Spring 2022

Essays on Crime and Law

Morgan Elaine Stockham
Claremont Graduate University

Essays on Crime and Law

Morgan Stockham

Claremont Graduate University
2022

Approval of the Dissertation Committee

This dissertation has been duly read, reviewed, and critiqued by the Committee listed below, which hereby approves the manuscript of Morgan Stockham as fulfilling the scope and quality requirements for meriting the degree of Doctor of Philosophy in Economics.

Gregory DeAngelo, Chair
Claremont Graduate University
Associate Professor of Economic Sciences

Thomas Kniesner
Claremont Graduate University
University Professor of Economic Sciences

Melissa Rogers
Claremont Graduate University
Associate Professor of International Studies

Rebecca Thornton
University of Illinois at Urbana-Champaign
Associate Professor of Economic Sciences

Abstract

Essays on Crime and Law
Morgan Stockham
Claremont Graduate University: 2022

Civil law and public policy often are related to crime in complex and interesting ways. The following dissertation explores the intersections of divorce law and public policy with criminal outcomes.

Within my first chapter, we[1] attempt to identify the causal effect of wages on a prosecutor's effort by studying an exogenous salary increase in New York. We measure the performance by the likelihood that a conviction is upheld when appealed. If the efficiency wage theory explains behavior, then the exogenous wage shock should entice better performance. Alternatively, if individuals who hold office are motivated primarily by an intrinsic motivations 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 a pay increase has a null effect on prosecutor performance.

In my second chapter, we[2] revisit Stevenson and Wolfers (2006) which theorized that unilateral divorce laws shifted power from one party to a distribution of power across both parties in the marriage. They theorized that this effectively providing an application of Coasian Bargaining. Utilizing a two-way fixed effects (TWFE) difference-in-difference estimator, they find that unilateral divorce had significantly reduced suicide rates, domestic violence, and intimate homicide. Innovations in econometric theory have raised concerns regarding the use of TWFE with differential timing in the treatment variable, leading to biased estimation. We revisit Stevenson and Wolfers (2006) with more modern estimators for suicide and intimate homicide rates and utilize appropriate estimators to examine the effect of unilateral divorce laws on suicides and intimate partner homicide rates. In contrast to the original research, we do not find significant effects of unilateral divorce on suicide and intimate homicide rates.

Finally, I explore divorce and crime within Cook County, Illinois. Divorce in the United States is a common phenomena with the most recent estimates indicating that 35 percent of adults over 20 have been divorced at least once (Mayol-García et al., 2021). Two of the main consequences of divorce are income and social exclusion. These, in turn, have been shown

[1] This is joint work with Gregory DeAngelo and Bryan McCannon
[2] This is joint work with Anuar Assamidanov and Muhammad Salman Khalid

to be determinants of the propensity to commit crime (Buonanno and Bicocca, 2021). This paper investigates the impact of divorce on the propensity to commit crime using individual level data. Utilizing the random assignment of judges and their calendars in Cook County, Illinois to divorce cases, I measure the impact of a longer case on an individuals' propensity to commit low-level crimes. This paper finds that longer divorce cases increase an individual's likelihood of committing low-level crimes by up to 16 % over the mean when a case length is increased by 100 days.

This work is dedicated to my parents for their endless and eternal love.

Acknowledgments

Accomplishments are nothing without first acknowledging those that have supported me through my PhD. First, I'd like to thank my committee chair, Greg DeAngelo, for taking a chance on me and for seeing something in me I didn't yet see. Thank you to my committee members Thomas Kneisner, Melissa Rogers, and Rebecca Thornton, for supporting me endlessly and giving amazing feedback. Thank you to the crime economists of my life, Bryan McCannon, Scott Cunningham, and Mark Hoekstra, for supporting me within my research and studies. Thank you, especially, to Menna Bizuneh, who is the economist that led me to even be here at all. I am inspired to be a better economist because of each and every one of you.

Thank you to my lab-mates, of which a list would be too long, for the laughter that brightened my days. Thank you to Ami Bhatt, Richard Parnell, and Rainita Narender for making CGU my home. Thank you to my best friends, Esma Dollaku, Kyla Pickell, and Kaitlyn Swift, for being the most incredible friends I could have ever asked for. Thank you to Kimberly Swift, Servete Dollaku, Sado Dollaku, and Kathy Pickell for being such loving second-parents to me.

Thank you to my partner and best friend, Saimun Habib, for loving me and supporting me through this PhD. You are the wind beneath my wings and the warmth in my heart. I am lucky to love and be loved by you.

Thank you to my grandmother, Elaine Crowder, for being the strength in my bloodline and a great supporter of my education. Finally, thank you to my parents, Michael and Sarah Stockham. I am only here because of you two and I am truly the luckiest daughter in the world. Words will never be able to convey my intense gratitude for your work being the pillars beneath me and supporting me through *every* success and defeat.

For all of those I have mentioned, and the many more whose names are just not written here, I pull the wagon up-hill for us. I am eternally grateful to have stood on the shoulders of giants.

Contents

1 Chapter 1: District Attorney Compensation and Performance

Coauthored with Gregory Deangelo and Bryan McCannon

1.1 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).[3] Compensation in

[3]As articulated over 100 years ago by the famous Supreme Court justice Charles Evans Hughes, "But

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.[4]

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

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).

[4]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.

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 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.[5] 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.

[5] 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.

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

1.2 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.[6] 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

[6] 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.

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

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

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.[7] 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 $\frac{\partial f}{\partial \epsilon} > 0$.

[7] 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.

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.

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.

1.3 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.[8] 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

[8] 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.

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 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.

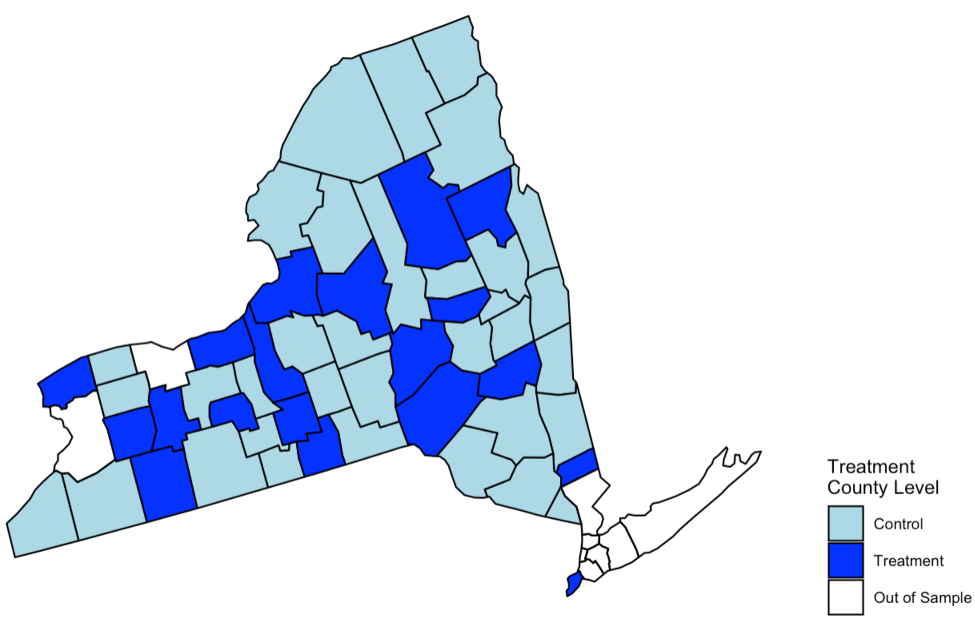
1.4 Data and Methods

1.4.1 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.[9] 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 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.

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

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.[10]

[10]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.

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 (%)	3.46ˆ	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.[11] The treat-

[11]These demographic variables come from the U.S. Census's American Community Survey which provides

ment 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 the policy is implemented.

1.4.2 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:

$$\begin{split} Upheld_{ikmy} = \alpha_0 + \alpha_1 Post_{imy} + \alpha_2 Treated_{ik} + \alpha_3 Post_{imy} \times Treated_{ik} + \nu_m + \\ X_{ikmy}\theta + \epsilon_{ikmy}, \end{split} \tag{5}$$

and

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

annual, county-level information.

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.[12] 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 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

[12] 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.

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.

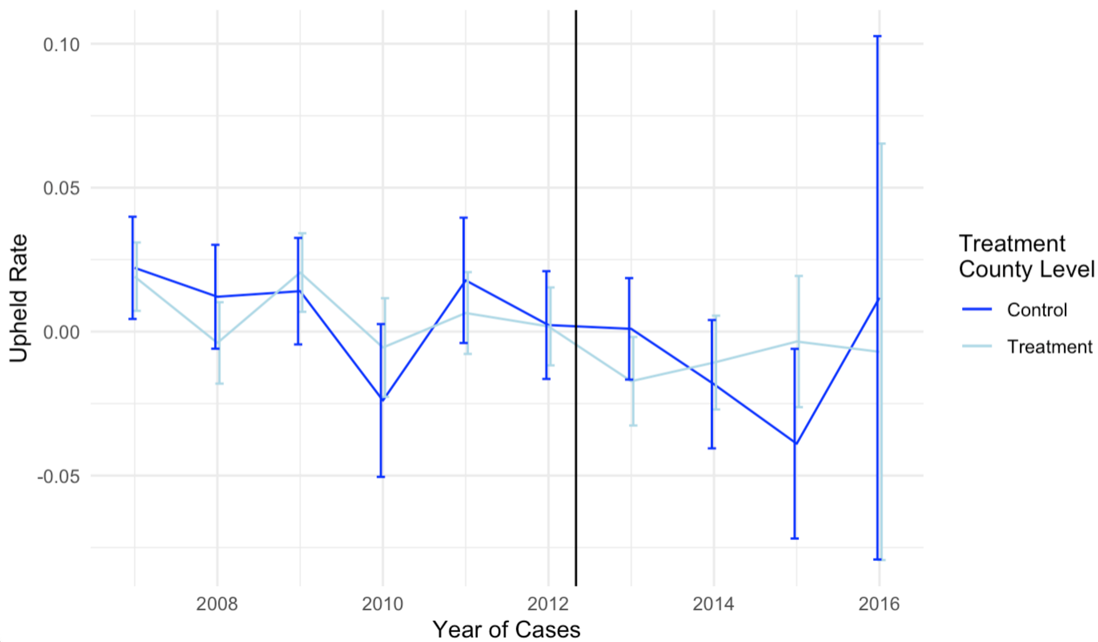


Figure 2: Parallel Trends in the Affirmation Rates

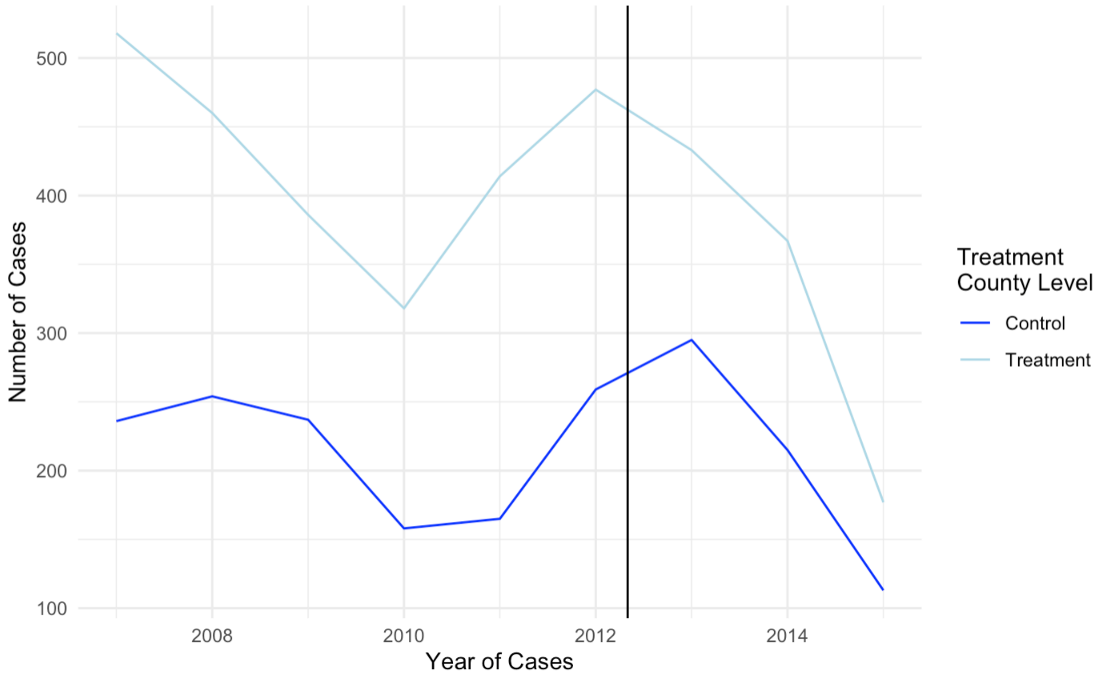


Figure 3: Parallel Trends in the Number of Cases Appealed

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.

1.5 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.[13]

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.

1.6 Prosecutor Effect 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".[14] The vast majority of criminal convictions arise from guilty pleas that are commonly facilitated from a plea bargaining process.[15]

A frequent concern about plea bargaining is that it relieves the prosecutor from having

[13]These various difference-in-difference specifications can be viewed in Tables A.4, A.5, and A.6 within the appendix.

[14]See *Lafler v Cooper* (2012).

[15]A typical state, along with the Federal government, has more than 95% of convictions arising from guilty pleas.

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.[16]

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.

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

[16]The calculations only include those observations where both the crime committed and the mode of conviction are known.

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

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.[17] Overall, whether we consider the full data set, or the subsample of violent crimes, the compensation change continues to fail to register an effect.

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)

[17] 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 ($N = 243$).

1.7 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.

2 Chapter 2: Revisiting Unilateral Divorce, Health, and Crime

Coauthored with Anuar Assamidanov and Muhammad Salman Khalid

2.1 Motivation

The seminal work of Stevenson and Wolfers (2006) examined the impact of unilateral divorce laws on suicide, domestic violence, and intimate homicide rates. The mechanism by which unilateral divorces laws are theorized to impact bargaining power from one party to the middle is through Coasian Bargaining. It is argued that the shift in power would have positive impacts on outcomes, primarily for women, wherein each party would have more equalized power to leave the marriage. Women would feel empowered to leave relationships where they were dealing with thoughts of suicide, experiencing domestic violence, or possibly at risk of becoming the victim of intimate partner homicide. They estimated that unilateral divorce laws caused decreases in all three outcomes.

