The Effects of Prosecutor Turnover on Criminal Case Outcomes

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Abstract

How does election turnover influence prosecutor behavior and the outcomes of criminal cases? This study explores how first-term district attorneys (DAs) adjust case strategies under unique electoral and institutional pressures to build public credibility, by examining how new prosecutors signal competence to voters through case outcomes, including plea bargains, dismissals, and jury convictions. Employing a dynamic difference-in-difference (DID) approach across three U.S. states, the analysis reveals that prosecutor turnover is associated with an increase in plea bargain convictions as reelection approaches. In contrast to prior research findings, we find that new prosecutors do not significantly increase jury trial convictions before elections. These findings suggest that new prosecutors' electoral vulnerability promotes a strategic shift towards risk-averse behavior, prioritizing plea bargains over jury trials to manage caseloads and signal effectiveness. This study expands existing knowledge of how electoral incentives and career concerns shape prosecutor discretion and case outcomes in the criminal justice system.

Keywords: Incumbency, Elections, Prosecutors, Plea Bargaining, Trials

JEL Classification: D72 , K41

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1 Introduction

Local district attorneys (DAs) play a pivotal role in shaping criminal case outcomes through their discretionary decision-making. Election pressures magnify the impact of these decisions, where they may adjust case strategies to signal competence to voters. Consequently, prosecutor discretion creates a principal-agent problem, wherein misaligned incentives can prompt prosecutorial adjustments to political and institutional pressures (Bibas, 2008; McCannon and Pruitt, 2018). Since prosecutors wish to signal their competence to voters, aligning the incentives elected prosecutors face with their constituents' interests plays an important role in determining prosecutors' legal strategies.

Existing research highlights that DAs often use plea bargains or select cases for trial as ways to project effectiveness, balancing public safety concerns with practical constraints on caseload management (Bandyopadhyay and McCannon, 2017, 2014; Hessick, 2022). The strong incentives prosecutors face often lead to distortions which adversely affect judicial efficiency and fairness concerns, including increased case backlogs (Bandyopadhyay and McCannon, 2017), more jury trial convictions (Bandyopadhyay and McCannon, 2014), and reduced or reversed felony convictions (McCannon, 2013). While research has previously documented such generalized prosecutors' responses to political pressures, little is known about whether newly elected, first-term prosecutors approach these strategic trade-offs differently than seasoned incumbents, especially under the heightened pressures of their first term.

Across all career stages, prosecutors face an asymmetric information problem, where they wish to signal their quality to voters. In order to maximize their chances at reelection, prosecutors face strong incentives to signal "toughness" on crime. In practice, prior research shows this usually emerges through increasing the number of convictions obtained at trials (Okafor et al, 2022; Bandyopadhyay and McCannon, 2014; Dyke, 2007). Given that the majority of criminal convictions result from plea

bargains rather than jury trials, election cycles present a strategic trade-off: increased trial activity signals a prosecutor's ability to obtain convictions, while plea bargains allow DAs to manage case backlogs more efficiently, which can be especially critical during election cycles (Bandyopadhyay and McCannon, 2017). New prosecutors face additional constraints early in their careers. They face additional uncertainty and risk associated with pursuing aggressive sentencing outcomes if their actions are incongruent with other legal actors. Consequently, new prosecutors must demonstrate competence while facing limited information about local institutional constraints. While a small but growing body of research examines how elections influence prosecutors' case handling methods, there has been relatively little attention paid to how political incentives interact with an incumbent prosecutors' tenure, or how it may differ from the incentives prosecutors face early in their career.

For first-term DAs, the choice is especially complex. First-term prosecutors are more susceptible to electoral pressures and the need to build credibility both with voters and with other legal actors than experienced prosecutors. Hessick and Morse (2020)'s National Study of Prosecutor Elections finds that 34% of prosecutors with less than five years in office are contested in elections, compared to only 22% of those with 5-10 years of tenure. Moreover, first-term prosecutors are more vulnerable, with 67% of contested elections resulting in the incumbent winning, whereas incumbents with over five years of tenure win 94-95% of the time (Hessick and Morse, 2020, 1567-1568), (Hessick et al, 2023; Hessick and Morse, 2019; Hessick, 2022). These statistics underscore how tenure increases electoral security, allowing experienced prosecutors greater flexibility in strategic choices. Without this advantage, first-term DAs must carefully balance more predictable plea bargain outcomes with the riskier rewards of trial convictions, while also managing relationships with judges and public defenders (McCannon, 2019, 2021). Given heightened pressures due to their limited incumbency

advantages, first-term DAs walk a delicate tightrope—signaling competence to voters while fostering institutional relationships key to long-term success.

The principal-agent problem DAs face means they must balance voter expectations with institutional constraints. While prior studies (Okafor, 2021; Bandyopadhyay and McCannon, 2014; Dyke, 2007) show that electoral pressures generally lead prosecutors to favor trials as visible signals of "toughness," my findings indicate that first-term prosecutors tend to rely more heavily on plea bargains. By focusing on a prosecutors' first term, this study adds a new dimension to the existing literature, suggesting that early-career prosecutors leverage the predictability of plea bargains to meet voter expectations while managing the unique pressures of initial tenure.

To investigate the effects of prosecutor tenure and turnover, this study employs a dynamic difference-in-difference (DID) approach (de Chaisemartin and D'Haultfœuille, 2024; de Chaisemartin et al, 2024), enabling a nuanced analysis of time-varying prosecutor behavior across jurisdictions with staggered turnover. This framework adapts to evolving strategies over time, isolating the distinct impact of early-career pressures on first-term prosecutors by comparing their behaviors to those of experienced incumbents. By comparing turnover-driven changes in case outcomes between newly elected and tenured prosecutors, this approach captures how tenure shapes prosecutor discretion under electoral and institutional constraints. Studies such as Okafor (2021) and Agan et al (2021) similarly use DID models and event studies to examine election-related shifts in prosecutor behavior, though our focus on early career stages is unique.

This study examines how prosecutor turnover during a DA's first term influences key case outcomes, specifically jury trial convictions, pre-trial plea bargains, and case dismissals. These outcomes reflect the strategic balance prosecutors strike between signaling toughness through trials, expediting disposals via plea bargains, and conserving

resources with dismissals. The findings of this study reveal that first-term DAs significantly First-term DAs increase pre-trial plea bargains, a shift contrasting with prior research emphasizing heightened trial activity or fluctuating dismissals as signaling tools in election cycles. Constrained by limited political experience and the absence of incumbency advantages, first-term prosecutors appear to prioritize plea bargains to establish credibility early on and avoid the adversarial risks associated with trials. Case dismissals decrease notably as first-term DAs approach a reelection year, suggesting a strong response to electoral pressures, which aligns with prior research showing that tenured incumbents also reduce dismissals to signal competence. Constrained by limited political experience and the absence of incumbency advantages, first-term prosecutors appear to prioritize plea bargains to establish credibility early on and avoid the adversarial risks associated with trials. This intensified, risk-averse approach may be unique to early-career prosecutors, given the differing career-stage pressures.

Additionally, the variability in trial conviction effects across states and contexts implies that prosecutor strategies are responsive to varied local dynamics. Supporting insights from DeAngelo and McCannon (2019) on risk-averse legal behavior and Gordon and Huber (2002) on strategic signaling contextualize these findings, suggesting that early-career prosecutors may over-invest in predictable outcomes to meet institutional and electoral expectations. In prior studies, this over-investment typically presents itself through jury trials, but given additional career pressures, our results posit that prosecutor strategy is career-stage dependent. This study illustrates how turnover drives first-term prosecutors to favor pre-trial resolutions, balancing political and tenure dynamics.

In the following sections, I systematically examine the impact of prosecutor (DA) turnover and tenure on decision-making. Section 2 introduces the dynamic DID methodology used to capture tenure-specific prosecutor behaviors. Section 3 outlines

the data sources, variables, and sample selection process. Section 4 presents the empirical results, discussing how turnover influences outcomes, particularly the increase in plea bargains and fluctuating dismissal rates, while considering the role of jury trial convictions given their prominence in prior research. Section 5 discusses the findings, emphasizing the role of tenure and career-stage dynamics in shaping prosecutor strategies, along with robustness testing. Lastly, section 6 concludes by summarizing the study's contributions, limitations, and implications for understanding prosecutor behavior within an electoral context, underscoring how tenure and electoral incentives shape prosecutor discretion and resource allocation in criminal justice.

2 Empirical Strategy

2.1 Theory

prosecutor behavior is heavily influenced by institutional incentives and public accountability pressures, especially regarding plea bargaining and trial decisions. In an experimental study by Ralston et al (2023), they demonstrate that prosecutors' conviction metrics are sensitive to how plea bargains are valued compared to trial outcomes. When plea bargains contribute to conviction rates, prosecutors are more likely to offer them, resulting in a larger "trial penalty"—the differential between the plea offer and potential trial sentence. A similar body of work theorizes that information asymmetries among parties leads to suboptimal legal outcomes, driven in part by risk aversion (Bibas, 2008; Grossman and Katz, 1983; Baker and Mezzetti, 2001). The choice of metrics evaluated by agents influence prosecutor behavior. Rasmusen et al (2009) note that when conviction rates are prioritized as a performance metric, prosecutors are incentivized to focus on high-certainty cases to boost observable success, potentially neglecting complex cases with greater social value. This selective focus reflects a moral hazard, where case selection is driven by performance metrics rather

than justice. Their model predicts that politically motivated prosecutors may favor conviction rates over the socially optimal allocation of resources, in order to satisfy voter's perceived efficacy. Collectively, these theories present many key factors which can lead prosecutors to adopt risk-averse, plea-centric strategies under asymmetric information, leading to potentially inefficient uses of judicial resources.

