage points (16% relative to the mean, $p < .05$). In columns 3 and 4 results are directionally consistent but smaller in magnitude, again providing compelling evidence that reform prosecutors pursued lower misdemeanor conviction rates after taking office. Corresponding event studies are presented in Figures 1 (charging rates) and 2 (conviction rates), indicating that counties with a reform prosecutor have similar misdemeanor charging and conviction rates relative to other counties prior to the relevant prosecutors' inauguration dates, and that misdemeanor charging and conviction rates fall after a reformer takes office.

We also explore which offense types drive the observed decreases in misdemeanor charging and conviction rates. In Table 4, we present misdemeanor charging effects separately by offense type. In Panel A, we find no large or statistically significant effects on violent misdemeanor charging rates in any of the four samples, and the sign of the point estimate varies. In column 1 of Panel B, we estimate a statistically significant decrease in the misdemeanor property charging rate of 141 offenses per 100,000 people per year (25%), but this effect is not directionally consistent or statistically significant across the samples used columns 2-4. In Panel C, we find by far the largest and most robust decrease in charging rates, in both relative and absolute terms, for misdemeanor drug offenses. Column 1 indicates a decrease of 242 misdemeanor drug charges per 100,000 residents per year in the sample that overlaps with that used in the reported local crime rates analysis in Section 4.2, a 46% reduction relative to the mean ($p < .01$). Point estimates are also large and negative in the remaining samples and are statistically significant in the largest sample (column 4).

Results in Table 5 indicate that decreases in misdemeanor conviction rates are directionally consistent and of similar magnitude across all offense types and all samples, and are statistically significant in at least one sample for all offense types. This finding is consistent with reform prosecutors’ stated policy goal to reduce the use of misdemeanor pretrial detention (Petersen et al., 2024; Mitchell and Petersen, 2025), which increases the probability of conviction (Dobbie et al., 2018).

We conduct the same analysis for felony charging and conviction rates and present results in Table 6 and Figures 3 and 4. In jurisdictions represented in the reported local crime rates sample (column 1), we find that reform prosecutors reduce felony charging rates by approximately 339 charges per 100,000 residents per year ($p < .05$). However, this reduction in felony charging rates is not directionally robust across samples. We also find a small but statistically significant decrease in felony conviction rates in the reported local crime rates sample (2.5 percentage points or about 5%, $p < .01$), but this finding is also not directionally robust across the other samples. These results are suggestive that some reform prosecutors may reduce charging and conviction rates for some felony offenses, but the extent may vary more across jurisdictions than for misdemeanors.

As in the misdemeanor analysis, event studies in Figures 3 and 4 show similar felony charging and conviction rates in treated and untreated counties prior to a reform prosecutor’s inauguration date. Like the aggregated difference-in-differences estimates in Table 6, these event studies show that the estimated effect of reform prosecutors on felony charging and conviction rates is ambiguous, varying in direction across the different samples used.

Finally, we again subset by felony offense type in Tables 7 and 8. We find no statistically significant changes in charges or convictions for any individual felony offense type. Point estimates suggest that any potential decreases in felony charging and conviction rates are likely driven primarily by felony drug offenses. We see no evidence of large or directionally consistent changes in charging or conviction rates for violent or property felony offenses.

We then investigate whether the prosecutor-driven decreases in misdemeanor charging and conviction rates have any downstream effects on county-level prison incarceration rates. Note that the incarceration sample is overlapping with but different from the charging and convictions sample, as court data and prison incarceration data have different coverage in CJARS. In some reform prosecutors’ multi-county jurisdictions, prison incarceration data are available for multiple counties, so we perform our analysis at the county-year level but cluster by jurisdiction for treated counties. Results are presented in Table 9, with corresponding event studies in Figure 5. Point estimates are small and negative across all four samples, and marginally statistically significant ($p < 0.1$) in the full available sample in column 4. If we were to take these estimates at face value, they would indicate a decrease in the

prison incarceration rate of 24-62 incarcerated people per 100,000 residents per year (4-11%) depending on the sample. Overall, we do not find robust evidence that the reformers' policy changes resulted in a substantial change in county-level prison incarceration rates.

We have established that reform-minded prosecutors' policies do appear to impact charging and conviction outcomes. Their policies are reducing charging and convictions, robustly for nonviolent misdemeanors (primarily drug offenses) with some suggestive evidence of more modest reductions for some nonviolent felonies as well. However, we do not find robust evidence that these reductions in charging and conviction rates result in downstream changes in prison incarceration rates in their jurisdictions. That reform prosecutors produce little to no reduction in prison incarceration rates immediately after taking office is consistent with the finding that most of the reductions in charging and convictions are driven by nonviolent misdemeanor offenses that typically do not lead to prison sentences.

4.2 Local Reported Crime

Having established that reform prosecutors' policies are having real effects on how cases are managed, we now turn to the concern that reducing charging and conviction rates for nonviolent misdemeanor offenses in particular could lead to increases in reported crime via reduced deterrence and incapacitation.

Tables 12 and 13 present our main estimates of the effects of reform prosecutors on local reported crime rates using the full unbalanced panel of jurisdictions for all months available, estimated using the staggered difference-in-differences estimator in Callaway and Sant'Anna (2021). Table 12 aggregates offense types into categories to reduce noise and improve statistical power, including only jurisdiction-month observations for which all of the relevant offense types are reported in the data. Table 13 presents results for individual offense types, trading noisier estimates for any given offense type for the ability to include more jurisdictions in the analysis.

Column 1 of Table 12 estimates the effects of the reform prosecutors' inaugurations on aggregated crime rates for all offense types included in the analysis, and as such covers the fewest jurisdictions due to inconsistent reporting of offense types across jurisdictions. We do not detect a statistically significant effect of reform prosecutors' inauguration on overall crime rates, but the estimate — 4 fewer incidents per 10,000 residents per month — is imprecise, and the corresponding 95% confidence interval implies between a 30% decrease and a 7% increase in overall crime rates. Nonetheless, we can rule out large increases in the overall crime rate as a result of the prosecutors' policy changes. Column 1 of Table 14 reports similar estimates using the stacked difference-in-differences estimator in Wing et al.

(2024) on event-balanced panels covering the six months (Panel A) or 12 months (Panel B) before and after a prosecutor’s inauguration. Figure 6 presents both unbalanced (Panel A) and balanced (Panel B) event studies, as well as trends for each balanced sample (Panels C and D), indicating similar trends (albeit different levels) in crime rates both before and after each prosecutor’s inauguration between earlier-treated and later-treated (comparison group) jurisdictions. If anything, trends suggest that crime rates are more volatile in the earlier-treated jurisdictions, exhibiting larger swings both before and after the reform prosecutors take office, but the crime rate tends to increase and decrease at the same time across both sets of jurisdictions, indicating no evidence of a change in overall crime trends after a reform prosecutor takes office.

Columns 2 and 3 of Table 12 present our main estimates of the effects of reform prosecutors on violent (homicide, assault and robbery) and property (burglary, theft, and vandalism) offense rates, respectively. We find no statistically significant changes in reported local crime rates for either category; our estimates rule out large ($>10\%$) increases in either category, but cannot rule out a large (40%) decrease in property offenses. We present balanced-panel results in columns 2 and 3 of Table 14 and event studies in Figures 7 and 8, indicating no evidence of any significant effects on violent or property offense rates. Further, columns 4 and 5 of Table 12 present estimates of effects on income-generating (robbery, theft, and burglary) and non-income generating (homicide, assault, and vandalism) offenses, respectively.⁶ We again find no evidence of significant effects in either category, and the null result is robust to balanced-panel estimates (columns 4 and 5 of Table 14), with event studies and trends presented in Figures 9 and 10. Overall, we find no evidence of significant effects, and the 95% confidence intervals on our main estimates rule out more than a 10% increase in any category.

Table 13 presents our main results for individual offense types, with analogous event-balanced estimates presented in Table 15 and event studies and trends presented in Figures 11-17. We find no statistically significant effects ($p < .05$) for any offense type, but estimates are quite imprecise across most offense types. Column 3 of Table 13 suggests a large (about 36%) and marginally significant ($p < 0.1$) decrease in the robbery rate, but this finding is not robust to the event-balanced estimation (column 3 of Table 15).

Recall from Section 4.1 that reform prosecutors decreased charging and conviction rates for nonviolent misdemeanor offenses immediately after taking office, with the largest and most robust reductions for misdemeanor drug offenses. We find no evidence of any changes in charging for violent offenses, and ambiguous and directionally inconsistent effects on

⁶Drug offenses are excluded from both categories, as we cannot distinguish drug possession for sale and drug possession for personal use in many of the incident-level datasets provided by local police departments.

nonviolent felony charging depending on the estimation sample used. If deterrence and/or incapacitation effects dominate, we would generally expect increases in reported nonviolent offense rates and no changes in reported violent offense rates. Similarly, if beneficial aversion of collateral consequences dominates, we would expect a reduction in reported income-generating offense rates and no changes in reported non-income-generating offense rates. We estimate no significant changes in any category and estimated effects are similar across categories, indicating that these mechanisms either are not present or largely offset each other in the months immediately after reform prosecutors take office. It appears that these reform prosecutors were able to reduce criminal justice consequences for low-level offenses without adverse effects on public safety.

4.3 Drug Overdose Mortality

One limitation of our analysis of the effects of reform prosecutors on local reported crime rates is that police may change their enforcement behavior if they know that certain offenses are unlikely to be prosecuted. Existing evidence shows that some police officers may indicate a preference for harsher expected sanctions in the way that they allocate their enforcement activity (Gonçalves and Mello, 2023; Soliman, 2022). If adjustments in police enforcement behavior result in decreased propensity to report offenses after reform prosecutors take office, our estimated effects on reported local crime rates could be biased downward. To address this concern, we estimate effects on drug overdose mortality rates as a measure of drug use that should be unaffected by any changes in police incident recording behavior. Further, if deterrence effects were present, we would expect an increase in drug use, as reform prosecutors' largest reductions in charging rates are for misdemeanor drug offenses (see Tables 4 and 7). Table 18 and Figure 18 present estimates of the effect of reform prosecutors on drug mortality rates. As noted above in Section 3, only counties with 10 or more drug overdose deaths per year are represented in the data, so we present estimates for log drug mortality rates (Panel A, interpreted as percentage changes) in addition to the rate per 100,000 residents per year (Panel B). Most estimates indicate no significant changes in drug mortality rates following a reform prosecutor's inauguration. We find a marginally significant increase in drug mortality rates in column 1 of Panel B, but this result is noisily estimated and not robust to any other sample or specification.

5 Conclusion

The totality of the results in this paper show that reform-minded prosecutors reduce misdemeanor charging and conviction rates, primarily for drug misdemeanor offenses, shortly after they take office. This reduction in sanctions translates to little to no downstream effect on prison incarceration rates, reported local crime rates, or drug mortality rates in the years after reform prosecutors take office.

In theory, we might expect that any significant reductions in charging and conviction rates could decrease the incapacitation of potential offenders by reducing jail and prison incarceration. Yet the largest and most consistent reductions in charging and conviction rates after the inauguration of reform prosecutors are for nonviolent misdemeanor offenses that typically result in neither pretrial detention nor post-conviction incarceration sentences. Although we do not observe jail incarceration in our data, [Ouss and Stevenson \(2023\)](#) found that a reform prosecutor’s decision to not request cash bail for lower level offenses had no effect on pretrial detention rates. We likewise find few to no effects on prison incarceration rates after reform prosecutors take office, consistent with this prior work finding that reform-minded prosecutors’ policy changes have few effects on incapacitation through incarceration.

We might also expect that the reductions in nonviolent misdemeanor charging and conviction rates observed after reform prosecutors take office might reduce the extent to which potential offenders are deterred from engaging in those offenses. However, a growing body of literature documents that charges and convictions for less serious offenses can impose serious consequences on defendants, including harsher sanctions for future offenses ([Harrington et al., 2025](#)) and collateral consequences outside the criminal justice system like reduced employment opportunities ([Agan and Starr, 2017](#); [Mueller-Smith and Schnepel, 2019](#)) and exclusion from social safety net programs ([Mueller-Smith et al., 2023](#)). These collateral consequences may increase reoffending behavior, potentially offsetting deterrence effects and creating room for policy changes that reduce sanctions for low level offenses without increasing crime. Our findings suggest that reform prosecutors can successfully reduce the imposition of legal sanctions for low level offenses without compromising public safety, at least at the level at which leniency is currently being implemented. 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.

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6 Figures and Tables

6.1 Charging, Conviction, and Prison Incarceration

Table 1: Analysis Sample: Charging and Conviction Rates

Name	Title	Jurisdiction	State	Inauguration	Years	Crime Sample
Buta Biberaj	Commonwealth's Attorney	Loudoun County	Virginia	2019	2009-2019	Yes
Aisha Braveboy	State's Attorney	Prince George's County	Maryland	2019	2005-2017	Yes
John Chisholm	District Attorney	Milwaukee County	Wisconsin	2007	2005-2018	Yes
John Choi	County Attorney	Ramsey County	Minnesota	2011	2009-2021	No
Laura Conover	County Attorney	Pima County	Arizona	2020	2005-2021	Yes
John Creuzot	District Attorney	Dallas County	Texas	2019	2005-2012	Yes
Satana Deberry	District Attorney	Durham County	North Carolina	2019	2005-2021	No
Parisa Dehghani-Tafti	Commonwealth's Attorney	Arlington County/City of Falls Church	Virginia	2019	2009-2019	Yes
Steve Descano	Commonwealth's Attorney	Fairfax County	Virginia	2019	2009-2019	Yes
Matthew Ellis	District Attorney	Wasco County	Oregon	2021	2005-2021	Yes
José Garza	District Attorney	Travis County	Texas	2021	2005-2017	Yes
Joe Gonzales	District Attorney	Bexar County	Texas	2019	2005-2021	No
Mark Gonzalez	District Attorney	Nueces County	Texas	2017	2005-2012	Yes
Christian Gossett	District Attorney	Winnebago County	Wisconsin	2007	2005-2018	No
Jim Hingeley	Commonwealth's Attorney	Albemarle County	Virginia	2019	2009-2019	Yes
Larry Krasner	District Attorney	Philadelphia	Pennsylvania	2018	2009-2018	Yes
Karen McDonald	Prosecuting Attorney	Oakland County	Michigan	2021	2005-2021	Yes
Stephanie Morales	Commonwealth's Attorney	City of Portsmouth	Virginia	2015	2009-2019	No
Marilyn Mosby	State's Attorney	Baltimore City	Maryland	2015	2005-2017	Yes
Melissa Nelson	State Attorney	Fourth Judicial Circuit	Florida	2017	2005-2021	No
Harold Pryor	State Attorney	17th Judicial Circuit	Florida	2021	2005-2021	Yes
Eli Savit	Prosecuting Attorney	Washtenaw County	Michigan	2021	2005-2021	Yes
Mike Schmidt	District Attorney	Multnomah County	Oregon	2021	2005-2021	Yes
Carol Siemon	Prosecuting Attorney	Ingham County	Michigan	2017	2005-2021	Yes
Andrew Warren	State Attorney	13th Judicial Circuit	Florida	2017	2005-2021	No
Monique Worrell	State Attorney	9th Judicial Circuit	Florida	2021	2005-2021	Yes

