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District Attorney Compensation and Performance

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Abstract

Does prosecutor pay impact performance? We attempt to identify the causal effect of wages on a prosecutor's effort by studying a large (41%), exogenous salary increase for district attorneys in New York state. We measure the performance of prosecutors by the likelihood that a conviction is upheld when appealed. If the efficiency wage theory accurately explains non-market actor behavior, then the exogenous wage shock should entice better performance. Alternatively, if individuals who hold public office are motivated primarily by an intrinsic desire to carry out their office duties to the best of their ability rather than strictly financial compensation, then their performance would be unrelated to changes in their salary. We mostly find, inconsistent with efficiency wage theory, that an exogenous pay increase has a null effect on prosecutor performance.

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1 Introduction

Many services that matter for our quality of life are provided by local public actors. One especially crucial public service is the prosecution of crime. Communities thrive when law enforcement punishes wrongdoers, avoids overly harsh, disparate, and erroneous treatment, and secures the property and safety of its residents. Much of the discretion associated with the disposition of criminal cases is exercised by local prosecutors. They choose whether to file charges and which (and how many) charges to file. They engage in plea bargaining and represent "the people" at trial.

As with any career decision, individuals who choose to work in this job have a number of considerations that enter their occupational-choice decision making. While they care deeply about doing their job well, in an interesting survey of active prosecutors Wright and Levine [2018] record that job security, predictable hours, and not having to advertise for clients ranks high on the list of features that attract attorneys to the profession. Of course, salary is also important. Prosecutors are asked to exert substantial time and energy to clear the case backlog, effectively screen out those not culpable, and piece together a case to secure convictions of those who are guilty.

The public has a difficult principal-agent problem to overcome. Citizens are poorly informed about the office's functioning. Monitoring prosecutor behavior is costly and the typical individual does not have the human capital to be able to evaluate the quality of the decision making. Nevertheless, the United States is unique in the world in its use of popular elections to select and retain local prosecutors. Thus, elections act as the primary accountability check on the quality of prosecution services provided.

Consequently, the community has two tools to incentivize prosecutorial effort. As articulated by Gordon and Huber [2002], voters can conceivably use the threat of not re-electing an incumbent to encourage the elected prosecutor to motivate effort exertion. The second tool available to the community is the compensation paid to the elected prosecutor. While the effects of the election mechanism on prosecutor decision making has received attention, to the best of our knowledge an exploration of the consequences of changes in prosecutor salary has not been documented.

Our research question revolves around the theory of efficiency wages. In an early, seminal contribution Shapiro and Stiglitz [1984] argue that compensating individuals above the "going wage" can act to encourage extra effort exertion (or at least discourage shirking) as the threat of being fired becomes a motivation to work hard. An alternative argument for efficiency wages, put forth by Akerlof [1982, 1984], is that high wages act similar to a gift exchange where the reciprocity motivation of workers encourages high effort, without monitoring being necessary. Regardless, wage increases should increase effort provision.

As a famous historical case study, Henry Ford paid workers in his automobile assembly factories $5.00 per day; far above prevailing market wages and a doubling of their previous compensation [Raff and Summers, 1987]. With long lines of job seekers and, presumably, effort-enhancing goodwill developed with the workforce, productivity improved. Recent scholarship has conducted RCT field experiments that have reinforced the effect of efficiency wages [Gilchrist et al., 2016].

Scholarship in public administration calls this presumption into question. It has long been argued that monetary incentives can harm intrinsic motivations [Frey, 1997, Frey and Jegen, 2001]. This may be especially prominent in public service jobs [Perry, 2018] and may counteract a public-service motivation [Taylor and Taylor, 2011].[1] Compensation in the public sector tends to be below private market salaries. Thus, sorting in labor markets can lead to individuals with strong service-minded motivations being those employed in these jobs. Individuals working as prosecutors may have intrinsic motivations to "protect and serve", putting relatively more weight on job performance and relatively less weight on financial compensation. In fact, evidence exists that greater monetary rewards may harm the warm glow created by serving others in public service professions [Deserranno, 2002]. Hence, improved compensation may even harm the desire to exert effort. Finally, while the existence of efficiency wages may discourage shirking, what matters for policymakers is how a *further* increase in compensation affects effort provision *on the margin*. If an additional pay increase does not lead to additional effort (maybe because full effort is already being exerted), then public funds can be redirected to improve community well-being through other public services.[2]

In light of potentially conflicting theoretical explanations for the impact of wage increases on public sector performance, especially for elected prosecutors, we empirically explore whether improved financial compensation leads to improved effort in prosecution services. There are two challenges we must overcome in performing this analysis. First, we must identify a reliable measurement of effort. This is difficult because the inability to use monitoring to solve this principal-agent problem hinges on the inability of voters to measure prosecutor effort. We use an innovative metric introduced by McCannon [2013] and DeAngelo and McCannon [2020] to measure prosecutor and judge effort, respectively. While the quality of case handling cannot be directly observed, appellate courts exist to evaluate the correctness of convictions obtained in lower courts. Appellate justices will have superior information about the handling of the case and whether it follows legal requirements. While numerous factors may play into the decision to modify or reverse a conviction, patterns to affirmations reveal changes in the incentive effects faced by legal actors. For example, McCannon [2013] links appellate cases in New York state to the election cycle of prosecutors. He finds that convictions which arise in the time window just prior to a prosecutor's re-election are less likely to be upheld, conditional on appeal. DeAngelo and McCannon [2020] build on this by expanding the data set and including judge election cycles. They show that judges who were formerly prosecutors have higher accuracy in the criminal cases they are involved with, and this quality improves when they run for re-election. Judges who were not formerly prosecutors have a lower baseline quality, and get worse when up for re-election. We

[1] As articulated over 100 years ago by the famous Supreme Court justice Charles Evans Hughes, "But when we come to the higher offices I am not one of those who think that mere increases of salary will prove an adequate solution of the problem. I also share the feeling that we should be cautious about increasing the chance of drawing men to the public service who seek it for the sake of the compensation. It is idle to suppose that emoluments can be given which can rival those obtainable by men of first rate ability in their lines of chosen effort .... [J]udges must be content to serve for annual pay less in amount than may be received in a single case by the lawyers arguing before them." The quote is taken from Frank [2003] originating in Hughes [1910].

[2] Boylan and Mocan [1987] provide an illustration by exploring judge-mandated increases in spending on criminal justice and show that government provision of welfare services suffer. They use spending on jails as the shock to the public finance expenses.

follow the lead set in these papers by using data on appeals from New York state as a measurement of errors in the case handling to proxy for legal actors' effort provision.

Second, we need an exogenous shock that affects prosecutor compensation to engage in a causal identification. This is, of course, challenging in that compensation of public actors tends to be endogenously determined to solve a particular problem. For example, in the context of judicial pay, it is commonly argued that compensation needs to be improved out of concern that higher private sector pay will draw the most talented individuals away from public service. Further, in many states prosecutor compensation is uniform across the state, so that identifying the counterfactual outcomes are frustrated.

We leverage a unique, quasi-natural experiment in New York state. In 2011, after twelve years of no change in nominal pay, New York introduced legislation to increase state judge pay to the level of Federal judge salaries. Over a four-year phase in, it increased their compensation by more than 41%. Importantly, New York state law requires that head prosecutors are paid 95% of the judges' salary. The prosecutor's compensation is paid out of county funds, while judge salaries are paid from state funds. There is no evidence that the state legislators even considered the spillover effect of this radical pay increase. In fact, this unintended, unfunded spending mandate resulted in one county threatening to ignore the law and challenge it in court.[3] Interestingly, New York state law relaxes the compensation rule for counties with populations under 100,000 residents. Thus, this quasi-natural experiment provides the opportunity, using a difference-in-difference estimation, to identify the causal effect of prosecutor compensation on the quality of the convictions obtained.

Our primary results fail to find evidence of an increase in efficiency wages as being an important motivating factor in the prosecution of local crime. The substantial pay increase has a strongly insignificant relationship with outcomes of appealed cases in the treated counties. Moreover, we show that the null result is robust to numerous model specifications.

We then proceed to further explore prosecutor discretion. Specifically, we consider the willingness to plea bargain a case. Plea bargaining dominates the U.S. criminal justice system. Proponents point to the benefit from resource conservation [Landes, 1971], sorting function improving the asymmetric information problem [Reinganum, 1988], and insurance value for risk averse individuals [Grossman and Katz, 1983]. One concern, though, is that prosecutors can substitute generous plea offers for diligence in thoroughly investigating cases [Baker and Mezzetti, 2001]. Rather than fully invest the time and effort on a case, a prosecutor can reduce effort by plea bargaining. This is expected to be especially pronounced in cases that would have taken a substantial amount of effort to prosecute. If higher salaries encourage effort provision by prosecutors, then the cases that are plea bargained will be executed "correctly", so that later appeals will be unsuccessful.