This study treats the inauguration of newly elected prosecutors as a local policy shock to prosecutor practices, with election turnovers serving as indicators of 'treatment', which may trigger shifts in case outcomes beginning shortly after the new prosecutor takes office. Similar to Agan et al (2021), who assess the effects of electing a progressive prosecutor on crime rates, this study examines the effects within the electing a new prosecutor, and its effects on aggregated case dispositions. These impacts are captured using a dynamic DID framework, adapted from de Chaisemartin et al (2024); de Chaisemartin and D'Haultfœuille (2024, 2022), which allows for multiple turnovers within the sample period, while accommodating non-absorbing treatment effects. I use this approach to examine how prosecutor strategies change in the first four years of a DA's tenure. Districts with newly elected first-term prosecutors serve as treated units, while tenured incumbents serve as the control group[1]. The model establishes a stable baseline by designating pre-treatment years, wherein control groups consist of districts that either do not experience turnover or are under incumbents with more than one term of service, enabling robust comparison between new and experienced prosecutors. Detailed operationalization of this method appears in Section 2.2.

The judicial district serves as the group level unit of analysis, since many prosecutors oversee cases spanning multiple counties. Thus, district-level assessment of three criminal case outcomes—jury trials, dismissals, and pre-trial plea bargains— capture the effects of turnover, when a prosecutor assumes office. If turnover drives strategic changes in case handling, shifts in the proportions of these outcomes should capture

[1] In practice, the DiD estimators of Callaway and Sant'Anna (2021) are similar in function to those used by de Chaisemartin et al (2024, 7)

the adjustments linked to new leadership. Together, the DID results and event studies shown for each case outcome reflect shifts following regular election turnovers.

2.2 Model

2.2.1 Design Details

The dynamic DID approach captures the temporal variation in prosecutor turnover effects across distinct periods, while the ATCE summarizes the cumulative impact across post-treatment periods. I capture the year-specific effects of turnover on the case outcome variables using the `did_multiplegt_dyn` package in *Stata*. While other staggered treatment models assume a one-time, irreversible treatment, this approach is uniquely suited to studying the recurrent impacts of first-term prosecutor turnover. The method thus mitigates biases common in traditional staggered-treatment DiD approaches that assume constant effects over time (Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2022). A key advantage of this dynamic framework is its capacity to track treatment status changes, which mirrors the real-world nature of prosecutor turnover as a plausible exogenous shock to local policies. This structure enables analysis of new prosecutors' behavior under electoral and institutional pressures.

The model estimates both time-period-specific impacts and the average total cumulative effect (ATCE) as a cumulative metric across the post-turnover period. While time-period estimates reveal trends across different stages of the prosecutor's tenure, the ATCE provides a cumulative insight into the overall impact on prosecutor outcomes, including plea bargains. Together, they capture the period-specific shifts in a prosecutor's first term, and a summary measure of turnover's overall impact on case outcomes.

Period-specific effects help capture how prosecutor turnover influences behavior, reflecting both the immediate adjustments associated with a transition and any cumulative impacts sustained across a prosecutor's first term. By examining these effects, the study identifies both short-term behavioral shifts due to turnover and the broader, lasting influence of turnover on case outcomes under a new prosecutor's oversight. This approach provides deeper insight into how prosecutors respond to early-career political and institutional pressures, along with short-term adjustment effects. By distinguishing between transitional 'growing pains' as they adapt to their new roles, and the more fundamental strategical shift in case handling, we capture two metrics of potential changes following turnover.

In order to narrow the interpretation of our results to factors associated with normal turnover, I restrict the sample to cases of election-based, infrequent turnover within districts. This mitigates the impact of any confounding effects which could otherwise misrepresent the true effects of turnover. To accomplish this, the sample used for this approach excludes years with irregular prosecutor turnover—such as appointments or non-standard prosecutor transitions—as these can introduce unobserved influences unrelated to typical electoral pressures. Interim appointments or unexpected departures, for example, impose unique demands that may obscure the interpretation of changes in prosecutor strategies[2]. Additionally, using pre-treatment years with experienced incumbents as a baseline provides flexibility to account for non-linear treatment effects, capturing shifts in prosecutor priorities under new leadership. By focusing only on standard, election-based turnovers, this approach isolates the treatment effects within comparable, election-driven transitions, while accounting for an unbalanced sample.

This analysis defines treatment as beginning in $t = 0$ (the year when the new prosecutor is elected, typically near the end of the calendar year), with a standardized

[2]Most excluded years involve consecutive turnovers or interim appointments, such as a replacement serving a deceased prosecutor's term, which complicate treatment effect identification in this model.

pre-treatment window from $t-3$ to $t-1$, ensuring that both treated and control groups consist of experienced incumbents during the baseline period[3]. Due to the model's restrictions, the number of placebo periods estimated cannot be greater than the number of post-treatment periods estimated. The district's treatment effects are estimated for periods $t+1$ through $t+3$, corresponding to the second to fourth years of the first term. By design, treated and control groups include only districts with incumbents who have served more than one term prior to $t=0$, ensuring that they were experienced incumbents not in their first term in the pre-treatment period. This sampling approach enhances the precision of the comparison, limiting bias by isolating standard election-driven turnover. By imposing these restrictions, this study identifies plausibly exogenous shifts in prosecutor discretion and behavior. The event-study plots 1 and A2 illustrate these effects across the sample period.

While this model's flexibility mitigates biases inherent in traditional DiD approaches, it has its limitations. The data are limited to comparable state outcomes, and the identification strategy cannot account for the effects of consecutive prosecutor turnovers. Endogenous turnover factors, if unobserved, could introduce bias[4]. In addition, the reliance on comparable state outcomes imposes some constraints on the measures of case outcomes and sample selection. Nonetheless, most model specifications show little evidence of anticipation effects or violations of parallel trends. Section 4.4 discusses further robustness tests of the results' validity.

2.2.2 Mathematical Formulation

The formulae here summarize structure of treatment and placebo effects in capturing the influence of prosecutor turnover, along with placebo tests' estimators. The model specifications used here correspond to design 2 in de Chaisemartin et al (2024). For

[3]Most turnovers occur in November general elections, though some result from uncontested primaries or midterm cycles, with new prosecutors typically assuming office at the start of the next calendar year.

[4]While concerns regarding differences between open and incumbent elections deserve attention in further changes, most turnovers-both in our sample and generally- occur from open elections (Hessick et al, 2023). In context of this sample, turnover circumstances commonly follow prior incumbents' retirements, rather than contested elections.

context, the general specifications of the base model[5], the event-study estimators, and the differences-in-differences estimates are presented here.

Here, prosecutor turnover is considered treatment, and the length of the treatment period consists of their first term in office. This design allows treatment status to "switch" off once a new prosecutor's first term ends. The binary treatment variable $D_{g,t}$ is defined below:

Treatment [Turnover] status:

$$\forall(g,t),\ D_{g,t} = 1\{E_g \geq t \geq F_g\},\ \text{with } 2 \leq F_g \leq E_g \tag{1}$$

where F_g is the first period of treatment, and E_g is the last period (year) of treatment for group (district) g. $D_{g,t}$ is equal to 1 if group g is treated, and is 0 otherwise.

The estimates of the event-study effects in the post-treatment period ℓ are defined by the formula:

$$\text{DID}_\ell = \frac{1}{N_\ell} \sum_{g:F_g-1+\ell \leq T_g} S_g \text{DID}_{g,\ell} \tag{2}$$

This provides the average treatment effect for each specific period ℓ after the treatment begins. In all models used, the first year in which $D = 1$ (often referred to as the treatment initiation year or $t = 0$) is treated as a baseline period. Thus, we estimate the three remaining post-treatment periods in which a new prosecutor is in office during their first term. This baseline year serves as a reference point for estimating treatment effects in subsequent periods, and it is omitted from the post-treatment period estimates, in order to avoid multicollinearity issues. For example, $\text{DID}_{\ell=1}$ estimates the treatment effect for the second year that a new prosecutor is in office.