Table 2: Analysis Sample: Prison Incarceration Rates

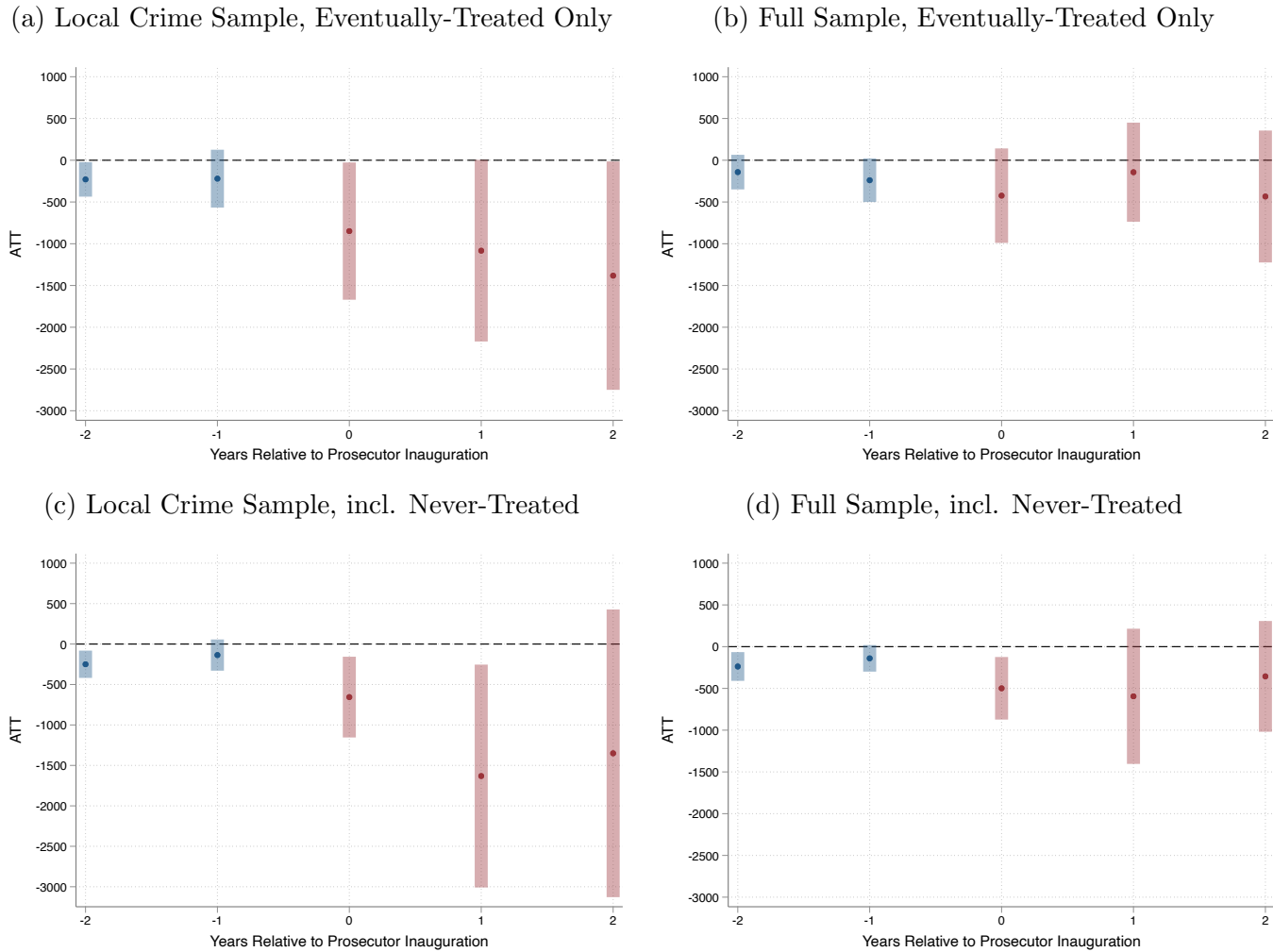
Name	Title	Jurisdiction	State	Inauguration	Years	Crime Sample
John Chisholm	District Attorney	Milwaukee County	Wisconsin	2007	2005-2021	Yes
John Choi	County Attorney	Ramsey County	Minnesota	2011	2005-2021	No
Dave Clegg	District Attorney	Ulster County	New York	2020	2016-2021	Yes
Laura Conover	County Attorney	Pima County	Arizona	2020	2005-2021	Yes
John Creuzot	District Attorney	Dallas County	Texas	2019	2005-2017	Yes
Satana Deberry	District Attorney	Durham County	North Carolina	2019	2005-2021	No
Michael Dougherty	District Attorney	20th Judicial District	Colorado	2018	2005-2021	Yes
Mark Dupree	District Attorney	Wyandotte County	Kansas	2017	2005-2021	Yes
Kim Foxx	State's Attorney	Cook County	Illinois	2016	2006-2020	Yes
José Garza	District Attorney	Travis County	Texas	2021	2005-2017	Yes
Joe Gonzales	District Attorney	Bexar County	Texas	2019	2005-2017	No
Mark Gonzalez	District Attorney	Nueces County	Texas	2017	2005-2017	Yes
Eric Gonzalez	District Attorney	Kings County	New York	2016	2016-2021	No
Christian Gossett	District Attorney	Winnebago County	Wisconsin	2007	2005-2021	No
Alexis King	District Attorney	First Judicial District	Colorado	2021	2005-2021	Yes
Larry Krasner	District Attorney	Philadelphia	Pennsylvania	2018	2005-2021	Yes
Beth McCann	District Attorney	Second Judicial District	Colorado	2017	2005-2021	Yes
Brian Middleton	District Attorney	Fort Bend County	Texas	2019	2005-2017	Yes
Melissa Nelson	State Attorney	Fourth Judicial Circuit	Florida	2017	2005-2021	No
Alonzo Payne	District Attorney	12th Judicial District	Colorado	2021	2005-2021	Yes
Harold Pryor	State Attorney	17th Judicial Circuit	Florida	2021	2005-2021	Yes
Eric Rinehart	State's Attorney	Lake County	Illinois	2020	2006-2020	No
Mimi Rocah	District Attorney	Westchester County	New York	2021	2016-2021	Yes
Andrew Warren	State Attorney	13th Judicial Circuit	Florida	2017	2005-2021	No
Monique Worrell	State Attorney	9th Judicial Circuit	Florida	2021	2005-2021	Yes

Table 3: Effect of reform-minded prosecutors on all misdemeanor charges and convictions

	(1) Local Crime Sample	(2) Full Sample	(3) Local Crime Sample	(4) Full Sample
Panel A: Misdemeanor Charges per 100,000 Residents				
ATT	-737.3 * (309.5)	-258.1 (304.6)	-548.2 (390.5)	-482.8 * (227.8)
Pre-Treatment Mean	3153.0	3485.9	3153.0	3485.9
Percentage Effect	-23.38	-7.404	-17.39	-13.85
Treatment-Observed Counties	15	22	15	22
Eventually-Treated Counties	19	26	20	27
Never-Treated Counties	0	0	1062	1062
Total Counties	19	26	1081	1088
Panel B: Proportion of Misdemeanor Charges Convicted				
ATT	-0.0979 *** (0.0293)	-0.0824 * (0.0368)	-0.0719 (0.0462)	-0.0353 (0.0363)
Pre-Treatment Mean	0.553	0.516	0.553	0.516
Percentage Effect	-17.72	-15.95	-13.01	-6.843
Treatment-Observed Counties	14	21	14	21
Eventually-Treated Counties	17	24	18	25
Never-Treated Counties	0	0	995	995
Total Counties	17	24	1012	1019

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. Charging and conviction data are available for only one county per jurisdiction. Column 1 includes only the eventually-treated counties that are available both in the CJARS data on charges and convictions and in the local-reported-crime sample, while column 2 includes all eventually-treated counties available in the CJARS charges and convictions data. Columns 3 and 4 include the eventually-treated counties of columns 1 and 2, respectively, plus all never-treated counties with available charge and conviction data. The analysis is at the county-year level, and results 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.

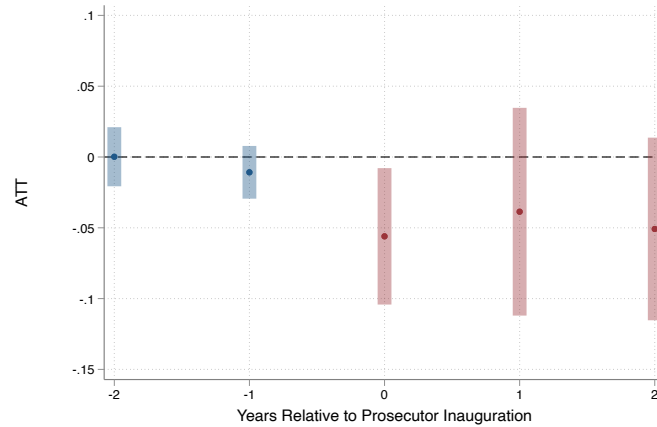
Figure 1: Misdemeanor Charging Rates



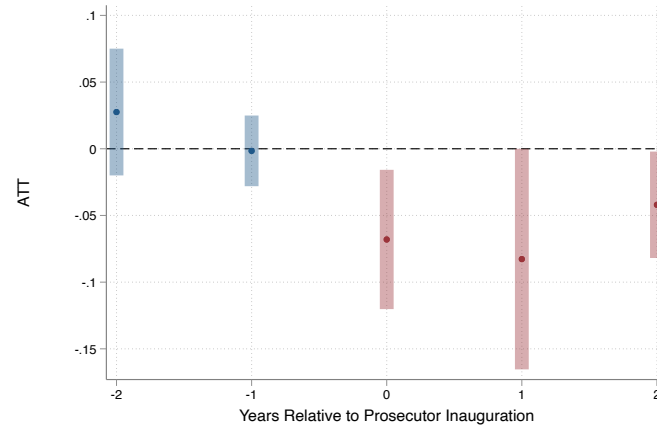
Notes. Event study of the effect of prosecutors' inauguration on misdemeanor charges per 1000 residents by year. Time 0 is the inauguration year. Figure generated by the csdid Stata package, using the event option and weighting by population.

Figure 2: Proportion of Misdemeanor Charges Convicted

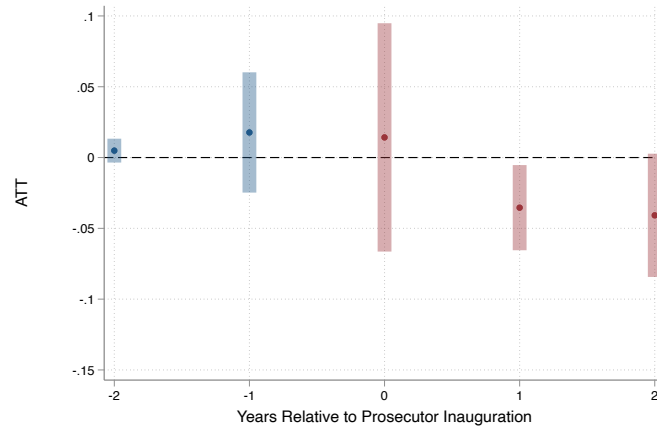
(a) Local Crime Sample, Eventually-Treated Only



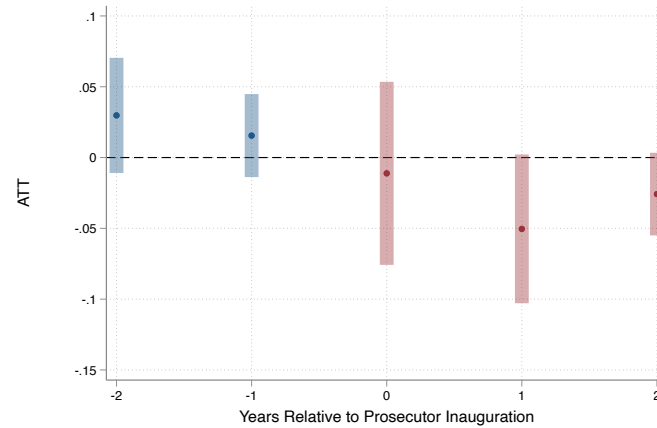
(b) Full Sample, Eventually-Treated Only



(c) Local Crime Sample, incl. Never-Treated



(d) Full Sample, incl. Never-Treated



Notes. Event study of the effect of prosecutors' inauguration on proportion of misdemeanor charges convicted. Time 0 is the inauguration year, and conviction outcomes are among cases initially charged in a given year. Figure generated by the csdid Stata package, using the event option and weighting by population.

Table 4: Effect of reform-minded prosecutors on misdemeanor charges by offense type

	(1)	(2)	(3)	(4)
	Local Crime Sample	Full Sample	Local Crime Sample	Full Sample
Panel A: Violent Misdemeanor Charges per 100,000 Residents				
ATT	-31.92 (26.33)	2.249 (23.40)	-23.66 (27.99)	-14.32 (18.30)
Pre-Treatment Mean	426.1	438.8	426.1	438.8
Percentage Effect	-7.491	0.513	-5.552	-3.263
Treatment-Observed Counties	15	22	15	22
Eventually-Treated Counties	19	26	20	27
Never-Treated Counties	0	0	1062	1062
Total Counties	19	26	1081	1088
Panel B: Misdemeanor Property Charges per 100,000 Residents				
ATT	-141.1 * (64.98)	0.573 (73.82)	-85.36 (70.39)	-35.43 (42.19)
Pre-Treatment Mean	550.8	662.4	550.8	662.4
Percentage Effect	-25.61	0.0864	-15.50	-5.349
Treatment-Observed Counties	15	22	15	22
Eventually-Treated Counties	19	26	20	27
Never-Treated Counties	0	0	1062	1062
Total Counties	19	26	1081	1088
Panel C: Misdemeanor Drug Charges per 100,000 Residents				
ATT	-242.5 ** (83.54)	-118.3 (92.43)	-206.1 (137.8)	-158.1 + (84.11)
Pre-Treatment Mean	526.4	567.2	526.4	567.2
Percentage Effect	-46.06	-20.85	-39.16	-27.87
Treatment-Observed Counties	15	22	15	22
Eventually-Treated Counties	19	26	20	27
Never-Treated Counties	0	0	1062	1062
Total Counties	19	26	1081	1088

Notes. The dependent variables in Panel A, B, and C are the misdemeanor charging rate per 100,000 residents for violent, property, and drugs offenses, respectively. Charging data are available for only one county per jurisdiction. Column 1 includes only the eventually-treated counties that are available both in the CJARS data on charges and in the local-reported-crime sample, while Column 2 includes all eventually-treated counties available in the CJARS charges and convictions data. Columns 3 and 4 include the eventually-treated counties of Columns 1 and 2, respectively, plus all never-treated counties with available charge data. The analysis is at the county-year level, and results 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.