Our results directly support this hypothesis. In a triple-difference specification, we consider cases involv-

[3]Allegany County refused to increase pay for the district attorney in the spring of 2012, which was slated to increase from $119,800 to $140,300 in one year (a 17.1% increase). The district attorney is the highest paid government employee in that county. Private market salaries for lawyers in Allegany County are recorded by the U.S. Census at $79,000, and median household income is $48,412. Thus, the district attorney was to be paid 77.6% more than a typical attorney in the county and almost three times the median income. Eventually the County Board relented as they judged the cost of the lawsuit to exceed the enhanced compensation. See https://www.wellsvilledaily.com/article/20120515/News/305159998.

ing violent crimes that reached final disposition through a guilty plea and were appealed. In the treated counties, we find higher affirmation rates of convictions reached by plea bargain involving violent crimes. No similar effect exists for other crime categories.

Therefore, overall, a substantial pay increase for head prosecutors does not have a measurable effect on the effort exerted on cases, as measured by successful appeals. This suggests that, at the margin, efficiency wages are not driving prosecutor behavior. For violent crimes, where effort exertion is crucial, the decision to proceed to trial is potentially very costly, and a generous plea offer can save the prosecutor substantial resources. It is in these situations where improved compensation results in significantly higher affirmation rates.

Our work complements the growing literature studying the incentives of legal actors. While the decision making of judges has received substantial attention, recent investigations have begun to evaluate the drivers of prosecutor decision making. As a prominent example, criminal justice outcomes are strongly correlated with the prosecutor's election cycle. When up for re-election, they are less likely to dismiss cases [Dyke, 2007], more likely to pursue a conviction at trial than plea bargain the case [Bandyopadhyay and McCannon, 2014, 2015], create more incarcerations [Nadel et al., 2017], and commit more mistakes that are overturned upon appeal [McCannon, 2013]. To the best of our knowledge, ours is the first investigation into how prosecutor compensation impacts performance.

In a related application, Boylan and Long [2005] provide evidence that Federal Assistant U.S. Attorneys take more cases to trial when private market lawyer salaries are greater. They argue that career concerns are affecting their professional decision making. While our focus is on local prosecutors, we consider wage effects rather than outside labor market opportunities.

A handful of studies have considered judicial compensation. Similar to our results, Choi et al. [2009] and Baker [2008] suggest that increased judicial compensation has little to no measurable effect on performance. DeAngelo and McCannon [2017] consider the same pay increase in New York state and disentangle improved effort by established judges from the quality of new judges who choose to serve in the profession because of better compensation. They provide evidence that the pay increase primarily effects judges' effort, not selection to the bench. Anderson and Helland [2012] evaluate variation in appellate judges salaries from 1977 to 2007. They find that there is a small reduction in the likelihood a judge leaves the bench, suggesting that higher pay does encourage judges to serve longer. Therefore, like our findings, there seems to be a mostly negligible impact of futher pay increases on the effort of legal actors. Changes in outcomes can only be registered on small, specific margins.

Other than prosecutors and judges, studies have investigated the incentives of defense attorneys. Roach [2014] argues that changes in court-appointed attorney pay influences the effort exerted and, ultimately, the severity of the outcomes received by the defendant. Agan et al. [2020] compare defense attorneys representing paying clients and those representing indigent defendants (at a lower fee) and show that differences in effort exertion can explain a substantial amount of the difference between outcomes received by the indigent and

non-indigent defendants. Therefore, to achieve improvements in the effort of legal actors, it may be best to focus improved compensation on publicly-provided defense, rather than on prosecutors and judges.

In Section 2 we provide a simple theoretical model to clarify how changes in prosecutor effort corresponds to modifications and reversals of convictions upon appeal. Section 3 explains the quasi-natural experiment we use for our causal identification. The data and estimation strategy is described in Section 4. Section 5 presents the primary results, including the robustness checks. Section 6 specifically evaluates plea bargaining practices in violent crimes. Section 7 concludes.

2 Theory

This section lays out a simple model that predicts how increased prosecutorial effort can affect appealed cases. This is a useful exercise since our data is limited to only those convictions that are appealed. To do so, we consider a representative case (i.e., a randomly-selected case) and identify the probability that the case is from a wrongfully accused individual, conditional on it being appealed.

We consider the situation where an individual is arrested and the prosecutor has decided to pursue a conviction (rather than dismiss the charges). We suppose that the defendant is actually innocent or guilty.[4] Let γ denote the probability a randomly-selected cases involves a guilty individual. Hence, $1-\gamma$ is the probability a randomly-selected, charged individual is innocent. Let κ_t be the probability a type t defendant is convicted, $t \in \{g, i\}$. Let α_t denote the probability that a convicted individual of type t appeals his conviction. Finally, let μ_t denote the probability an appealed case by a type t defendant is modified (or reversed) by an appellate court, conditional on the case being appealed. We assume $\mu_i > \mu_g$.

There are numerous factors that could conceivably influence whether an individual appeals his conviction. It could be affected by income/wealth (i.e., ability to appeal), wishful thinking, intervention by a nonprofit defense group (e.g., Innocence Project), or the unexpected arrival of new information. We treat all of these factors as an exogenous probability of appeal (α_i). Importantly, we assume that these drivers are unrelated to prosecutor effort. Similarly, we take the decision to commit the crime and law enforcement's efficacy in correctly apprehending criminals as exogenous. The conviction probability and appellate decisions are endogenous variables.

With this setup, consider the probability a defendant is innocent, conditional on the case being appealed. Denote this conditional probability as I. Given that our data set consists of only those cases that result in both a conviction and an appeal, this value will tell us what proportion of the sample is expected to be innocent individuals who have been wrongfully convicted.

Using the variables defined, it follows that

$$I = \frac{(1-\gamma)\kappa_i\alpha_i}{(1-\gamma)\kappa_i\alpha_i + \gamma\kappa_g\alpha_g}. \tag{1}$$

[4]This is a simplification as the correct distinction might be whether the individual actually engaged in the activities claimed in the charges levied. Thus, one should think of guilt and innocence in the broadest possible sense.

To incorporate prosecutor effort, assume that the probability of convicting a defendant is

$$\kappa_g = \pi_g + f(\epsilon)$$

and

$$\kappa_i = \pi_g - \lambda f(\epsilon),$$

where ϵ is the effort exerted by the prosecutor in securing the conviction. Since the prosecutor does not know the defendant's type (and presumably is pursuing the conviction because she believes he is guilty), ϵ is not type dependent. The term π_t is the portion of the conviction probability that is driven by the evidence and context of the case (e.g., quality of the defense attorney, judicial decision making, jury composition, etc.), which can be expected to depend on the defendant's culpability. Assume $\pi_g > \pi_i$. The parameter $\lambda > 0$ captures any difference between effort obtaining convictions on the guilty and clearing the names of the wrongfully accused.[5] If $\lambda > 1$, then effort is more effective at reducing wrongful convictions, while if $\lambda \in (0, 1)$ effort primarily obtains convictions on the guilty. The function f is the productive transformation of effort into case outcomes. Thus, we assume $\frac{\partial f}{\partial \epsilon} > 0$.

Hence, it follows that

$$\frac{\partial I}{\partial \kappa_g} = \frac{-(1-\gamma)\gamma\kappa_i\alpha_i\alpha_g}{[(1-\gamma)\kappa_i\alpha_i + \gamma\kappa_g\alpha_g]^2} < 0$$

and

$$\frac{\partial I}{\partial \kappa_i} = \frac{(1-\gamma)\gamma\kappa_g\alpha_i\alpha_g}{[(1-\gamma)\kappa_i\alpha_i + \gamma\kappa_g\alpha_g]^2} > 0.$$

Therefore, since $\frac{\partial I}{\partial \epsilon} = \frac{\partial I}{\partial \kappa_i}\frac{\partial \kappa_i}{\partial \epsilon} + \frac{\partial I}{\partial \kappa_g}\frac{\partial \kappa_g}{\partial \epsilon}$, which simplifies to $\frac{\partial I}{\partial \epsilon} = \left[\frac{\partial I}{\partial \kappa_g} - \lambda\frac{\partial I}{\partial \kappa_i}\right]\frac{\partial f}{\partial \epsilon}$, it follows that

$$\frac{\partial I}{\partial \epsilon} = \left(\frac{(1-\gamma)\gamma\alpha_i\alpha_g[-\lambda\kappa_g - \kappa_i]}{[(1-\gamma)\kappa_i\alpha_i + \gamma\kappa_g\alpha_g]^2}\right)\frac{\partial f}{\partial \epsilon} < 0. \tag{2}$$

As a result, a randomly-selected appealed case is less likely to be an innocent individual when the prosecutor exerts more effort. Higher effort alters the pool of convicts to one comprised mostly of guilty individuals.