Although unilateral divorce laws may shift bargaining power from the holding party to the middle, it is possible that these laws may not be monotonically good or effective. In the case of no-fault divorce the decision to be divorced is only required by one party *and* the courts will not hear that a party is at fault and therefore not offset the costs to the "not-at-fault party". Similarly, many states still require cooling-off periods which can be sped up with agreements between parties, but even in the case of no-fault divorces, a party can choose to hold out on agreeing to a speedier divorce, which would continue to create an imbalance in power.

Their paper also utilized two-way fixed effects (TWFE) estimators with differential timing in treatment. In more recent years, conversations about the validity of TWFE have risen, with papers such as Goodman-Bacon (2018) calling into question the weighting of comparison control and treatment groups in an aggregate treatment effect. Within Goodman-Bacon (2018) he presents a case-study of decomposing Stevenson and Wolfers (2006) using different data sources. He reveals that the estimations of Stevenson and Wolfers (2006) may be incorrect because of the problematic comparison groups within the Average Treatment Effect (ATE) of TWFE.

According to a popular AI citation search-engine, Scite, this research has been cited 214 times by multiple researchers (Scite, 2021). If this research is inappropriately estimated, as we believe it is, then it is inappropriate research to use in supporting claims that no-fault divorce laws are wholly positive for society at large. We believe that the inappropriateness of the original author's estimation strategies coupled with the potential usage of that original research to be used in a harmful way to be strong reasons to revisit this paper.

While using new estimators, we find that there is not a positive net benefit for women's intimate homicide rates and that the benefit for women's suicide rates follows 15 years after the adoption of the laws. The former indicates that there is no net benefit of unilateral divorce laws for women's intimate homicide rate. This could be positive for some women, but on net the effect is null. The latter finding provides evidence that women's positive returns on suicide rates, may not be driven solely by unilateral divorce. Decreases in suicide rates 15 years after adoption is unlikely to be driven only by unilateral divorce laws because this is an extraordinarily long time period after adoption.

This paper revisits Stevenson and Wolfers (2006) but utilizes appropriate estimators from modern econometric theory. The paper is structured as follows: section 2 is our replication of their initial findings, section 3 is a decomposition of these estimations, section 4 presents more modern estimators across suicide rates and intimate homicide rates, and section 5 concludes.

2.2 Replication

In this paper, we used the same data as Stevenson and Wolfers (2006) to test the effect of unilateral divorce laws on number of suicide, domestic violence, and intimate partner homicide. Particularly, the data captures the years from 1964 through to 1996, 36 states and District of Columbia adopted unilateral divorce law, and 14 states did not adopt, which was considered as the control population. Additionally, there is a differential timing of adoption of the law across the states. Data on suicide and homicide were from the National Center for Health Statistics (NCHS) and the FBI Uniform Crime Reports (UCR). We replicate the findings of the original paper in the following sub-sections.

2.2.1 Two-Way Fixed Effects Method

To provide evidence that our data is similar we replicate the tables in the original paper. The original method utilized in Stevenson and Wolfers (2006) is the two-way fixed effects method difference-in-differences. This method compares treatment and control groups in the pre- and post-periods. The estimation takes on the following form:

$$Outcome_{sy} = \beta_0 + \beta_1 Unilateral + \nu_y + \kappa_s + X_{sy}\theta + \epsilon_{sy}. \tag{1}$$

Each specification includes year fixed effects (ν_y), state fixed effects (κ_s), and a set of control variables (X_{sy}). The control variables included in the original estimations include the maximum AFDC rate for a family of four, the natural log of state personal income per capita, the unemployment rate, the female-to-male employment rate, age composition variables indicating the share of states' populations aged 14 –19, and then ten-year cohorts beginning with age 20 up to a variable for 90, and the share of the state's population that is Black, White, and other (Stevenson and Wolfers, 2006). The variable of interest, $Unilateral$, is expected to be negative and significant across all specifications.

2.2.2 Replications

Column 1 of Tables 1 and 2 displays the TWFE estimation with state and year FE's in the original paper. Column 2 adds controls. Columns 3 and 4 present our replication of their TWFE results. Column 5 adds clustering at the state level, which was absent in the original paper. Abadie et al. (2017) showed that the use of normal standard errors may lead to incorrect inference and using clustering at the treatment level can improve the inference (Abadie et al., 2017). Our replications find the similar result that unilateral divorce had a statistically significant and negative effect on female suicides beginning eight years after adoption. Male suicides are also impacted 9-10 years after the adoption of unilateral divorce. However, these effects become insignificant when clustered standard errors were used.

Table 1: Effect Of Unilateral Divorce Laws On Female Suicide Rates

Variable	(1)	(2)	(3)	(4)	(5)
Year of Change	1.6%	1.3%	1.6%	1.1%	1.1%
	(3.8)	(3.4)	(3.8)	(3.3)	(3.5)
1-2 years later	-1.5%	-1.4%	-1.5%	-1.2%	-1.2%
	(3.7)	(3.5)	(3.7)	(3.5)	(4.8)
3-4 years later	-1.5%	-1.1%	-1.5%	-1.1%	-1.1%
	(3.1)	(3.1)	(3.1)	(3.1)	(4.2)
5-6 years later	-3.0%	-2.0%	-3.0%	-1.9%	-1.9%
	(2.9)	(2.9)	(2.9)	(2.9)	(4.2)
7-8 years later	-8.0%	-6.6%	-8.0%***	-6.5%**	-6.5%
	(3.0)	(3.0)	(3.0)	(3.0)	(4.5)
9-10 years later	-10.0%	-8.5%	-10.0%***	-8.4%***	-8.4%*
	(3.0)	(3.0)	(3.0)	(3.0)	(4.5)
11-12 years later	-11.9%	-10.2%	-11.9%***	-10.2%***	-10.2%*
	(3.1)	(3.2)	(3.1)	(3.2)	(5.2)
13-14 years later	-12.8%	-11.1%	-12.8%***	-11.0%***	-11.0%**
	(3.2)	(3.1)	(3.2)	(3.1)	(5.0)
15-16 years later	-13.3%	-11.7%	-13.3%***	-11.7%***	-11.7%**
	(3.7)	(3.6)	(3.7)	(3.6)	(5.8)
17-18 years later	-16.4%	-13.9%	-16.4%***	-13.8%***	-13.8%**
	(3.6)	(3.6)	(3.6)	(3.6)	(5.5)
>=19 years later	-18.7%	-16.4%	-18.6%***	-16.3%***	-16.3%**
	(3.2)	(3.3)	(3.2)	(3.3)	(6.7)
F-test of joint significance	p = 0.00	p = 0.00	p = 0.00	p = 0.00	p = 0.32
Average Effect	-9.7	-8.3	-9.5	-8.1	-8.1
	(2.3)	(2.3)	()	()	()
Original Paper	Yes	Yes	No	No	No
State and Year FE	Yes	Yes	Yes	Yes	Yes
Control Variables	No	Yes	No	Yes	Yes
Robust SE	Yes	Yes	Yes	Yes	Yes
Cluster	No	No	No	No	At State

Standard Errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2: Effect Of Unilateral Divorce Laws On Male Suicide Rates

Variable	(1)	(2)	(3)	(4)	(5)
Year of Change	-0.8%	-1.4%	-0.8%	-1.4%	-1.4%
	(2.2)	(2.1)	(2.2)	2.1	(2.5)
1-2 years later	1.2%	0.5%	1.2%	0.5%	0.5%
	(1.5)	(1.4)	(1.5)	(1.4)	(2.0)
3-4 years later	0.0%	-0.9%	0.0%	-0.9%	-0.9%
	(1.6)	(1.5)	(1.6)	(1.5)	(2.1)
5-6 years later	0.4%	-0.2%	0.4%	-0.2%	-0.2%
	(1.5)	(1.5)	(1.5)	(1.5)	(2.0)
7-8 years later	-1.0%	-1.3%	-1.0%	-1.3%	-1.3%
	(1.8)	(1.8)	(1.8)	(1.8)	(2.6)
9-10 years later	-3.5%	-3.9%	-3.5%**	-3.9%**	-3.9%
	(1.7)	(1.7)	(1.7)	(1.7)	(2.7)
11-12 years later	-2.2%	-2.6%	-2.2%	-2.7%	-2.7%
	(2.0)	(2.0)	(2.0)	(2.0)	(3.2)
13-14 years later	-3.2%	-3.6%	-3.2%	-3.6%*	-3.6%
	(2.0)	(2.0)	(2.0)	(2.0)	(3.5)
15-16 years later	-1.6%	-2.0%	-1.6%	-2.0%	-2.0%
	(2.0)	(1.9)	(2.0)	(2.0)	(3.3)
17-18 years later	-1.6%	-1.9%	-1.6%	-1.9%	-1.9%
	(2.1)	(2.0)	(2.1)	(2.0)	(3.4)
>=19 years later	-3.9%	-4.3%	-3.9%*	-4.3%**	-4.3%
	(2.0)	(2.0)	(2.0)	(2.0)	(4.2)
F-test of joint significance	p = 0.36	p = 0.37	p = 0.36	p = 0.36	p = 0.59
Average Effect	-1.5	-2.0			
	(1.3)	(1.3)	()	()	()
Original Paper	Yes	Yes	No	No	No
State and Year FE	Yes	Yes	Yes	Yes	Yes
Control Variables	No	Yes	No	Yes	Yes
Robust SE	Yes	Yes	Yes	Yes	Yes
Cluster	No	No	No	No	At State

Standard Errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3 presents our replication results for the impact of no-fault divorce laws on domestic violence. Table 3 provides estimates using OLS with state fixed effects, controls, time-varying controls, and probit estimations across overall and severe violence with male and female spouses as victims. These results are nearly identical to the original paper and indicate that domestic violence decreases in overall husband to wife violence and in severe violence between both husband to wife and wife to husband domestic violence.

Table 3: Effect of Unilateral Divorce on Domestic Violence

	Overall violence		Severe Violence	
	Husband to wife	Wife to husband	Husband to wife	Wife to husband
	Average incidence of each type of violence			
	11.7%	11.9%	3.4%	4.5%
OLS(Diffs-in-diffs)	-4.3%**	-2.7%	-1.1%	-2.9%***
	(1.9)	(1.8)	(1.3)	(1.0)
Add state fixed effects	-5.5%***	-3.2%**	-2.0%**	-3.6%
	(1.8)	(1.5)	(0.9)	(0.7)
Add individual controls	-5.0%***	-1.9%	-1.8%*	-3.4%***
	(1.8)	(1.4)	(1.0)	(0.9)
Add state-level time-varying controls	-3.6%**	-1.8%	-1.8%*	-3.0%***
	(1.5)	(1.3)	(1.0)	(0.7)
Probit with individual controls	-4.7%***	-2.0%	-1.2%*	-2.1%***
	(1.6)	(1.3)	(0.7)	(0.7)

Table 4 is a replication of the results in Stevenson Wolfers (2006), which we replicate nearly identically. We find that unilateral divorce has a significant reductions on intimate homicide against women with and without controls. This result holds across violence from spouses, family members, and known assailants. These effects do not extend to non-intimate homicides (defined as the homicide rate less the intimate homicide rate or triple difference homicides (intimate less non-intimate homicide rates). We do not estimate significant effects for men except when including controls and estimating the effect of unilateral divorce on murders of men by known assailants

Table 4: Effect of Unilateral Divorce on Intimate Homicide

	No Controls	Including Controls		
	Intimate homicide	Intimate homicide	Placebo nonintimate homicide	Diffs-in-Diffs-in-Diffs (intimate less nonintimate)
	(1)	(2)	(3)	(4)
	Women Murdered by Intimates			
By Spouse	-10.5*	-11.8**	-4.3	-7.55
	(5.7)	(5.8)	(3.5)	(6.3)
By Family	-8.9**	-9.1**	-3.4	-5.7
	(4.2)	(4.4)	(4.1)	(5.5)
By Known	-8.7**	-8.9**	-3.2	-8.6
	(3.5)	(3.6)	(5.2)	(5.6)
	Men Murdered by Intimates			
By Spouse	12.3	4.4	-4.0	8.4
	(8.9)	(8.5)	(2.6)	(8.3)
By Family	1.8	-4.1	-3.4	-.72
	(5.1)	(5.2)	(2.7)	(5.1)
By Known	-2.0	-5.8*	-2.8	-5.5
	(3.0)	(3.0)	(4.1)	(4.7)

clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

2.3 Decomposition of Results

Goodman-Bacon (2018) proposed a method to decompose the average treatment effect on the treated (ATT) into different comparison groups. He identifies that certain groups (Later treated observations versus any treatment level) are problematic treatment group comparisons. Intuitively, this is because these treatment groups are comparing samples that are newly treated to samples that have been treated before. This is inappropriate because difference-in-differences econometric design should only produce estimations with comparison groups that are treated and untreated.

In addition, two other groups, which compare the earlier treatment group with the later control group and the later treatment group with the earlier control, impose stronger parallel trend assumption across time between different groups. This indicates that these comparison groups are also inappropriate because these parallel trends assumptions may not hold up, indicating an inappropriate ATE estimation.

It is also possible to think of a scenario where the heterogeneity of treatment effects may effect the estimate. If early adopting states have a smaller effect than late adopters, then the estimated effect between these two groups could be biased downwards.

The Goodman-Bacon decompositions shows the ATTs for each group and the weights assigned to them. If these inappropriate comparison groups have more weight, then the ATT should be seen with caution and is more likely to be biased. We used the Goodman-Bacon decomposition to see the distribution of ATTs of unilateral divorce on suicide rates with and without co-variates and intimate homicide with and without co-variates. We find that the ATTs from the comparison of inappropriate groups are assigned higher weights, indicating that the original treatment estimations are biased.

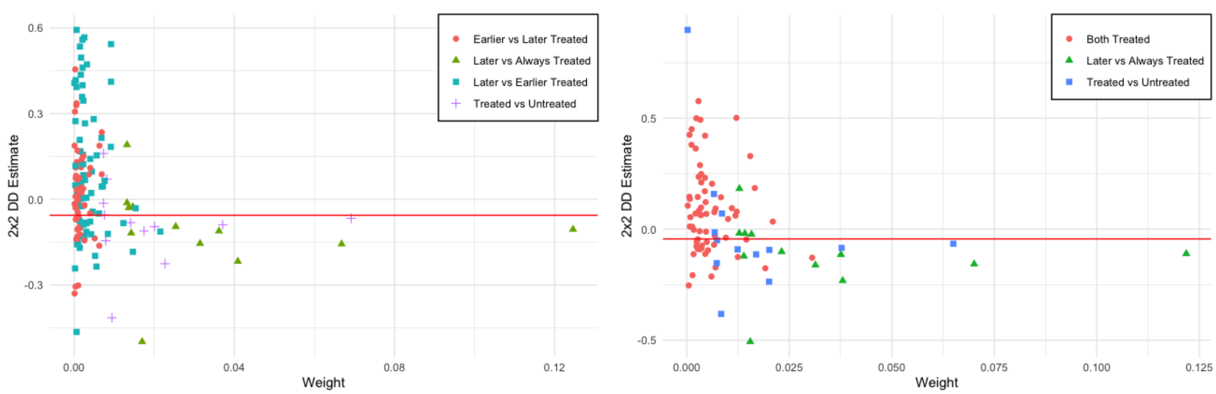
2.3.1 Suicide

For each of the following figures, the red line represents the ATE of TWFE estimations and each of the points represents an ATT of a different group-time cohort comparison group. These group-time cohorts are represented by different types of points for what kind of comparison group it is.