Table 5: Effect of reform-minded prosecutors on misdemeanor conviction rates
by offense type

	(1)	(2)	(3)	(4)
	Local Crime Sample	Full Sample	Local Crime Sample	Full Sample
Panel A: Proportion of Violent Misdemeanor Charges Convicted				
ATT	-0.118 ** (0.0399)	-0.0698 + (0.0358)	-0.0864 (0.0622)	-0.0348 (0.0438)
Pre-Treatment Mean	0.484	0.436	0.484	0.436
Percentage Effect	-24.40	-16.01	-17.86	-7.970
Treatment-Observed Counties	14	21	14	21
Eventually-Treated Counties	17	24	18	25
Never-Treated Counties	0	0	793	797
Total Counties	17	24	810	821
Panel B: Proportion of Misdemeanor Property Charges Convicted				
ATT	-0.0935 ** (0.0305)	-0.0771 * (0.0390)	-0.0895 + (0.0463)	-0.0506 (0.0362)
Pre-Treatment Mean	0.555	0.514	0.555	0.514
Percentage Effect	-16.86	-15.00	-16.14	-9.843
Treatment-Observed Counties	14	21	14	21
Eventually-Treated Counties	17	24	18	25
Never-Treated Counties	0	0	874	876
Total Counties	17	24	891	900
Panel C: Proportion of Misdemeanor Drug Charges Convicted				
ATT	-0.107 *** (0.0313)	-0.0783 (0.0648)	-0.0651 (0.0642)	-0.0247 (0.0602)
Pre-Treatment Mean	0.558	0.514	0.558	0.514
Percentage Effect	-19.23	-15.24	-11.66	-4.800
Treatment-Observed Counties	14	21	14	21
Eventually-Treated Counties	17	24	18	25
Never-Treated Counties	0	0	847	847
Total Counties	17	24	864	871

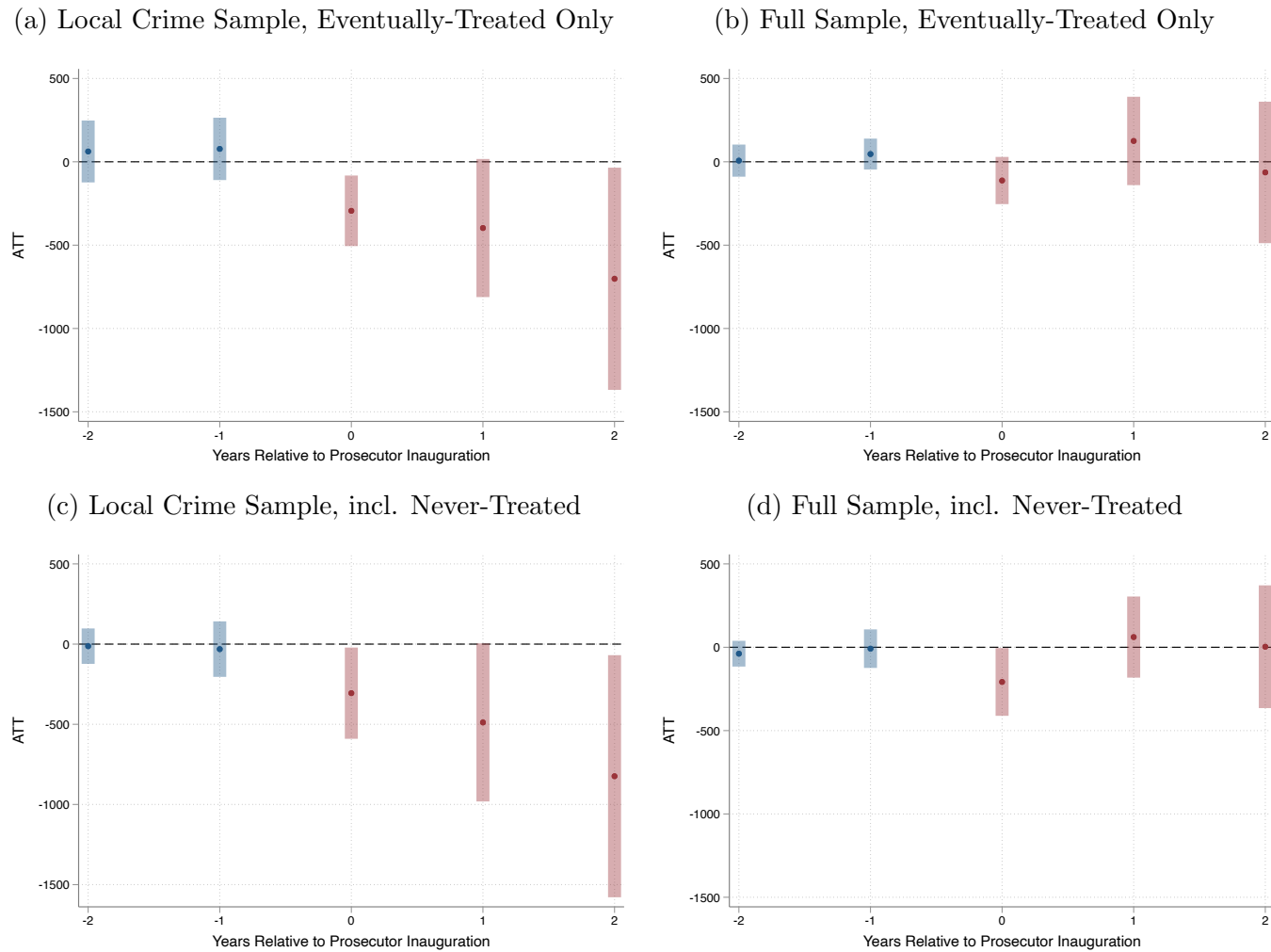
Notes. The dependent variables in Panel A, B, and C are the share of misdemeanor charges resulting in a conviction for violent, property, and drugs offenses, respectively, measured as of the year the case was initially charged. Conviction data are available for only one county per jurisdiction. Column 1 includes only the eventually-treated counties that are available both in the CJARS data on convictions and in the local-reported-crime sample, while column 2 includes all eventually-treated counties available in the CJARS charges and convictions data. Columns 3 and 4 include the eventually-treated counties of columns 1 and 2, respectively, plus all never-treated counties with available conviction data. The analysis is at the county-year level, and results 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.

Table 6: Effect of reform-minded prosecutors on all felony charges and convictions

	(1)	(2)	(3)	(4)
	Local Crime Sample	Full Sample	Local Crime Sample	Full Sample
Panel A: Felony Charges per 100,000 Residents				
ATT	-338.6 * (146.7)	53.80 (116.0)	-319.8 + (167.1)	-104.3 (81.75)
Pre-Treatment Mean	1499.7	1587.8	1499.7	1587.8
Percentage Effect	-22.58	3.388	-21.33	-6.572
Treatment-Observed Counties	12	18	12	18
Eventually-Treated Counties	16	22	17	23
Never-Treated Counties	0	0	1126	1126
Total Counties	16	22	1142	1148
Panel B: Proportion of Felony Charges Convicted				
ATT	-0.0254 * (0.0100)	-0.0323 (0.0237)	0.0206 (0.0542)	-0.0156 (0.0386)
Pre-Treatment Mean	0.485	0.499	0.485	0.499
Percentage Effect	-5.233	-6.461	4.251	-3.119
Treatment-Observed Counties	12	18	12	18
Eventually-Treated Counties	15	21	16	22
Never-Treated Counties	0	0	1002	1004
Total Counties	15	21	1017	1025

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. Charging and conviction data are available for only one county per jurisdiction. Column 1 includes only the eventually-treated counties that are available both in the CJARS data on charges and convictions and in the local-reported-crime sample, while column 2 includes all eventually-treated counties available in the CJARS charges and convictions data. Columns 3 and 4 include the eventually-treated counties of columns 1 and 2, respectively, plus all never-treated counties with available charge and conviction data. The analysis is at the county-year level, and results 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.

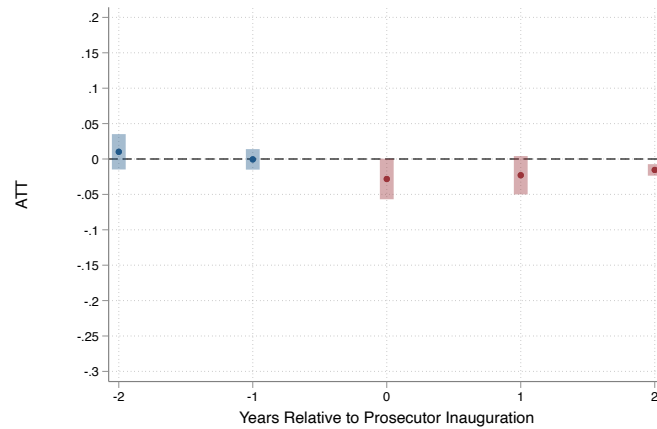
Figure 3: Felony Charging Rates



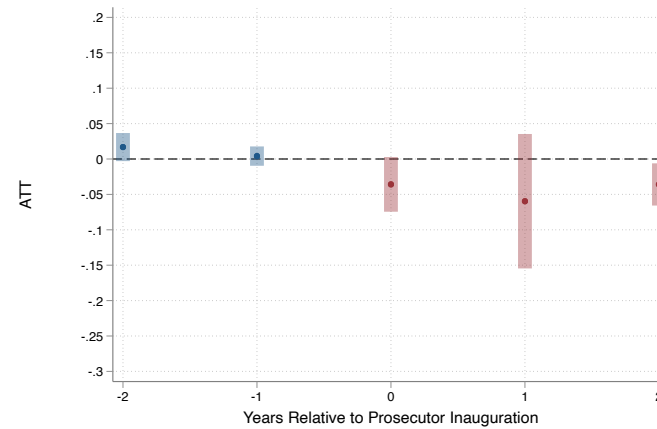
Notes. Event study of the effect of prosecutors' inauguration on felony charges per 100,000 residents by year. Time 0 is the inauguration year. Figure generated by the csdid Stata package, using the event option and weighting by population.

Figure 4: Proportion of Felony Charges Convicted

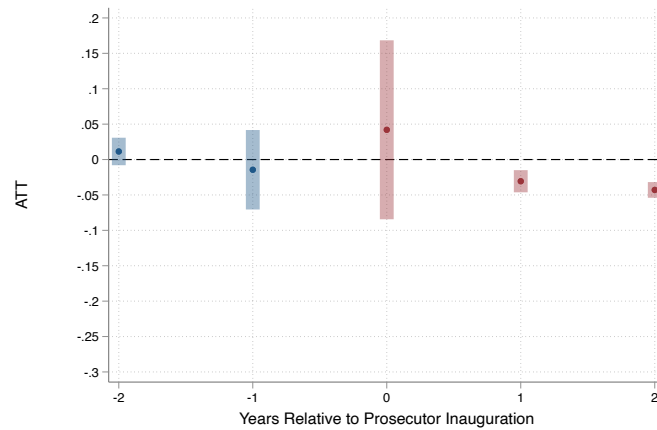
(a) Local Crime Sample, Eventually-Treated Only



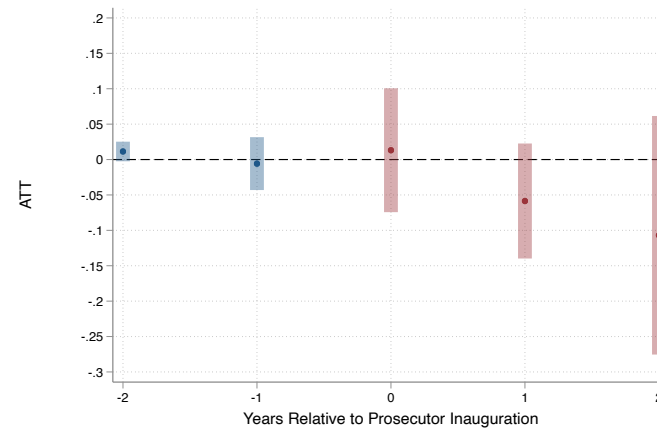
(b) Expanded Sample, Eventually-Treated Only



(c) Local Crime Sample, incl. Never-Treated



(d) Expanded Sample, incl. Never-Treated



Notes. Event study of the effect of prosecutors' inauguration on proportion of felony charges convicted. Time 0 is the inauguration year, and conviction outcomes are among cases initially charged in a given year. Figure generated by the csdid Stata package, using the event option and weighting by population.

Table 7: Effect of reform-minded prosecutors on felony charges by offense type

	(1)	(2)	(3)	(4)
	Local Crime Sample	Full Sample	Local Crime Sample	Full Sample
Panel A: Violent Felony Charges per 100,000 Residents				
ATT	-18.25 (15.37)	26.69 (20.51)	-40.37 (34.91)	-5.487 (16.25)
Pre-Treatment Mean	323.9	319.0	323.9	319.0
Percentage Effect	-5.633	8.366	-12.46	-1.720
Treatment-Observed Counties	12	18	12	18
Eventually-Treated Counties	16	22	17	23
Never-Treated Counties	0	0	1126	1126
Total Counties	16	22	1142	1148
Panel B: Felony Property Charges per 100,000 Residents				
ATT	-40.57 (38.57)	-10.88 (37.93)	-56.70 (41.68)	-42.38 (41.07)
Pre-Treatment Mean	468.1	511.5	468.1	511.5
Percentage Effect	-8.667	-2.127	-12.11	-8.285
Treatment-Observed Counties	12	18	12	18
Eventually-Treated Counties	16	22	17	23
Never-Treated Counties	0	0	1126	1126
Total Counties	16	22	1142	1148
Panel C: Felony Drug Charges per 100,000 Residents				
ATT	-280.6 (233.5)	1.850 (76.78)	-168.9 (118.1)	-29.03 (50.65)
Pre-Treatment Mean	409.7	439.7	409.7	439.7
Percentage Effect	-68.50	0.421	-41.23	-6.602
Treatment-Observed Counties	12	18	12	18
Eventually-Treated Counties	16	22	17	23
Never-Treated Counties	0	0	1126	1126
Total Counties	16	22	1142	1148

Notes. The dependent variables in Panel A, B, and C are the felony charging rate per 100,000 residents for violent, property, and drugs offenses, respectively. Charging data are available for only one county per jurisdiction. Column 1 includes only the eventually-treated counties that are available both in the CJARS data on charges and convictions and in the local-reported-crime sample, while Column 2 includes all eventually-treated counties available in the CJARS charges data. Columns 3 and 4 include the eventually-treated counties of columns 1 and 2, respectively, plus all never-treated counties with available charge data. The analysis is at the county-year level, and results 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.