Consequently, the probability that a randomly-selected appealed case is modified is

$$\mu = \mu_i I + \mu_g(1 - I). \tag{3}$$

It follows that

$$\frac{\partial \mu}{\partial \epsilon} = (\mu_i - \mu_g)\frac{\partial I}{\partial \epsilon} < 0. \tag{4}$$

Therefore, considering cases which have been appealed, the probability that a randomly-selected case is reversed or modified should decrease with effort exerted. Stated differently, greater effort should lead to more decisions to uphold convictions. If an increase in wages leads to more effort, then we have our primary prediction.

[5]The assumption that λ is greater than zero is equivalent to assuming that prosecutor effort "clarifies" the asymmetric information problem. This is in the same spirit as study of plea bargaining in Bjerk [2007], which presumes that the effort put into trial preparation, when plea bargaining fails, acts to improve the information available to jurors. One can think of our assumption here as also coming from pressure put on prosecutors by voters to make accurate decisions, which includes minimizing both type I and type II errors.

Hypothesis: *The probability an appealed case is upheld increases when prosecutor pay increases.*

We will test this hypothesis empirically using data from New York state.

3 Prosecutor Pay in New York

Our empirical analysis focuses on the criminal justice system in New York state. Each county has a prosecution office headed by the District Attorney. The District Attorney (hereafter DA) is selected in a partisan, popular election to serve a four year term. The head prosecutor oversees a staff of Assistant District Attorneys (hereafter ADA) and supporting staff. DAs prosecute crimes at both the county court and the state's trial court, known in New York as the Supreme Court.[6] Justices in county and Supreme courts are also selected in popular, partisan elections and serve 10 and 14 year terms, respectively.

As a county office, the salary of the head prosecutor is paid from county funds. Thus, the budget available to a prosecutor's office is controlled by the county legislative board. Importantly for our analysis, though, the salary of the head prosecutor is determined by the state government.

New York state did not raise the pay for justices for a twelve year period prior to 2011. Under intense pressure, in 2011 a commission was created to assess the impact of this nominal wage freeze and determine whether a pay increase was needed. The primary argument for the pay increase was that the best legal minds were not choosing to be judges, but rather were picking careers in the private sector. The commission concurred with this belief and recommended that New York state bring its Supreme Court justices' pay up to the level of Federal judges. This constituted a 41 percent pay increase starting on April 1, 2012. The increase was rolled out over time so that the justices pay equaled Federal judges by 2016 [Pfau, 2011].

What was overlooked by this commission was that state law ties DA salary to judge salaries. Section 183-A of New York state law dictates:

> Notwithstanding any other provision of law, the district attorney of each county having a population of more than five hundred thousand according to the last federal census, exclusive of the counties of New York, Bronx, Kings, Queens and Richmond, shall receive an annual salary equivalent to that of a justice of the state supreme court together with such additional compensation as the legislative body of such county may provide by local law. Further, that the district attorney of each county having a population of more than one hundred thousand and less than five hundred thousand according to the last federal census, exclusive of the county of Richmond, and the district attorney of any county, the board of supervisors of which has designated such office as a full-time position pursuant to subdivision eight of section seven hundred of the county law, shall receive an annual salary equivalent to that of county judge in the county in which the district attorney is elected or appointed, together with such additional compensation as the legislative body of such county may provide by local law.

County court justices are paid 95% of the salary for state Supreme Court justices. Hence, New York state law fixes prosecutor salaries to judge salaries. Counties with populations less than 100,000, though, are exempt. Thus, the substantial pay increase received by justices in New York affect prosecutors' salaries

[6] Felony crimes can be prosecuted in either court. The important distinction is that civil lawsuits over $25,000 are heard in the Supreme Court. Civil disputes less than $25,000 are handled in the county courts.

only in those counties with populations in excess of 100,000. Evaluating the text of the Commission's report and every supporting document submitted by interested parties, the Commission did not make a single mention of the effect that judge compensation had on prosecutors. Thus, the pay increase was unexpected and shocking to county prosecutors and county officials in charge of budgeting.

The unanticipated, unfunded mandate provoked responses after the fact. The New York State Association of Counties called on the state legislature to fund the increase in salaries. Previously, the head prosecutor salary was fully financed in the county budgets. Effective lobbying has since resulted in a state subsidy to offset this increased expense [NYSAC, 2016]. Local newspaper coverage at the time reported that although county leaders called for state support, the counties will most likely initially be paying for the pay raises themselves [Raymo, 2016, Hughes, 2016]. These calls for legislative action, and the lack of mention of the effect on prosecutors in the Commission report, strongly supports the exogeneity of the salary change. Although endogenous for judges, the substantial, exogenous shock to prosecutor compensation acts as a quasi-natural experiment. Since low population counties were exempt from the change, a difference-in-difference estimation strategy will produce causal identification of how changes in salary may affect district attorney's behavior and criminal justice outcomes.

Two additional notes regarding the identification strategy are worth emphasizing. First, the policy change affects justices in all jurisdictions, regardless of population size. If it was the case that both judge and prosecutor pay increased in the treated counties (those with populations over 100,000) and neither judge nor prosecutor pay changed in the untreated counties, then there would be no way to differentiate the effect of prosecutor effort from judicial effort. Here, though, justices receive a pay increase *regardless* of the county's population. Therefore, changes in the difference between the treated and control counties after the pay increase cannot be due to judicial compensation, but rather is a consequence of prosecutorial pay. Therefore, while compensation can be expected to alter judicial decision making, a difference-in-difference estimation will single out the marginal effect of prosecutor pay. Second, ADA and supporting staff salaries remain line-item expenses on the county budgets. The state policy did not affect their compensation. Therefore, our identification strategy isolates head prosecutor pay and how it incentivizes the leadership to encourage and motivate the employees' effort exertion.

4 Data and Methods

4.1 Data

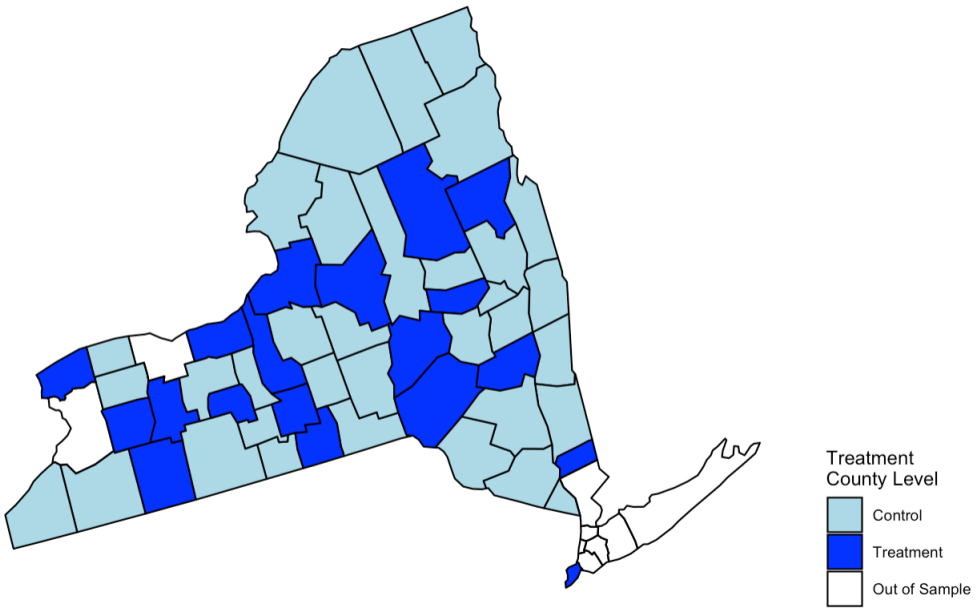
The data set for our empirical analysis is an extension of data used in DeAngelo and McCannon [2020]. All appealed convictions between 2008 and 2016 are obtained from each appellate division's website.[7] Recordable information on the justice, prosecutor, and case-specific variables is collected. This includes the date of the initial conviction and the court within which the cases was decided.