The overall effect of treatment for a given number of treated periods ℓ is computed using the average total cumulative effect, which captures the total effect of treatment

[5]To preserve space and brevity, the formulae including covariates is excluded from the method section, but can be found in de Chaisemartin et al (2024)'s appendix.

for each group over time, accumulated from the period when treatment begins (post-baseline) up until the last observed period. This is formally estimated as Equation 2.2.2. In this formulation, the numerator sums up the total treatment effect by accumulating each incremental effect, $\delta_{g,\ell,k}$, across all periods and groups. The denominator normalizes this total by accounting for variations in treatment intensity across groups and periods, represented by changes in the treatment status, $(D_{g,F_g-1+\ell} - D_{g,1})$. This normalization ensures that the ATCE reflects an average impact per unit of treatment, providing a comprehensive measure of the cumulative effect of treatment over time[6].

$$\begin{aligned}\delta =& \frac{\sum_{g:F_g \leq T_g} \sum_{\ell=1}^{T_g-F_g+1} \sum_{k=0}^{\ell-1} \delta_{g,\ell,k}}{\sum_{g:F_g \leq T_g} \sum_{\ell=1}^{T_g-F_g+1} (D_{g,F_g-1+\ell} - D_{g,1})} \\ =& \frac{\sum_{g:F_g \leq T_g} \sum_{k=0}^{T_g-F_g} \sum_{\ell=k+1}^{T_g-F_g+1} \delta_{g,\ell,k}}{\sum_{g:D_{g,1}=0,F_g \leq T_g} \sum_{k=0}^{T_g-F_g} (D_{g,F_g+k} - D_{g,1})}.\end{aligned} \tag{3}$$

Similar to the individual period event-study estimates, the period-specific placebo estimates are used to test for anticipation tests. Formally, a given period's placebo estimate is defined by:

$$DID_{\ell}^{pl} = \frac{1}{N_{\ell}^{pl}} \sum_{g:1 \leq F_g-1-\ell, F_g-1+\ell \leq T_g} S_g DID_{g,\ell}^{pl} \tag{4}$$

The placebo estimator (aggregated across all groups) DID_{ℓ}^{pl} provides an average placebo effect across all groups for period ℓ, enabling an evaluation of whether treated and control groups exhibited similar trends before treatment began. N_{ℓ}^{pl} denotes the number of groups for which the placebo estimator DID_{ℓ}^{pl} is computable for the specified period, and S_g adjusts for the direction of treatment "switching" to ensure

[6] k represents an index for each incremental effect 'lag' within a given period after treatment onset. A full description can be found in de Chaisemartin et al (2024, 20-24).

consistency in aggregation[7]. In this non-absorbing treatment design, groups can transition in and out of treatment across different periods. If DID_{ℓ}^{pl} is close to zero, this supports the parallel trends assumption, suggesting that treated and control groups follow similar trends pre-treatment on average. Lastly, the F-test reports the p-value for the joint insignificance of the placebo estimates, providing further evidence on whether the pre-treatment trends support the parallel trends assumption.

3 Data

The study's sample includes judicial outcomes from Florida, North Carolina, and Ohio, selected due to their consistent, well-documented data. Not all states provide uniformly reported judicial outcomes relevant to this study, and differences in legal structures further limit the choice of comparable outcomes. These states provide reliable election and court data across relevant years for the chosen outcomes, making them suitable for this research design. The final sample spans 1990 to 2019 and contains an unbalanced panel due to data exclusions detailed below[8]. Appendix tables A1 and A2 provide summary statistics, including data on covariates.

Jury trial convictions, pre-trial plea bargains, and total dismissals are aggregated annually to maintain consistency across states. The observation periods include 1990-2019 for Florida, 2011-2019 for North Carolina[9], and 2013-2019 for Ohio. Data after 2019 is excluded to due to COVID-19-related biases in prosecutor practices[10].

Only standard election turnovers are included. Exclusions cover non-standard appointments, consecutive turnovers, and cases where new prosecutors do not seek re-election. These omissions improve identification and comparability between treatment

[7] S_g is computed within did_multiplegt_dyn to determine if $D_{g,t}$ switches from 0 to 1, or vice versa.
[8] Earlier drafts of the paper extended the sample included 2020 to 2023, but due to systematic distortions in key outcomes following COVID-19, they are excluded here. Thus, the current sample runs from 1990 to 2019.
[9] Data for North Carolina covers fiscal years, running from July 1st to June 30th of each year. As a result, our selection of years for North Carolina also includes gaps to exclude years which had more than one prosecutor.
[10] Earlier drafts of the paper find that including the years 2020 to 2021 introduce COVID-related variability to the study's measured outcomes.

and control groups. The sample comprises Florida's 67 counties, North Carolina's 43 districts (accounting for redistricting)[11], and 64 of Ohio's 88 counties, enhancing comparability through control of local contexts and institutional consistency.

3.1 Court Data

For consistency, case outcomes are limited to criminal prosecutions for felonies and non-traffic misdemeanors. These are the commonly considered case types in prior research on prosecutor behavior (e.g., Agan et al (2021); McCannon (2019); Bandyopadhyay and McCannon (2014). This study compiles court data from each state's official court reports, compiled yearly [12].

While some research uses the proportion of case disposals or convictions arising from jury trials or plea bargains, differences in reporting and court structure make using this measure unfit for cross-state comparisons in the data used here. Instead, we use covariates and population-weighting to mitigate the differences occurring due to differences in states' counties and districts' relative sizes.

3.2 Election Data

In order to identify prosecutor turnover, I use each states' official election results[13], in conjunction with newspaper documentation where gaps in recording occur. In order to identify prosecutor turnover, I identify in each election cycle whether a given seat is contested or open, and whether a participating candidate is an incumbent running for reelection. I categorize the observations according to whether prosecutor turnover

[11] To account for North Carolina's redistricting, only observations with consistent district composition are included to avoid compositional changes and potential for omitted variable bias within groups.

[12] More information on the Florida data can be found at: http://www.flcourts.org. The data in use can be found here: http://trialstats.flcourts.org.. North Carolina data can be found here: https://www.nccourts.gov/documents/publications/criminalinfraction-case-activity-report-by-prosecutor-district. Ohio's court of common pleas data is found at https://www.supremecourt.ohio.gov/courts/services-to-courts/court-services/dashboards/

[13] Election data can be found at the following sites. FL: https://results.elections.myflorida.com; NC: https://www.ncsbe.gov/results-data; OH: https://www.ohiosos.gov/elections/election-results-and-data/

occurs, and whether they run again in a subsequent term of office for the same position[14].

3.3 Covariate Data

The model's covariates include socioeconomic and demographic variables, which help account for differences in local conditions associated with higher crime rates. Demographic variables are obtained from U.S. Census data. In addition to population aggregates, which are used to calculate weights and population density, our covariates used include the measures $Youth$, $White$, and $Male$. $Youth$ measures the district's percentage of the population within the ages 15-24 for a given year. Similarly, $White$ and $Male$ measure the percent of the population that is non-hispanic white and the percent of the population that is male, respectively. We also include the yearly unemployment rates UR from the Bureau of Labor Statistics, calculated by district for a given year, as a proxy for the opportunity cost of crime.

4 Results

4.1 Plea Bargains

Table 1 presents the primary results for plea bargains, showing significant increases in plea bargaining rates following prosecutor turnover. Event-study plots in Figures 1 and A2 confirm these effects, particularly in the first three years post-turnover. This suggests that newly elected prosecutors, particularly in their first terms, may strategically increase plea bargains to expedite case processing and build a strong track record early in their tenure.

The results are less consistent for period four, where confidence intervals widen and significance diminishes. This may reflect the reduced sample size due to data availability. Another possibility is that this may reflect varying prosecutor strategies,

[14]Where possible, data is cross-referenced with the data set used by Hessick and Morse (2020), found at https://doi.org/10.15139/S3/ILI4LC.

such as a shift toward trial convictions for some prosecutors nearing reelection. Such heterogeneity in strategies is also evident in later robustness tests, with consistent effects observed in most specifications, especially for the first three periods following turnover.

The ATCE is consistently positive and often statistically significant for plea bargains post-treatment, indicating that prosecutor turnover generally increases the number of cases resolved through plea bargains when averaged across the treatment window. This measure of overall effects is consistent with the event-study estimators, supporting a robust, positive overall effect even as individual period impacts vary. The tests for the joint equality of the effects, along with the event study figures, show little evidence that any individual year's estimated effects are significantly different from others, although the increase in pleas is smallest in period 1.

While the magnitude of the effects varies somewhat across periods, states, and by model specification, as shown in figure 1, table 1 and in the additional results A.2, the direction and statistical significance remain largely consistent across model specifications. The Average Total Cumulative Effect is consistently significant, and in most cases, is roughly estimated as eleven additional pre-trial plea bargains per 10,000 residents per year. This adds further support the robustness of findings across different jurisdictions with varying population sizes, indicating that the observed increase in plea bargains is not solely driven by larger counties, districts, or higher-population areas.

Dividing the event studies figures by state shows that all states experience an initial uptick in plea bargains following turnover, although individually most of them are insignificant. While the magnitudes and duration of positive trends in plea bargains vary by state following turnover, the event study effects are all positive for year $t+2$ A1. The consistency of these results across states indicates that even conservative

interpretations of the evidence indicate an initial increase in plea bargains following prosecutor turnover.