"Earlier versus Later Treated" and "Treated versus Untreated" groups are intuitive to TWFE. In each of these comparison types we're comparing treated units to either untreated or not-yet treated units. This akin to treated versus untreated units. "Later versus Always Treated" and "Later versus Earlier Treated" comparison groups are comparing two types of treated units and are inappropriate estimation groups. In these we would be comparing treated units to control units that are either always treated or treated before the treatment unit.

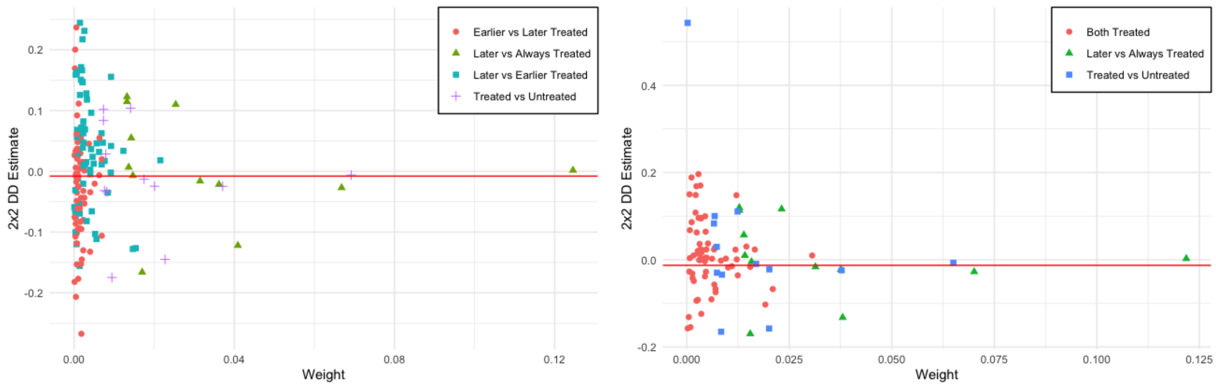
Figures 1 and 2 provide graphical representations of the group-time effects. As can be visually observed, the original estimate is influenced by biased groups that are pulling the estimate negative.

Appendix Tables B.1 and B.2 provide decompositions of effects of female and male suicide rates with and without controls, respectively. We find that the effects are driven by mostly "Later versus Always" and "Later versus Earlier Treated", as well as "Both Treated". This indicates that the original estimated effects are biased and may not indicate the correct effect of no-fault divorce on suicide rates.



Without co-variates　　　　　　　　　　With co-variates

Figure 1: Goodman-Bacon Decompositions by Group-Time for Female Suicide Rates



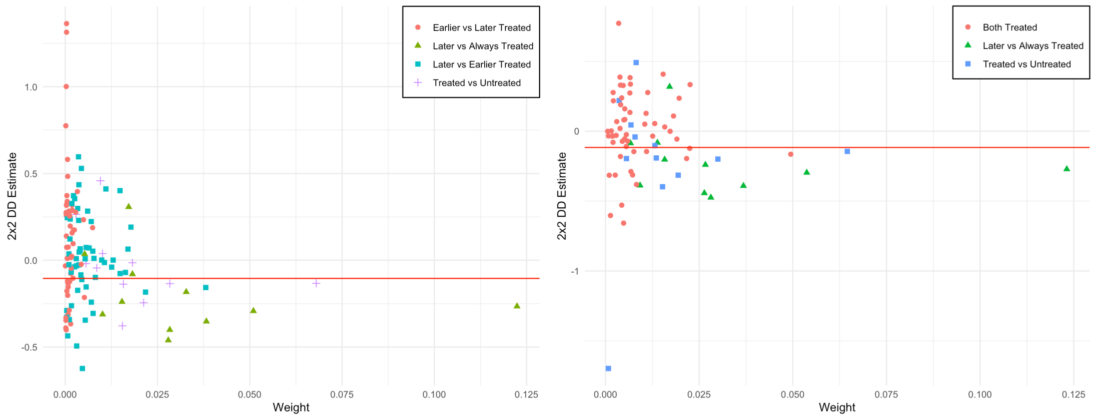
Without co-variates With co-variates

Figure 2: Goodman-Bacon Decompositions by Group-Time for Male Suicide Rates

2.3.2 Intimate Homicides

Figure 3 presents visualizations of ATTs by group-time cohorts. The initially estimated effects of -10.5 percentage points without co-variates and -11.8 percentage points with co-variates are presented as red lines in each figure. Both figures indicate that some inappropriate groupings are being given heavier weights and therefore are biasing the estimated results.

The "Later vs Earlier" Treated group receives the bulk of the weight in the estimated ATE when excluding co-variates. The "Earlier vs Later" Treated group receives most of the weight when including co-variates. The prevailing belief of Stevenson and Wolfers (2006) is that the estimation should be driven by treated versus untreated comparison groups, but we find that most of the estimation is driven by groups that bias estimation. This indicates that estimated effects may be driven by comparison groups that are inappropriate and require that we estimate the impact of unilateral divorce on intimate homicide using different estimators. Appendix table B.3 and B.4 present the decompositions of the effect into groups by weight and average estimate.



Without co-variates With co-variates

Figure 3: Goodman-Bacon Decompositions by Group-Time for Female Intimate Homicide Rates

In sum, the results of the decomposition indicate that the TWFE estimators are being influenced by "Later versus Always Treated" and "Later versus Earlier Treated". This indicates the need for research to re-examine the impact of unilateral divorce laws on suicides and intimate partner homicides. In the next section, we propose new estimators to be used to estimate the true effect of no-fault divorce law adoption on suicide and intimate partner homicide.

2.4 New Estimators

The logic of the two-way fixed effect model does not naturally extend to differential timings (Goodman-Bacon, 2018; Imai and Kim, 2020; Chaisemartin and D'haultfoeuille, 2021). From the derivation of the linear regression, it provides us a variance weighted approximation. However, in the panel setting with staggered timing, these weights are not always appropriate. The model can put heavier weights on inappropriate comparison groups, which calls into question the interpretability of the ATE.

Recent work in econometric theory has proposed new estimation techniques for differential timing difference-in-differences, as opposed to TWFE. Callaway and C Sant (2020) propose an inverse probability weighting estimator which doesn't include post-treatment controls for treated units and only compares treated and either untreated or not-yet treated units for estimated ATTs. Sant'Anna and Zhao (2018) proposed a doubly robust estimator that combines propensity scores and linear regression providing two opportunities to correctly specify your estimation. We utilize the *did* package in R to estimate the effects of unilateral divorce on suicide and intimate homicide rates using the IPW and doubly robust estimators (Callaway and Sant'Anna, 2022).

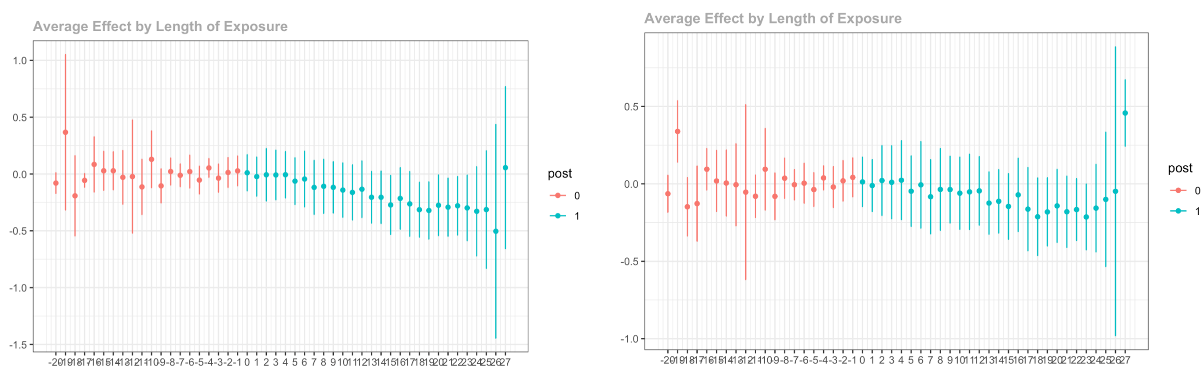
2.4.1 Suicide

Table 5 presents our estimation of unilateral divorce on female suicide rates with the Inverse Probability Weighting Estimator. This estimation doesn't include any co-variates. From this table we can discern that the unilateral divorce law changes had significant impacts on female suicide rates with a significant ATT of -15.23 percent, however this is without controls. However, when including controls, the ATT is insignificant.

Table 5: Group ATT of Effect of Unilateral Divorce on Female Suicide Rates

	(1)	(2)	(3)	(4)
Group	ATT	ATT	ATT	ATT
1969	-0.0984	0.0944	-0.0963*	-0.0475
1970	-0.5623	-0.3203	-0.1607	-0.0081
1971	-0.1896	-0.1531	-0.0269	-0.0182
1972	-0.1452	-0.1003	-0.0450	-0.0151
1973	-0.0834	-0.0577	-0.0211	0.0012
1974	-0.1358	-0.0018	-0.0360	0.0178
1975	-0.0798	-0.0029	-0.0398	-0.0098
1976	-0.2483*	0.0005	-0.0605	-0.0152
1977	-0.2696	-0.0994	-0.1532	-0.1038
1980	-0.1347	-0.1502	0.0229	0.0913
1984	-0.1510*	-0.1307	-0.0224	0.0109
1985	0.2763*	0.3690*	-0.0636*	-0.0576
Aggregate ATT	-0.1523*	-0.0724	-0.0497	-0.014
Gender	Female	Female	Male	Male
co-variates	No	Yes	No	Yes

Figure 4 we examine the effect of unilateral divorce on female suicide by length of exposure. Each point represents an estimate of a year either before or after treatment. Suicide rates are significantly impacted by treatment 15 years after being treated. This is a larger window that initially estimated by Stevenson and Wolfers (2006). Their initial findings and our replication find that suicide rates only become statistically significantly different from zero eight years after the law change. Therefore, other unknown factors outside the scope of Coasian argument might be in play here threatening the validity of the estimators.

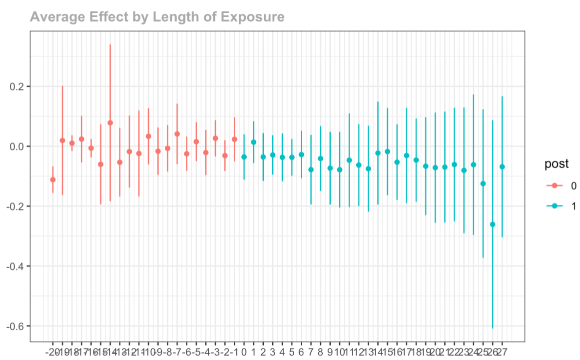


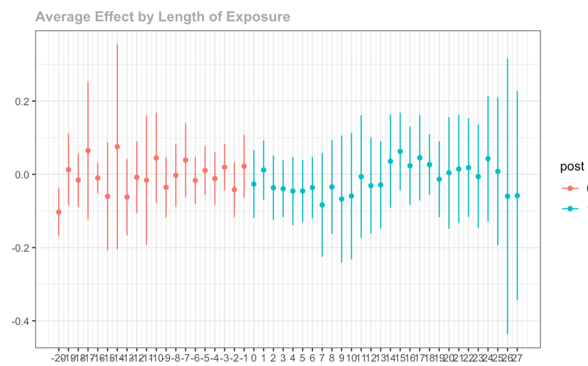
Without co-variates With co-variates

Figure 4: Effect of Unilateral Divorce on Female Suicide by Year

Table 6 presents an estimation using a regression method with our own manual estimations of propensity scores on treatment. The *did* package in R only allows for a small number of co-variates with the inverse probability weighting and doubly robust methods, therefore we use a regression method to check that null results aren't driven by differences in co-variates. This method mimics a doubly robust estimation because we specify a propensity score method and then a linear regression method but are not constrained. From this table we still discern that the unilateral divorce law changes had no significant impact on female suicide rates when controlling for co-variates. The estimated ATT of unilateral divorce laws on female suicide rate is -4.97 percent and is insignificant.

Figure 5 presents the effect of unilateral divorce on male suicide by length of exposure while controlling for co-variates. We don't find effects for male suicide rates. This could be due to the fact that male suicide rates are already high comparatively and no-fault divorce wouldn't be strong enough to impact it.





Without co-variates With co-variates

Figure 5: Effect of Unilateral Divorce on Male Suicide by Year

We decompose the effect of divorce laws on female suicide rates by age group using CDC Wonder data. Our new estimators do not reveal any significant effects of divorce laws on female suicide rates when we include controls. But we do find statistically significant effect when the controls were not used. Table 6 provides evidence that the estimated significant effects are positive and driven mostly by young women (under age of 20), along with some cohorts of 20-54 year old women. This raises suspicion on the estimators as significant effect of divorce laws on younger women that are less likely to be married does not support Coasian argument. It is highly likely that these young women are experiencing positive externalities of culture change leading lower suicide rate among them rather than the input of no-fault divorce.