Table 8: Effect of reform-minded prosecutors on felony conviction rates by offense type

	(1)	(2)	(3)	(4)
	Local Crime Sample	Full Sample	Local Crime Sample	Full Sample
Panel A: Proportion of Violent Felony Charges Convicted				
ATT	-0.00670 (0.0153)	-0.0247 (0.0225)	0.0370 (0.0554)	-0.0154 (0.0438)
Pre-Treatment Mean	0.415	0.443	0.415	0.443
Percentage Effect	-1.613	-5.586	8.912	-3.488
Treatment-Observed Counties	12	18	12	18
Eventually-Treated Counties	15	21	16	22
Never-Treated Counties	0	0	814	816
Total Counties	15	21	829	837
Panel B: Proportion of Felony Property Charges Convicted				
ATT	-0.00790 (0.0105)	-0.0171 (0.0117)	0.0372 (0.0570)	-0.00135 (0.0348)
Pre-Treatment Mean	0.501	0.507	0.501	0.507
Percentage Effect	-1.578	-3.373	7.421	-0.267
Treatment-Observed Counties	12	18	12	18
Eventually-Treated Counties	15	21	16	22
Never-Treated Counties	0	0	889	891
Total Counties	15	21	904	912
Panel C: Proportion of Felony Drug Charges Convicted				
ATT	-0.0438 (0.0323)	-0.0378 (0.0372)	0.00759 (0.0593)	-0.0146 (0.0383)
Pre-Treatment Mean	0.508	0.519	0.508	0.519
Percentage Effect	-8.621	-7.288	1.494	-2.808
Treatment-Observed Counties	12	18	12	18
Eventually-Treated Counties	15	21	16	22
Never-Treated Counties	0	0	909	911
Total Counties	15	21	924	932

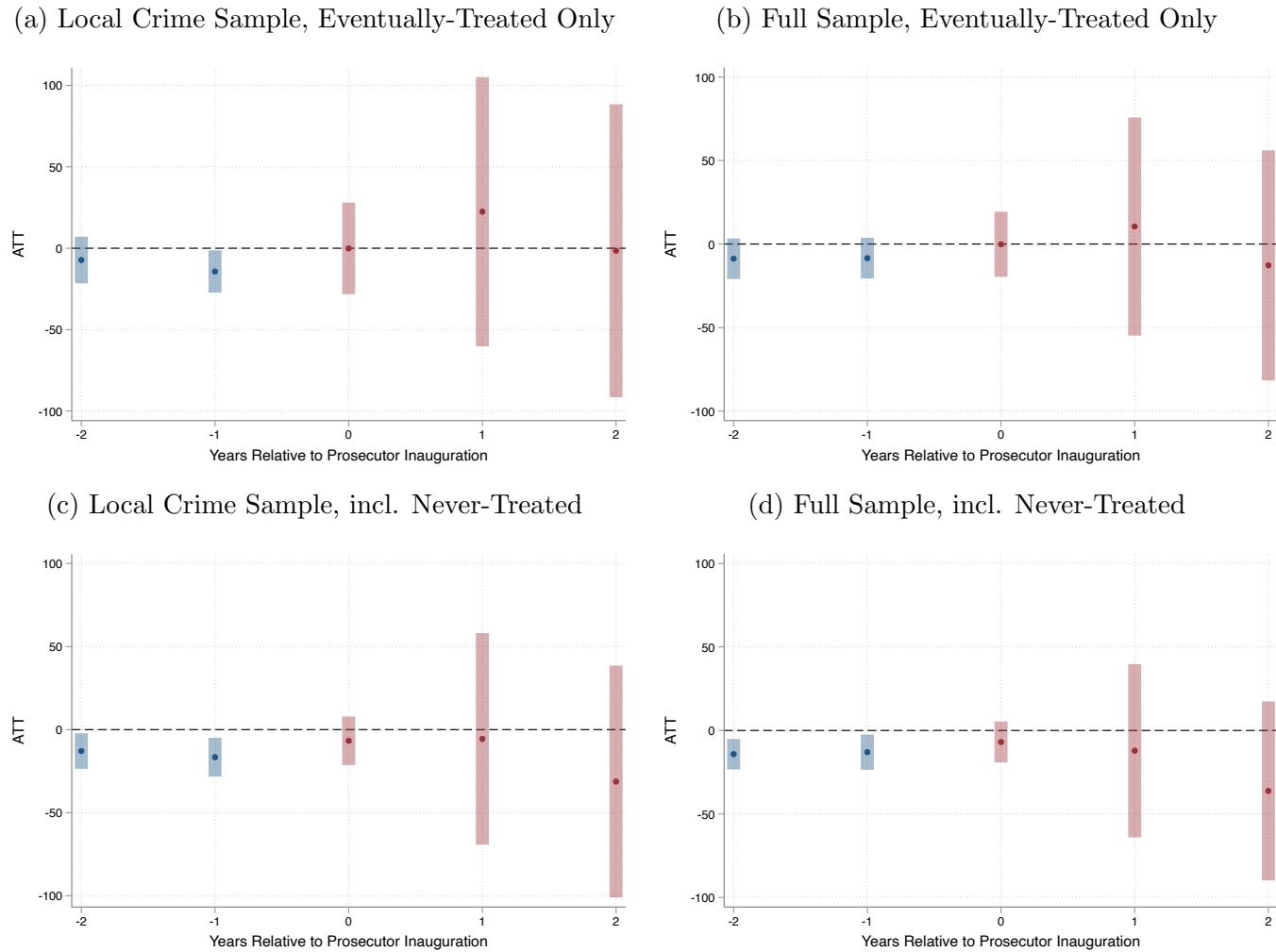
Notes. The dependent variables in Panel A, B, and C are the share of felony charges resulting in a conviction for violent, property, and drugs offenses, respectively, measured as of the year the case was initially charged. Conviction data are available for only one county per jurisdiction. Column 1 includes only the eventually-treated counties that are available both in the CJARS data on convictions and in the local-reported-crime sample, while column 2 includes all eventually-treated counties available in the CJARS charges and convictions data. Columns 3 and 4 include the eventually-treated counties of columns 1 and 2, respectively, plus all never-treated counties with available conviction data. The analysis is at the county-year level, and results 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.

Table 9: Effect of reform-minded prosecutors on prison incarceration rates

	(1)	(2)	(3)	(4)
	Local Crime Sample	Full Sample	Local Crime Sample	Full Sample
Incarceration Rate per 100,000 Residents				
ATT	-27.62 (24.41)	-24.23 (37.21)	-61.53 (46.18)	-55.58 ⁺ (33.64)
Pre-Treatment Mean	545.8	547.6	545.8	547.6
Percentage Effect	-5.059	-4.424	-11.27	-10.15
Treatment-Observed Jurisdictions	14	20	14	20
Treatment-Observed Counties	21	29	21	29
Eventually-Treated Jurisdictions	17	24	17	24
Eventually-Treated Counties	24	33	24	33
Never-Treated Counties	0	0	1176	1176
Total Clusters	17	24	1193	1200
Total Counties	24	33	1200	1209

Notes. The dependent variable is the prison incarceration rate per 100,000 residents. Prison incarceration data are available for multiple counties within prosecutors' multi-county jurisdictions. Column 1 includes only the eventually-treated counties that are available both in the CJARS data on prison incarceration rates and in the local-reported-crime sample, while column 2 includes all eventually-treated counties available in the CJARS prison incarceration data. Columns 3 and 4 include the eventually-treated counties of columns 1 and 2, respectively, plus all never-treated counties with available prison incarceration data. The analysis is at the county-year level, and results are estimated using the [Callaway and Sant'Anna \(2021\)](#) estimator. Standard errors, clustered at the jurisdiction level for treated counties, are reported in parentheses. +10%, *5%, **1%, and ***0.1% significance levels.

Figure 5: Incarceration Rates



Notes. Event study of the effect of prosecutors' inauguration on prison incarceration rates per 100,000 residents. Time 0 is the inauguration year, and all county residents in prison in a given year are included in the incarceration rate. Figure generated by the csdid Stata package, using the event option and weighting by population.

6.2 Local Reported Crime Rates

Table 10: Analysis Sample: Local Reported Crime Rates

Name	Title	Jurisdiction	State	Inauguration	Data Source	Begin	End
Wesley Bell	Prosecuting Attorney	St Louis County	Missouri	1/1/19	St. Louis County Police Department	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 Police Department	6/16	6/21
Chesa Boudin	District Attorney	City and County of San Francisco	California	1/8/20	San Francisco Police Department	1/18	6/21
Aisha Braveboy	State's Attorney	Prince George's County	Maryland	7/1/19	Prince George's County Police Department	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 County Sheriff's Office	1/19	7/22
Laura Conover	County Attorney	Pima County	Arizona	1/2/20	Tucson Police Department	1/18	3/21
Shalena Cook Jones	District Attorney	Chatham County	Georgia	1/4/21	Chatham County Police Department	2/18	6/21
John Creuzot	District Attorney	Dallas County	Texas	1/1/19	Dallas Police Department	6/14	4/21
Parisa Dehghani-Tafti	Commonwealth's Attorney	Arlington County & City of Falls Church	Virginia	12/16/19	Arlington County Police Department	1/15	6/21
Steve Descano	Commonwealth's Attorney	Fairfax County	Virginia	12/16/19	NIBRS (Fairfax County/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 Police Department	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 Police Department	2/14	12/23
Kim Gardner	Circuit Attorney	City of St Louis	Missouri	1/6/17	St. Louis Metropolitan Police Department	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 Police Department	12/11	3/21
Mark Gonzalez	District Attorney	Nueces County	Texas	1/12/17	Corpus Christi Police Department	10/14	7/21
Andrea Harrington	District Attorney	Berkshire County	Massachusetts	1/2/19	Pittsfield Police Department	1/17	4/21
Jim Hingeley	Commonwealth's Attorney	Albemarle County	Virginia	12/17/19	NIBRS (Albemarle County)	1/05	12/16
Alexis King	District Attorney	First Judicial District	Colorado	1/12/21	Golden Police Department	10/19	5/21
Larry Krasner	District Attorney	Philadelphia	Pennsylvania	1/2/18	Philadelphia Police Department	1/16	12/20
Beth McCann	District Attorney	Second Judicial District	Colorado	1/10/17	Denver Police Department	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 Police Department	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 Police Department	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 Police Department	1/19	8/21
Rachael Rollins	District Attorney	Suffolk County	Massachusetts	1/2/19	Boston Police Department	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 Police Department	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 County Sheriff's Office	1/19	7/21
Monique Worrell	State Attorney	9th Judicial Circuit	Florida	1/5/21	Orlando Police Department	1/19	9/21

Table 11: Summary Statistics: Local Reported Crime Rates

	Mean	Std. Dev.	Min	Max	N
Population	571914.89	565337.45	16809.00	2745196.00	3267
Homicide Rate	0.10	0.16	0.00	1.90	3215
Assault Rate	8.18	5.48	0.00	28.40	3251
Robbery Rate	1.28	1.62	0.00	9.79	3173
Violent Offense Rate	9.85	6.75	0.00	33.93	3114
Burglary Rate	3.93	3.75	0.00	27.68	3242
Theft Rate	15.96	10.71	0.02	60.67	3262
Vandalism Rate	6.14	3.83	0.00	24.17	2883
Property Offense Rate	26.34	15.54	0.13	82.94	2883
Drug Offense Rate	3.04	2.23	0.00	18.20	2935

Notes. Local reported crime rate data cover 39 jurisdictions. For 26 jurisdictions, data were collected from local police departments (FOIA/online portals); for 13, from the FBI's NIBRS. Reported crime rates are offenses per 10,000 residents per month. The panel is unbalanced; some jurisdictions lack certain offenses or years. Observations are at the jurisdiction-month level.

Table 12: Effect of reform-minded prosecutors on reported local crime rates, by pooled crime category

	(1) All Offenses	(2) Violent	(3) Property	(4) Income-Generating	(5) Non-Income
Reported incidents per 10,000 residents per month					
ATT	-4.304 (3.476)	-0.139 (0.557)	-3.751 (3.076)	-2.937 (2.286)	-0.735 (0.761)
Jurisdictions	29	36	31	36	31
Pre-Inauguration Mean	37.08	9.769	23.46	18.75	14.55
Percentage Effect	-11.61	-1.419	-15.99	-15.66	-5.049
95% CI (Percentage Effect)	[-29.98, 6.76]	[-12.6, 9.76]	[-41.68, 9.7]	[-39.56, 8.24]	[-15.3, 5.2]
Observations	2249	2555	2344	2579	2332

Notes. The dependent variables in columns 1 to 5 are the overall reported crime rate (homicide, assault, robbery, burglary, theft, vandalism, and drug offenses), followed by the crime rate of 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 samples in each column include only jurisdiction-month observations for which all relevant offense types for the dependent variables are reported in the data. The analysis is at the jurisdiction-month level, and results 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 13: Effect of reform-minded prosecutors on reported local crime rates, by offense type

	(1) Homicide	(2) Assault	(3) Robbery	(4) Burglary	(5) Theft	(6) Vandalism	(7) Drug
Reported incidents per 10,000 residents per month							
ATT	0.0131 (0.0327)	0.367 (0.362)	-0.496 ⁺ (0.255)	-0.234 (0.493)	-2.163 (1.826)	-0.870 (0.606)	0.0889 (0.233)
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 (Pct. Effect)	[-51.78, 78.48]	[-4.28, 13.42]	[-73.2, .32]	[-34.6, 21.12]	[-40.92, 10.1]	[-33.36, 5.14]	[-11.2, 16.6]
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, respectively. The analysis is at the jurisdiction-month level, and results 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 14: Effect of reform prosecutors on reported local crime rates,
event-balanced panels by pooled crime category

	(1) All Offenses	(2) Violent	(3) Property	(4) Income-Generating	(5) Non-Income
Panel A: Offenses per 10,000 residents per month, Event-Balanced [-6,6]					
ATT	-1.833 (1.468)	-0.317 (0.346)	-2.331 (1.390)	-1.653 (1.277)	-0.431 (0.443)
Jurisdictions	24	31	27	32	26
Pre-Inauguration Mean	35.58	8.453	24.38	19.56	13.02
Percentage Effect	-5.152	-3.754	-9.562	-8.450	-3.313
95% CI (Percentage Effect)	[-13.68, 3.38]	[-12.1, 4.6]	[-21.28, 2.16]	[-21.77, 4.87]	[-10.33, 3.7]
Observations	897	1144	975	1170	949
Panel B: Offenses per 10,000 residents per month, Event-Balanced [-12,12]					
ATT	-0.522 (1.474)	-0.273 (0.409)	-1.849 (1.927)	-1.392 (1.791)	-0.663 (0.603)
Jurisdictions	20	25	23	27	21
Pre-Inauguration Mean	35.42	8.307	23.78	19.36	12.82
Percentage Effect	-1.475	-3.283	-7.778	-7.187	-5.170
95% CI (Percentage Effect)	[-10.18, 7.23]	[-13.45, 6.89]	[-24.58, 9.03]	[-26.2, 11.83]	[-14.98, 4.64]
Observations	1475	1800	1575	1850	1525