There are 62 counties in New York state. For our study, we exclude the nine metropolitan counties with

[7]Eight cases were appealed in 2007. We include these observations.

more than 500,000 residents since the prosecution of crime can be expected to be substantially different in these counties. Further, the greater New York City metropolitan area organizes a distinct criminal court system. Thus, we exclude these large counties and analyze appeals of criminal convictions in 53 counties in New York state. Based on New York state law, there are 19 counties treated by the salary increase (those with a population between 100,000 and 500,000 residents) leaving 34 counties (those with less than 100,000 residents) to act as our control group. Figure 1 depicts a map of New York state with color indication for which counties are in the treatment, control, and out of sample.

Figure 1: Treated Counties in New York



Map of New York state with coloring/shading to match the population size and, hence, treatment variable.

The original convictions of the appealed criminal convictions occured between 2007 and 2016. The full sample of data contains 26,414 appeals, but the sample we use consists of 5,499 appeals of criminal convictions in the non-NYC metro counties with less than 500,000 residents.

There are 84 DAs and 364 ADAs in our sample. Our treated counties average 12.5 ADAs per county, while our control counties average 7.0 ADAs. The counties not included in our analysis average 60.2 ADAs, as they differ substantially in size. This is further evidence that the largest counties should not be included in our analysis. Staffing is in line with population sizes.

Our primary outcome variable of interest, *Upheld*, is an indicator variable equal to one if the lower court's conviction is affirmed. The variable *Upheld* is equal to zero if the appellate court either reverses or modifies the conviction. Occasionally, the appellant may have multiple charges in the conviction being appealed. Hence, it is possible that one conviction is affirmed while another is not. In these rare circumstances we record them as $Upheld = 0$ since our objective is to measure the existence of any procedural errors or

miscarriages of justice. In our sample, approximately 80% of appealled criminal convictions are affirmed.

Tables 1 and 2 provide summary statistics for the treatment and control groups. Table 1 reports means for the affirmation rate, population size, and the number of appealed cases. Table 2 reports relevant demographic variables.[8]

Table 1: Summary Statistics

	Pre-Period (low pay regime)	Post-Period (high pay regime)
Treated Counties		
% Upheld = 1	81.7%	80.0%
Population	266,707	266,521
# obs.	2205	1355
Control Counties		
% Upheld = 1	76.5%	75.5%
Population	64,596	66,479
# obs.	1120	819

The differences in means is statistically significant for pre-period vs. post-period population of the control counties. The difference in affirmation rate means between the treated and control counties for both before the pay increase (first column) and after the pay increase (second column) are statistically significant at the 5% and 1% level of significance, respectively.

Table 1 reports a lower affirmation rate for the counties in the control group. These differences are significant for both the low-pay regime and the high-pay regime. The difference in the mean affirmation rates between the pre-period and post-period (for either the control or treated counties) are statistically insignificant. This indicates that the pay increase has, potentially, no effect on upheld rates.

Table 2: County Demographics

	Control (1)	Treatment (2)	Differences in means (2) − (1)
% Population Under 25	31.17%	33.38%	2.21 **
Hispanic Population (%)	3.46^	6.99%	3.54 ***
Black Population (%)	3.64%	7.91%	4.27 ***
% without a Bachelor's	79.30%	70.10%	-9.20 ***
% Unemployed	8.24%	7.85%	-0.39 **
% in Poverty	13.49%	12.99%	-0.51 **

Asterisks represent the result of a difference-in-means t-test; *** 1%; ** 5%, * 10% level of significance.

Table 2 reports demographic information across the two groups of counties.[9] The treatment and control groups are significantly different along all these dimensions. Treatment counties have populations that are younger, more racially diverse, less educated, less unemployed, and have a smaller percentage of the population in poverty. A difference-in-difference estimation strategy, by controlling for the difference between these two groups, will disentangle how this distinction between the control and treated counties change as

[8]Tables A.1, A.2, and A.3 in the appendix provide summary statistics of the grounds for appeals and crime types between treatment and control groups across the pre- and post-periods.

[9]These demographic variables come from the U.S. Census's American Community Survey which provides annual, county-level information.

the policy is implemented.

4.2 Methods

We utilize a difference-in-differences method to estimate the effects of the salary change on whether an appealed case is upheld. The method allows us to parse out baseline distinctions between the treatment and control groups and isolate the causal effect of the salary increase on prosecutor performance. Our dependent variable, as previously discussed, indicates whether a conviction is upheld when appealed. This allows us to capture when a prosecutor's work raises concerns and whether the lower court's outcome is deemed a mistake.

We estimate a model that includes a binary variable for treated locations, $Treated$, a binary variable for being in the period after the prosecutor wage increase, $Post$, and an interaction of those two variables for our variable of interest: $Post \times Treated$. In addition, we will consider two-way fixed effects models. Specifically, we will estimate:

$$Upheld_{ikmy} = \alpha_0 + \alpha_1 Post_{imy} + \alpha_2 Treated_{ik} + \alpha_3 Post_{imy} \times Treated_{ik} + \nu_m + X_{ikmy}\theta + \epsilon_{ikmy}, \quad (5)$$

and

$$Upheld_{ikmy} = \beta_0 + \beta_1 Post_{imy} \times Treated_{ik} + \nu_m + \upsilon_y + \kappa_k + X_{ikmy}\theta + \epsilon_{ikmy}. \quad (6)$$

Each specification includes month-of-year fixed effects (ν_m) and a set of control variables (X_{ikmy}). Equation 6 includes year controls (υ_y) and county fixed effects (κ_k). We will vary estimations by what is included in the set of control variables. Namely, we will consider controls for DA, crime committed, grounds for appeal, and a set of case-specific information.[10] We include these time and cross-sectional fixed effects to measure within-county, year, and month effects and to provide evidence that our results are not indicative of unobserved yearly, monthly, or spatial trends. These controls are consistent with those used in DeAngelo and McCannon [2020].

One concern is that it is possible that our results could be driven by changes in case types over time. If different crime types are appealed over time, or those that are appealed are argued on different grounds, then our estimation strategy will not necessarily be capturing prosecutor effort. If the appeals that arise after the pay increase come from markedly different types of cases, then we would be mistakenly assigning the distinct success of these appealed convictions to prosecutor effort. To investigate this concern, we predict our dependent variable, *Upheld*, on our fixed effects and controls. Figures A.1 and A.2 in the appendix show the distributions of our predicted upheld rates. Figure A.1 compares the differences in the treatment and control groups and Figure A.2 compares the differences in the pre- and post-periods. The distributions are

[10] This set includes whether the case was heard in the county court or the Supreme Court, whether the appellate decision was unanimous, the number of days that elapsed between the initial conviction and the appeal, indicators for the type of defense representation, indicator variables for the mode of conviction, the slip opinion's length in words, an indicator variable for whether the DA was up for re-election at the time of the initial conviction, and an indicator variable for whether the defendant was the respondent to the appeal.

all centered around similar means. Hence, the upheld rate of appealed cases does not appear to differ in the pre- versus post-period or in treated versus control counties.

Tables A.1, A.2, and A.3 in the appendix compare average grounds for appeal and crime types in the pre- and post-periods. There are not statistically significant differences in the grounds for appeal and crime types in the appealed cases. Thus, it does not appear that there are changes in charging or appealing behavior that resulted from the DA wage increase.

Another requirement for our results to be interpreted as causal when using a difference-in-difference estimation strategy is that we successfully show that parallel trends holds. Figures 2 and 3 present parallel trend graphs that display the upheld rate and number of cases over time. The figures present averages from the treatment and control groups in each year and include error bars representing the 95% confidence intervals.