Table 6: Effect Of Unilateral Divorce Laws On Female Suicide Rates by Age Group

VARIABLES	(1) Less than 20	(2) 20 - 34	(3) 35 - 44	(4) 45 - 54	(5) 55 - 64	(6) 65 - 74	(7) 75 - 84	(8) Greater than 84
>=1 year later	-2.10e-05	0.000128	-0.000222	0.000243**	0.000200	-6.35e-05	-6.05e-05	3.99e-05
	(7.09e-05)	(0.000205)	(0.000146)	(0.000110)	(0.000204)	(0.000133)	(7.99e-05)	(2.91e-05)
1-2 years later	0.000249***	0.000244	-9.97e-05	0.000262*	3.62e-05	-8.47e-05	4.24e-05	3.42e-05
	(8.61e-05)	(0.000155)	(0.000122)	(0.000144)	(0.000158)	(9.60e-05)	(7.95e-05)	(4.22e-05)
3-4 years later	4.91e-05	0.000175	-0.000142	0.000150	6.64e-05	9.43e-05	4.38e-05	1.91e-06
	(7.27e-05)	(0.000158)	(0.000153)	(0.000121)	(0.000116)	(8.71e-05)	(7.65e-05)	(2.76e-05)
5-6 years later	0.000110	0.000332**	-7.06e-05	0.000281**	6.86e-05	7.67e-05	8.89e-05	3.74e-05
	(6.84e-05)	(0.000165)	(0.000124)	(0.000117)	(0.000103)	(8.30e-05)	(6.79e-05)	(2.92e-05)
7-8 years later	0.000190**	0.000361**	-0.000201	0.000170	5.59e-05	0.000132	6.41e-05	2.05e-05
	(7.42e-05)	(0.000149)	(0.000123)	(0.000118)	(0.000106)	(8.52e-05)	(6.83e-05)	(2.97e-05)
9-10 years later	0.000254***	0.000338**	-0.000189	0.000125	6.35e-05	0.000123	4.80e-05	5.17e-06
	(7.78e-05)	(0.000149)	(0.000129)	(0.000126)	(0.000110)	(8.69e-05)	(6.92e-05)	(2.92e-05)
11-12 years later	0.000254***	0.000271*	-0.000182	0.000227*	-8.70e-06	0.000160*	4.07e-05	3.06e-05
	(7.66e-05)	(0.000157)	(0.000140)	(0.000132)	(0.000111)	(9.11e-05)	(7.15e-05)	(3.08e-05)
13-14 years later	0.000292***	0.000249	-0.000180	0.000190	3.08e-06	0.000152	8.03e-05	3.33e-05
	(8.47e-05)	(0.000166)	(0.000149)	(0.000146)	(0.000117)	(9.56e-05)	(7.53e-05)	(3.17e-05)
15-16 years later	0.000285***	0.000353**	-0.000167	0.000173	-6.81e-06	0.000164	6.23e-05	3.81e-06
	(8.77e-05)	(0.000171)	(0.000158)	(0.000153)	(0.000121)	(9.98e-05)	(7.87e-05)	(3.17e-05)
17-18 years later	0.000299***	0.000297	-0.000250	0.000175	-0.000103	0.000190*	4.09e-05	6.34e-06
	(8.90e-05)	(0.000184)	(0.000180)	(0.000164)	(0.000128)	(0.000106)	(8.01e-05)	(3.25e-05)
>=19 years later	0.000285***	0.000254	-0.000246	0.000221	-8.55e-05	0.000198*	1.64e-05	2.41e-06
	(9.12e-05)	(0.000195)	(0.000175)	(0.000166)	(0.000130)	(0.000107)	(8.01e-05)	(3.26e-05)
Constant	-0.00101	0.000468	0.0190*	-0.00864	-0.000776	-4.06e-05	0.000886	-0.00174
	(0.00660)	(0.0143)	(0.0112)	(0.00962)	(0.00849)	(0.00702)	(0.00471)	(0.00166)
F-Test	0.0035	0.3033	0.8114	0.1500	0.3298	0.2814	0.3273	0.0575
Observations	785	785	785	785	785	785	785	785
R-squared	0.508	0.608	0.466	0.420	0.521	0.433	0.487	0.475

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

2.4.2 Intimate Homicide

Table 7 presents an estimation using the IPW method. This estimation does not include any controls. From Table 7 we discern that the unilateral divorce law changes had no significant impacts on the variables of interest, intimate homicide on women by spouses, family, or known assailants, placebos, or the triple difference dependent variable across men or women.

Table 7: Effect of Unilateral Divorce on Intimate Homicide - IPW Estimator

	Intimate homicide (1)	Placebo nonintimate homicide (2)	Diffs-in-Diffs-in-Diffs (intimate less nonintimate) (3)
Women Murdered by Intimates			
By Spouse	-4.8	-12.1	7.2
	(16.1)	(10.5)	(15.8)
By Family	-19.2	-2.8	-16.4
	(16.5)	(9.5)	(17.5)
By Known	-6.3	-18.8	12.5
	(14.1)	(10.2)	(16.9)
Men Murdered by Intimates			
By Spouse	15.3	-9.0	24.3*
	(13.8)	(10.9)	(13.4)
By Family	-18.0	-5.2	-12.8
	(12.7)	(14.0)	(19.2)
By Known	2.1	-21.5	23.7
	(16.3)	(29.4)	(43.0)

Bootstrapped standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8 presents estimations of the Sant'anna-Zhao doubly robust method without controls. The estimates of this method don't change from the inverse probability weighting method. From this table we discern that the unilateral divorce law changes had no significant impacts on the variables of interest, intimate partner homicide on women by spouses, family, or known assailants, placebos, or the triple difference dependent variable across men or women. Estimates from the linear regression method are not included but also find insignificant effects across all specifications.

Table 8: Effect of Unilateral Divorce on Intimate Homicide - Doubly Robust Estimator

	Intimate homicide (1)	Placebo nonintimate homicide (2)	Diffs-in-Diffs-in-Diffs (intimate less nonintimate) (3)
Women Murdered by Intimates			
By Spouse	-4.8	-12.1	7.2
	(15.7)	(10.3)	(15.3)
By Family	-19.2	-2.8	-16.9
	(15.7)	(9.7)	(16.7)
By Known	-6.3	-18.8*	-16.4
	(14.0)	(9.5)	(15.9)
Men Murdered by Intimates			
By Spouse	15.3	-9.0	24.3*
	(14.0)	(10.1)	(12.8)
By Family	-18.0	-5.2	-12.8
	(11.9)	(13.2)	(21.6)
By Known	2.1	-21.5	23.7
	(14.5)	(30.3)	(40.7)

Bootstrapped standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

2.5 Conclusion

Stevenson and Wolfers (2006) published a paper that theorized that unilateral divorce laws shifted bargaining rights to the middle for couples. They used TWFE and found that unilateral divorce laws decreased female suicide rates 8 years after law adoption, decreased domestic violence, and decreased the murder of women by intimate contacts. This paper has been cited and used in support of a multitude of research, which indicates that it's an important piece of research. We have revisited their data and research question with more modern econometric techniques to study whether or not these effects are still precisely measured today. We find that although we replicated their results well using TWFE, we did not find any effects of unilateral divorce on suicide rates or intimate homicide with more appropriate estimators. This indicates that the original results were contaminated by TWFE estimations and should not be relied on for understanding the true effects of unilateral divorce on health and crime.

Within our paper, we provide a decomposition of TWFE model using the Goodman-Bacon's decomposition method and find that there are problematic weights associated with control and treatment comparison groups in the initial analysis. These inappropriately measured control and treatment groups biased the initial results and pulled their estimates downward indicating strong effects of unilateral divorce on outcomes. Our results do not necessarily prove that unilateral divorce laws and ineffective in preventing violence for *all* women, but rather that *on net* these laws are not necessarily effective. This indicates that there may be some women that were positively affected by these laws, and some women that were negatively affected. For example, many states have "cooling-off" periods, wherein an upset spouse can still keep the divorcing party beholden trapped in a marriage past their filing date.

Using more Callaway and Sant'anna (2020) methods nullifies the effects presented in the original paper. Similarly, Stevenson and Wolfers (2007), expressed how culture was changing at the same time as these divorce laws were being adopted. This indicates that the initial estimates, although biased, could have captured a change in culture rather than adoption of unilateral divorce laws.

3 Chapter 3: All is Fair in Love and War: Divorce and Crime in Cook County, Illinois

3.1 Introduction

In the United States, 35 percent of individuals have been divorced at least once (Mayol-García et al., 2021). For the individual, this will cost an average of $16,000, assuming an average hourly lawyer fee of $250 per hour Stebbins (2018). These estimates do not include the time and financial costs of cases continuing to determine child support and parental agreements. Americans over the age of 50 who experience divorce lose up to 50 percent of their wealth and never recover financially (Lin et al., 2019). There are also emotional costs of divorce: divorced individuals are less satisfied with their life, families, income, housework, and living standard compared to married individuals. Divorced individuals have lower mental health scores than married individuals and they smoke more often (Leopold, 2018). Divorced adults also experience increases in drinking and drug use (Collins et al., 2007; Power et al., 1999). Consequentially, These are determinants of crime.

The cost of divorce for the individual also transfers to society. Judges in the US cost upwards of $200,000 in salaries every year (Courts, 2022). The demand for divorce judges costs taxpayers millions of dollars every year driven by of the high expense of judges. In addition, researchers at Georgia State estimated that divorce and single-parenthood cost the US 115 billion dollars every year due to increases in welfare, education, criminal justice, and health care payments (Scafidi and Investigator, 2008).

Previous studies have looked at the determinants of crime, but research that has examined divorce as a determinant of crime are few and focus at the state level (Buonanno and Bicocca, 2021; Cáceres-Delpiano and Giolito, 2012; Stevenson and Wolfers, 2006). This research is the first to leverage individual-level data to explore divorce as a determinant of crime. I present suggestive causal evidence that a longer divorce case leads to an increase of an individual's propensity to commit crime between 6 and 16 % above the mean when a case length is increased by 100 days. Suggestive evidence of the mechanism of the impact of social exclusion through divorce on individuals' propensity to commit crime is presented in the heterogeneity of effects. The only type of crime that is statistically significantly affected by divorce is drug crimes. This suggests that longer divorces leads to greater loses in income and social exclusion which then increases their propensity to commit drug crimes. This is consistent with psychology literature which connects drug usage, income, and social exclusion (Cacioppo et al., 2015; DeWall and Pond Jr, 2011; Dyal and Valente, 2015; Baptiste-Roberts and Hossain, 2018).

The remainder of this paper is structured as follows. Section two discusses the current literature on divorce, crime, and the connections. Section three discusses the data and section four discusses the methods of estimation. Section five presents the findings and section six presents tests for robustness. Section seven concludes.

3.2 Literature Review

There exists a body of literature that explores the determinants of crime (Buonanno and Bicocca, 2021). There are two main determinants of crime that are impacted by divorce: income and social exclusion. Lower income and higher poverty increase the payoff of criminal activity, because the opportunity cost of committing crime is lower. Similarly, low income individuals face psychological strain of their income level and income inequality, which leads to criminal activity. This psychological effect of lower income is also seen in the social exclusion that individuals can face (Buonanno and Bicocca, 2021).

An individual's community can also impact their propensity to commit crime. In neighborhoods with high criminal activity, the social cost of committing crime is low, leading to a higher chance of criminal behavior. This is also exacerbated by the fact that poverty may affect perceived returns of crime (Buonanno and Bicocca, 2021). Alternatively, individuals who experience social exclusion are more likely to commit crime. Individuals who experience social exclusion face lower costs of crime because they have less social support (Buonanno and Bicocca, 2021). Socially excluded individuals are more likely to use drugs, which could increase likelihood of comitting drug crimes (Foster, 2000; Dyal and Valente, 2015; DeWall and Pond Jr, 2011; Cacioppo et al., 2015).

These two main determinants are impacted by divorce, which in turn, makes divorce a determinant of crime. The way that divorce impacts both of these determinants is that divorce decreases income and separates individuals from their community. Immediately, individuals face an average cost of $16,000 for a divorce with high lawyer fees (Stebbins, 2018). Throughout the divorce, individuals split their assets and wealth and often never recover financially from this (Lin et al., 2019).

Indirectly, there is also an opportunity cost of income. When individuals must be in court, they are unable to use that time to produce income. Divorce may come at inopportune times and research has shown decreases in divorce during recessions (Cohen, 2014). Couples that are divorcing during a recession may be under greater economic strain because of unemployment or lowered pay, or through market inequalities. A couple selling their house during a divorce will lose on potential income gained from the sale because of the recession. This indicates a process in which divorce will decrease income and affect their probability of committing crime.

An individual is not only impacted by their income in a divorce, but also how divorce shifts their social networks. Their spouse, who has been a member of their social network, is now partially or completely lost. Their family and friends are also lost from their direct and indirect community and this is isolating to many (Gerstel, 1987). This social exclusion can impact the cost of crime in two main ways: opportunity cost and substance abuse. Divorced individuals' networks shift which decreases their social support. This creates a lower cost threshold for the individual to commit crime (Buonanno and Bicocca, 2021). Divorced individuals are, on average, more likely to drink and use illegal drugs (Collins et al., 2007; Power et al., 1999). Further evidence of this change in social standing is seen in the decrease of mental health and psychological health of divorced individuals (Bruce, 1998; Lin et al., 2019; Tosi and van den Broek, 2020). This can increase the likelihood of crime through the increased cost of goods to an individual's consumption function, decreased cost of crime because of a shifting social network, and increased likelihood to have drugs in one's possession and be arrested for it.

This paper measures the effect randomly longer divorce cases, as opposed to divorce, on crime. It is challenging to estimate divorce on crime because divorce is a non-random event. Longer divorce cases are a good proxy for the financial and social costs of a divorce, and therefore impact the determinants of crime in a strong way. For example, longer divorce cases monotonically increase the economic cost of a case, as an individual will have to miss work and pay their lawyers more money. Longer divorces will also continue to isolate individuals and increase their likelihood to use substances because they will possibly need to wait longer to continue to heal (Leopold, 2018).

In sum, longer divorce cases isolate individuals from their social circles, increase their likelihood of substance abuse, and decrease their income, which are determinants of crime. However, to look at these impacts on society we need to consider data that is at the individual level and takes into account the random assignment to a judge, the length of a case, and case co-variates, among many variables. Suggestive evidence of this mechanism is explored through heterogeneous treatment of longer divorces on types of crimes, including drug crimes. Section three explains the way in which the final panel data were constructed to empirically test how divorce is a determinant of crime.

3.3 Data

To examine the question of how the length of divorce impacts crime, I use divorce court data from Cook County, Illinois at the individual level. This geography includes Chicago and surrounding suburban district. This data includes the population of divorce cases and Cook County prison and parole data from 2010-2020. The crime data includes felonies of classes 4 to 1 and M in Illinois. The final panel data is constructed by taking into account five processes: name matching, race and gender prediction, sub-setting, random assignment, and dependent variable creation.

3.3.1 Matching

Divorce court data is matched with prison and parole data from Cook County using record linkage. This matching is determined by Levenshtein distances. These matches are all distanced with a value of 1 meaning the matches are exact matches or have a difference of 1 letter. [1] This indicates provides a degree of freedom for possible misspellings, but names are within 1 letter of each other. The only information provided by divorce courts is the name of individuals, therefore I matched only on the names of individuals. The inclusion of race and gender, which are sampled through probabilities, would introduce bias to matching through the commonness of names. Within the pooled sample 30,762 criminals are matched with divorcing 55,926 individuals. The main results use this matching technique, but results robust to estimating effects with matches that are 1 to 1, include gender, race, and controls for name complexity.

[1] A match with a distance of 1 means the names are exactly the same or have up to 1 insertion, deletion, or transmutation of the name. For example, there is one level distance between "Smith" and "Smithe", "Smish", and "Smit".