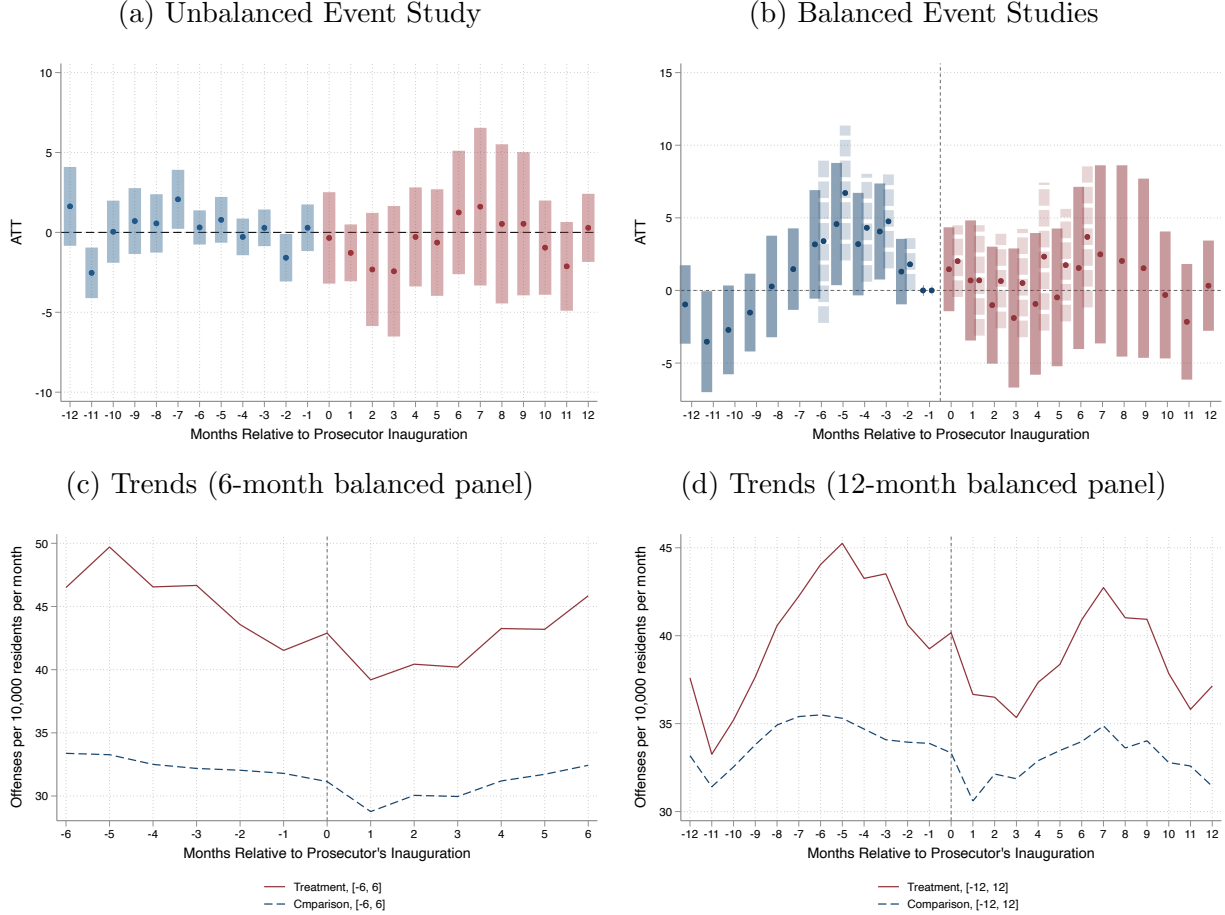
Notes. The dependent variables in columns 1 to 5 are the overall reported crime rate (homicide, assault, robbery, burglary, theft, vandalism, and drug offenses), followed by the crime rate of violent (homicide, assault and robbery), property (burglary, theft, and vandalism), income-generating (robbery, theft, and burglary), and non-income generating (homicide, assault, and vandalism) offenses, respectively. The samples in each column include only jurisdiction-month observations for which all relevant offense types for the dependent variables are reported in the data. The analysis is at the jurisdiction-month level, and results are estimated using the stacked difference-in-differences estimator in [Wing et al. \(2024\)](#) on event-balanced panels covering the six months (Panel A) or 12 months (Panel B) before and after a prosecutor's inauguration. Standard errors, clustered at the jurisdiction level, are reported in parentheses. +10%, *5%, **1%, and ***0.1% significance levels.

Table 15: Effect of reform prosecutors on reported local crime rates, event-balanced panels by offense type

	(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 (0.0123)	-0.184 (0.259)	-0.127 (0.0999)	-0.441 (0.460)	-1.535 (0.987)	-0.148 (0.219)	0.0686 (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 (Percentage Effect)	[-28.29, 32.94]	[-10.43, 5.03]	[-26.35, 6.15]	[-30.14, 10.84]	[-24.95, 3.32]	[-10.69, 5.4]	[-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 (0.0129)	-0.261 (0.289)	0.0526 (0.0989)	-0.189 (0.655)	-1.405 (1.131)	-0.311 (0.328)	-0.0331 (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 (Percentage Effect)	[-21.11, 49.94]	[-12.61, 4.9]	[-13.08, 22.21]	[-33.33, 25.1]	[-26.08, 6.39]	[-18.46, 6.89]	[-19.32, 17.4]
Observations	2025	2075	1850	2075	2075	1575	1600

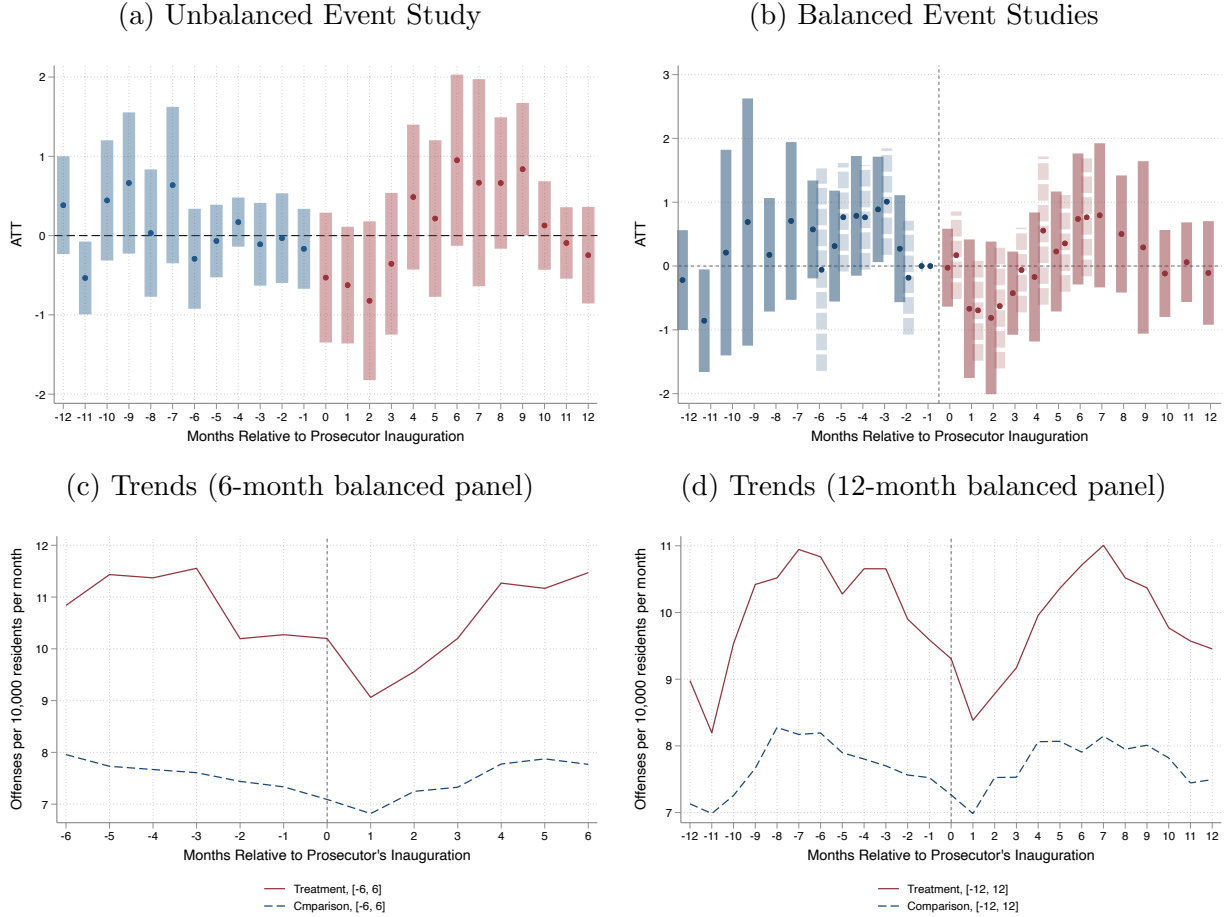
Notes. The dependent variables in columns 1 to 7 are the reported crime rate for homicide, assault, robbery, burglary, theft, vandalism, and drug offenses, respectively. The analysis is at the jurisdiction-month level, and results are estimated using the stacked difference-in-differences estimator in [Wing et al. \(2024\)](#) on event-balanced panels covering the six months (Panel A) or 12 months (Panel B) before and after a prosecutor's inauguration. Standard errors, clustered at the jurisdiction level, are reported in parentheses. +10%, *5%, **1%, and ***0.1% significance levels.

Figure 6: Overall Reported Crime Rate (All Offenses)



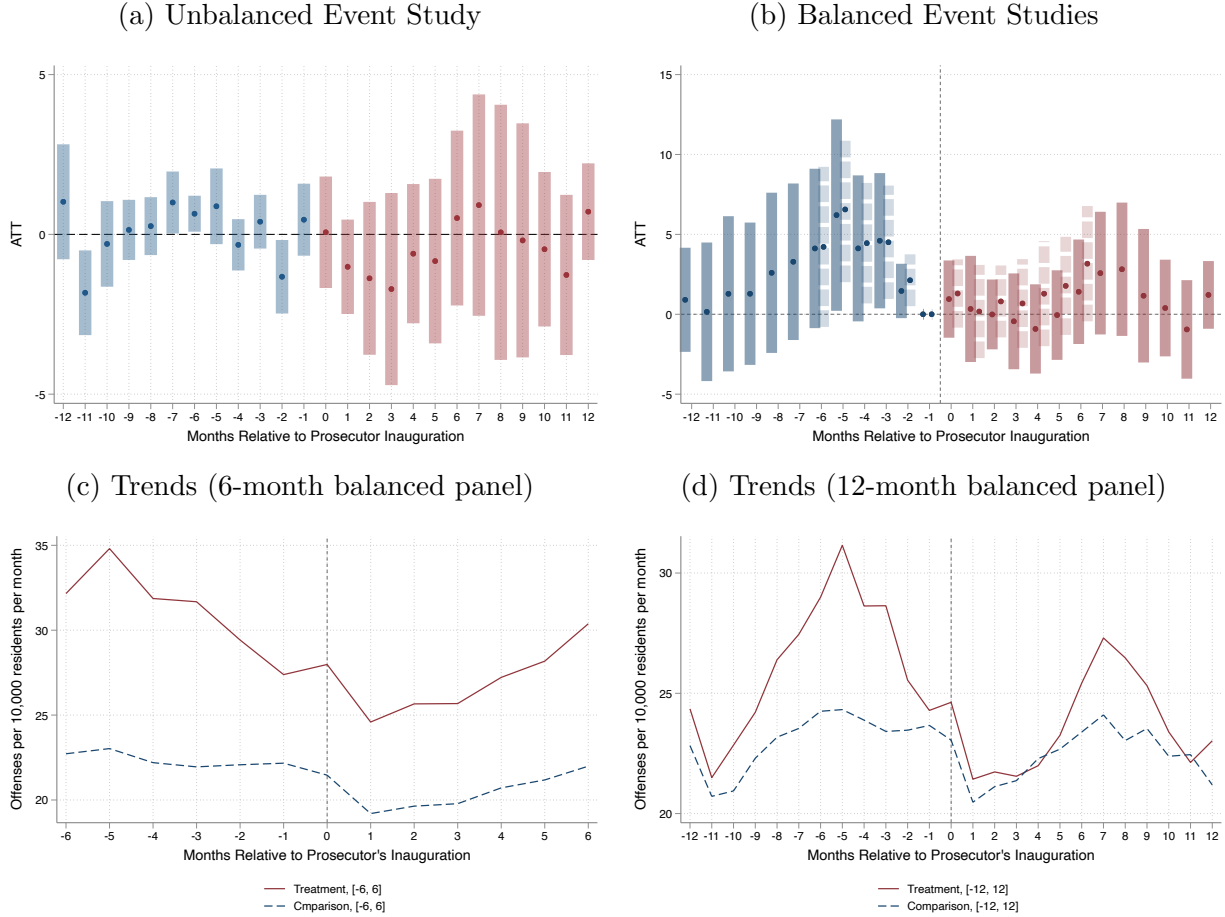
Notes. Panels (a) and (b) present event studies of the effect of prosecutors' inauguration on overall reported crime rates (homicide, assault, robbery, burglary, theft, vandalism, and drug offenses) by month. Includes only jurisdiction-month observations where all offense types are reported in the data. Time 0 is the inauguration month. Rates are reported incidents per 10,000 residents per month. Unbalanced event study in panel (a) generated by the `csdid` Stata package (Callaway and Sant'Anna, 2021), using the event option and weighting by population. Balanced event studies in panel (b) estimated using the stacked event-study method in Wing et al. (2024) and weighting by population, with the lighter dashed confidence intervals representing an event-balanced sample for months [-6, 6] and darker solid confidence intervals representing an event-balanced sample for months [-12, 12] around the prosecutor's inauguration. Panels (c) and (d) present outcome trends for the stacked, population-weighted, event-balanced samples used in panel (b).

Figure 7: Violent Offense Rate



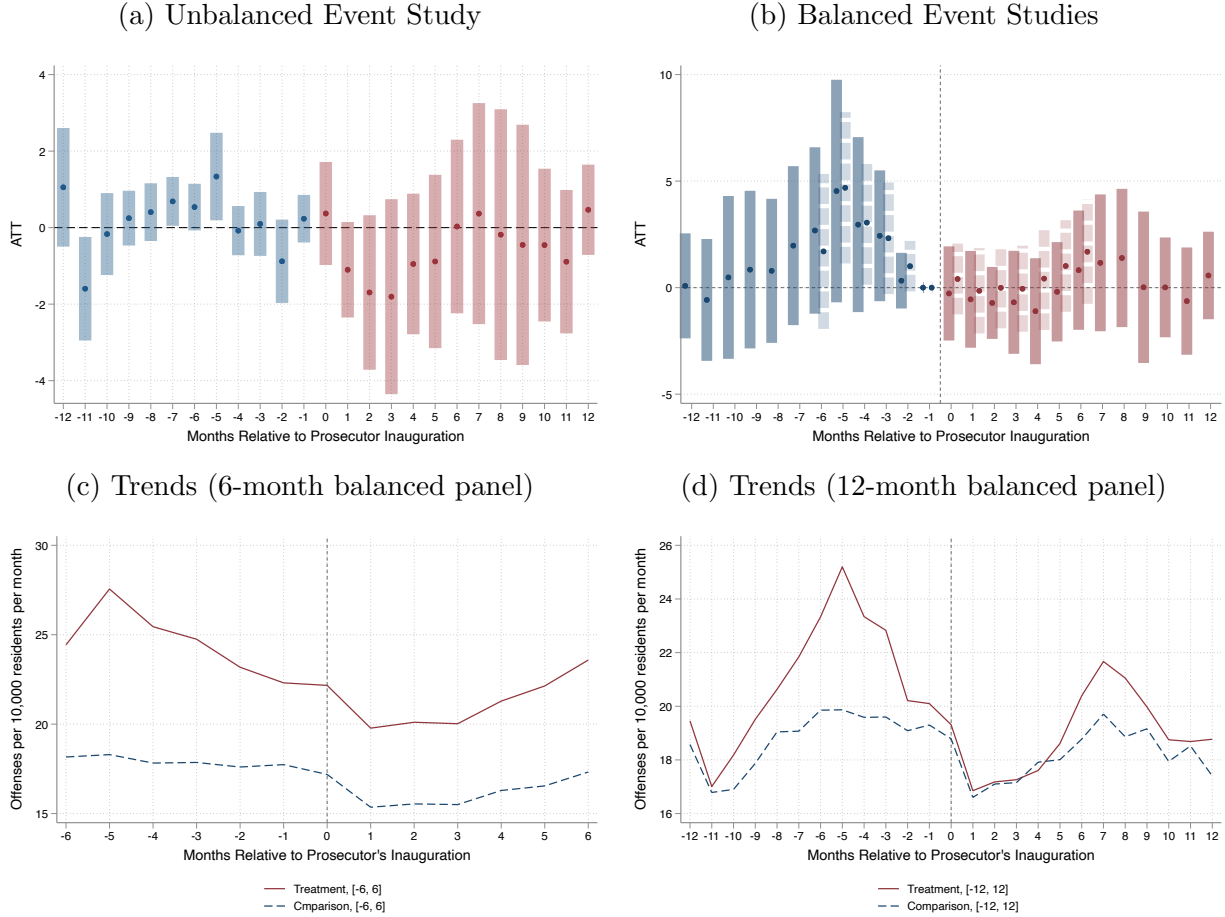
Notes. Panels (a) and (b) present event studies of the effect of prosecutors' inauguration on violent offense (homicide, assault, and robbery) rates by month. Includes only jurisdiction-month observations where homicide, assault, and robbery offenses are all reported in the data. Time 0 is the inauguration month. Rates are reported incidents per 10,000 residents per month. Unbalanced event study in panel (a) generated by the `csdid` Stata package (Callaway and Sant'Anna, 2021), using the event option and weighting by population. Balanced event studies in panel (b) estimated using the stacked event-study method in Wing et al. (2024) and weighting by population, with the lighter dashed confidence intervals representing an event-balanced sample for months [-6, 6] and darker solid confidence intervals representing an event-balanced sample for months [-12, 12] around the prosecutor's inauguration. Panels (c) and (d) present outcome trends for the stacked, population-weighted, event-balanced samples used in panel (b).