Table 1 Results: Plea Bargains Per 10,000 Residents

	Base Model	Base Model with Controls	Same Switchers Model	Same Switchers, Truncated
$\hat{\delta}_3^{pl}$	2.111	3.553	16.544	
	(4.545)	(4.703)	(14.577)	
$\hat{\delta}_2^{pl}$	2.174	3.138	10.747	2.176
	(4.358)	(4.406)	(17.149)	(4.756)
$\hat{\delta}_1^{pl}$	3.162	3.704	4.773	2.820
	(3.406)	(3.451)	(11.710)	(4.003)
$\hat{\delta}_1$	2.617	2.497	18.659*	2.876
	(2.663)	(2.712)	(9.318)	(2.877)
$\hat{\delta}_2$	12.393***	12.133***	32.281***	12.221***
	(2.893)	(2.869)	(7.475)	(3.135)
$\hat{\delta}_3$	18.708***	18.187***	31.936***	18.916***
	(4.200)	(4.095)	(6.553)	(4.396)
Avg. Total Cumulative Effect	11.006***	10.711***	27.625***	11.338***
	(2.719)	(2.698)	(7.119)	(2.977)
Joint Eq. Effects	0.001	0.001	0.010	0.001
Joint Sig. Placebo	0.759	0.742	0.007	0.749
N obs. used to estimate ATCE	709.000	709.000	407.000	663.000

Standard errors in parentheses; All standard errors are clustered at the district level
Covariates Included: No, Yes, Yes, Yes
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

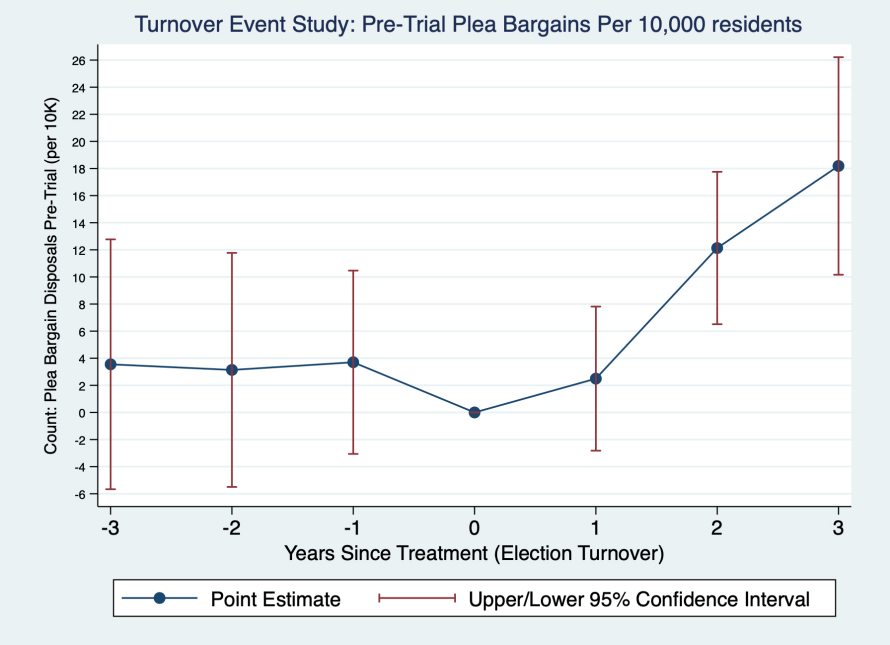

Fig. 1 Pre-Trial Plea Bargains per 10,000 residents

4.2 Case Dismissals

Regression results for the effects of case dismissals complement the results for pre-trial plea bargains, by providing additional context. Looking at figure 2, we see that turnover has no significant effect on case dismissals, even from residual effects after a prosecutors' first term. While some results not shown here exhibit limited, short-term effects, these effects do not hold when non-parametric trends by state are used to account for differences across states not captured by covariates and fixed effects within the model. Second, treatment is sensitive to the number of post-treatment periods included. While the sample design does not allow for the estimation of periods past eight (equivalent to $t + 7$ in figure 2), the models used for dismissals exhibit no consistent trends for dismissal changes. While the confidence intervals are large, most estimates are close to zero. This supports the argument that prosecutors' increased number of plea bargains cannot be explained by a change in dismissals. Since dismissals

are not significantly changing following turnover, this suggests that an increase in plea bargains is not due to a major shift in the choices dismissed, on aggregate.

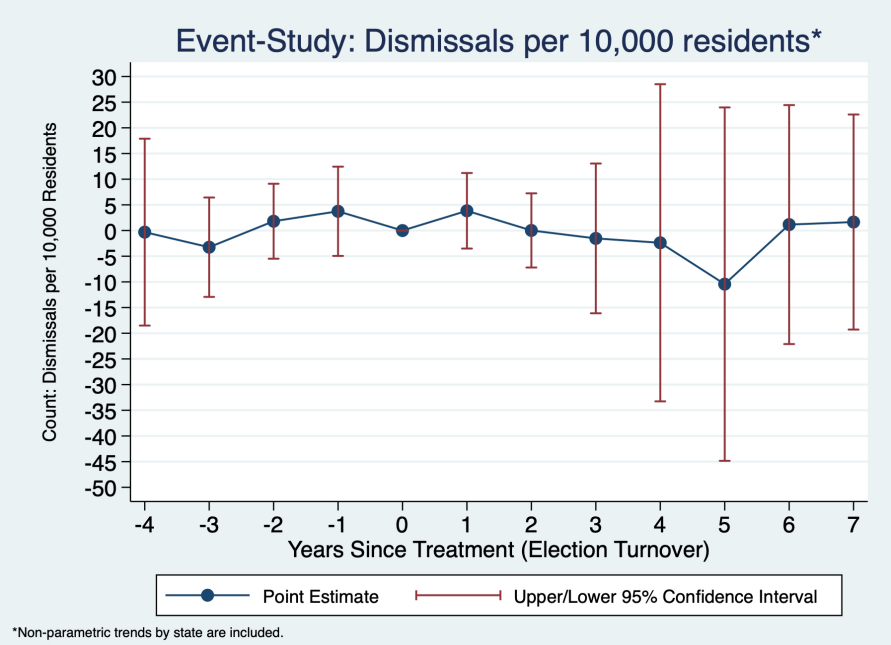

Fig. 2 Case Dismissals per 10,000 Residents, Two Terms

The lack of consistent change in dismissal counts aligns with prior theories of election pressures driving prosecutors to a "tough on crime" approach. If both experienced and new incumbents expect that conviction rates are a relevant measure of importance, neither group would wish to increase dismissals. Rather, jury trials and plea bargains are the primary means available to prosecutors to demonstrate competence.

4.3 Jury Trial Convictions

Surprisingly, the results show few statistically significant effects of prosecutor turnover on jury trial convictions [15]. While some models do exhibit significant effects in individual periods, the most consistent of these is in period 7, corresponding to the *second* re-election year post-turnover. This added context provides evidence supporting the theory that prosecutors favor plea bargain disposals earlier in their career. While this may seem contrary to prior findings (e.g., Bandyopadhyay and McCannon (2014); Dyke (2007)), but the results can also support an interpretation that first-term incumbents do not pursue the same strategy when they seek their first retention election. One possible interpretation of heterogeneity of prosecutor behavior is that prosecutors pursue less risky strategies early in their career, and alter their behavior in subsequent reelection bids.

In conjunction with the trends and mixed significance indicated in table A.5, our findings are not wholly contrary to prior studies, since the most common periods where turnover has significant lagged effects on jury trial convictions occurs in their second term in office. Since our sample is restricted to prosecutors who do not experience consecutive turnover, this limits the external validity of findings for jury trials. However, the period-specific estimates for jury trial convictions post-turnover generally trend upward in a prosecutors' second term. While these results are more inconsistent than results for plea bargain disposals, they are overall consistent with the findings of prior work which find increased jury trial convictions in re-election years. It should be noted, however, that all results for the second-effects ($t+4$ to $t+7$) originate from Florida data, and that heterogeneity across states (see figure A6) leads to inconsistent effects after a prosecutors' first term in the current dataset. In sum, the trends for jury trial convictions in conjunction with the trends for plea bargains highlight career-stage specific differences in how prosecutors obtain convictions.

[15] Figures A4 and A5, plus table A.5 are found in the appendix, but no specification shows *consistently* significant effects, and are thus omitted from the main body of the paper.

These results have caveats; Differences in case handling lead to recording differences influencing the results for some states. While North Carolina and Florida report plea bargain and jury trial outcomes similarly, omitted data for the nature of dismissed, transferred, or 'other' disposals of cases may mask important variation that happens at the state-level due to reporting differences. In addition, North Carolina only reports their data in fiscal years, which limits the accuracy in years where two district attorneys' terms overlap. To address this, fiscal years with overlapping terms are omitted, but this also limits the sample treatment period to effectively three years in several cases, along with some truncation of the beginning and ends of some prosecutors' terms recorded.

While the lack of consistently significant change in jury trials shows no evidence that new prosecutors increase jury trials, it should be noted that this result does not rule out smaller shifts in jury trial activity that might fall below the sensitivity threshold of the current model. The sample size also decreases in later periods, due to sample sizes for Ohio and North Carolina covering a shorter timespan than for Florida. Additionally, the figure showing results by state show significant differences in trends across states. Thus some insignificance in periods past the first term is likely driven by imprecision and smaller sample size in later periods rather than differences in model selection. Overall, the results paint a picture of heterogeneous outcomes for prosecutors throughout their careers.

When combined, the imprecise estimates for jury trials, in conjunction with the low proportion of total cases, offer inconclusive results that differ from the earlier findings across multiple disciplines showing that jury trials are typically distorted during election cycles (Bandyopadhyay and McCannon, 2017; McCannon, 2013; Yntiso, 2022; Nadel et al, 2017). This inconsistency may also be due to the lack of granularity in this analysis. If for instance, these non-results are due to a lack of charge-level information, these results would be unable to capture individual-level differences in sentencing

severity or other unobserved factors by which first-term prosecutors selectively distort jury trial outcomes, such as racially-based differences in outcomes. While this model controls for minority population data, differences in outcomes such as those shown by Okafor et al (2022) are unlikely to be noticeable in aggregate measures used here.

4.4 Robustness Testing

To test the stability of our findings, we conduct a series of robustness tests that vary model specifications and assumptions. This section discusses these robustness checks, including alternate model specifications with non-parametric trends by state, using only never-treated districts for control groups, and heterogeneity tests for state-level differences. [16]. To address anticipation effect concerns, we use leads of treatment to test if placebo treatments show signs of parallel trends violations. To determine if state-level differences drive results, I conduct heterogeneity analysis by state, examine whether the treatment effects vary across jurisdictions. Similarly, I test to see if covariate changes are associated with treatment status. To test this, regressions are reported using the covariates as the outcomes to test if socioeconomic or demographic conditions exhibit significant effects following turnover. The results, presented in tables A9, A8, and figure A3 indicate that the treatment effects do not significantly differ by state in any of the effect periods or placebo periods, suggesting that the estimated impacts are consistent across regions.