3.3.2 Prediction

The original divorce court data does not provide information on individual demographics, so the *predictrace* package in R is used to predict race and gender (Kaplan, 2020). This package assigns probabilities of gender based on first name and race based on first and last names with data trained on census data from Cook County, Illinois. I sample gender and race characteristics for the defendant, plaintiff, and judge based off these probabilities to estimate the impact that race and gender has on case and crime outcomes.

3.3.3 Summary Statistics

Table 2 provides information on the averages of the panel. 63 percent of individuals are white, 9 percent are Black, 5 percent are Asian, and 23 percent are Hispanic, which is consistent with American Community Survey Data of the population of Cook County (Bureau, 2020). There is an average of 502 days in an entire case, which includes 168 days until a divorce is finalized and 334 days of other activities associated with a case [2]. 4.5 percent of individuals are matched with a crime within two years after their divorce case is filed. 46 percent of cases do not include children, 22 percent include a military spouse, and 66 percent of cases are with couples whose predicted and sampled races are the same.

[2] These activities include textual information associated with a case. For example, these activities capture paperwork filings, whether a case includes children, and updates associated with a case. These activities were transformed into a matrix of binaries. For example, binaries exist for if a case has activities associated with children, if the divorce is finalized, and if there is a parenting agreement

Table 1: Summary Statistics

Crime	4.572
	(0.039)
Number of Divorce Activities	16.235
	(0.096)
Length of Case (Days)	377.619
	(1.030)
Number of Total Activities	28.177
	(0.096)
Length of Divorce (Days)	149.884
	(0.622)
Divorce Finalized	43.211
	(0.093)
No Children Involved	43.387
	(0.093)
Contested Divorce Case	17.585
	(0.071)
Not Previously Filed (Mentioned)	70.422
	(0.086)
Military Spouse	17.769
	(0.072)
Same Last Name	51.175
	(0.094)
Self-Representation	30.357
	(0.086)
White	41.802
	(0.092)
Black	5.887
	(0.044)
Asian	3.232
	(0.033)
Hispanic	14.552
	(0.066)
Female	44.458
	(0.093)
N	284896

Standard errors in parentheses.

3.3.4 Subsetting

Each estimation of the instrumental variable method will be on five sub-samples of the initial panel. The first is the pooled sample of individuals. The second is the sample of team calendars, the third is the team calendars conditional on a case reaching divorce completion and given adequate time to finish, and the final two are individual calendars and individual calendars with some conditionality.

Cook County randomly assigns individuals to calendars instead of just to individual judges. There are two types of calendars: Individual and Team. In an individual calendar, the individual is assigned to one judge, like a traditional divorce court. In the team calendar, an individual is assigned to a team of three judges. Each judges presides over a different part of the case: preliminary, trial (if necessary), and post-decree. This process in no ways hinders the estimation of a causal model but may present differences in the case length. Individual and team calendars have statistically different case lengths regardless of case co-variates so it is necessary to complete the analysis in order to determine that the effects are not driven by the type of judges a case is assigned to. The co-variates across these two different are not statistically different, but team calendar cases are on average 70 days faster. There are multiple different districts within Cook County for individuals to file and are separated between suburban and the downtown district. Individual and team calendar sub-sampling is only provided for the downtown Chicago district where there is documentation on these calendar types. This district is the largest of the districts and is where most couples file for divorce. These cases are more representative of cases seen in other large cities and provide an opportunity to externally validate findings as well as separate estimations among calendar types. Table 2 presents summary statistics across the two types of calendars. These summary statistics indicate that there isn't much variation across calendar type except for average case length.

I also include conditions for two of the sub-samples because I also want to compare cases that have had adequate time for the case to be finished. Therefore, I also provide estimates in the sub-sample of cases who file at least in 2018 and have finished the initial divorce proceedings. This allows ample time for the case to be filed and a prove-up, which finalizes the initial divorce, to be filed.

Table 2: Summary Statistics by Calendar Type

	(1) Individual	(2) Team
Crime	4.622	4.796
	(0.062)	(0.071)
Number of Divorce Activities	24.475	11.474
	(0.257)	(0.106)
Length of Case (Days)	407.920	324.328
	(1.781)	(1.474)
Number of Total Activities	32.558	27.234
	(0.187)	(0.148)
Length of Divorce (Days)	204.577	104.109
	(1.450)	(0.707)
Divorce Finalized	28.823	71.911
	(0.133)	(0.149)
No Children Involved	43.524	45.324
	(0.146)	(0.165)
Contested Divorce Case	19.899	16.905
	(0.118)	(0.124)
Not Previously Filed (Mentioned)	70.727	70.160
	(0.134)	(0.151)
Military Spouse	20.870	23.227
	(0.120)	(0.140)
Same Last Name	48.514	48.234
	(0.147)	(0.165)
Self-Representation	28.934	28.786
	(0.133)	(0.150)
White	40.342	40.347
	(0.144)	(0.162)
Black	5.664	5.875
	(0.068)	(0.078)
Asian	3.449	3.358
	(0.054)	(0.060)
Hispanic	15.928	15.779
	(0.108)	(0.121)
Female	44.581	44.539
	(0.146)	(0.164)
N	115422	91488

Standard errors in parentheses.

3.3.5 Random Assignment

Cook County, Illinois electronically randomly assigns divorce cases to calendars (cir, 1980). This assignment is empirically random while taking into account the district in which a couple files, and the year and month of filing, which motivates my use of this level of fixed effects in the main results. Table 3 verifies that assignment of cases to divorce judge calendars is random after conditioning for district-by-time fixed effects. F-statistics of each of these co-variates, except for 3, are close to zero. *Counsel Listed*, *Attorney*, and *Military* have F-statistics greater than 3 indicating that judge assignment has some predictive power in these co-variates. Judges may affect whether or not counsel is listed in the case, if the case goes to trial and requires some representation, and whether or not military individuals include that information in their filing. I'm unable to parse when some of this information is filed, and if it's after judge assignment. Therefore, any failings of these tests may be due to judge interference in activity filings.

Table 3: Test of Randomization

Variable	F-Statistics
Crime	1.70
Children	1.24
Not Previously Filed	1.49
Military	3.66
Attorney	3.42
Counsel Listed	4.26
Female	0.94
Male	0.35
White	1.62
Black	1.48
Hispanic	1.35
Asian	1.65

This table provides estimates of the first stage F-statistic of the instrument, judge calendar assignment, on co-variates that *should not* be effected. These estimations include district X year X month fixed effects as well as two-way clustering by case id and judge calendar. This estimation is presented with the pooled sample of all data points.

3.3.6 Outcome

The dependent variables, *Crime*, is a strictly coded variable that takes into account several factors. The first is the fact that an individual's name is matched to prison and parole data in Cook County. Second, that the salient crimes should only be impacted by the case length if they occur after the filing date and within two years of filing. This variable is a binary variable that is coded as a value of 1 if the individual is matched with a criminal in Cook County, Illinois who has who has committed any type of felony within the two years after their filing date. It is coded as a 0 otherwise.

3.4 Methods

I aim to examine the effect of divorce case length on crime. For individual i and case c, consider the fixed-effects model in Equation 4 that naïvely relates outcomes to the length of an individual's divorce case:

$$\mathrm{Y}_{ict} = \gamma_0 + \gamma_1 \text{Case Length}_i + \mathbf{\Gamma}\text{Calendar}_i + +\gamma_3 \mathrm{X}_{ict} + \gamma_4 \mathrm{D}_{ict} + \psi_i, \tag{1}$$

where Y_{ict} is the outcome of interest for individual i in case c in year t, $Calendar_i$ is a matrix of judge calendar fixed effects, X_{ict} is a matrix of district-by-time fixed effects, D_{ict} is a vector of divorce case- and individual-level control variables, and ψ_{ict} is the error term. In this estimation, γ_1 should be positive and significant, wherein a longer divorce case should increase an individual's propensity to commit crime. It follows that $\mathbf{\Gamma}$ would include a vector of effects based on the calendar assignment. Table 4 presents naïve OLS results for the impact of case length on crime while including judge calendar fixed effects. These estimates indicate that naïve OLS will underestimate the impact of case length on crime, and therefore is an inappropriate estimation strategy.

Table 4: Naïve OLS

	Dependent variable:				
	Crime				
	Pooled Sample	Team Calendar		Individual Calendar	
	(1)	(2)	(3)	(4)	(5)
Case Length - 100s of Days	0.0001**	0.001*	0.001**	0.00000	−0.00002
	(0.0001)	(0.0003)	(0.0003)	(0.0001)	(0.0001)
No Children	0.0003	−0.001	−0.002	−0.002**	−0.001
	(0.0004)	(0.001)	(0.002)	(0.001)	(0.002)
Contested Divorce	−0.002**	−0.005**	−0.005**	−0.003	−0.004*
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Not Previously Filed	0.0003	−0.001	−0.002	0.003**	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Military Spouse	0.004***	0.007***	0.009***	0.008***	0.005***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)
Same Last Name	−0.001	−0.001	−0.0004	−0.001	−0.001
	(0.0004)	(0.001)	(0.001)	(0.001)	(0.001)
Pro Se	0.001*	0.003**	0.002**	0.003***	0.004**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Asian	0.006***	0.019***	0.022***	0.013***	0.016***
	(0.001)	(0.004)	(0.005)	(0.001)	(0.002)
Black	0.025***	0.050***	0.060***	0.050***	0.054***
	(0.002)	(0.006)	(0.004)	(0.005)	(0.011)
Hispanic	0.013***	0.039***	0.043***	0.037***	0.050***
	(0.001)	(0.004)	(0.002)	(0.003)	(0.004)
White	0.016***	0.033***	0.038***	0.031***	0.035***
	(0.001)	(0.003)	(0.002)	(0.002)	(0.003)
Female	−0.015***	−0.031***	−0.034***	−0.029***	−0.033***
	(0.001)	(0.003)	(0.002)	(0.002)	(0.003)
Plaintiff	−0.003***	−0.005**	−0.005**	−0.005***	−0.006***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
Crime Mean	.027	.028	.027	.032	.032
Case Length Mean	377.62	324.33	407.92	557.96	305.64
District by Year by Month FE	X	X	X	X	
Judge Calendar FE	X	X	X	X	
Observations	284,896	91,488	60,852	115,422	31,242
Adjusted R^2	0.014	0.029	0.027	0.027	0.026
Note:	*p<0.1; **p<0.05; ***p<0.01				
	s.e. are two-way clustered at the judge assignment and case level				

This table provides estimates of the naïve OLS estimations of the dependent variable of interest, *crime* on an individual's case length. These estimations include district X year X month and judge calendar fixed effects. Estimates are provided without bracketing and standard errors are provided in parentheses.

The issue with inference is that the estimates of this model will be biased by the reverse causality of case length and crime, even when including judge calendar fixed effects. For example, an individual who commits crime may have a shorter case length because they hold less bargaining power in court or that their crime makes a couple more likely to divorce. Therefore, any estimates will be biased finding that case length is attenuated downwards where γ_1 would be close to zero or negative, as is presented in table 4. To address this issue, I estimate the causal impact of case length on crime by measuring the tendency for a randomly assigned divorce judge or group of judges to make a case longer. In this model, I interpret the outcomes for individuals assigned to judges with longer case preferences as the causal effect of the change in the case length associated with judge assignment. This empirical design estimates the local average treatment effect (LATE) of the causal effect of case length on an individual's propensity to commit crime.

This identification strategy follows previous research that exploits the random assignment of judges as an instrument for court decisions (Stevenson, 2018; Kling, 2006; Dobbie et al., 2018; Angrist et al., 1999). For example, Stevenson (2018) used magistrate random assignment to measure whether pre-trial detention impacted outcomes for individuals that had been arrested. This paper also follows in the work of Stevenson (2018) and Angrist et al. (1999) by utilizing a jackknife instrumental variable approach wherein the effect of calendar assignment on individual i is not included in the estimation of the effect of calendar assignment on that individual. This identifies the causal effect of calendar assignment without biasing the instrument with the effect of the individual's case.

The first stage is estimated as:

$$\text{Case Length}_i = \alpha_0 + \alpha_1 \text{Calendar}_i + \alpha_2 X_i + \alpha_3 D_i + \epsilon_i. \tag{2}$$

This estimation includes district-by-year-by-month fixed effects, X_i, case co-variates and demographics, D_i, and a matrix of judge assignment variable, $Calendar_i$. District-by-year-by-month fixed effects were chosen because calendar assignment is conditional on the district a couple files in as well as the year and month of filing. District determines which judge calendars are available, and year and month control for the possibility of a judge retiring or a judge on vacation. For example, I want to include a fixed effect for a district in a given year in a given month when a particular judge may be on vacation.

Table 5: First Stage

	Dependent variable:				
	Case Length - 100s of Days				
	Pooled Sample	Team Calendar		Individual Calendar	
	(1)	(2)	(3)	(4)	(5)
No Children Involved	-2.067***	-1.563***	-1.521***	-2.220***	-2.492***
	(0.0268)	(0.0407)	(0.0463)	(0.0451)	(0.0900)
Contested Divorce Case	6.652***	5.408***	6.092***	7.494***	7.407***
	(0.0456)	(0.0682)	(0.100)	(0.0719)	(0.133)
Not Previously Filed (Mentioned)	-0.257***	-0.227***	-0.234***	-0.328***	-0.477***
	(0.0348)	(0.0507)	(0.0590)	(0.0594)	(0.124)
Military Spouse	0.0215	0.0567	0.0878*	0.0112	-0.126
	(0.0301)	(0.0403)	(0.0472)	(0.0476)	(0.100)
Same Last Name	0.395***	0.357***	0.392***	0.476***	0.569***
	(0.0239)	(0.0355)	(0.0441)	(0.0405)	(0.0909)
Self-Representation	-0.0370**	-0.146***	-0.111***	-0.00464	0.0669
	(0.0147)	(0.0221)	(0.0261)	(0.0250)	(0.0583)
White	0.176***	0.119***	0.120***	0.287***	0.204**
	(0.0227)	(0.0329)	(0.0406)	(0.0383)	(0.0857)
Black	0.234***	0.215***	0.114	0.296***	0.0810
	(0.0393)	(0.0603)	(0.0749)	(0.0666)	(0.144)
Asian	-0.140***	-0.200***	-0.234***	-0.249***	-0.384**
	(0.0492)	(0.0673)	(0.0807)	(0.0800)	(0.180)
Hispanic	0.170***	0.196***	0.209***	0.163***	0.112
	(0.0309)	(0.0446)	(0.0556)	(0.0504)	(0.112)
Female	0.0215**	0.0261*	0.0385**	0.0282*	0.0702**
	(0.00904)	(0.0134)	(0.0163)	(0.0151)	(0.0331)
Plaintiff	0.00847**	0.0272***	0.0146***	0.00102	-0.0199**
	(0.00380)	(0.00477)	(0.00490)	(0.00509)	(0.00957)
Case Length Mean	377.62	324.33	407.92	557.96	305.64
District by Year by Month FE	X	X	X	X	X
Judge Calendar FE	X	X	X	X	X
Observations	284,896	91,488	60,852	115,422	31,242
R^2	0.346	0.311	0.305	0.374	0.387
Note:	*p<0.1; **p<0.05; ***p<0.01 s.e. are two-way clustered at the judge assignment and case level				

This table provides estimates of the first stage estimations of the variable of interest, *Fitted Case Length - 100s of Days* on an vector of controls. These estimations include district X year X month and judge calendar fixed effects. Estimates are provided without bracketing and standard errors are provided in parentheses.