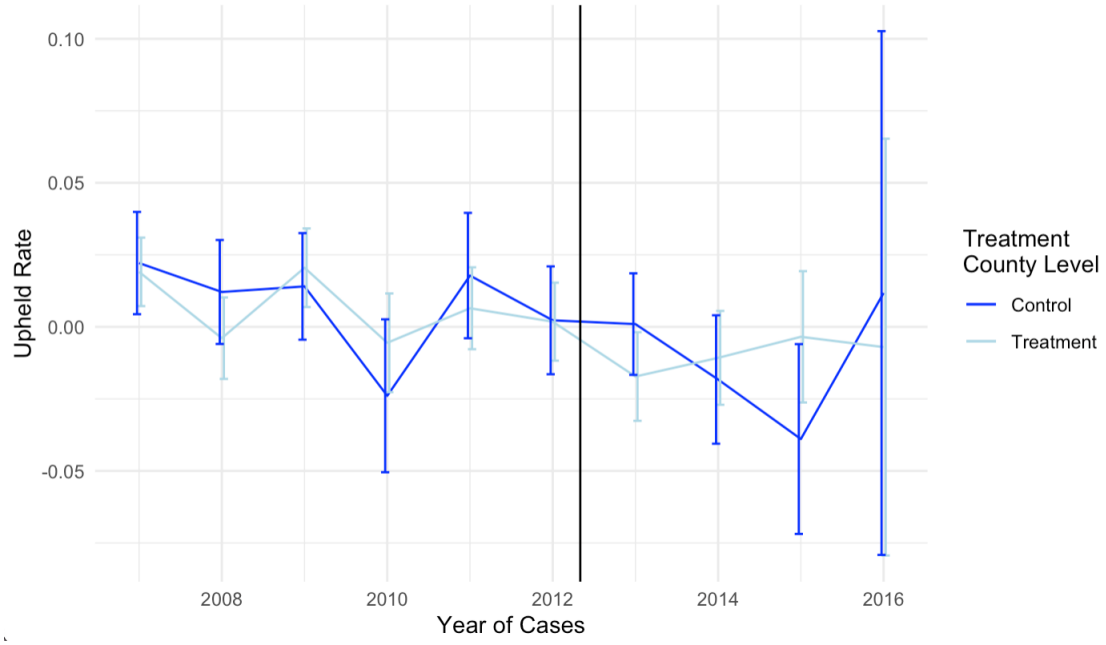


Figure 2: Parallel Trends in the Affirmation Rates

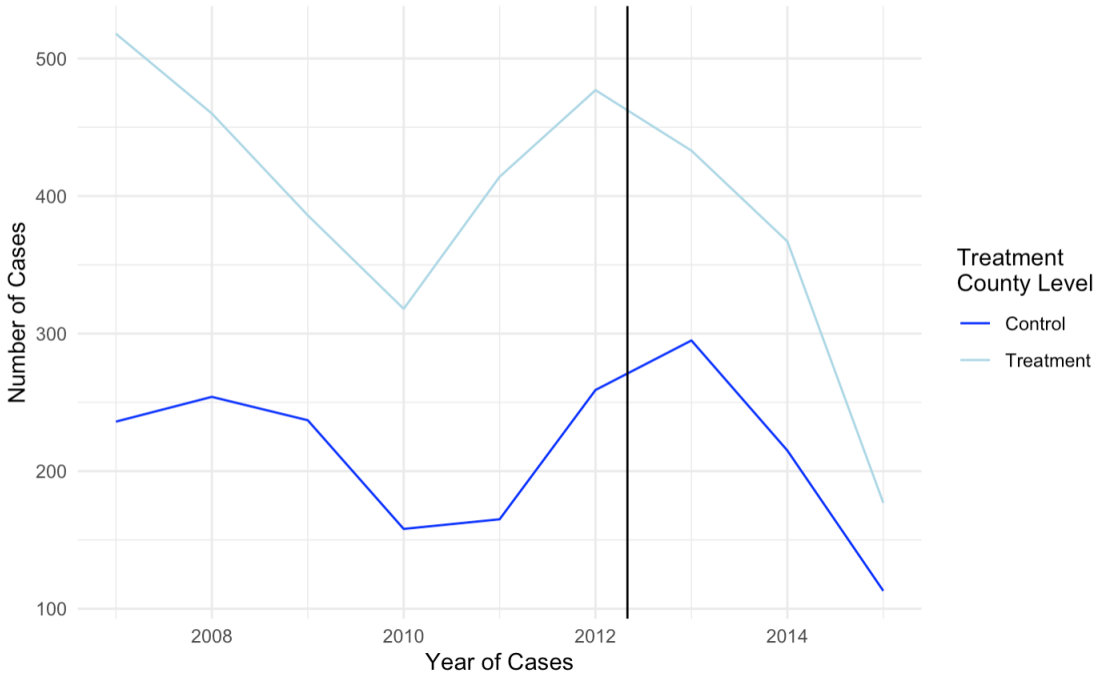


Figure 3: Parallel Trends in the Number of Cases Appealed

Figure 2 indicates that there is not a statistical difference between the treatment and control groups in the pre-period. The success of appealed cases is similar in each year. Similarly, Figure 3 indicates that, although case loads change over time, these trends are followed by both treatment and control counties. These two figures indicate that the parallel trends assumption likely holds.

Finally, to interpret our estimates as causal, we need to ensure that SUTVA is not violated. It is unlikely that the treatment of one county is affected by the treatment of other counties, though. The salary increase of one county's district attorney will not lead to a salary increase for a control county because the treatment is not driven by the district attorney. Moreover, it is unlikely that a district attorney in a control county could cause a spike in population growth, as measured in a previous year census, to gain a salary increase, and we do not see counties with populations on the cusp of switching back and forth between treatment and control counties. Therefore, although SUTVA cannot be tested, we feel confident that it is not violated.

Based on these results, the difference-in-difference coefficients can be interpreted as giving a causal identification of the impact of the change in the head prosecutor's pay on performance.

5 Results

We estimate our model across numerous specifications. We start by considering the difference-in-difference model presented in Equation 6. Column 1 of Table 3 presents the results with only time and county fixed effects, while column 2 adds the DA indicators and control variables. The final specification in column 3 is the full model with controls for the crime committed and the grounds for appeal. Standard errors are

clustered at the county level. We feel this is the most appropriate treatment of the standard errors as the "experiment" is at the county level. We suppress the presentation of the fixed effects and controls and only report the difference-in-difference coefficient.

Table 3: Prosecutor Compensation and Appeals

	(1)	(2)	(3)
Post x Treated	-0.0149	-0.0279	-0.0219
	(0.0231)	(0.0319)	(0.0328)
County Fixed Effects?	Yes	Yes	Yes
Year Fixed Effects?	Yes	Yes	Yes
Month of Year Fixed Effects?	Yes	Yes	Yes
DA Fixed Effects?	No	Yes	Yes
Controls?	No	Yes	Yes
Crime Controls?	No	No	Yes
Grounds for Appeal Controls?	No	No	Yes
R^2	0.0306	0.1015	0.1351
AIC	5542.4	5148.8	4917.2

This table presents linear probability estimates with an indicator variable equal to one if the conviction was upheld as the dependent variable. Standard errors clustered by county are presented in parentheses; *** 1%, ** 5%, * 10% level of significance. Data set includes all appeals in the 53 counties with populations under 500,000 between 2008 and 2016 except for those dismissed by the appellate court; $N = 5,499$. There are 15 indicator variables for grounds for appeal and 41 indicator variables for the crime committed. Controls include indicator variables for county court and other/missing court (with Supreme Court as the omitted category), an indicator for whether the decision was unanimous, the number of days between the trial conviction and the appellate decision, indicator variables for whether the defense is from a legal aid society, other/missing defense (with a public defender as the omitted category), indicator variables for mode of conviction (jury trial, nonjury trial, other/missing - with a guilty plea as the omitted category), length of the slip opinion (in words), an indicator variable for whether the prosecutor is up for re-election, and an indicator for whether the defendant is the respondent in the appeal.

For each specification, the difference-in-difference coefficient is small and statistically insignificant. For example, in column 1 the p-value exceeds 0.6. In fact, the coefficient is recorded as negative, which would suggest that the increased compensation *reduces* the effort put in by the prosecutor. The coefficient moves farther from the range of positive values, and the standard errors grow, as controls are included.

Numerous additional specifications are considered. We choose not to present them all here. In these specifications we vary the sets of control variables included and the formulation of the fixed effects. Namely, we further saturate the model with either month by year time controls, keeping county fixed effects, or by including county by year fixed effects, and including month of year controls. In addition, we consider clustering the standard errors at the county by year level. In each of these 30 additional specifications, the difference-in-difference coefficient is insignificant and remains close to zero. Further, we estimate Equation 5, which considers a standard difference-in-difference specification. Again, we vary the inclusion/exclusion of controls, grounds for appeal, crime committed, and vary the form of the fixed effects. Once again, we consider multiple standard error calculations for each specification and find nearly identical results. Further, since we have a binary dependent variable, we also re-estimate Equation 5 but with a probit model. The fixed effects

and controls are varied, along with the calculation of the standard errors. These last two checks evaluate another 20 specifications. Regardless of these efforts, the difference-in-difference coefficient is statistically insignificant and remains near zero in each specification.[11]

Thus, our results point to a robust insignificant relationship between prosecutor pay and effort, as measured by success of appealed convictions, despite a 41% pay increase. This provides strong evidence that, at the margin, the overall effect of the pay increase on the quality of the convictions obtained is a precisely estimated zero effect.