Overall, our robustness tests results reinforce our primary findings. While some specifications show signs of parallel trends-mainly the same-switchers model-most models find no significant evidence of pre-treatment trends. Across the majority of the models used, we note two common findings across almost all specifications: (1) turnover effects are positive and significant for plea bargains in years two and three, and (2) the average total cumulative effects of turnover on plea bargains are positive

[16] In some additional results, we also examine outcomes in terms of case disposals using analytical population weights. These results are not shown here due to space limitations. The results are consistent with the overall findings.

across all valid estimation models where parallel trends hold. Moreover, these results are similar in terms of magnitude across specifications.

The regressions tests for anticipation effects using leads for treatment are presented in appendix section A.4. If anticipation effects based on upcoming elections lead to systematic violations of parallel trends assumptions, reported treatment effects are likely subject to biased estimates in the post-treatment period. Thus, we give model specifications with treatment reassigned as happening in the years prior (primarily years one and two, to avoid overlap with election cycle fluctuations unrelated to turnover). These models with treatment leads of one and two years serve as checks for any premature response to anticipated turnover. Models where treatment is artificially shifted to earlier periods assess pre-treatment parallel trends assumptions. If results indicated significant effects for periods before true treatment, this would indicate evidence of anticipation effects. Results indicate estimated effects for periods preceding treatment in table A6, with coefficients representing the last placebo treatment period written in bold. The estimated effects in the placebo periods are statistically insignificant, indicating no evidence of anticipation effects, with confidence intervals for the leaded treatment periods intersecting zero. This provides further support for the validity of the parallel trends assumption required for causal inference in our study.

4.4.1 Limitations and Considerations

While these robustness checks support the validity of our findings, there are certain limitations. For instance, removing observations with multiple treatment switches limits our sample size, potentially affecting generalizability, particularly for groups where competitive elections and turnover are a common occurrence. Although this seems particularly limiting, prior studies have shown that a large proportion of prosecutor elections are neither contested nor competitive, with most prosecutor turnover arising from open election bids (Hessick and Morse, 2019; Hessick et al, 2023). Despite these challenges, the convergence of results across multiple specifications and robustness

checks suggests a robust and durable prosecutor impact on plea bargains following turnover.

5 Discussion & Future Directions

5.1 Varied Prosecutor Behavior

Contrary to prior theory and findings, which suggests that political pressures drive prosecutors toward an over-reliance on jury trials, the results here show that new prosecutors increase plea bargaining convictions (Dyke, 2007; Bandyopadhyay and McCannon, 2014, 2017). This likely reflects the heightened vulnerability of new prosecutors early in their careers, where pressures to quickly demonstrate competence push them toward risk-averse strategies. Plea bargains offer a reliable means to achieve favorable case resolutions without the uncertainty of jury trials. In this context, turnover pushes new prosecutors toward case disposition strategies focused on lower-risk disposals and operational efficiency, distinct from the cyclical jury trial increases observed around election periods for more experienced prosecutors.

This study also relates to the efficiency-fairness trade-off between sentencing accuracy and efficiency within prosecutor decision-making. On one hand, plea bargains, within the framework of an ideal "social planner" (Rasmusen et al, 2009), promote efficient case resolution. On the other hand, focusing on expediency can come at the expense of fairness, as plea deals may pressure defendants to accept resolutions that do not fully reflect case merits (Grossman and Katz, 1983). Instead, case metrics may drive prosecutors to base their case selection on performance quality metrics with less risk. Similarly, Baker and Mezzetti (2001) describe how resource constraints can lead prosecutors to overemphasize expedient case disposals, which may disadvantage defendants by limiting their legal options. The increased reliance on plea bargains in this

context suggests that turnover prompts new prosecutors to prioritize efficient case resolution, a strategy which, while pragmatic, raises questions about its implications for long-term justice outcomes.

As noted by Reinganum (1988), prosecutors' decision-making is highly sensitive to perceived risks and rewards, especially early in their terms, when the incentive to establish a record of successful case resolutions is strongest. Turnover shifts case handling, with new prosecutors increasing pre-trial plea bargain convictions as elections near. Institutional pressures motivate first-term prosecutors to adopt low-risk, performance-focused strategies—increasing plea bargains, balancing caution with the need to demonstrate effectiveness.

While turnover results do not yield statistically significant effects on jury trial convictions consistently across states and specifications, these results suggest that early-career prosecutors balance risk aversion with a strong drive to demonstrate competence by prioritizing plea bargain convictions. The varying results for jury trial convictions suggests heterogeneity across states and years. Within the context of prior findings, their is little evidence that these effects diverge new prosecutors' behavior in the long term from prior findings and predictions indicating increased jury trial convictions during election years. Thus, this study suggests that first-term prosecutors differ from experienced incumbents by increasing plea bargain convictions, rather than obtaining more convictions via jury trials.

One potential explanation for the uptick in plea bargaining is the need for newly elected prosecutors to address case backlogs left by their predecessors. If the prior prosecutor prioritized jury trials, or if backlogs grew due to election-related activities, successors may seek to "clear the slate" by accelerating case resolutions through plea deals. Bandyopadhyay and McCannon (2017) notes that competitive elections frequently lead to increased case backlogs. While this "cleaning house" interpretation

is speculative, it opens avenues for future research into how turnover impacts prosecutor strategies for managing inherited caseloads, particularly when both tenure and electoral competitiveness are considered.

This strategic shift toward plea bargaining may reflect new prosecutors' desire to balance institutional pressures for expediency with the risks of uncertain jury trials. This approach to case disposition serves as an adjustment mechanism, allowing prosecutors to manage workloads while adapting to the demands of the political and institutional environment. Yet, prioritizing plea deals over other forms of case disposal might overshadow the pursuit of balanced justice, as the need to signal effectiveness supersedes fairness considerations.

Furthermore, unobserved heterogeneity likely affects prosecutor behavior across states. Variations in court structures, resources, and sentencing guidelines may alter prosecutor incentives in ways that complicate cross-state comparisons. Future studies should consider methods to address this heterogeneity, perhaps through case studies that explore institutional differences across jurisdictions.[17]

Collectively, this study's findings provide a nuanced understanding of how prosecutor turnover interacts with political and institutional pressures, with broader implications for public choice theory, criminal justice practices, and economics. First, results suggest that elections may both discipline and distort newly elected prosecutors' case strategies as they seek to meet early performance expectations. Second, this analysis highlights the evolution of prosecutor behavior across career stages, underscoring strategic differences between new and incumbent prosecutors. The results also touch upon principal-agent dynamics, as early-term prosecutors may adopt more conservative approaches to mitigate career uncertainty. Lastly, further research should investigate how experience shapes prosecutor strategy, particularly in balancing risk and political incentives across career phases.

[17] For example, North Carolina's strict sentencing guidelines could affect a prosecutor's tendency to favor plea bargains over jury trials.

5.2 Future Plans & Changes

Future additions and modifications may enhance sample robustness and external validity by expanding datasets to include additional states and years, addressing the variations in case classification and data collection across jurisdictions[18]. Incorporating institutional covariates, such as the timing of judicial and public defender elections, could further clarify the broader effects of turnover on prosecutor strategies. Such additions will enable more accurate cross-state comparisons and control for institutional differences that may influence prosecutor discretion and case outcomes.

5.3 Policy Implications and Future Research

This study highlights how prosecutor turnover reshapes case handling strategies, with a marked increase in plea bargains during their first term and a decline in dismissals in the year preceding an election. These adaptations align with public choice theory, showing that new prosecutors, driven by early-term pressures, tend to favor strategies that demonstrate competence and minimize risk. In particular, turnover acts as a shock to local case strategies, prompting early-career prosecutors to adopt risk-averse methods, such as prioritizing plea bargains over jury trials. For risk-averse prosecutors who wish to avoid adversarial relationships with existing legal actors, or uncertain case outcomes, prioritizing plea bargain convictions is a relatively more attractive strategy.

Using a dynamic DID approach to capture prosecutor behavior over time reveals that first-term incumbents may prioritize efficient case disposal through plea bargains, rather than focusing on securing more jury trial convictions. This approach underscores a significant trade-off between expediency and fairness, as the efficiency of plea bargains may overshadow considerations of balanced justice outcomes.

The changes in case outcomes following turnover have broader implications for public choice economics, criminal justice, and principal-agent theory, suggesting that

[18]For example, California has extensive case disposal data, but Proposition 47 caused a distortion in the legal classification of some forms of felonies to be reclassified as misdemeanors(Pendleton, 2024).

election cycles can both discipline and potentially distort prosecutor behavior, particularly in early stages. However, limitations related to data consistency across states and restricted covariates warrant caution. Future work should address these gaps by incorporating additional datasets and institutional covariates to refine results and improve generalizability. Despite these limitations, this study contributes a nuanced understanding of how elections shape prosecutor behavior and decision-making, particularly as new incumbents adapt to evolving political and institutional expectations in ways that directly impact criminal justice outcomes, mostv notably through plea bargains.

6 Conclusion

This study provides insight into prosecutor behavior through the lens of public choice theory and law and economics research, illustrating how prosecutors use case outcomes to signal their performance quality to voters, while balancing other institutional incentives and career considerations. Contrary to prior findings linking election cycles with increased trial activity, this analysis shows only limited and inconsistent evidence of significant jury trial distortions following turnover, depending on model specifications. While the true effects of turnover on jury trial convictions may be masked in this study's outcome measures, the distinction from prior research is clear: prosecutor turnover is followed by an increase in plea bargains, even in election years.