Table 5 includes first stage estimates of the instrumented variables *Fitted Case Length - 100s of Days* on controls with judge calendar fixed-effects. The case controls and demographics included in this estimation are whether or not a case includes children, if individuals have filed before, if they are representing themselves, if one of the spouses is in the military, if they share a last name, if they are female, and their probable race. Each of these co-variates can impact the length of a case and an individual's willingness to commit crime. For example, women are less likely to commit crime (Buonanno and Bicocca, 2021). Individuals with children will be potentially more likely to commit crime because divorce has a larger economic impact. Individuals with the same last name could indicate a higher cost of divorce and longer divorce because they must file to change their name. Race of an individual affects their propensity to commit crime (Buonanno and Bicocca, 2021). Individuals in the military are able to increase the length of their cases through special considerations. The Soldiers and Sailors Civil Relief Act allows divorce court to postpone a divorce proceeding for the entire length of their military service and up to 60 days after they return. Couples that have previously filed might have a shorter case because they have previously been through this process before. Self-representing individuals do not pay the cost of lawyers indicating that they may be lower income, which may increase in individual's likelihood of committing crime. Finally, couples with the same last name make up around two thirds of couples and may be different in terms of commitment to each other, affecting case length (Davies, 2011).

Table 6 presents F-statistics of the significance of calendar assignment on case length. Four of the sub-samples provide evidence of a strong relationship between judge assignment and case length as their respective F-statistics are larger than 10. Individual calendars that are conditional on finishing have an F-statistics of 8, indicating judge assignment is a poor predictor of case length. This identifies that judge assignment is a good instrument for a longer or shorter case length in most settings.

Further evidence of this strong relationship is provided in figure 1, which provides a graphical representation of the first-stage relationship between case length preferences of calendars, indicated by the red line, and the prediction of a longer case length with 95 percent confidence intervals, indicated by the pink lines. The individual rate of case length is monotonic and approximately linearly increasing in the case length preference measure. This indicates that a case randomly assigned to a judge that has a preference for longer cases is associated with longer case processing time.

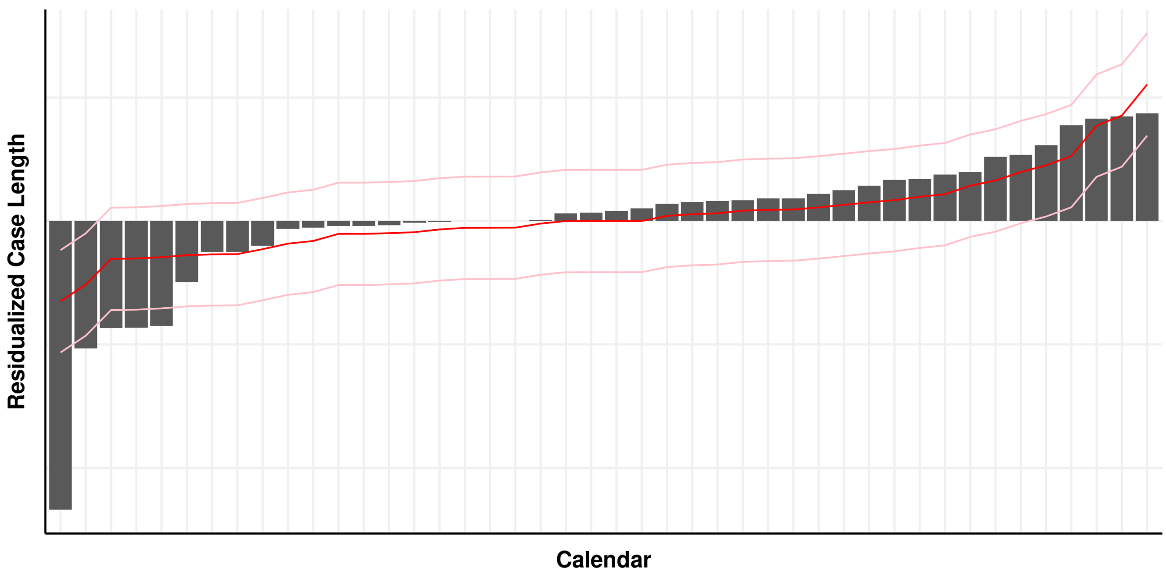


Figure 1: Distribution of Residualized LOO Mean of Case Length

The second stage of the estimation takes on the following form:

$$\text{Crime}_i = \beta_0 + \beta_1 \widehat{CaseLength}_i + \beta_2 X_i + \beta_3 D_i + \upsilon_i. \tag{3}$$

This estimation includes a vector of controls of the case and individual, X_i, and district-by-time fixed effects, D_i. This second stage estimates the causal impact of the fitted case length value, $\widehat{CaseLength}_i$, in 100s of days, on the dependent variable, $Crime_i$.

The final requirement for this instrumental variable estimation to be causal besides is the exclusion restriction. The exclusion restriction requires that there must not be any way for the calendar assignment to affect the dependent variable except through divorce case length. An example of the violation is if calendar assignment impacts future outcomes, such as a divorce judge from a calendar seeing an individual in a criminal case in another court. Although this assumption is untestable, I find no evidence that divorce judges oversee criminal cases in Cook County, Illinois. This is also tested empirically by estimating the impact of the instrument, judge calendar, on crime. Table 3, presents the joint f-statistics of the judges on various co-variates. The F-statistic of judge fixed effects on crime is 1.7 indicating that judge assignment is a poor predictor of committing crime.

The next section presents the results of the JIVE estimation strategy across all 5 sub-samples, as well as heterogeneous treatment effects of judge assignment on crime types.

3.5 Results

3.5.1 Main Results

Table 6 provides the impact of a randomly longer case length on an individual committing crime. Column 1 provides estimates in the pooled sample of all individuals. Columns 2 and 3 present estimates on the sub-sample of team calendars. Columns 4 and 5 are estimates in the sub-sample of individual calendars. Columns 3 and 5 present estimates on cases filed between 2010 and 2018 and are conditional on the divorce portion of a case being finalized.

Estimates of the variable of interest *Fitted Case Length - 100s of Days* are positive and significant across three specifications.[3] This provides evidence that being randomly assigned a longer case calendar in the pooled sample or when an individual is being seen by a team of judges will increase an individual's likelihood to commit crime. The estimates range from 0.3 to .78 percentage points above the mean crime, which ranges from 4.5 to 5.1 percent of individuals. Therefore, longer divorce cases are causally linked to increases in crime and estimates range between a 6 percent and 16 percent increase above the mean within the

[3] This is consistent when including increased restrictions on matching between individuals in the divorce and crime data-sets. As noted earlier, results are still positive and significant when restricting matching to restrict for matches that are 1 to 1 between divorce individuals and criminals, as well as for matches that also match on race, sex, and race and sex.

entire set of cases or individuals are assigned to teams of judges.

All other co-variates included in the estimations are also significant, except for *Not Previously Filed* which is not robustly significant. Children not being involved, military spouses, self-representation, and each race variable are all significant and have a positive impact on an individual's willingness to commit crime. Contested cases, couples with the same last name, females, and plaintiffs are less likely to commit a crime. This is likely because individuals with children have a duty to stay out of prison to provide, self-representation is a proxy for low-income, and couples with a military spouse are likely to have lower career opportunities (Bogen, 2019). Each race variable is significant and positive because the committed race variable is individuals who are not matched to a race. This could indicate that compared to individuals that have common names, which are easier to match to crime data and race data, all other races are more likely to commit crime. Couples that have more assets or have hired attorneys, and are more likely to have a contested care, are higher income, women are less likely to commit crime, and plaintiffs are more likely to be female. In the case of *Same Last Name*, 2/3 of couples choose to have the same last name of each other. This is a traditional marriage decision and may be a proxy for information that is unavailable in the textual data (Davies, 2011). Finally, *1/(Length of Name)* is included to control for the possibility that shorter names are more likely to be matched to other short names when a Levenshtein distance is included. This variable is positive and significant, which indicates that shorter names are more likely to be matched to individuals with shorter names, which could lead to false positives. However, The variable of interest, *Fitted Case Length - 100s of Days*, is still positive and significant indicating that even when controlling for name length, judges are still more likely to increase an individual's propensity to commit crime.

Table 6: Effect of Divorce Judge Calendar Randomization on Crime

	Dependent variable:				
	Crime				
	Pooled Sample	Team Calendar		Individual Calendar	
	(1)	(2)	(3)	(4)	(5)
Fitted Case Length - 100s Days	0.00306***	0.00778***	0.00777***	-0.00157	0.00184
	(0.000931)	(0.00166)	(0.00269)	(0.00243)	(0.00189)
No Children Involved	0.00249	0.00737**	0.00553	-0.00949*	-0.000850
	(0.00216)	(0.00319)	(0.00458)	(0.00562)	(0.00567)
Contested Divorce Case	-0.0238***	-0.0470***	-0.0514***	0.00690	-0.0225
	(0.00627)	(0.00911)	(0.0167)	(0.0182)	(0.0151)
Not Previously Filed (Mentioned)	0.00125	4.11e-05	0.000674	0.000615	-0.000215
	(0.00113)	(0.00208)	(0.00254)	(0.00189)	(0.00351)
Military Spouse	0.0126***	0.0139***	0.0149***	0.0133***	0.0119***
	(0.00113)	(0.00181)	(0.00220)	(0.00165)	(0.00330)
Same Last Name	-0.00303***	-0.00404**	-0.00462**	-0.00191	-0.00520*
	(0.000895)	(0.00160)	(0.00220)	(0.00172)	(0.00297)
Self-Representation	0.00284***	0.00518***	0.00507**	0.00291**	0.000942
	(0.000849)	(0.00159)	(0.00200)	(0.00135)	(0.00295)
White	0.0499***	0.0530***	0.0618***	0.0527***	0.0623***
	(0.000796)	(0.00145)	(0.00192)	(0.00140)	(0.00264)
Black	0.0777***	0.0815***	0.0965***	0.0810***	0.0947***
	(0.00222)	(0.00399)	(0.00533)	(0.00359)	(0.00717)
Asian	0.0301***	0.0329***	0.0383***	0.0276***	0.0437***
	(0.00203)	(0.00363)	(0.00466)	(0.00311)	(0.00711)
Hispanic	0.0697***	0.0720***	0.0821***	0.0723***	0.0927***
	(0.00136)	(0.00236)	(0.00310)	(0.00206)	(0.00430)
Female	-0.0507***	-0.0536***	-0.0596***	-0.0513***	-0.0626***
	(0.000749)	(0.00136)	(0.00178)	(0.00118)	(0.00252)
Plaintiff	-0.00775***	-0.00736***	-0.00885***	-0.00822***	-0.0102***
	(0.000776)	(0.00139)	(0.00180)	(0.00121)	(0.00257)
1/(Length of Name)	0.214***	0.245***	0.293***	0.150***	0.152*
	(0.0248)	(0.0441)	(0.0583)	(0.0394)	(0.0839)
Crime Mean	0.045	0.047	0.051	0.046	0.049
Case Length Mean	377.62	324.33	407.92	557.96	305.64
First Stage F-Statistic	44.97	40.78	27.76	17.07	8.09
District by Year by Month FE	X	X	X	X	X
Observations	284,896	91,488	60,852	115,422	31,242

Note: *p<0.1; **p<0.05; ***p<0.01
robust s.e. reported in parentheses.

This table provides estimates of the instrument, *Fitted Case Length - 100s of Days* on the dependent variable, *Crime*. textitCrime is defined as a binary variable that is coded as a 1 if an individual is matched with a crime that is within the two years after filing for divorce. These estimations include district X year X month fixed effects as well as robust standard errors. Clustering is not available for JIVE estimations. Within this table, columns 3 and 5 are conditional on finalized divorce and within the 2010-2018 period.

3.5.2 Heterogeneous Treatment Effects

Table 7 provides the impact of a randomly longer case length on an individual's willingness to commit crime across 5 different dependent variables. These 5 different dependent variables are classified by crime class which includes crime classes 4, 3, 2, 1, and M. Crime class 4 is the weakest, while 1 is the strongest, and M stands for murder. Row 1 provides estimates in the pooled sample of all individuals. Rows 2 and 3 present estimates on the sub-sample of team calendars. Rows 4 and 5 are estimates in the sub-sample of individual calendars. Rows 3 and 5 present estimates on cases filed between 2010 and 2018 and are conditional on the divorce portion of a case being finalized. These estimations include the same controls as the main results. Estimates are provided without bracketing, standard errors are provided in parentheses, and dependent variable means are provided in square brackets.

Estimates of the variable of interest *Fitted Case Length - 100s of Days* are positive and significant across four of the crime specifications and four of the sub-samples. This provides causal evidence that being randomly assigned a longer case calendar will increase an individual's likelihood to commit crime across 3 different crime levels, but not murder or crime class 2. The estimates range in .19 to .34 percentage points above the mean crime which ranges from .04 to 1.9 percentage points. This is evidence that longer divorce cases are more impactful on lower level crimes, rather than high level crimes.

Table 7: Effect of Divorce Judge Calendar Randomization on Crime by Crime Class

	Class 4	Class 3	Class 2	Class 1	Class Murder
Pooled Sample	0.00119**	0.000932**	0.000581	0.000370	-4.49e-05
	(0.000563)	(0.000379)	(0.000509)	(0.000374)	(4.20e-05)
	[0.0163]	[0.0169]	[0.0192]	[0.0165]	[0.0201]
Team Calendar	0.00235**	0.00120*	0.00127	0.00156**	-2.00e-05
	(0.00101)	(0.000668)	(0.000874)	(0.000675)	(7.10e-05)
	[0.0069]	[0.0069]	[0.0077]	[0.0070]	[0.0075]
Team and Conditional	0.00347**	0.000763	0.000858	0.00166	-4.28e-05
	(0.00166)	(0.00109)	(0.00142)	(0.00106)	(4.03e-05)
	[0.0123]	[0.0127]	[0.0148]	[0.0125]	[0.0152]
Individual Calendar	-0.000912	0.00243**	-0.000958	-0.00135	1.67e-05
	(0.00151)	(0.000978)	(0.00130)	(0.000974)	(8.20e-05)
	[0.0068]	[0.0073]	[0.0085]	[0.0069]	[0.0098]
Individual and Conditional	0.00130	0.00103	-0.000556	0.000574	0
	(0.00117)	(0.000718)	(0.00103)	(0.000824)	(0)
	[0.0001]	[0.0001]	[0.0001]	[0.0001]	[0.000]
First Stage F-Statistic	44.97	40.78	27.76	17.07	8.09
Controls	X	X	X	X	X
District by Year by Month FE	X	X	X	X	X
Observations	284,896	91,488	60,852	115,422	31,242
Note:	*p<0.1; **p<0.05; ***p<0.01 robust s.e. reported in parentheses.				