6 Prosecutor Effort and Plea Bargaining

A distinguishing feature of the U.S. criminal justice system is the prevalence of plea bargaining. It was not an understatement when Justice Anthony Kennedy referred to the U.S. as having a "system of pleas".[12] The vast majority of criminal convictions arise from guilty pleas that are commonly facilitated from a plea bargaining process.[13]

A frequent concern about plea bargaining is that it relieves the prosecutor from having to make the effort investments that a jury trial requires. Rather than expend the resources to further investigate the case, a prosecutor can simply make a generous plea offer to the defense to save herself the costs of the investigative efforts.

The plea bargaining rate varies by the seriousness of the crime committed. In Table 4 we categorize those convictions appealed into five broad categories. The frequency at which the initial conviction was plea bargained is provided.[14]

Table 4: Plea Bargaining Rates

	% of obs. with a guilty plea
Violent Crimes	70.85%
Property Crimes	86.61%
Drug Violations	85.12%
Sex-Related Crimes	86.40%
Minor Offenses	81.72%

While these rates are derived from our set of appealed cases, and not the universe of convictions, the results mirror common findings. Property crimes, drug violations, and minor offenses rarely go to a jury trial. When plea bargaining fails, it fails in the prosecution of violent crimes. Therefore, since violent crimes often go to trial and require a substantial amount of time, effort, and financial resources to prosecute (relative to nonviolent crimes), it is a natural place to search for changes in a prosecutor's incentives.

[11] These various difference-in-difference specifications can be viewed in Tables A.4, A.5, and A.6 within the appendix.
[12] See *Lafler v Cooper* (2012).
[13] A typical state, along with the Federal government, has more than 95% of convictions arising from guilty pleas.
[14] The calculations only include those observations where both the crime committed and the mode of conviction are known.

As a consequence, we consider a triple-difference specification. Not only are we interested in the difference between outcomes in those counties treated with the compensation policy and those not in the years prior to and after the policy was put into place, but also whether outcomes differ by those with a guilty plea and those without. Specifically, we estimate

$$\begin{aligned} Upheld_{ikmy} =& \gamma_0 + \gamma_1 Post_{imy} + \gamma_2 Treated_{ik} + \gamma_3 Plea_{ikmy} + \gamma_4 Post_{imy} \times Plea_{ikmy} \\ &+ \gamma_5 Post_{imy} \times Treated_{ik} + \gamma_6 Plea_{ikmy} \times Treated_{ik} \\ &+ \gamma_7 Post_{imy} \times Treated_{ik} \times Plea_{ikmy} + \epsilon_{ikmy}. \end{aligned} \tag{7}$$

The triple-difference coefficient, γ_7, is of primary interest. Equation 7 will be estimated on both the full sample as well as the subsample of violent crimes. Table 5 presents the results.

Table 5: Plea Bargaining and Violent Crimes

	Diff in Diff		*Triple Difference*	
	(1)	(2)	(3)	(4)
Treated	0.052 **	-0.087	0.55 *	0.368 ***
	(0.024)	(0.077)	(0.032)	(0.084)
Post	-0.011	-0.031	-0.029	0.667
	(0.030)	(0.068)	(0.041)	(0.042)
Plea			0.414 ***	0.634 ***
			(0.027)	(0.029)
Treat x Plea			-0.002	-0.436 ***
			(0.036)	(0.099)
Post x Plea			0.012	-0.756 ***
			(0.037)	(0.049)
Post x Treat	-0.006	0.073	0.006	-0.575 ***
	(0.035)	(0.094)	(0.051)	(0.154)
Post x Treat x Plea			-0.011	0.666 ***
			(0.048)	(0.174)
R^2	0.0038	0.0071	0.0342	0.0759
AIC	56588	270.0	5496.0	249.4
sample	all	violent	all	violent
N	5499	343	5499	343

Results from linear probability models presented. Standard errors clustered by County presented in parentheses; *** 1%, ** 5%, * 10% level of significance. The first and second columns estimate a standard difference-in-difference model, while the third and fourth columns estimate a triple-difference model.

The first two columns ignore plea bargaining and estimate the difference-in-difference model.[15] Overall, whether we consider the full data set, or the subsample of violent crimes, the compensation change continues to fail to register an effect.

[15]The specifications presented do not include any control variables. As argued previously, the results are unaffected by their exclusion. We choose to consider the models without controls because in specifications of only violent crimes the number of observations is small, and we wish to preserve the degrees of freedom. There are 343 observations where the appeal is from a known violent crime. Plea bargaining occurs in just over 70% of these observations ($N = 243$).

Column 4 provides an interesting result. If we consider the difference between jury trial convictions and guilty pleas, the improved prosecutor compensation corresponds to increased affirmations of violent crime convictions. This effect does not exist for nonviolent crimes and explains the lack of results in the full sample (Column 3). The statistical significance of the triple-difference coefficient persists if standard errors are clustered at the county by year level and if controls and month of year fixed effects are included. Hence, the significance of this coefficient in Column 4 is not sensitive to the specification.

We replicate this exercise for the other crime categories. Table 6 presents the triple difference coefficients. Whether it is minor offenses, drug violations, property crimes, or sex-related offenses, there is no measurable difference in the success of appealed convictions that come from guilty pleas, as compared to jury trial convictions. It is within the set of violent crimes that prosecutor effort matters. Here, the incentives created by higher compensation have an influence.

Table 6: Plea Bargaining in Nonviolent Crimes

Crime Category	DDD coefficient	std error
Sex Offenses	0.051	(0.136)
Drug Violations	-0.159	(0.125)
Minor Offenses	0.166	(0.133)
Property Crimes	-0.043	(0.096)

7 Conclusion

We leverage a quasi-natural experiment which increased pay for head prosecutors in counties in New York state to evaluate the impact of higher compensation on prosecutorial effort. In a theoretical model, we show that improved effort can be measured by changes in the rate at which appealed convictions are upheld. Using data from New York state, we show that the pay increase did not have a meaningful effect on prosecutor performance, even though the change was substantial. This suggests that, using the conceptual framework of efficiency wages, further increases in pay do not necessarily lead to additional effort exertion.

Given commonly voiced concerns about plea bargaining practices in the United States, we further investigate whether plea bargaining a case, rather than taking it to trial, acts as a mechanism for prosecutor shirking. Focusing on violent crimes that, presumably, require substantial time and effort to prosecute at trial, we find that there is a statistically significant effect of the pay increase on the likelihood violent crime cases that were plea bargained are upheld on appeal. While cases with guilty pleas are upheld at a higher baseline rate, the quality of the plea bargained cases, relative to jury trial convictions, improves after the salary increase in the treated counties. This suggests that the improved efficiency wage, whether it is driven by reciprocal motives or the threat of losing the job, affects effort exertion in the plea bargaining process of violent crime cases.

An important issue that our analysis is unable to explore is how the incentives of the head, elected

prosecutor trickles down to the behavior of subordinates in the office. While the voting public selects the individual who leads the office, this head prosecutor has a staff of assistant prosecutors who handle most prosecution decisions. Through hiring, firing, monitoring, and office policymaking, this head prosecutor is presumably able to influence the case handling decisions. How this internal mechanism functions is not observable in our data. Therefore, an important area for future research is understanding how the incentives of the head of the office influence the case handling of the subordinates. Further, we do not explore the compensation of these assistant prosecutors. It may very well be that the disparity between the head prosecutor's compensation and pay to assistant prosecutors is meaningful in that, while we show that little change occurs when the head prosecutor's pay increases, improvements in ADA compensation may have substantial improvements.

Another limitation worth acknowledging is external validity. We look at a radical pay increase in New York state. It may be the case that compensation in other states differ so that the lack of effect we highlight may not hold elsewhere. For example, a feature of New York state is the substantial difference between the cost of living in urban versus rural areas. With pay fixed by the state government, prosecutors in the numerous rural counties of upstate New York are paid well above private market lawyer salaries. It is reasonable, then, to presume that the motivations created by efficiency wages are already in effect so that a further pay increase has no *marginal* effect. In a state without this discrepancy, pay increases may be more important. Nevertheless, since we are evaluating a substantial improvement in pay, the observation that it does not have a meaningful impact is likely telling of what smaller increases in pay would do in other jurisdictions.

Finally, we focus on the effect that financial compensation has on effort provision. The discussion of salary of public actors includes other dimensions that we do not study. Most prominently, in the context of judicial pay, it is primarily argued that improved pay reduces the loss of highly skilled, intelligent individuals to the private market. Improving prosecutor pay may not only halt any loss of skilled individuals to the private market, but may discourage turnover by junior prosecutors in the office. We are unable to investigate retention here. In related work, McCannon [2021] provides evidence that the pay increase expanded the incumbency advantage in elections. As another concern, the prosecutors' salaries come from public funds. This must either crowd out other publicly provided services or come from increased taxation. However, the full welfare consequences are beyond the scope of our analysis.