Systematic differences in prosecutor behavior following turnover illustrate key dynamics in principal-agent relationships for legal actors, demonstrating how agents manage risk and credibility as they adapt to their early-term pressures. Turnover-driven volatility may boost short-term case clearance rates but risks compromising consistency in legal outcomes. While the prioritization of efficient disposals addresses certain institutional incentives, it raises broader questions about the balance between accountability and fairness in the criminal justice system. If first-term incumbents'

incentives to increase plea bargain disposals are too strong, they incur greater pressures on defendants regardless of guilt.

In this context, prosecutor turnover acts as both a corrective mechanism and a source of volatility, altering case outcomes as agents balance responsiveness to political pressures with operational efficiency. This duality raises important questions within public choice theory about the role of elections as both a mechanism for accountability and a driver of strategic case dispositions that may prioritize observable performance over consistent legal standards. These insights contribute to the literature by demonstrating how electoral incentives and principal-agent dynamics shape the public sector's operational strategies, with implications for understanding the intersection of institutional design and agent behavior within public choice frameworks.

Acknowledgements. This work received support from the Institute for Humane Studies under grant no. IHS018106.

Appendix A Additional Results & Summary Statistics

This appendix section contains additional information that may be helpful in providing a more comprehensive understanding of the research problem of ideal DD specification, as well as supplemental figures.

A.1 Summary Statistics

Table A1 Summary Statistics for Key Variables (Overall Sample)

	(1)			
	mean	sd	min	max
Year	2011.018	7.927113	1990	2019
Percent White	73.56766	18.63546	12.90698	98.10567
Percent Youth (Aged 15-24)	12.97394	2.841253	8.685388	24.07046
Percent Male	49.21495	1.134837	46.89163	54.58212
Pre-trial Plea Bargains	6497.053	7700.765	0	44429
Jury Trial Convictions	171.7671	188.2896	0	1244
Case Dismissals	2633.662	3892.769	0	39356
Population Estimate	473710.4	522998.5	13654	2716940
Population Density	388.3166	469.7471	26.20405	2767.742
Plea Bargains per 10k	121.681	76.32184	.3479108	454.7002
Dismissals per 10k	69.76718	107.9542	.2609195	654.244
Jury Trials per 10k	4.985782	8.143995	.1308995	84.36196
Observations	1284			

Table A2 Summary Statistics for Key Variables by State

	Florida				North Carolina				Ohio			
	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
Year	2004.48	8.37	1990.00	2019.00	2015.26	2.52	2011.00	2019.00	2016.00	2.00	2013.00	2019.00
Unemployment Rate	5.61	2.48	1.89	14.24	6.39	2.51	2.99	14.22	5.73	1.69	3.03	12.68
Percent White	63.25	17.53	12.91	89.86	68.74	15.97	24.72	92.88	89.14	8.48	58.59	98.11
Percent Youth (Aged 15-24)	12.94	3.51	8.69	24.07	13.20	2.56	9.31	22.57	12.86	2.00	10.03	21.67
Percent Male	49.19	1.22	46.89	53.40	48.77	0.90	47.02	53.00	49.54	1.07	47.53	54.58
Pre-trial Plea Bargains	12884.69	7995.77	440.00	44429.00	3720.57	2429.59	762.00	14742.00	713.96	1457.80	0.00	9649.00
Jury Trial Convictions	262.34	182.39	0.00	1244.00	236.67	188.97	20.00	990.00	19.94	47.88	0.00	385.00
Case Dismissals	3678.42	2909.82	80.00	17321.00	4594.28	5847.27	112.00	39356.00	70.78	136.96	0.00	1125.00
Population Estimate	874979.25	550873.64	72627.00	2716940.00	234782.24	198391.82	85743.00	1111761.00	153617.40	247955.50	13654.00	1316756.00
Population Density	479.56	451.51	26.20	1623.47	293.95	342.91	34.09	2120.59	342.34	540.95	29.96	2767.74
Plea Bargains per 10k	158.90	48.26	59.62	335.22	177.40	69.65	19.13	454.70	39.84	15.14	0.35	96.00
Dismissals per 10k	50.29	37.29	6.01	295.76	198.69	155.14	8.04	654.24	6.73	9.71	0.26	130.82
Jury Trials per 10k	3.46	2.01	0.14	14.89	13.36	13.42	1.01	84.36	1.21	0.93	0.13	9.67
Observations	536				300				448			

A.2 Additional Results: Pleas

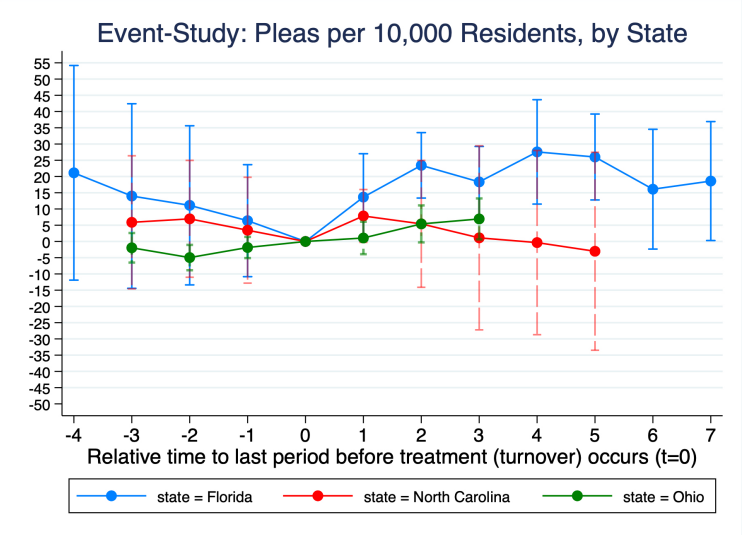

Fig. A1 Plea Bargains per 10,000 Residents, By State

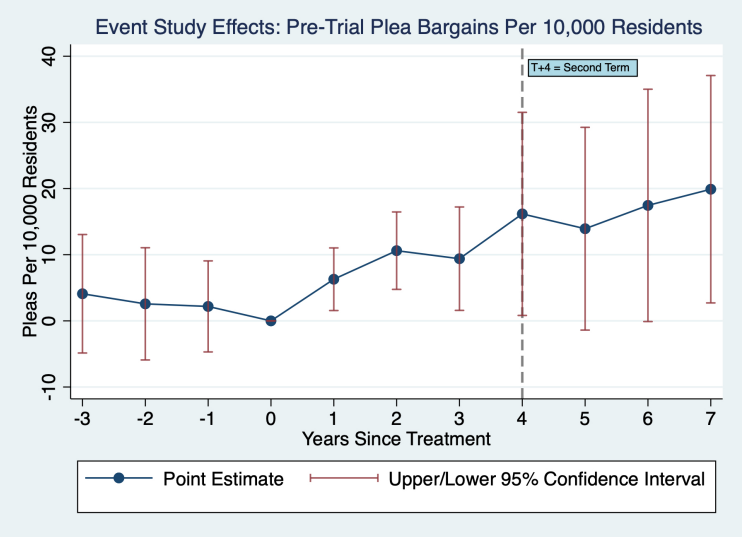

Fig. A2 Plea Bargains per 10,000 Residents, Residual Effects

Non-parametric trends by state included above.

Table A3 Results: Plea Bargains, Additional Model Specifications

	Base Model, Non-Parametric State Trends	Never-Switched as Controls, Non-Parametric State Trends	(1), with Bootstrapped Clustered Standard Errors	Same-Switchers with Bootstrapped Clustered Standard Errors
$\hat{\delta}_3^{pl}$	4.096	4.121	4.096	12.598
	(4.568)	(4.750)	(4.799)	(17.776)
$\hat{\delta}_2^{pl}$	2.572	1.775	2.572	8.632
	(4.328)	(4.455)	(4.345)	(19.707)
$\hat{\delta}_1^{pl}$	2.183	1.611	2.183	3.455
	(3.511)	(3.487)	(3.874)	(13.495)
$\hat{\delta}_1$	6.293**	5.459*	6.293*	17.837
	(2.415)	(2.391)	(2.897)	(12.462)
$\hat{\delta}_2$	10.610***	9.133**	10.610*	28.834*
	(2.985)	(3.283)	(4.303)	(14.582)
$\hat{\delta}_3$	9.397*	8.004	9.397	23.802
	(3.988)	(4.362)	(5.282)	(21.348)
$\hat{\delta}_4$	16.167*	13.660		
	(7.831)	(7.943)		
$\hat{\delta}_5$	13.927	7.827		
	(7.818)	(7.514)		
$\hat{\delta}_6$	17.452	8.482		
	(8.958)	(9.458)		
$\hat{\delta}_7$	19.897*	17.039		
	(8.768)	(9.617)		
Average Total Cumulative Effect	13.419***	10.652*	8.699*	23.491
	(4.056)	(4.232)	(3.773)	(15.320)
Joint Eq. Effects	0.265	0.196	0.288	0.110
Joint Sig. Placebo	0.808	0.705	0.845	0.308
N obs. used to estimate ATCE	644.000	524.000	492.000	138.000

Standard errors in parentheses

Covariates: No, Yes, Yes, Yes

All standard errors are clustered at the district level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A4 Results: Plea Bargains with Analytical Population Weights