This table provides estimates of the instrument, *Fitted Case Length - 100s of Days* on five separate crime variables that are separated by crime class. Each dependent variable is defined as a binary variable that is coded as a 1 if an individual is matched with a crime that is of the class specified within the two years after filing for divorce. These estimations include district X year X month fixed effects as well as robust standard errors. These estimations also include the vector of controls included in the main results. Within this table, columns 3 and 5 are conditional on finalized divorce and within the 2010-2018 period. Estimates are provided without bracketing, standard errors are provided in parentheses, and dependent variable means are provided in square brackets.

Table 8 provides the impact of a randomly longer case length on an individual's willingness to commit crime across 12 different crime types. Each crime case included textual data on the type of crime committed. From this text, I parsed the types of crimes. These crimes include sex crime, DUI, domestic, drug, traffic, firearm, larceny, property, assault, robbery, burglary, and aggravated crimes. Each estimation is the effect of the instrument on the specified crime type. These estimations are performed on the pooled data because the main results find evidence of an effect regardless of sub-sample. These estimations include the same controls as the main results. Estimates are provided without bracketing, standard errors are provided in parentheses, and dependent variable means are provided in square brackets.

Estimates of the variable of interest *Fitted Case Length - 100s of Days* are positive and significant within salient crimes. This provides evidence that individuals assigned to a judge or group of judges with longer case preferences are more likely to commit drug crimes. This is suggestive that the strongest connection between case length and crime is that individuals in longer cases may feel the mental and psychological effects of their case and turn to drugs to cope, which is consistent with the literature. This is further evidence of a mental, emotional, and psychological mechanism in the connection of divorce and crime.

Table 8: Effect of Divorce Judge Calendar Randomization on Crime by Crime Type

Sex	Drug	DUI	Domestic	Traffic (Non-DUI)	Property
0.000194	0.00194*	0.000263	4.23e-05	0.000690	6.99e-05
(0.000475)	(0.00101)	(0.000507)	(0.000294)	(0.000549)	(0.000146)
[0.0126]	[0.0615]	[0.0146]	[0.0049]	[0.0164]	[0.0011]
Crime with Firearm	Theft	Assault	Robbery	Burglary	Aggravated
0.00102	0.000271	0.000748	0.000542	0.000776	0.000321
(0.000697)	(0.000531)	(0.000592)	(0.000504)	(0.000559)	(0.000785)
[0.0274]	[0.0154]	[0.0204]	[0.0141]	[0.0172]	[0.0361]

Note: *p<0.1; **p<0.05; ***p<0.01
robust s.e. reported in parentheses.

This table provides estimates of the instrument, *Fitted Case Length - 100s of Days* on twelve separate crime variables that are separated by crime type. Each dependent variable is defined as a binary variable that is coded as a 1 if an individual is matched with a crime that is of the class specified. These estimations include district X year X month fixed effects as well as robust standard errors. These estimations also include the vector of controls included in the main results. Estimates are provided without bracketing, standard errors are provided in parentheses, and dependent variable means are provided in square brackets. The number of observations is 284,896. The first stage F-statistic is 44.97.

3.6 Robustness

To check the appropriateness of the instrument, I propose checking the impact of the instrument and its effect on crimes that shouldn't be related. These "impossible crimes" are those committed anytime before an individual's filing date outside of Cook County. Judges shouldn't impact behavior before the individual files their divorce case and individuals filing in Cook County should be less likely to commit crimes outside of the county. The results of this test should be insignificant which would determine that the instrument does not predict behavior that is outside the impact of the variable interest.

Table 9 provides the impact of a randomly longer case length on an individual committing an "impossible crime". Column 1 provides estimates in the pooled sample of all individuals. Columns 2 and 3 present estimates on the sub-sample of team calendars. Columns 4 and 5 are estimates in the sub-sample of individual calendars. Columns 3 and 5 present estimates on cases filed between 2010 and 2018 and are conditional on the divorce portion of a case being finalized.

Estimates of the variable of interest *Fitted Case Length - 100s of Days* are insignificant across four of the sub-samples, save the pooled sample. This provides evidence that being randomly assigned a longer case calendar will not affect the likelihood an individual committed crimes outside of the salient time period and outside of the county.

Table 9: Effect of Divorce Judge Calendar Randomization on Impossible Crimes

	Dependent variable:				
	Impossible Crime				
	Pooled Sample	Team Calendar		Individual Calendar	
	(1)	(2)	(3)	(4)	(5)
Fitted Case Length - 100s of Days	0.00142*	0.00143	0.000734	-0.00190	-0.000516
	(0.000766)	(0.00138)	(0.00233)	(0.00328)	(0.00184)
No Children Involved	0.000834	-0.00144	-0.00385	-0.00520	-0.00164
	(0.00178)	(0.00259)	(0.00391)	(0.00747)	(0.00539)
Contested Divorce Case	-0.0107**	-0.0104	-0.00899	0.0137	0.00389
	(0.00514)	(0.00749)	(0.0146)	(0.0246)	(0.0146)
Not Previously Filed (Mentioned)	0.000455	-0.00203	-0.00197	-0.000377	0.000841
	(0.000909)	(0.00165)	(0.00203)	(0.00175)	(0.00284)
Military Spouse	0.00327***	0.00520***	0.00584***	0.00365***	0.00514**
	(0.000862)	(0.00138)	(0.00170)	(0.00127)	(0.00260)
Same Last Name	9.47e-05	0.00148	0.00112	0.000372	-0.00177
	(0.000714)	(0.00125)	(0.00177)	(0.00188)	(0.00245)
Self-Representation	0.00145**	0.00160	0.000978	0.00149	0.000628
	(0.000687)	(0.00125)	(0.00158)	(0.00109)	(0.00236)
White	0.0387***	0.0395***	0.0465***	0.0405***	0.0487***
	(0.000657)	(0.00117)	(0.00157)	(0.00135)	(0.00227)
Black	0.0454***	0.0451***	0.0500***	0.0512***	0.0567***
	(0.00172)	(0.00300)	(0.00393)	(0.00298)	(0.00570)
Asian	0.0163***	0.0162***	0.0196***	0.0124***	0.0268***
	(0.00150)	(0.00260)	(0.00343)	(0.00233)	(0.00561)
Hispanic	0.0303***	0.0331***	0.0383***	0.0309***	0.0371***
	(0.000937)	(0.00164)	(0.00218)	(0.00148)	(0.00292)
Female	-0.0276***	-0.0294***	-0.0327***	-0.0262***	-0.0321***
	(0.000608)	(0.00107)	(0.00140)	(0.000965)	(0.00207)
Plaintiff	-0.00309***	-0.00251**	-0.00142	-0.00336***	-0.00213
	(0.000632)	(0.00110)	(0.00143)	(0.000980)	(0.00208)
Crime Mean	0.028	0.028	0.032	0.028	0.035
Case Length Mean	377.62	324.33	407.92	557.96	305.64
First Stage F-Statistic	44.97	40.78	27.76	17.07	8.09
District by Year by Month FE	X	X	X	X	X
Observations	284,896	91,488	60,852	115,422	31,242
Note:	*p<0.1; **p<0.05; ***p<0.01 robust s.e. reported in parentheses.				

This table provides estimates of the instrument, *Fitted Case Length - 100s of Days* on an individual's likelihood of committing an impossible crime. Each dependent variable is defined as a binary variable that is coded as a 1 if an individual is matched with an impossible crime. These estimations include district X year X month fixed effects as well as robust standard errors. These estimations also include the vector of controls included in the main results. Estimates are provided without bracketing and standard errors are provided in parentheses.

3.7 Policy Considerations

The results of this analysis shed light on the effect of legal divorce proceedings on individuals' propensity to commit crime. One possible path of intervention is to assign cases to types of calendars that don't provide evidence of affecting criminal outcomes: Individual Calendars.

Individual judge calendars over-see 57,711 cases within the data-set, while teams oversee 45,744 cases. There are 15 individual judge calendars and 18 team calendars. This also indicates there are 15 individuals over-seeing 57,000 cases and 54 judges over-seeing 45,000 cases. This may indicate that, although individual calendars are slower, they may be more efficient in direct and indirect ways.

The adoption of assigning cases to only individual judges is low-cost because formation of teams of judges is an administrative cost. However, let's assume it takes approximately $10,000 worth of labor hours to put judges back as individuals on calendars.

The cost of the three crime types impacted by divorce calendars to Cook County, Illinois in 2019 was approximately $ 55,000,000 dollars (Lightfoot and O'Neal, 2022). If crime is committed by 4.7 percent of the individuals in this data set across 10 years, then the cost of crime from divorce is $ 258,500 dollars per year. If this decreased by 16.6 because individuals are assigned to judges that don't impact their likelihood of committing crime, then the reductions in cost of crime are up to $ 42,911 dollars per year. The net benefit is then $ 41,911 dollars per year when subtracting the assumed administrative cost.

3.8 Conclusion

The prevalence of divorce in the United States, coupled with the financial, mental, and emotional cost to the individual and society associated with it, makes makes divorce an important area of socio-economic research. Divorce and crime are connected through financial and social costs and a longer divorce case may make an individual more likely to commit crime. Thus, it is important to answer how a quasi-longer divorce case impacts individual's likelihood to commit crime. The findings in this analysis indicate that divorce case length being increased by 100 days leads to an 16 percent increase in an individual's propensity to commit crime. These large causal estimates indicate that divorces could cost society even more taxpayer dollars than expected. The implication then is that policies that are aimed at assigning cases to judge calendar types that don't affect crime, such as assignment to individual judges, could decrease crime and costs posed to society every year.

Cook County, Illinois, is representative of many different backgrounds of individuals and large metropolitan demographics, these findings may not be extendable to other counties in the US that do not have similar civil case courts. Similarly, this paper relies on the connection of divorce cases to names, which may be biased by commonness of a name [4]. Future efforts should include better identified individual data as well as a larger spread of geographic reach.

[4] When including controls for complexity of name, such as whether an individual is matched to census data or the length of a name, the results are still robust

4 Appendix

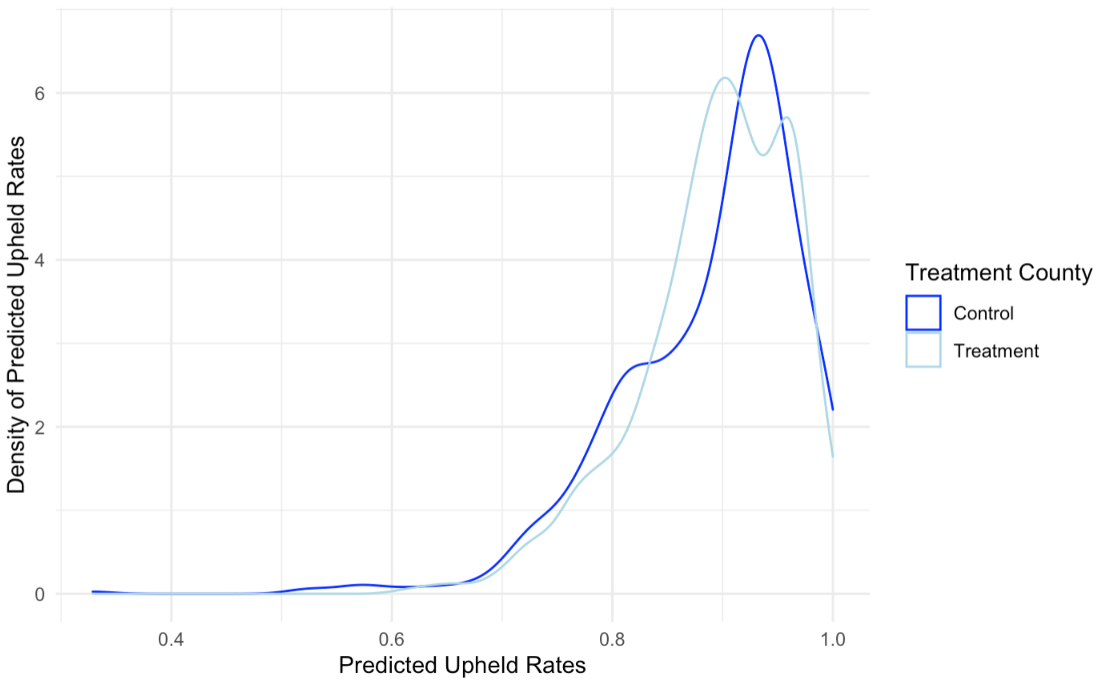


Figure A.1: Predicted Affirmation Rates: Treated and Control Counties

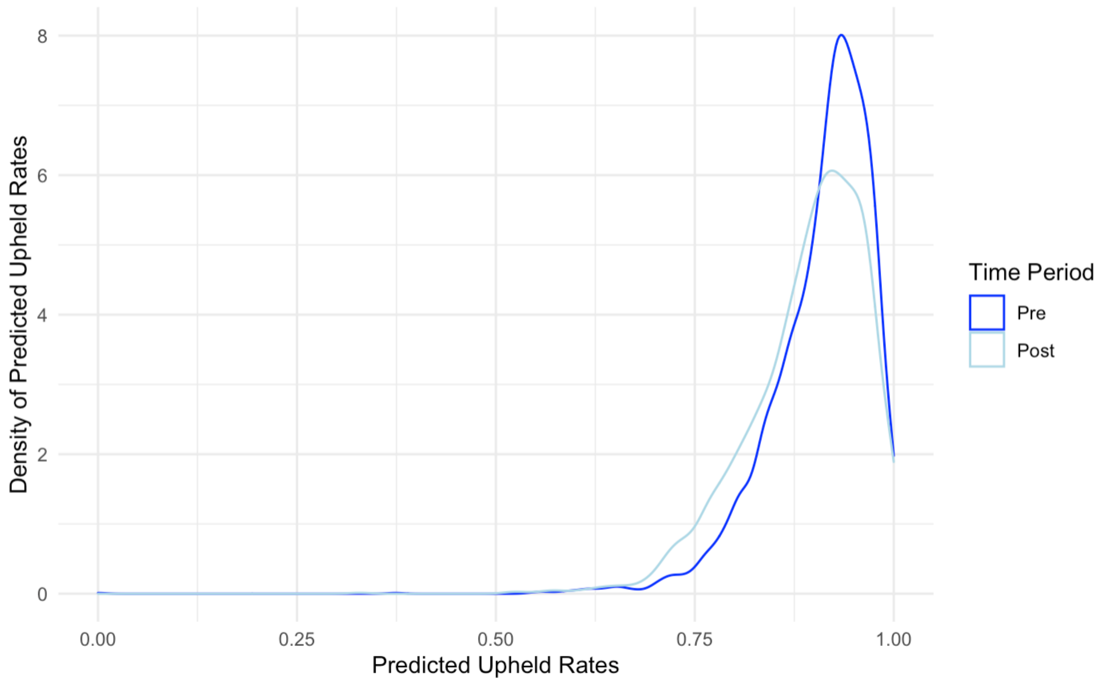


Figure A.2: Predicted Affirmation Rates: Pre-Period and Post-Period

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.