Figure 8: Property Offense Rate



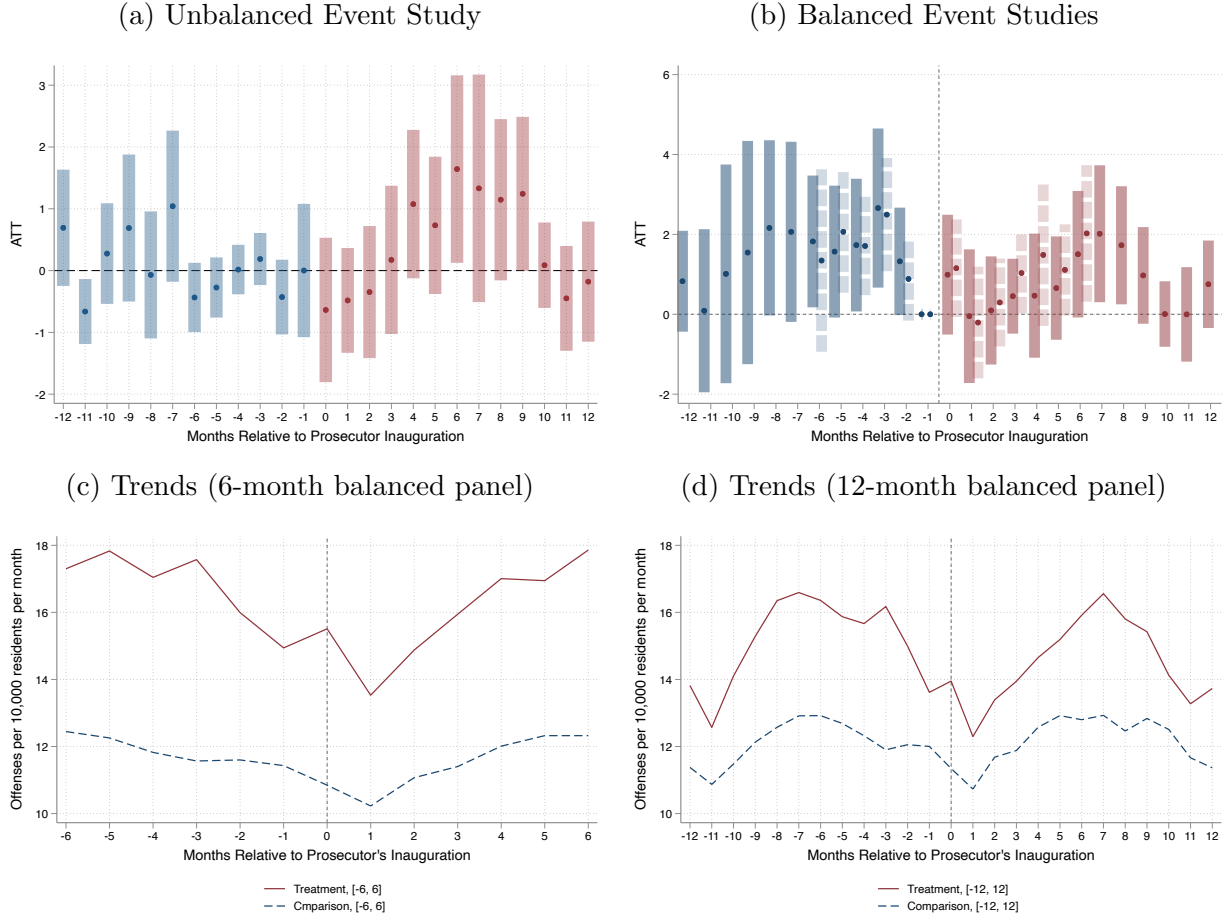
Notes. Event study of the effect of prosecutors' inauguration on property offense (burglary, theft, and vandalism) rates, by month. Time 0 is the inauguration month. Includes only jurisdiction-month observations where burglary, theft, and vandalism offenses are all reported in the data. Rates are reported incidents per 10,000 residents per month. Unbalanced event study in panel (a) generated by the `csdid` Stata package (Callaway and Sant'Anna, 2021), using the event option and weighting by population. Balanced event studies in panel (b) estimated using the stacked event-study method in Wing et al. (2024) and weighting by population, with the lighter dashed confidence intervals representing an event-balanced sample for months [-6, 6] and darker solid confidence intervals representing an event-balanced sample for months [-12, 12] around the prosecutor's inauguration. Panels (c) and (d) present outcome trends for the stacked, population-weighted, event-balanced samples used in panel (b).

Figure 9: Income-Generating Offense Rate



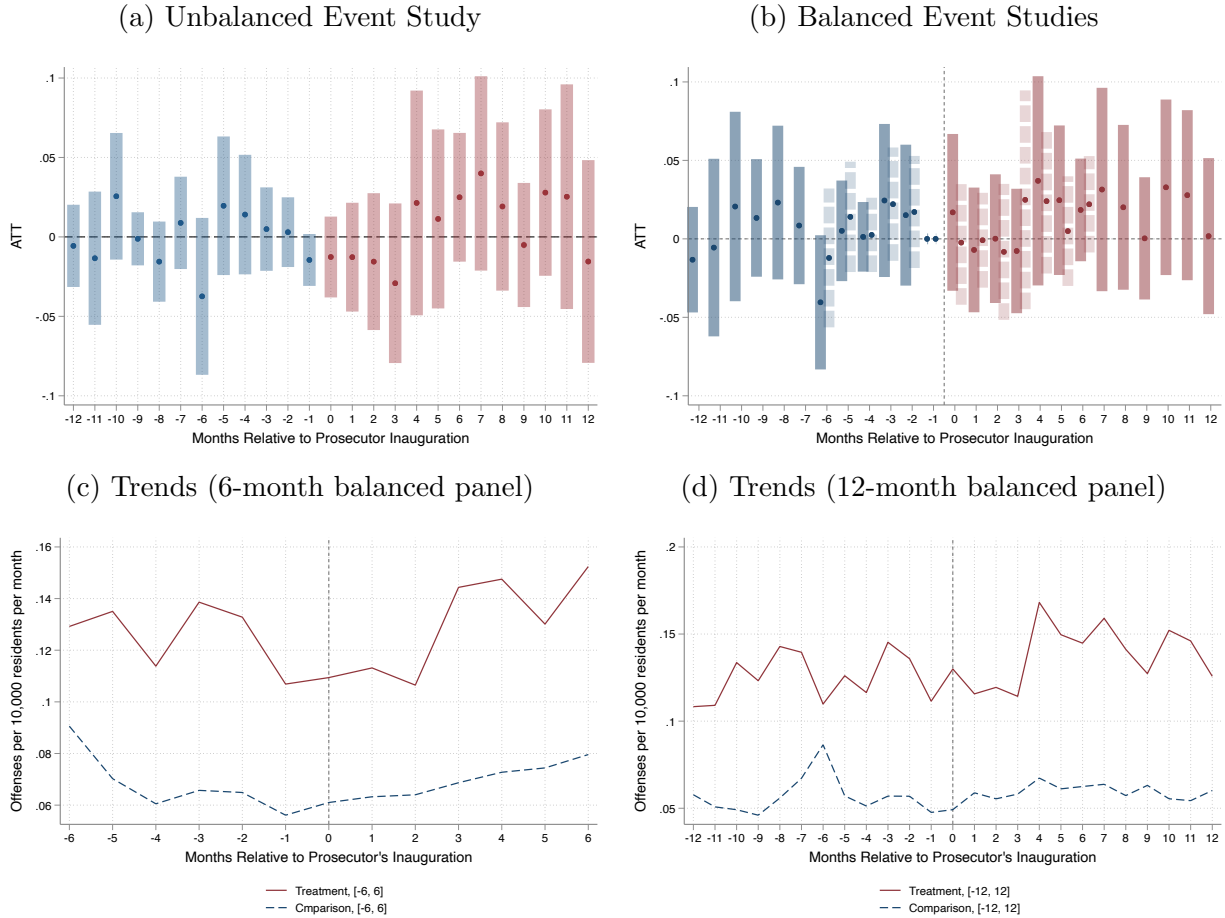
Notes. Event study of the effect of prosecutors' inauguration on income-generating offense (robbery, burglary, and theft) rates, by month. Time 0 is the inauguration month. Includes only jurisdiction-month observations where robbery, burglary, and theft offenses are all reported in the data. Rates are reported incidents per 10,000 residents per month. Unbalanced event study in panel (a) generated by the `csdid` Stata package (Callaway and Sant'Anna, 2021), using the event option and weighting by population. Balanced event studies in panel (b) estimated using the stacked event-study method in Wing et al. (2024) and weighting by population, with the lighter dashed confidence intervals representing an event-balanced sample for months [-6, 6] and darker solid confidence intervals representing an event-balanced sample for months [-12, 12] around the prosecutor's inauguration. Panels (c) and (d) present outcome trends for the stacked, population-weighted, event-balanced samples used in panel (b).

Figure 10: Non-Income-Generating Offense Rate



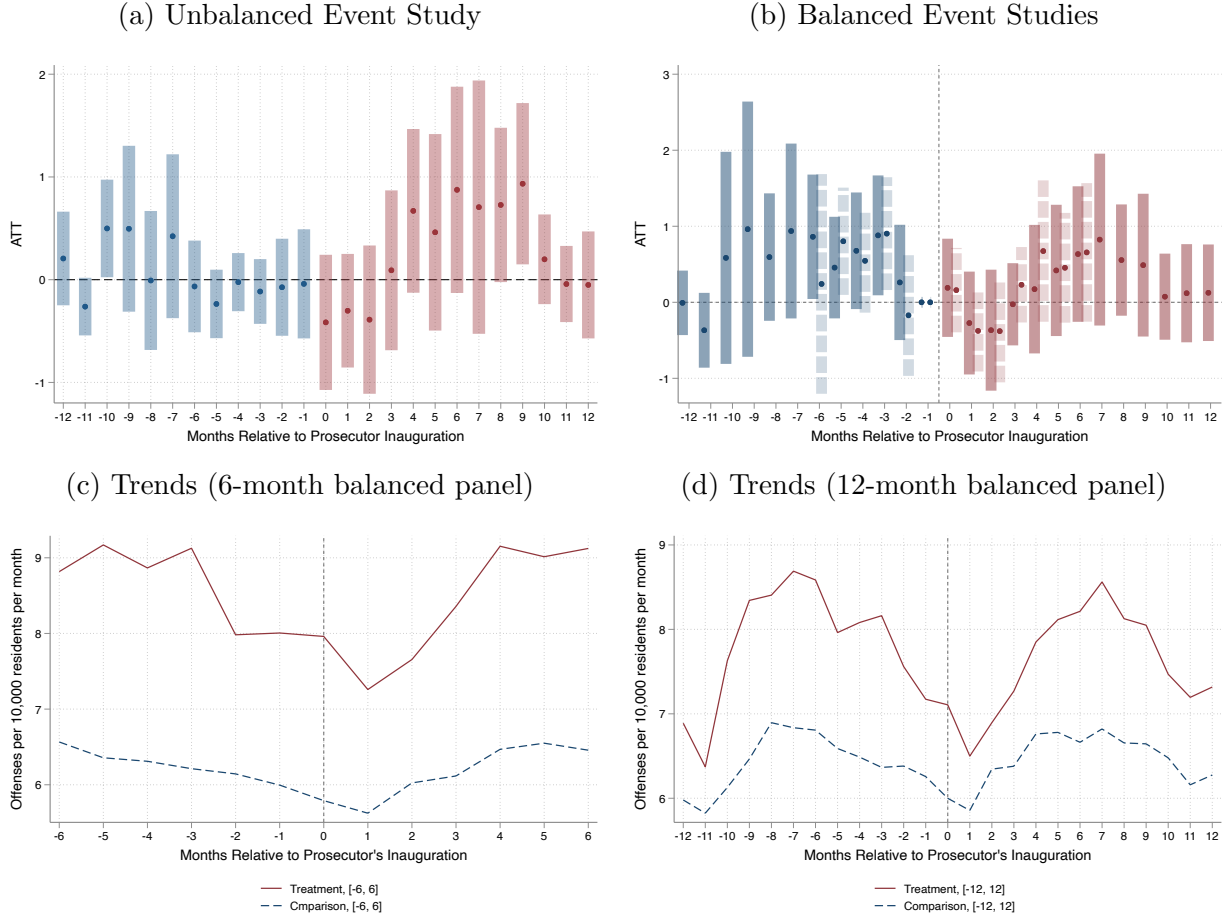
Notes. Event study of the effect of prosecutors' inauguration on non-income-generating offense (homicide, assault, and vandalism) rates, by month. Time 0 is the inauguration month. Includes only jurisdiction-month observations where homicide, assault, and vandalism offenses are all reported in the data. Rates are reported incidents per 10,000 residents per month. Unbalanced event study in panel (a) generated by the `csdid` Stata package (Callaway and Sant'Anna, 2021), using the event option and weighting by population. Balanced event studies in panel (b) estimated using the stacked event-study method in Wing et al. (2024) and weighting by population, with the lighter dashed confidence intervals representing an event-balanced sample for months [-6, 6] and darker solid confidence intervals representing an event-balanced sample for months [-12, 12] around the prosecutor's inauguration. Panels (c) and (d) present outcome trends for the stacked, population-weighted, event-balanced samples used in panel (b).