	(1) Base Model	(2) Base Model with Controls	(3) Same Switchers Model	(4) Same Switchers, Truncated
$\hat{\delta}_3^{pl}$	-1201.019	-765.440	-342.667	
	(1,062.744)	(887.922)	(1,108.182)	
$\hat{\delta}_2^{pl}$	-513.749	-286.910	-1000.393	-833.237
	(542.815)	(460.354)	(757.743)	(562.930)
$\hat{\delta}_1^{pl}$	-373.630	-313.956	-674.896	-618.197*
	(247.761)	(211.674)	(372.522)	(267.979)
$\hat{\delta}_1$	691.786*	601.136*	1,692.120**	1,000.548**
	(280.004)	(301.510)	(616.563)	(352.753)
$\hat{\delta}_2$	1,509.078***	1,392.411***	2,914.069***	1,667.075***
	(456.192)	(388.993)	(599.192)	(433.990)
$\hat{\delta}_3$	1,449.937*	1,235.852*	3,345.276**	1,611.118*
	(611.647)	(562.288)	(1,057.485)	(683.627)
$\hat{\delta}_4$	2,363.605*	2,352.478**	3,904.784**	
	(977.880)	(877.967)	(1,438.207)	
Average Total Cumulative Effect	1,442.407**	1,328.468**	2,977.734***	1,428.575**
	(499.829)	(450.794)	(824.903)	(442.608)
Joint Eq. Effects	0.052	0.002	0.122	0.163
Joint Sig. Placebo	0.272	0.260	0.001	0.051
N used to estimate ATCE	3.17e+08	3.17e+08	1.77e+08	2.46e+08

Standard errors in parentheses; All standard errors are clustered at the district level

Covariates Included: No, Yes, Yes, Yes

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A.3 Additional Results: Dismissals

Table A5 Results: Dismissals Per 10,000 Residents

	Base Model	Base Model with Controls	Same Switchers Model	Longer Time Period
$\hat{\delta}_4^{pl}$				-0.309
				(9.281)
$\hat{\delta}_3^{pl}$	-3.325	-3.245	4.254	-3.245
	(5.059)	(4.939)	(7.847)	(4.939)
$\hat{\delta}_2^{pl}$	1.819	1.813	9.745	1.813
	(3.752)	(3.723)	(8.382)	(3.723)
$\hat{\delta}_1^{pl}$	3.684	3.751	2.404	3.751
	(4.449)	(4.434)	(5.283)	(4.434)
$\hat{\delta}_1$	3.879	3.849	-0.694	3.849
	(3.770)	(3.752)	(7.820)	(3.752)
$\hat{\delta}_2$	-0.026	0.022	-4.205	0.022
	(3.710)	(3.685)	(9.071)	(3.685)
$\hat{\delta}_3$	-1.753	-1.531	-5.613	-1.531
	(7.446)	(7.439)	(6.370)	(7.439)
$\hat{\delta}_4$				-2.382
				(15.755)
$\hat{\delta}_5$				-10.433
				(17.554)
$\hat{\delta}_6$				1.160
				(11.872)
$\hat{\delta}_7$				1.660
				(10.682)
Avg. Total Cumulative Effect	0.786	0.863	-3.504	-0.557
	(3.841)	(3.831)	(7.097)	(7.516)
Joint Eq. Effects	0.506	0.521	0.740	0.576
Joint Sig. Placebo	0.309	0.299	0.208	0.451
N obs. used to estimate ATCE	492.000	492.000	138.000	644.000

Standard errors in parentheses. All standard errors are clustered at the district level

Covariates Included: No, Yes, Yes, Yes, Yes, Yes, Yes

Non-parametric trends are used throughout

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A.4 Additional Results: Robustness Checks

A.4.1 Lead Tests for Anticipation Effects

This section tests for the sensitivity of the treatment effect to the year chosen. Results show no strong evidence of anticipation, nor do they find significant effects in post-treatment periods when as-if treatment is assigned to years $t-3$ to $t-1$.

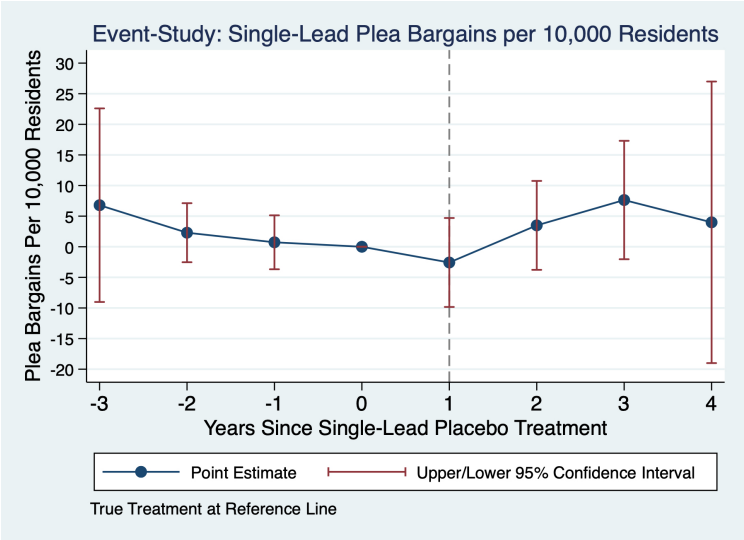

Fig. A3 One-Year Lead Event Study, Pleas

Table A6 Plea Bargains per 10,000 Residents with One, Two, and Three-Year Leads of Treatment

	One-Year Lead	Two-Year Lead	Three-Year Lead
$\hat{\delta}_3^{pl}$	3.753	-9.957	-24.768*
	(8.782)	(7.576)	(10.756)
$\hat{\delta}_2^{pl}$	1.105	3.701	-14.939*
	(2.469)	(7.293)	(7.540)
$\hat{\delta}_1^{pl}$	-0.674	3.393	-3.184
	(2.084)	(1.918)	(6.995)
$\hat{\delta}_1$	**0.293**	0.674	-3.393
	(2.819)	(2.084)	(1.918)
$\hat{\delta}_2$	4.459	**2.141**	-1.105
	(3.627)	(2.618)	(2.469)
$\hat{\delta}_3$	8.594	7.040*	**0.729**
	(4.818)	(3.331)	(3.444)
$\hat{\delta}_4$	4.881	8.971	-0.671
	(11.940)	(8.890)	(9.034)
$\hat{\delta}_5$		8.008	8.526
		(11.335)	(12.312)
$\hat{\delta}_6$			26.608*
			(11.711)
Avg. Total Cumulative Effect	4.406	4.934	0.966
	(4.443)	(3.982)	(3.989)
Joint Eq. Effects	0.007	0.242	0.052
Joint Sig. Placebo	0.851	0.010	0.079
N obs. used to estimate ATCE	574.000	585.000	520.000

Standard errors (clustered at the district level) in parentheses

Covariates included across all models

Relevant periods for anticipation effects are in bold. No results show evidence of significant anticipation effects.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A7 Dismissals per 10,000 Residents with One, Two, and Three-Year Leads of Treatment

	One-Year Lead	Two-Year Lead	Three-Year Lead
$\hat{\delta}_3^{pl}$	-4.432	0.319	-17.535
	(7.395)	(12.986)	(10.470)
$\hat{\delta}_2^{pl}$	-7.044	0.274	-1.907
	(4.159)	(8.662)	(10.939)
$\hat{\delta}_1^{pl}$	-0.769	-2.649	-0.268
	(4.273)	(2.815)	(9.499)
$\hat{\delta}_1$	**-2.718**	0.769	2.649
	(4.424)	(4.273)	(2.815)
$\hat{\delta}_2$	1.578	**-0.540**	7.044
	(4.898)	(3.533)	(4.159)
$\hat{\delta}_3$	-2.660	1.654	**3.169**
	(5.735)	(5.195)	(4.847)
$\hat{\delta}_4$	-14.986	-1.425	6.447
	(20.098)	(10.915)	(13.193)
$\hat{\delta}_5$		-17.353	2.083
		(20.040)	(10.875)
$\hat{\delta}_6$			-5.745
			(11.562)
Avg. Total Cumulative Effect	-3.081	-1.863	4.377
	(6.068)	(5.738)	(4.846)
Joint Eq. Effects	0.472	0.796	0.832
Joint Sig. Placebo	0.219	0.804	0.000
N obs. used to estimate ATCE	574.000	585.000	520.000

Standard errors in parentheses

Covariates included across all models

All standard errors are clustered at the district level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

A.4.2 Covariates Treatment Testing

This section replaces the outcome variable with each respective covariate, to see if treatment effects are driven by our covariate evolution rather than outcome variables. The summarized results for the base model with covariates as the regressors are shown here. Results show covariates have no significant effects in regards to treatment.