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8 Appendix

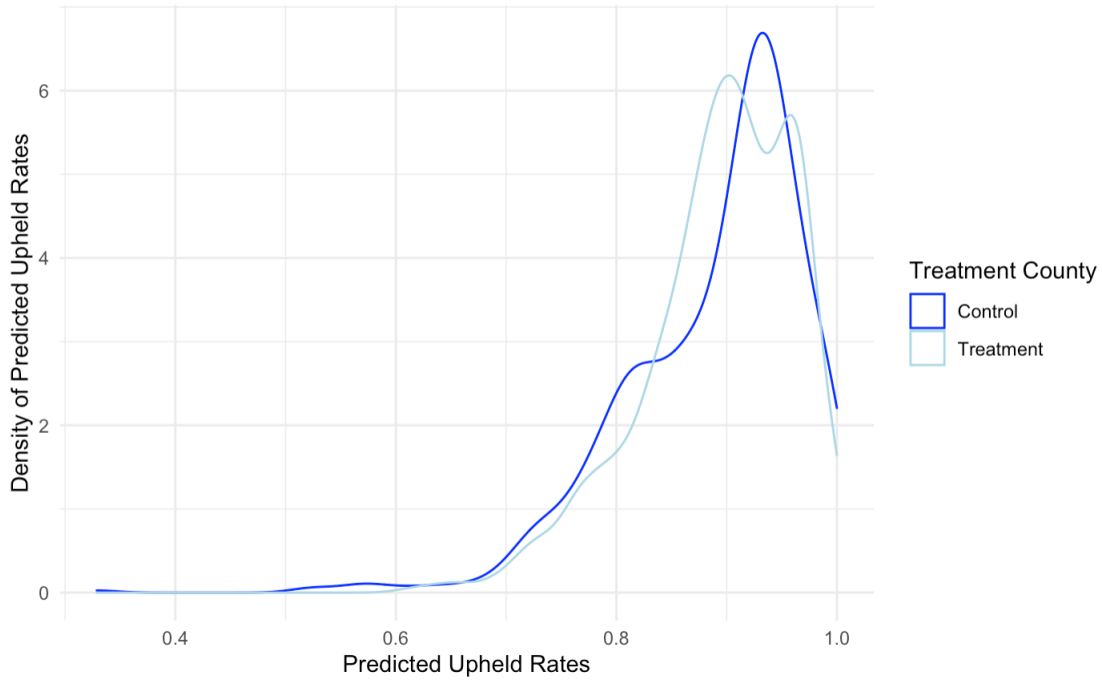

Figure A.1: Predicted Affirmation Rates: Treated and Control Counties

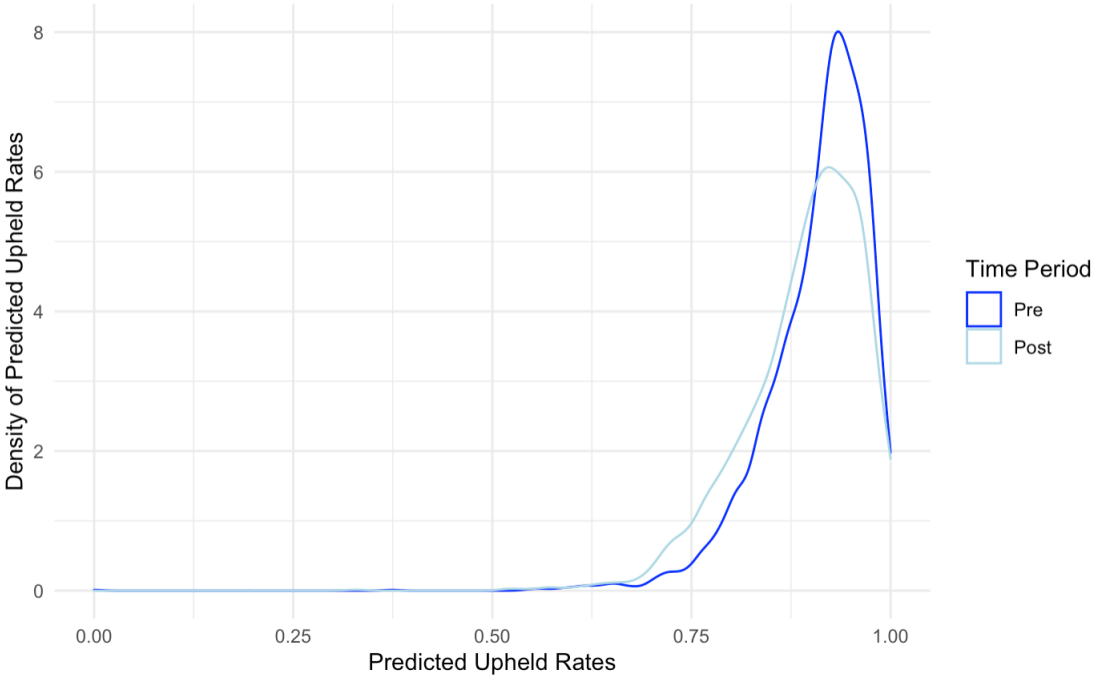

Figure A.2: Predicted Affirmation Rates: Pre-Period and Post-Period

Table A.1: Grounds for Appeal Summary Statistics

	Pre-Period Means	Post-Period Means	Differences in Means
Treatment			
Resentence	0.114	0.112	-0.003 *
Sufficient	0.303	0.290	-0.013
Severe	0.229	0.272	0.043 *
Juror	0.055	0.059	0.004
Mental	0.110	0.104	-0.007
Youth	0.029	0.032	0.003
Suppression	0.150	0.139	-0.012
Coerce	0.033	0.040	0.007
Instructions	0.047	0.041	-0.006
Speedy Trial	0.018	0.017	-0.001
Coercion	0.015	0.014	-0.001
Double Jeopardy	0.010	0.003	-0.007
Incapacitated	0.006	0.007	0.001
Control			
Resentence	0.151	0.115	-0.035 *
Sufficient	0.301	0.272	-0.029
Severe	0.272	0.324	0.052 *
Juror	0.055	0.053	-0.002
Mental	0.107	0.103	-0.004
Youth	0.030	0.027	-0.004
Suppression	0.118	0.117	-0.001
Coerce	0.044	0.033	-0.011
Instructions	0.046	0.044	-0.002
Speedy Trial	0.019	0.023	0.004
Coercion	0.021	0.017	-0.004
Double Jeopardy	0.009	0.007	-0.002
Incapacitated	0.005	0.011	0.006

An asterisk denotes significance at the 5% level.

Table A.2: Treated County Crimes Summary Statistics

	Pre-Period Means	Post-Period Means	Differences in Means
Larceny	0.019	0.018	-0.001
Robbery	0.041	0.043	0.002
Sex	0.087	0.089	0.002
Intoxicated	0.009	0.010	0.0005
Unlicensed	0.003	0.004	0.001
Vehicle	0.010	0.018	0.008 *
Assault	0.050	0.040	-0.011
Homicide	0.001	0.001	0.001
Arson	0.002	0.001	−0.002
Gang	0.002	0.003	0.001
Burglary	0.053	0.042	-0.011
Bail	0.0005	0.002	0.002
Conspiracy	0.006	0.004	−0.002
Weapon	0.049	0.051	0.001
Controlled	0.061	0.066	0.005
Sale	0.029	0.029	0.0004
Possession	0.104	0.102	-0.002
Child	0.017	0.016	−0.001
Contempt	0.013	0.007	−0.007
Drug	0.004	0.001	−0.003
Forgery	0.002	0.002	0.0004
Murder	0.024	0.022	−0.002
Rape	0.018	0.012	−0.006
Contraband	0.003	0.001	−0.002
Reckless	0.005	0.006	0.001
Endangerment	0.005	0.004	−0.0001
Marijuana	0.008	0.004	−0.004
Tampering	0.003	0.001	−0.002
Mischief	0.006	0.002	−0.004
Fraud	0.001	0.001	0.001
Kidnapping	0.003	0.004	0.001
Manslaughter	0.010	0.008	−0.002
Menacing	0.002	0.002	0.0004
Property	0.007	0.005	−0.002
Forge	0.005	0.008	0.003
Substance	0.061	0.066	0.005

An asterisk denotes significance at the 5% level.