Figure 11: Homicide Rate



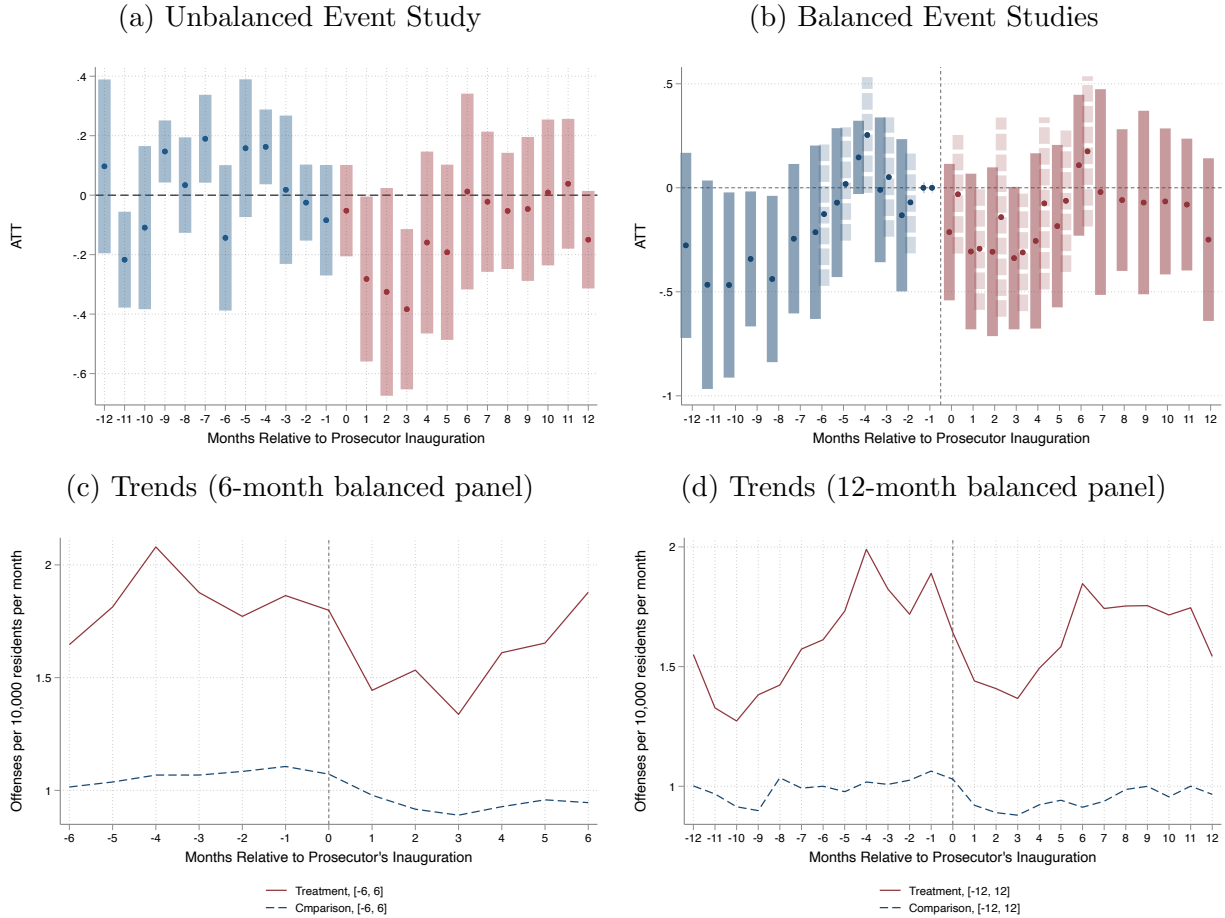
Notes. Event study of the effect of prosecutors' inauguration on homicide rates, by month. Time 0 is the inauguration month. Rates are reported incidents per 10,000 residents per month. Unbalanced event study in panel (a) generated by the `csdid` Stata package (Callaway and Sant'Anna, 2021), using the event option and weighting by population. Balanced event studies in panel (b) estimated using the stacked event-study method in Wing et al. (2024) and weighting by population, with the lighter dashed confidence intervals representing an event-balanced sample for months [-6, 6] and darker solid confidence intervals representing an event-balanced sample for months [-12, 12] around the prosecutor's inauguration. Panels (c) and (d) present outcome trends for the stacked, population-weighted, event-balanced samples used in panel (b).

Figure 12: Assault Rate



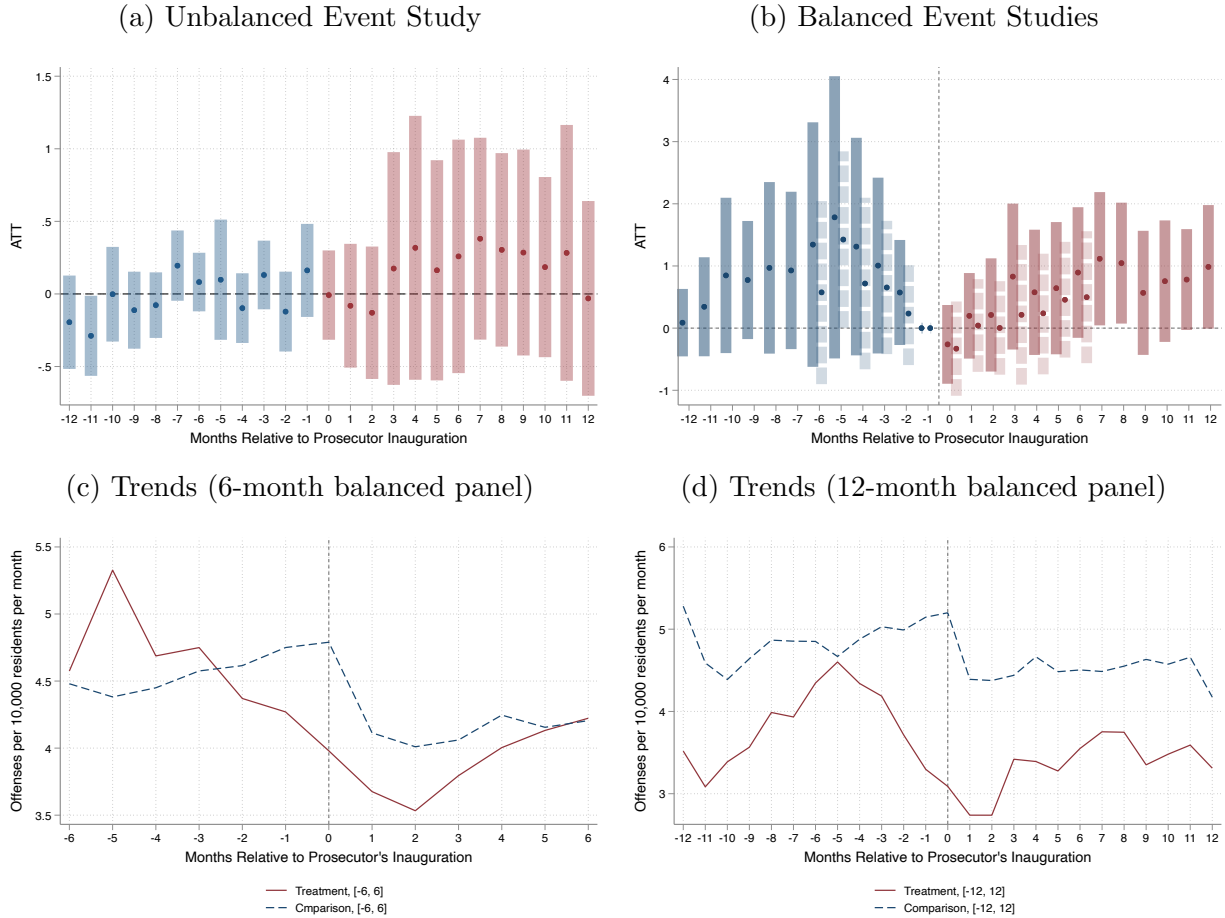
Notes. Event study of the effect of prosecutors' inauguration on assault rates, by month. Time 0 is the inauguration month. Rates are reported incidents per 10,000 residents per month. Unbalanced event study in panel (a) generated by the `csdid` Stata package (Callaway and Sant'Anna, 2021), using the event option and weighting by population. Balanced event studies in panel (b) estimated using the stacked event-study method in Wing et al. (2024) and weighting by population, with the lighter dashed confidence intervals representing an event-balanced sample for months [-6, 6] and darker solid confidence intervals representing an event-balanced sample for months [-12, 12] around the prosecutor's inauguration. Panels (c) and (d) present outcome trends for the stacked, population-weighted, event-balanced samples used in panel (b).

Figure 13: Robbery Rate



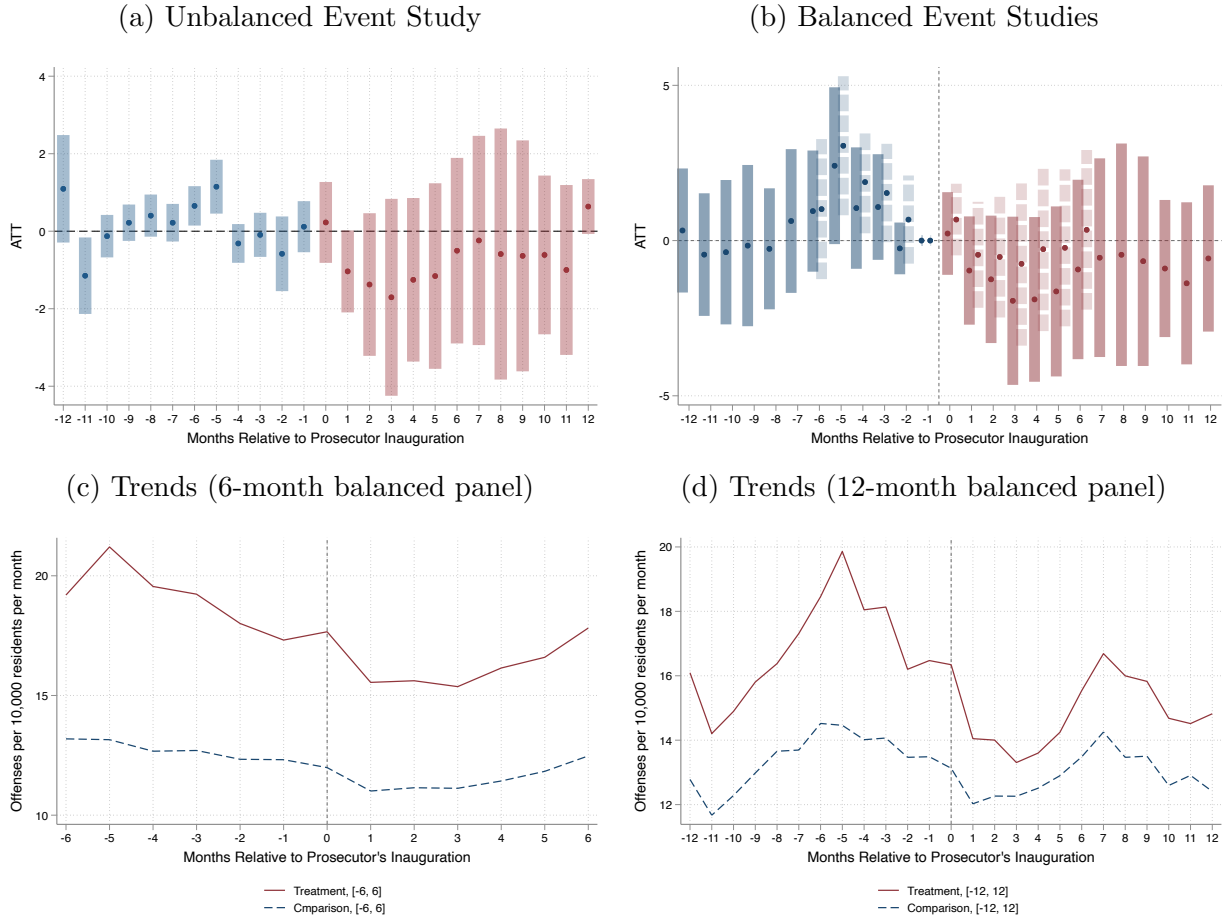
Notes. Event study of the effect of prosecutors' inauguration on robbery rates, by month. Time 0 is the inauguration month. Rates are reported incidents per 10,000 residents per month. Unbalanced event study in panel (a) generated by the `csdid` Stata package (Callaway and Sant'Anna, 2021), using the event option and weighting by population. Balanced event studies in panel (b) estimated using the stacked event-study method in Wing et al. (2024) and weighting by population, with the lighter dashed confidence intervals representing an event-balanced sample for months [-6, 6] and darker solid confidence intervals representing an event-balanced sample for months [-12, 12] around the prosecutor's inauguration. Panels (c) and (d) present outcome trends for the stacked, population-weighted, event-balanced samples used in panel (b).

Figure 14: Burglary Rate



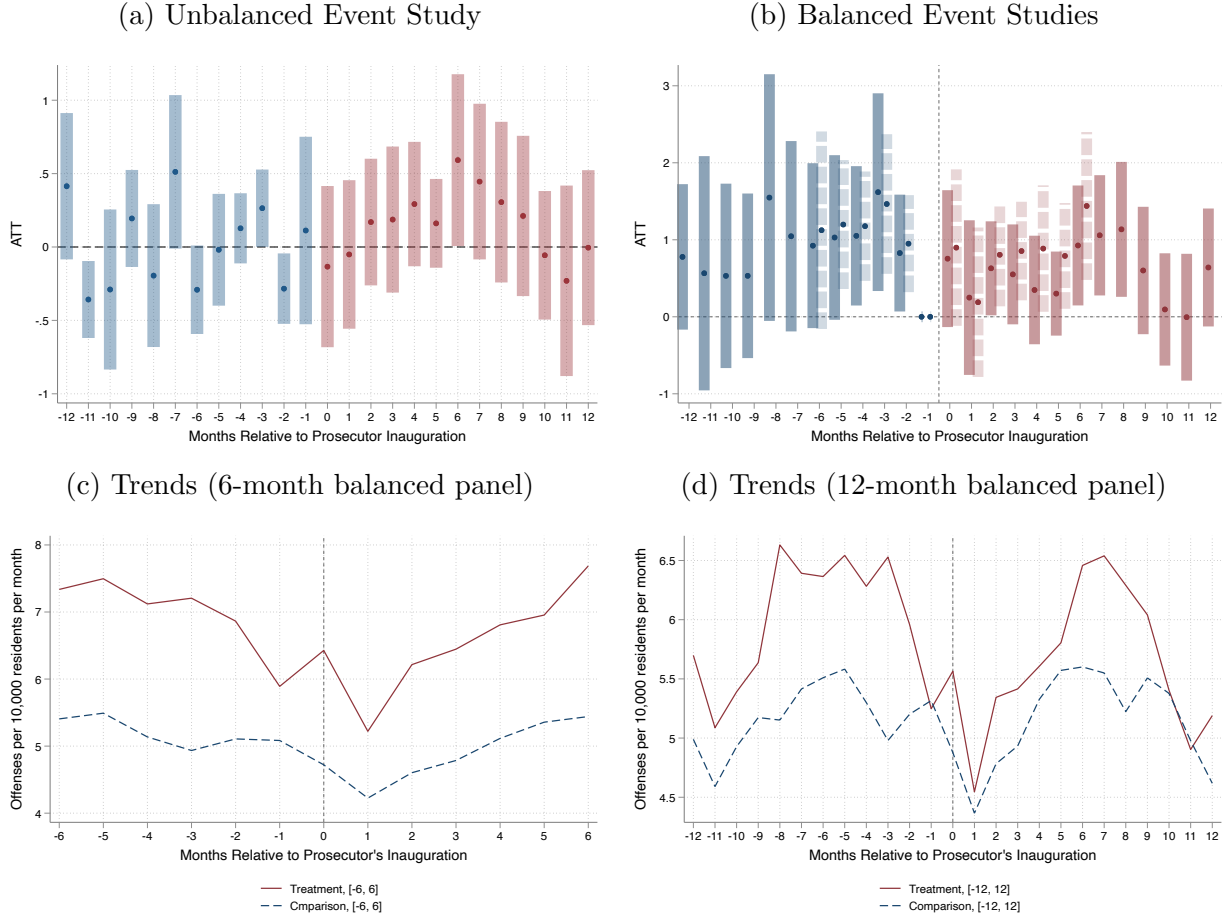
Notes. Event study of the effect of prosecutors' inauguration on burglary rates, by month. Time 0 is the inauguration month. Rates are reported incidents per 10,000 residents per month. Unbalanced event study in panel (a) generated by the `csdid` Stata package (Callaway and Sant'Anna, 2021), using the event option and weighting by population. Balanced event studies in panel (b) estimated using the stacked event-study method in Wing et al. (2024) and weighting by population, with the lighter dashed confidence intervals representing an event-balanced sample for months [-6, 6] and darker solid confidence intervals representing an event-balanced sample for months [-12, 12] around the prosecutor's inauguration. Panels (c) and (d) present outcome trends for the stacked, population-weighted, event-balanced samples used in panel (b).