Table B.1: Decomposition of Effect on Female Suicide Rates

Type	Weight	Average Estimate	Weight	Average Estimate
Earlier vs Later Treated	10.62	2.3	-	-
Later vs Always Treated	41.12	-12.72	40.70	-13.04
Later vs Earlier Treated	25.41	6.18	-	-
Treated vs Untreated	22.85	-9.57	21.76	-9.24
Both Treated	-	-	37.54	5.73
Aggregate ATT	100	-5.60	100	-4.42
	Without co-variates	Without co-variates	With co-variates	With co-variates

Table B.2: Decomposition of Effect on Male Suicide Rates

Type	Weight	Average Estimate	Weight	Average Estimate
Earlier vs Later Treated	10.62	-3.3	-	-
Later vs Always Treated	41.12	-0.97	40.70	-1.0
Later vs Earlier Treated	25.41	1.59	-	-
Treated vs Untreated	22.85	-1.96	21.76	-1.88
Both Treated	-	-	37.54	-0.3
Aggregate ATT	100	-0.79	100	-1.28
	Without co-variates	Without co-variates	With co-variates	With co-variates

Table B.3: Decomposition of Effect on Female Spousal Homicide Rate

Type	Weight	Average Estimate
Earlier vs Later Treated	7.13	10.43
Later vs Always Treated	36.68	-25.62
Later vs Earlier Treated	35.81	0.867
Treated vs Untreated	20.38	-10.46
Aggregate ATT	100	-10.50

Table B.4: Decomposition of Effect on Female Spousal Homicide Rate with co-variates

Type	Weight	Average Estimate
Earlier vs Later Treated	45.37	1.95
Later vs Always Treated	35.75	-27.49
Treated vs Untreated	18.88	-15.37
Aggregate ATT	100	-11.80

5 Bibliography

(1980). General Order No. 3.1,1.9 - Management of the Random Assignment System. Technical report.

Abadie, A., Athey, S., Imbens, G. W., and Wooldridge, J. (2017). When should you adjust standard errors for clustering? Working Paper 24003, National Bureau of Economic Research.

Agan, A., Freedman, M., and Owens, E. (2020). Is Your Lawyer a Lemon? Incentives and Selection in the Public Provision of Criminal Defense. *Review of Economics and Statistics*, 1(1):1–1.

Akerlof, G. A. (1982). Labor Contracts as Partial Gift Exchange. *Quarterly Journal of Economics*, 97(4):543–569.

Akerlof, G. A. (1984). Gift Exchange and Efficiency-Wage Theory: Four Views. *American Economic Review*, 74(2):79–83.

Anderson, J. M. and Helland, E. (2012). How Much Should Judges Be Paid? An Empirical Study on the Effect of Judicial Pay on the State Bench. *Stanford Law Review*, 64(5):1277–1341.

Angrist, J., Imbens, G., and Kreuger, A. (1999). Jackknife instrumental variables estimation. *NBER*.

Baker, S. (2008). Should We Pay Federal Circuit Court Judges More? *Boston Law Review*, 88(1):63–112.

Baker, S. and Mezzetti, C. (2001). Prosecutorial Resources, Plea Bargaining, and the Decision to Go to Trial. *Journal of Law, Economics, and Organization*, 17(1):149–167.

Bandyopadhyay, S. and McCannon, B. C. (2014). The Effect of the Election of Prosecutors on Criminal Trials. *Public Choice*, 161(3/4):141–156.

Bandyopadhyay, S. and McCannon, B. C. (2015). Prosecutorial Retention: Signaling by Trial. *Journal of Public Economic Theory*, 17(2):219–256.

Baptiste-Roberts, K. and Hossain, M. (2018). Socioeconomic disparities and self-reported substance abuse-related problems. *Addiction & health*, 10(2):112.

Bjerk, D. (2007). Guilt Shall Not Escape or Innocence Suffer? The Limits of Plea Bargaining When Defendant Guilt is Uncertain. *American Law and Economics Review*, 9(2):305–329.

Bogen, J. (2019). Military Families Are Struggling to Make Ends Meet - The Atlantic.

Boylan, R. T. and Long, C. X. (2005). Salaries, Plea Rates, and the Career Objectives of Federal Prosecutors. *Journal of Law and Economics*, 48(2):627–651.

Boylan, R. T. and Mocan, N. (1987). Intended and Unintended Consequences of Prison Reform. *Journal of Law, Economics, and Organization*, 30(3):S58–S86.

Bruce, M. L. (1998). Divorce and psychopathology. *Adversity, stress, and psychopathology (1st ed.) BT - Adversity, stress, and psychopathology (1st ed.)*, pages 219–232.

Buonanno, P. and Bicocca, M. (2021). The socioeconomic determinants of crime : A review of the literature the socioeconomic determinants of crime.

Bureau, U. C. (2020). U.S. Census Bureau QuickFacts: Cook County, Illinois.

Cacioppo, S., Grippo, A. J., London, S., Goossens, L., and Cacioppo, J. T. (2015). Loneliness: Clinical import and interventions. *Perspectives on Psychological Science*, 10(2):238–249.

Callaway, B. and C Sant, P. H. (2020). Difference-in-Differences with Multiple Time Periods * "Difference-in-Differences with Multiple Time Periods and an Application on the Minimum Wage and Employment".

Callaway, B. and Sant'Anna, P. (2022). *did: Treatment Effects with Multiple Periods and Groups*.

Chaisemartin, C. D. and D'haultfoeuille, X. (2021). Two-Way Fixed Effects and Differences-in-Differences with Heterogeneous Treatment Effects: A Survey *.

Choi, S. J., Gulati, G. M., and Posner, E. A. (2009). Are Judges Overpaid? A Skeptical Response to the Judicial Salary Debate. *Journal of Legal Analysis*, 1(1):47–117.

Cohen, P. N. (2014). Recession and Divorce in the United States, 2008-2011. *Population research and policy review*, 33(5):615.

Collins, R. L., Ellickson, P. L., and Klein, D. J. (2007). The role of substance use in young adult divorce. *Addiction*, 102:786–794.

Courts, U. S. (2022). Judicial Compensation.

Cáceres-Delpiano, J. and Giolito, E. (2012). The impact of unilateral divorce on crime. *Journal of Labor Economics*, 30:215–248.

Davies, H. (2011). Sharing Surnames: Children, Family and Kinship. *Sociology*, 45(4):554–569.

DeAngelo, G. and McCannon, B. C. (2017). Judicial Compensation and Performance. *Supreme Court Economic Review*, 25(1):129–147.

DeAngelo, G. and McCannon, B. C. (2020). Judicial Elections and Criminal Case Outcomes. *Journal of Legal Studies*, 49(1):199–242.

Deserranno, E. (2002). Financial Incentives as Signals: Experimental Evidence from the Recruitment of Village Promoters in Uganda. *American Economic Journal: Applied Economics*, 11(1):277–317.

DeWall, C. N. and Pond Jr, R. S. (2011). Loneliness and smoking: The costs of the desire to reconnect. *Self and Identity*, 10(3):375–385.

Dobbie, W., Goldin, J., Yang, C. S., Harvard, Y. ., Agan, A., Cox, A., Farber, H., Goldsmith-Pinkham, P., Kaplow, L., Looney, A., Mas, A., Mogstad, M., Mueller-Smith, M., Murphy, E., Rehavi, M., Shavell, S., Stevenson, M., Sukhatme, N., Bunke, M., Deluca, K., Lee, S., and Wickett, A. (2018). The Effects of Pretrial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges. *American Economic Review*, 108(2):201–240.

Dyal, S. R. and Valente, T. W. (2015). A systematic review of loneliness and smoking: small effects, big implications. *Substance use & misuse*, 50(13):1697–1716.

Dyke, A. (2007). Electoral Cycles in the Administration of Criminal Justice. *Public Choice*, 133(3/4):417–437.

Foster, J. (2000). Social Exclusion, Crime and Drugs. *Drugs: education, prevention and policy*, 7(4):317–330.

Frank, M. J. (2003). Judge Not, Lest Yee be Judged Unworthy of a Pay Raise: An Examination of the Federal Judicial Salary “Crisis”. *Marquette Law Review*, 87(1):55–122.

Frey, B. S. (1997). On the Relationship between Intrinsic and Extrinsic Work Motivation. *International Journal of Industrial Organization*, 15(2):427–439.

Frey, B. S. and Jegen, R. (2001). Motivation Crowding Theory. *Journal of Economic Surveys*, 15(2):589–611.

Gerstel, N. (1987). Divorce and stigma. *Social Problems*, 34:172–186.

Gilchrist, D. S., Luca, M., and Malhotra, D. (2016). When 3+1¿4: Gift Structure and Reciprocity in the Field. *Management Science*, 62(9):1–2.

Goodman-Bacon, A. (2018). Difference-in-Differences with Variation in Treatment Timing.

Gordon, S. C. and Huber, G. A. (2002). Citizen Oversight and the Electoral Incentives of Criminal Prosecutors. *American Journal of Political Science*, 46(2):334–351.

Grossman, G. M. and Katz, M. L. (1983). Plea Bargaining and Social Welfare. *American Economic Review*, 73(4):749–757.

Hughes, C. E. (1910). *Conditions of Progress in Democratic Government*. Yale University Press, New Haven, CT.

Hughes, K. (2016). N.Y. Counties Balk at Paying for Prosecutors' Mandated Pay Raises. *Daily Freeman*.

Imai, K. and Kim, I. S. (2020). On the Use of Two-Way Fixed Effects Regression Models for Causal Inference with Panel Data.

Kaplan, J. (2020). predictrace: Predict the Race and Gender of a Given Name Using Census and Social Security Administration Data. Technical report.

Kling, J. R. (2006). Incarceration Length, Employment, and Earnings. *American Economic Review*, 96(3):863–876.

Landes, W. L. (1971). An Economic Analysis of the Courts. *Journal of Law and Economics*, 14(1):61–107.

Leopold, T. (2018). Gender Differences in the Consequences of Divorce: A Study of Multiple Outcomes. *Demography*, 55(3):769–797.

Lightfoot, L. and O'Neal, D. (2022). Crime Statistics. Technical report, Chicago.

Lin, I. F., Brown, S. L., Wright, M. R., and Hammersmith, A. M. (2019). Depressive Symptoms Following Later-life Marital Dissolution and Subsequent Repartnering. *Journal of health and social behavior*, 60(2):153–168.

Mayol-García, Y., Gurrentz, B., and Kreider, R. M. (2021). Number, Timing, and Duration of Marriages and Divorces: 2016 1 Current Population Reports.

McCannon, B. C. (2013). Prosecutor Elections, Mistakes, and Appeals. *Journal of Empirical Legal Studies*, 10(4):696–714.

McCannon, B. C. (2021). Informational Value of Challenging an Incumbent Prosecutor. *Southern Economic Journal*, 1(1):1–2.

Nadel, M. R., Scaggs, S. J. A., and Bales, W. D. (2017). Politics in Punishment: The Effect of the State Attorney Election Cycle on Conviction and Sentencing Outcomes in Florida. *American Journal of Criminal Justice*, 42(4):845–862.

NYSAC (2016). DA Salary Increase Must Be Paid-For By State, Say County Leaders.

Perry, J. L. (2018). Merit Pay in the Public Sector: The Case for a Failure of Theory. *Review of Public Personnel Administration*, 7(1):57–69.

Pfau, A. (2011). 2011 Commission on Judicial Compensation Chief Administrative Judge of the State of New York. Technical report, New York State Unified Court System.

Power, C., Rodgers, B., and Hope, S. (1999). Heavy alcohol consumption and marital status: Disentangling the relationship in a national study of young adults. *Addiction*, 94:1477–1487.

Raff, D. M. and Summers, L. H. (1987). Did Henry Ford Pay Efficiency Wages? *Journal of Labor Economics*, 5(4):S57–S86.

Raymo, D. A. (2016). State Decision Sticks Counties with DA Pay Raises .

Reinganum, J. F. (1988). Plea Bargaining and Prosecutorial Discretion. *American Economic Review*, 78(4):713–728.

Roach, M. A. (2014). Indigent Defense Counsel, Attorney Quality, and Defendant Outcomes. *American Law and Economics Review*, 16(2):577–619.

Sant'Anna, P. H. C. and Zhao, J. B. (2018). Doubly Robust Difference-in-Differences Estimators. *Journal of Econometrics*, 219(1):101–122.

Scafidi, B. and Investigator, P. (2008). The taxpayer costs of divorce and unwed childbearing first-ever estimates for the nation and all fifty states.

Scite (2021). scite: see how research has been cited.

Shapiro, C. and Stiglitz, J. (1984). Equilibrium Unemployment as a Worker Discipline Device.

Stebbins, S. (2018). How much does it cost to get a divorce? Where you'll pay the most.

Stevenson, B. and Wolfers, J. (2006). Bargaining In The Shadow of The Law: Divorce Laws and Family Distress. Technical report.

Stevenson, B. and Wolfers, J. (2007). Marriage and divorce: Changes and their driving forces. *Journal of Economic Perspectives*, 21:27–52.

Stevenson, M. T. (2018). Distortion of justice: How the inability to pay bail affects case outcomes. *Journal of Law, Economics, and Organization*, 34:511–542.

Taylor, J. and Taylor, R. (2011). Working Hard for More Money or Working Hard to Make a Difference? Efficiency Wages, Public Service Motivation, and Effort. *Review of Public Personnel Administration*, 31(1):67–86.

Tosi, M. and van den Broek, T. (2020). Gray divorce and mental health in the united kingdom. *Social Science and Medicine*, 256:113030.

Wright, R. F. and Levine, K. L. (2018). Career Motivations of State Prosecutors. *George Washington Law Review*, 86(6):1667–1710.