Table A.3: Control County Crimes Summary Statistics

	Pre-Period Means	Post-Period Means	Differences in Means
Larceny	0.013	0.023	0.010
Robbery	0.010	0.019	0.010
Sex	0.082	0.100	0.017
Intoxicated	0.029	0.019	−0.010
Unlicensed	0.013	0.009	−0.005
Vehic	0.015	0.013	−0.002
Assault	0.022	0.043	0.020∗
Burglary	0.032	0.049	0.016
Weapon	0.007	0.010	0.003
Controlled	0.040	0.060	0.019∗
Sale	0.021	0.030	0.010
Possession	0.038	0.053	0.016
Child	0.005	0.019	0.014∗
Contempt	0.010	0.018	0.008
Drug	0.002	0.010	0.008∗
Forgery	0.002	0.001	−0.001
Murder	0.005	0.006	0.001
Rape	0.013	0.015	0.001
Contraband	0.006	0.009	0.002
Reckless	0.003	0.007	0.005
Endangerment	0.001	0.005	0.004
Marijuana	0.004	0.001	−0.002
Mischief	0.005	0.005	−0.0005
Fraud	0.001	0.001	0.0003
Menacing	0.001	0.006	0.005∗
Property	0.004	0.002	−0.002
Forge	0.007	0.007	0.0001
Substance	0.040	0.060	0.019∗

An asterisk denotes significance at the 5% level.

Table A.4: Difference-in-Difference Results

	Two-Way Fixed Effects:														
	fixed effects only			*adding controls*			*adding grounds*			*crime & grounds*			*adding crime*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Post x Treated	-0.0149	-0.0132	0.0081	-0.0279	-0.282	0.0044	-0.0234	-0.0261	0.0097	-0.0219	-0.0268	0.0231	-0.0272	-0.0295	0.0170
	(0.0317)	(0.0325)	(0.0511)	(0.0319)	(0.0341)	(0.0487)	(0.0328)	(0.0362)	(0.0462)	(0.0328)	(0.0362)	(0.0474)	(0.0317)	(0.0340)	(0.0497)
	{0.639}	{0.687}	{0.875}	{0.384}	{0.412}	{0.928}	{0.479}	{0.474}	{0.834}	{0.507}	{0.462}	{0.629}	{0.395}	{0.389}	{0.734}
	[0.0231]	[0.0253]	[0.0456]	[0.0240]	[0.0274]	[0.0425]	[0.0239]	[0.0281]	[0.0392]	[0.0239]	[0.0281]	[0.0395]	[0.0240]	[0.0274]	[0.0428]
	{0.518}	{0.602}	{0.860}	{0.244}	{0.374}	{0.917}	{0.327}	{0.352}	{0.805}	{0.361}	{0.340}	{0.560}	{0.258}	{0.283}	{0.692}
Year FEs?	Yes	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes	No	No
Month FEs?	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
County FEs?	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Month x Year FEs?	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes	No
County x Year FEs?	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Grounds for Appeal?	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Crimes?	No	No	No	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
DA FEs?	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls?	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.0306	0.0486	0.1218	0.1015	0.1199	0.1787	0.1277	0.1459	0.2054	0.1351	0.1531	0.2127	0.1088	0.1270	0.1857
AIC	5542.4	5501.3	4981.4	5148.8	5071.3	4636.7	5010.5	4906.0	4479.4	4975.2	4859.7	4458.2	5139.8	5026.3	4643.6

Results from linear probability models presented. Data set includes all appeals except for those dismissed; $N = 5499$. Standard errors clustered by county presented in parentheses. Standard errors clustered at the county by year level presented in brackets. Below each are the associated p-values in the curly brackets. The difference-in-difference coefficient is presented for each specification. There are 15 indicator variables for grounds for appeal and 41 indicator variables for the crime committed. Controls include indicator variables for County Court and other/missing court (with Supreme Court as the omitted category), an indicator for whether the decision was unanimous, the number of days between the trial conviction and the appellate decision, indicator variables for whether the defense is from a legal aid society, other/missing defense (with a public defender as the omitted category), indicator variables for mode of conviction (jury trial, nonjury trial, other/missing - with a guilty plea as the omitted category), length of the slip opinion (in words), and indicator variable for whether the prosecutor is up for re-election, and an indicator for whether the defendant is the respondent in the appeal.

Table A.5: Difference-in-Differences Results (continued)

	Standard DiD Specification:				
	(1)	(2)	(3)	(4)	(5)
Treated	0.0516	-0.0360	0.0602	0.0677	-0.0335
	(0.0235)	(0.0400)	(0.0441)	(0.0450)	(0.0404)
	{0.033} **	{0.371}	{0.178}	{0.138}	{0.411}
	[0.0178]	[0.0676]	[0.0694]	[0.0692]	[0.0677]
	{0.004} ***	{0.594}	{0.386}	{0.328}	{0.621}
Post	-0.0106	0.0036	-0.0024	-0.0011	0.0057
	(0.0296)	(0.0305)	(0.0325)	(0.0326)	(0.0306)
	{0.722}	{0.908}	{0.941}	{0.972}	{0.852}
	[0.0233]	[0.0234]	[0.0244]	[0.0246]	[0.0235]
	{0.649}	{0.879}	{0.922}	{0.963}	{0.808}
Post x Treated	-0.0062	-0.0234	-0.0198	-0.0210	-0.0252
	(0.0351)	(0.0353)	(0.0372)	(0.0371)	(0.0351)
	{0.861}	{0.510}	{0.596}	{0.575}	{0.477}
	[0.0297]	[0.0272]	[0.0277]	[0.0278]	[0.0273]
	{0.835}	{0.390}	{0.474}	{0.451}	{0.356}
Month FEs?	No	Yes	Yes	Yes	Yes
DA FEs?	No	Yes	Yes	Yes	Yes
Controls?	No	Yes	Yes	Yes	Yes
Grounds?	No	No	Yes	Yes	No
Crime?	No	No	No	Yes	Yes
R^2	0.0083	0.1001	0.1267	0.1341	0.1074
AIC	5658.8	5141.2	5000.8	4982.0	5148.5

Results from linear probability models presented. Data set includes all appeals except for those dismissed; $N = 5499$. Standard errors clustered by county presented in parentheses. Standard errors clustered at the county by year level presented in brackets. Below each are the associated p-values in curly brackets.

Table A.6: Difference-in-Differences Probit Results

	Standard DiD Specification:				
	(1)	(2)	(3)	(4)	(5)
Treated	0.1788	-3.1608	-2.4835	-2.6310	-3.1827
	(0.0815)	(0.2680)	(0.2968)	(0.2910)	(0.2678)
	{0.028} **	{0.001} ***	{0.001} ***	{0.001} ***	{0.001} ***
	0.0510	-0.8548	-0.6600	-0.6924	-0.8541
	(0.229) **	(0.0743) ***	(0.0812) ***	(0.0792)	(0.0735)
	{0.026}	{0.001}	{0.001}	{0.001} ***	{0.001} ***
Post	-0.0310	0.0146	-0.0016	0.0005	0.0219
	(0.0938)	(0.1078)	(0.1172)	(0.1186)	(0.1088)
	{0.741}	{0.892}	{0.989}	{0.997}	{ 0.840}
	-0.0088	0.0040	-0.0004	0.0001	0.0059
	(0.268)	(0.292)	(0.0311)	(0.0312)	(0.0292)
	{0.742}	{0.892}	{0.989}	{0.997}	{0.840}
Post x Treated	-0.0244	-0.0969	-0.0936	-0.0971	-0.1041
	(01153)	(0.1278)	(0.1377)	(0.1388)	(0.1286)
	{0.832}	{0.448}	{0.497}	{0.484}	{0.418}
	-0.0070	-0.0262	-0.0249	-0.0256	-0.0279
	(0.329)	(0.346)	(0.0266)	(0.0364)	(0.0345)
	{0.832}	{0.449}	{0.497}	{0.484}	{0.418}
Month FEs?	Yes	Yes	Yes	Yes	Yes
DA FEs?	No	Yes	Yes	Yes	Yes
Controls?	No	Yes	Yes	Yes	Yes
Grounds?	No	No	Yes	Yes	No
Crime?	No	No	No	Yes	Yes
Pseudo R^2	0.0048	0.0895	0.154	0.1229	0.0968
AIC	5613.5	5161.2	5038.6	4988.0	5133.0
N	5499	5463	5463	5442	5442

Results from probit models presented. Data set includes all appeals except for those dismissed. For each explanatory varialbe the first estimate listed is the probit coefficient. The second is the marginal effect. Standard errors clustered by county presented in parentheses. Below each are the associated p-values.