Figure 15: Theft Rate



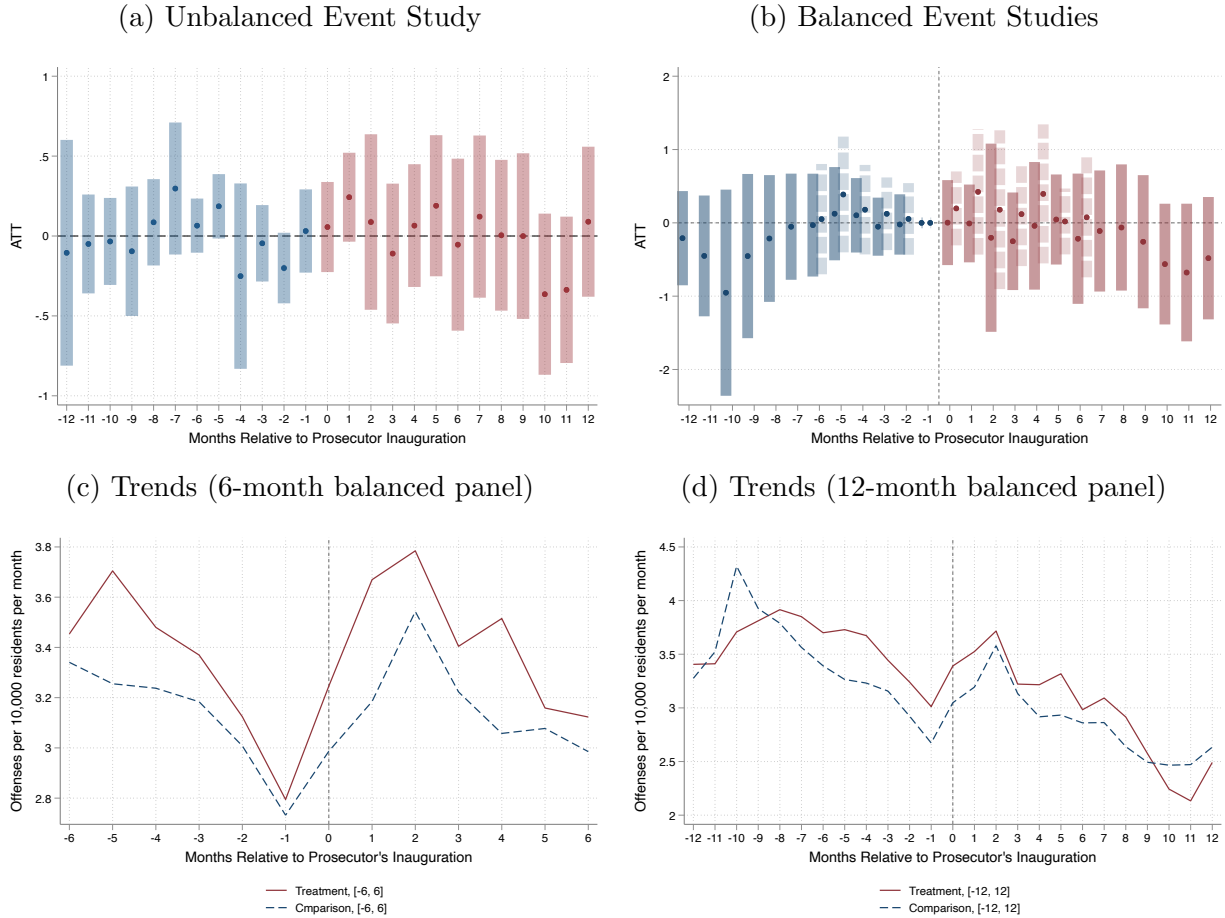
Notes. Event study of the effect of prosecutors' inauguration on theft rates, by month. Time 0 is the inauguration month. Rates are reported incidents per 10,000 residents per month. Unbalanced event study in panel (a) generated by the `csdid` Stata package (Callaway and Sant'Anna, 2021), using the event option and weighting by population. Balanced event studies in panel (b) estimated using the stacked event-study method in Wing et al. (2024) and weighting by population, with the lighter dashed confidence intervals representing an event-balanced sample for months [-6, 6] and darker solid confidence intervals representing an event-balanced sample for months [-12, 12] around the prosecutor's inauguration. Panels (c) and (d) present outcome trends for the stacked, population-weighted, event-balanced samples used in panel (b).

Figure 16: Vandalism Rate



Notes. Event study of the effect of prosecutors' inauguration on vandalism rates, by month. Time 0 is the inauguration month. Rates are reported incidents per 10,000 residents per month. Unbalanced event study in panel (a) generated by the `csdid` Stata package (Callaway and Sant'Anna, 2021), using the event option and weighting by population. Balanced event studies in panel (b) estimated using the stacked event-study method in Wing et al. (2024) and weighting by population, with the lighter dashed confidence intervals representing an event-balanced sample for months [-6, 6] and darker solid confidence intervals representing an event-balanced sample for months [-12, 12] around the prosecutor's inauguration. Panels (c) and (d) present outcome trends for the stacked, population-weighted, event-balanced samples used in panel (b).

Figure 17: Drug Offense Rate



Notes. Event study of the effect of prosecutors' inauguration on drug offense rates, by month. Time 0 is the inauguration month. Rates are reported incidents per 10,000 residents per month. Unbalanced event study in panel (a) generated by the `csdid` Stata package (Callaway and Sant'Anna, 2021), using the event option and weighting by population. Balanced event studies in panel (b) estimated using the stacked event-study method in Wing et al. (2024) and weighting by population, with the lighter dashed confidence intervals representing an event-balanced sample for months [-6, 6] and darker solid confidence intervals representing an event-balanced sample for months [-12, 12] around the prosecutor's inauguration. Panels (c) and (d) present outcome trends for the stacked, population-weighted, event-balanced samples used in panel (b).

Table 16: Comparison of local reported crime rates sample with [Petersen et al. \(2024\)](#)

Name	Title	Jursdiction	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

Notes. Table presents a list of prosecutors and corresponding jurisdictions used in the local reported crime rates analysis, sorted by whether it overlaps with the list of prosecutors classified as “progressive” in [Petersen et al. \(2024\)](#), whose sample includes the 100 largest counties in the United States and considers a county treated if and when they first elect a progressive prosecutor between 2000 and 2020. Panel A lists prosecutors included and classified as reform/progressive in both samples. Panel B lists prosecutors in our sample that would not have been included in [Petersen et al. \(2024\)](#) because their jurisdiction consists a county or counties outside the top 100 or an independent city, or because they were inaugurated in 2021, after their sampling period. Panel C lists prosecutors included in our sample of reform prosecutors but excluded from the list of “progressive” prosecutors considered in [Petersen et al. \(2024\)](#), usually due to their predecessors also being considered “progressive.” We include all reform prosecutors on the non-profit’s list without limitation based on predecessors.

6.3 Drug Overdose Mortality Rates

Table 17: Analysis Sample: Drug Overdose Mortality Rates

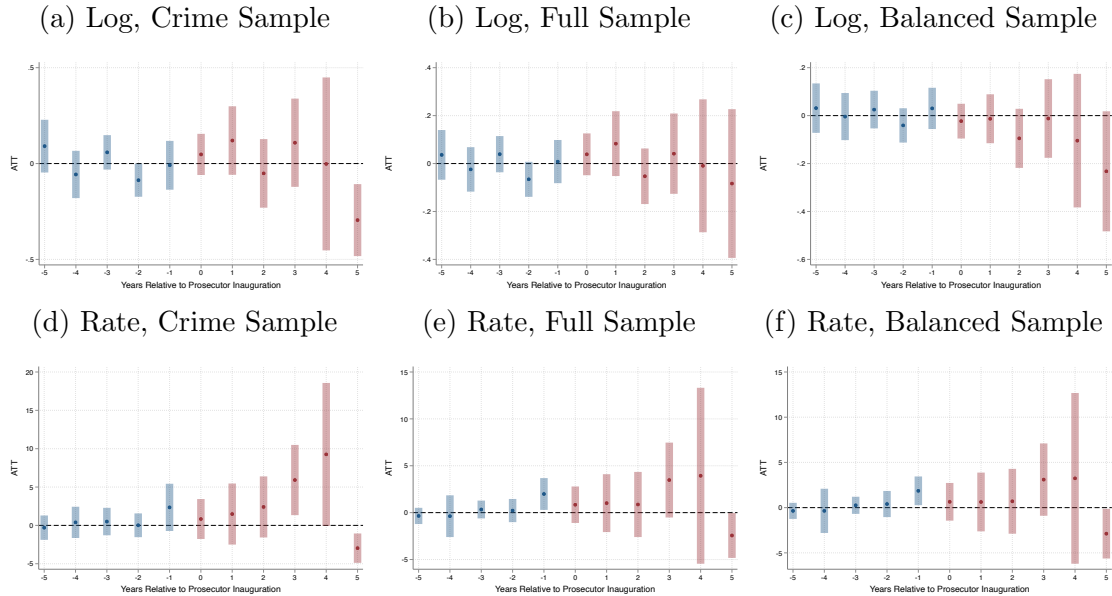
Name	Title	Jurisdiction	State	Inauguration	Years	Crime Sample
Diana Becton	District Attorney	Contra Costa County	California	2017	2005-2023	No
Wesley Bell	Prosecuting Attorney	St Louis County	Missouri	2019	2005-2023	Yes
Buta Biberaj	Commonwealth's Attorney	Loudoun County	Virginia	2019	2013-2023	Yes
Sherry Boston	District Attorney	DeKalb County	Georgia	2016	2005-2023	Yes
Chesa Boudin	District Attorney	City and County of San Francisco	California	2020	2005-2023	Yes
John Chisholm	District Attorney	Milwaukee County	Wisconsin	2007	2005-2023	Yes
John Choi	County Attorney	Ramsey County	Minnesota	2011	2005-2023	No
Laura Conover	County Attorney	Pima County	Arizona	2020	2005-2023	Yes
Shalena Cook Jones	District Attorney	Chatham County	Georgia	2021	2005-2023	Yes
John Creuzot	District Attorney	Dallas County	Texas	2019	2005-2023	Yes
Satana Deberry	District Attorney	Durham County	North Carolina	2019	2005-2023	No
Parisa Delghani-Tafti	Commonwealth's Attorney	Arlington County/City of Falls Church	Virginia	2019	2015-2023	Yes
Steve Descano	Commonwealth's Attorney	Fairfax County	Virginia	2019	2005-2023	Yes
Michael Dougherty	District Attorney	20th Judicial District	Colorado	2018	2005-2023	Yes
Kim Foxx	State's Attorney	Cook County	Illinois	2016	2005-2023	Yes
Kim Gardner	Circuit Attorney	City of St Louis	Missouri	2017	2005-2023	Yes
José Garza	District Attorney	Travis County	Texas	2021	2005-2023	Yes
George Gascón	District Attorney	Los Angeles County	California	2020	2005-2023	No
Joe Gonzales	District Attorney	Bexar County	Texas	2019	2005-2023	No
Eric Gonzalez	District Attorney	Kings County	New York	2016	2005-2023	No
Mark Gonzalez	District Attorney	Nueces County	Texas	2017	2005-2023	Yes
Christian Gossett	District Attorney	Winnebago County	Wisconsin	2007	2006-2023	No
Alexis King	District Attorney	First Judicial District	Colorado	2021	2005-2023	Yes
Larry Krasner	District Attorney	Philadelphia	Pennsylvania	2018	2005-2023	Yes
Beth McCann	District Attorney	Second Judicial District	Colorado	2017	2005-2023	Yes
Karen McDonald	Prosecuting Attorney	Oakland County	Michigan	2021	2005-2023	Yes
Ryan Mears	District Attorney	Marion County	Indiana	2019	2005-2023	Yes
Brian Middleton	District Attorney	Fort Bend County	Texas	2019	2005-2023	Yes
Harold Pryor	State Attorney	17th Judicial Circuit	Florida	2021	2005-2023	Yes
Eric Rinehart	State's Attorney	Lake County	Illinois	2020	2005-2023	No
Mimi Rocah	District Attorney	Westchester County	New York	2021	2006-2023	Yes
Rachael Rollins	District Attorney	Suffolk County	Massachusetts	2019	2005-2023	Yes
Dan Satterberg	Prosecuting Attorney	King County	Washington	2007	2005-2023	No
Eli Savit	Prosecuting Attorney	Washtenaw County	Michigan	2021	2008-2023	Yes
Mike Schmidt	District Attorney	Multnomah County	Oregon	2021	2005-2023	Yes
Carol Siemon	Prosecuting Attorney	Ingham County	Michigan	2017	2005-2023	Yes
Tori Verber Salazar	District Attorney	San Joaquin County	California	2015	2005-2023	No
Andrew Warren	State Attorney	13th Judicial Circuit	Florida	2017	2005-2023	No
Lynneice Washington	District Attorney	Jefferson County	Alabama	2017	2005-2023	No
Jared Williams	District Attorney	Augusta Judicial Circuit	Georgia	2020	2005-2023	Yes
Monique Worrell	State Attorney	9th Judicial Circuit	Florida	2021	2005-2023	Yes

Table 18: Effect of reform-minded prosecutors on drug overdose mortality rates

	(1)	(2)	(3)
	Local Crime Sample	Full Sample	Balanced Sample
Panel A: Log Mortality Rate			
ATT	0.0366 (0.0710)	0.000395 (0.0605)	-0.0640 (0.0562)
Jurisdictions	29	41	36
Panel B: Mortality Rate per 100,000 Residents			
ATT	3.223 ⁺ (1.872)	1.311 (1.649)	1.049 (1.692)
Jurisdictions	29	41	36
Pre-Treatment Mean	12.82	10.92	10.98
Percentage Effect	25.14	12.00	9.559

Notes. The dependent variable in Panel A is the logarithm of the drug overdose mortality rate per 100,000 residents; in Panel B, it is the drug overdose mortality rate. Column 1 includes only the jurisdictions that are available in both the drug overdose mortality data and the local-reported-crime sample, while column 2 includes all jurisdictions available in the drug overdose mortality data, and column 3 includes the balanced jurisdictions from column 2. The analysis is at the jurisdiction-year level, and results 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 18: Drug Overdose Mortality Rates



Notes. Event study of the effect of prosecutors’ inauguration 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. Figure generated by the csdid Stata package, using the event option and weighting by population.