Table A8 Dismissals per 10,000 Residents with One, Two, and Three-Year Leads of Treatment

	(1) Unemployment Rate	(2) Percent White	(3) Percent Male	(4) Percent Pop. ages 15-24	(5) Population Density
$\hat{\delta}_3^{pl}$	-0.059	-0.159	-0.017	0.027	3.227
	(0.177)	(0.378)	(0.027)	(0.053)	(2.831)
$\hat{\delta}_2^{pl}$	-0.106	-0.081	-0.023	0.029	1.613
	(0.130)	(0.125)	(0.023)	(0.036)	(1.901)
$\hat{\delta}_1^{pl}$	-0.049	0.012	-0.018	0.007	0.817
	(0.072)	(0.066)	(0.016)	(0.022)	(0.936)
$\hat{\delta}_1$	-0.093	0.006	-0.003	0.003	-0.961
	(0.074)	(0.057)	(0.014)	(0.019)	(1.034)
$\hat{\delta}_2$	-0.109	-0.205	0.017	-0.007	-1.403
	(0.095)	(0.317)	(0.020)	(0.032)	(2.081)
$\hat{\delta}_3$	-0.066	-0.174	-0.006	-0.025	-1.825
	(0.094)	(0.314)	(0.024)	(0.040)	(2.996)
Avg. Total Cumulative Effect	-0.090	-0.121	0.003	-0.009	-1.385
	(0.082)	(0.215)	(0.018)	(0.028)	(2.000)
Joint Eq. Effects	0.590	0.675	0.090	0.576	0.900
Joint Sig. Placebo	0.805	0.323	0.706	0.743	0.542
N obs. used to estimate ATCE	492.000	492.000	492.000	492.000	492.000

Standard errors in parentheses

Covariates are used as outcomes with no other controls across these models. Nonparametric trends across states are used.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A9 Heterogeneity by State Test for prosecutor Turnover Effects - Plea Bargains

	(1) PleaPer10k
$\hat{\delta}_1$	2.61724
	(2.66251)
$\hat{\delta}_2$	12.39339***
	(2.89304)
$\hat{\delta}_3$	18.70793***
	(4.20008)
Avg. Total Cumulative Effect	11.00649***
	(2.71948)
Effect_1 state	0.00000 (.)
Effect_2 state	0.00000 (.)
Effect_3 state	0.00000 (.)

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

For Table A9, values labeled as Effect_1, Effect_2, and Effect_3 represent p-values from a heterogeneity test assessing whether state-level factors predict variation in the estimated treatment effects. A p-value of 0.000 here indicates a high degree of statistical insignificance in these tests, suggesting that state-level indicators do not significantly account for differences in treatment effects across these periods. This differs from the typical interpretation of p-values in tests of joint significance or placebo effects, where a small p-value would imply a potential issue. In this case, the small p-values indicate that the observed treatment effects are consistent across states, with no significant heterogeneity.

A.5 Jury Trial Results

Event studies below correspond to Columns 2 and 5 in Table A10, Respectively.

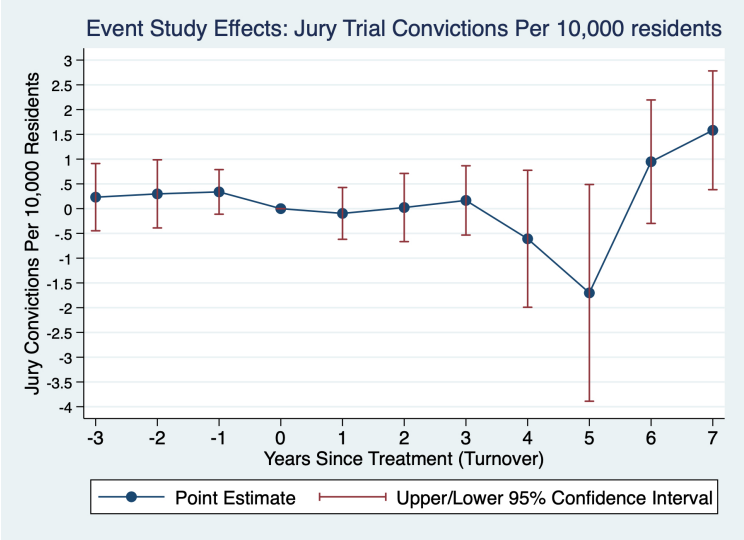

Fig. A4 Event Study Model 2: Jury Trial Convictions per 10,000 Residents

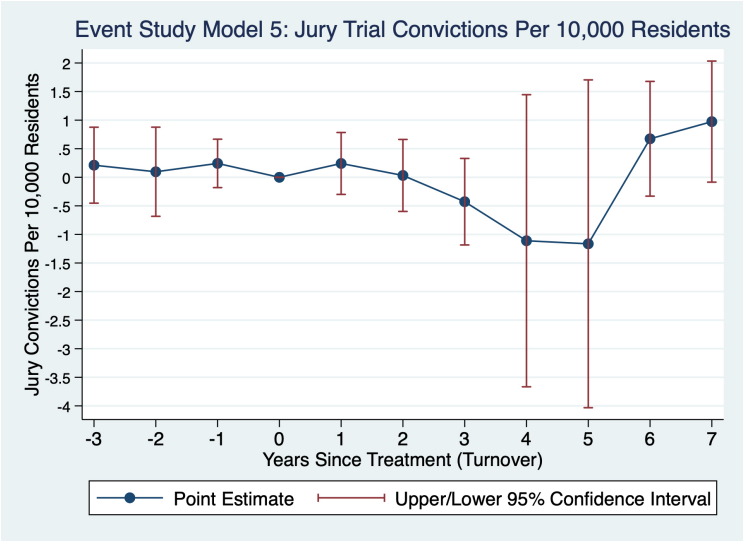

Fig. A5 Event Study Model 5: Jury Trial Convictions per 10,000 Residents

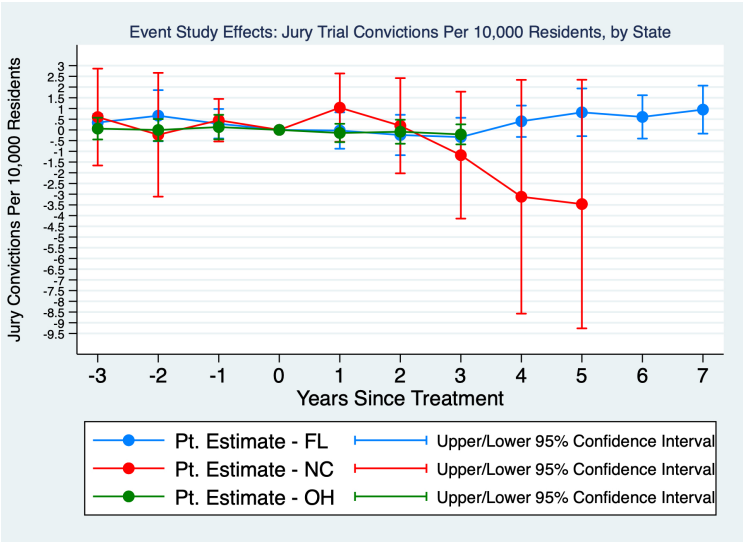

Fig. A6 Jury Trials Per 10,000 Residents, by state

Table A10 Jury Trial Event Study Results

	Base Model	Base Model with Controls	Same Switchers Model	Same Switchers, with State Non-Parametric Trends	Model 2, with State Non-Parametric Trends
$\hat{\delta}_3^{pl}$	0.364	0.232	0.406	0.403	0.212
	(0.248)	(0.346)	(0.258)	(0.249)	(0.339)
$\hat{\delta}_2^{pl}$	0.762	0.298	-0.051	-0.071	0.097
	(0.460)	(0.351)	(0.316)	(0.293)	(0.398)
$\hat{\delta}_1^{pl}$	0.248	0.338	0.742	0.292	0.243
	(0.201)	(0.230)	(0.491)	(0.462)	(0.216)
$\hat{\delta}_1$	0.182	-0.096	0.817	0.299	0.242
	(0.307)	(0.267)	(0.465)	(0.461)	(0.276)
$\hat{\delta}_2$	0.218	0.023	1.168	0.253	0.032
	(0.308)	(0.351)	(0.822)	(0.807)	(0.321)
$\hat{\delta}_3$	-0.109	0.166	**1.630***	0.631	-0.427
	(0.356)	(0.357)	(0.747)	(0.648)	(0.386)
$\hat{\delta}_4$	-0.070	-0.608	**1.620***	0.890	-1.110
	(0.349)	(0.706)	(0.644)	(0.573)	(1.304)
$\hat{\delta}_5$	-0.204	-1.701	**2.130****	**1.530***	-1.164
	(0.550)	(1.116)	(0.666)	(0.637)	(1.464)
$\hat{\delta}_6$	0.958	0.948	0.608	0.267	0.674
	(0.521)	(0.636)	(0.857)	(0.660)	(0.512)
$\hat{\delta}_7$	**1.223***	**1.582****	**2.093****	1.206	0.974
	(0.476)	(0.612)	(0.770)	(0.655)	(0.541)
Avg. Total Cumulative Effect	0.322	-0.096	**2.517***	1.269	-0.214
	(0.453)	(0.445)	(1.049)	(1.005)	(0.520)
Joint Eq. Effects	0.012	0.044	0.002	0.000	0.192
Joint Sig. Placebo	0.080	0.400	0.181	0.346	0.666
N obs. used to estimate ATCE	3.63e+08	838.000	308.000	182.000	644.000
N obs. for Effect 1	1.23e+08	349.000	66.000	38.000	205.000
N obs. for Effect 2	1.03e+08	277.000	66.000	38.000	152.000
N obs. for Effect 3	9.50e+07	234.000	53.000	38.000	151.000
N obs. for Effect 4	7.23e+07	140.000	53.000	38.000	80.000
N obs. for Effect 5	6.19e+07	124.000	41.000	27.000	65.000
N obs. for Effect 6	4.37e+07	56.000	41.000	27.000	42.000
N obs. for Effect 7	4.35e+07	54.000	39.000	27.000	42.000

Standard errors in parentheses

Covariates Included: No, Yes, Yes, Yes, Yes

All standard errors are clustered at the district level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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