ESSAYS ON THE CRIMINAL JUSTICE SYSTEM

AN ECONOMIC APPROACH

Radha Iyengar

A DISSERTATION

PRESENTED TO THE FACULTY

OF PRINCETON UNIVERSITY

IN CANDIDACY FOR THE DEGREE

OF DOCTOR OF PHILOSOPHY

RECOMMENDED FOR ACCEPTANCE

BY THE DEPARTMENT OF

ECONOMICS

June 2006

UMI Number: 3208871

UMI Microform 3208871

ProQuest Information and Learning Company
300 North Zeeb Road
P.O. Box 1346
Ann Arbor, MI 48106-1346

ABSTRACT

Two of the goals of the criminal justice system are deterring criminal activity and ensuring that individuals have a fair trial. In order to achieve these goals, the government uses policies which affect the incentives of individuals on both sides of the system. This dissertation evaluates three such policies.

The first chapter, "I'd rather be Hanged for a Sheep than a Lamb: The Unintended Consequences of 'Three-Strikes' Laws," evaluates the impact of longer sentences on the distribution of crimes committed by repeat offenders. To discourage repeat offenders, many states passed "Three-Strikes" laws, which impose enhanced penalties for multiple felony convictions. Assuming that more serious crimes have higher expected payoffs, the flattening of the penalty gradient implies that repeat offenders will commit more serious crimes when they do engage in criminal activity. Using data from California's criminal records for 1993-1995, I find that repeat offenders became more likely to commit serious crimes after Three-Strikes was implemented.

The second chapter "Does the Certainty of Arrest Reduce Domestic Violence? Theory and Evidence from Mandatory Arrest laws," explores the impact of policies that mandate arrest when a domestic violence incident is reported. These laws were justified by a randomized experiment which found that arrests reduced future violence. Using the FBI homicide data from 1980-2000, I provide evidence that mandatory arrest laws increased intimate partner homicides. I provide theoretical and empirical evidence that this increase in homicides is due to decreased reporting.

The third chapter, "An Analysis of the Performance of Federal Indigent Defense Counsel," measures performance differences between the two types of indigent defense

attorneys in the federal system. Exploiting the random assignment of cases between the types of attorneys, an analysis of cases from 1997-2001 from 51 districts indicates that public defenders secure lower conviction rates and sentence lengths than CJA attorneys. An analysis of data from three districts finds that attorney experience, wages, law school quality and average caseload account for over half of the overall difference in performance. This performance difference disproportionately affects minorities and as such may constitute a civil rights violation under Title VI of the Civil Rights Act.

ACKNOWLEDGEMENTS

This project represents the culmination of four years of graduate work. Like any extended endeavor, it was not done in a vacuum and would not have been possible without the help and support of numerous people.

I would like to thank my advisors Hank Farber, Cecilia Rouse, and Orley Ashenfelter. Hank Farber generously provided me with the help and support necessary to develop my fledgling ideas into academic papers. Cecilia Rouse provided me with technical training and moments of sanity without which this project would have been impossible. Orley Ashenfelter provided me with extraordinarily helpful comments as befits his reputation. Five minutes with him improved my work more than five months alone with myself. I am also grateful to my undergraduate advisor David Autor who invested in me as an undergraduate and encouraged me as a graduate student and without whom I would likely not have become an economist. I also benefited greatly from conversations with Alan Krueger, Jesse Rothstein, Bentley MacLeod, Anne Case, Francine Blau, and Steve Levitt.

I thank the Fellowship of Woodrow Wilson Scholars, and especially Stan Katz, for generous financial and intellectual support. I also thank the Industrial Relations Section which provided an outstanding intellectual climate as well as generous financial support throughout my studies. I am indebted to Charlotte Howard, Joyce Howell, and Linda Belfield who help create a collaborative and enjoyable (as well as tasty) work environment. I am also indebted to all the students in the IR Section over the past four years. I owe a particularly large debt to Courtney Stoddard for her moral and intellectual support as a colleague and her tolerance as an officemate. I also benefited from various discussions with Diane Whitmore Schanzenbach, Erica Field, Melissa Clark, Alex Mas, Ken Fortson, Giovanni Mastrobuoni, and Matthew Weinberg.

In addition, I am grateful to those individuals whose generosity helped me obtain the data used in this project. Frank Zimring and Amanda Packel generously provided me with data on California offenders. I thank the National Network to End Domestic Violence, their former president Lynn Rosenthal, and the member coalitions for their willingness to answer questions even while they devote endless hours to serving abused women and children. I am also grateful to Mark Motivans, the Bureau of Justice Statistics, and the Department of Justice for their willingness to share and explain federal criminal system data. I also appreciate the efforts of various people at the California Bar Association and Arizona Bar Association, as well as public defenders in the California Southern District, California Central District, and Arizona Federal District for their willingness to answer questions and provide information critical to this project

Finally, I thank my friends and family who have helped me through these projects in many ways. I am ever grateful to my parents, Ravi and Rama Iyengar, who tolerated my long years of graduate work with generous monetary and culinary support. I thank Rachel Beatty Reidl and Katie Gallagher for providing many enjoyable coffee breaks, runs and other necessary distractions. I thank Manda Wilson for her computer programming help as well as her moral support. Lastly, I thank James Lindley Wilson for his willingness to engage in endless discussions about my research, economics, and other such boring topics as well as his ever present love and support.

Table of Contents

Chapter One

I'd rather be Hanged for a Sheep than a Lamb: The Unintended Consequences of 'Three-Strikes' Laws*

The high crime rates of the 1980s coupled with the belief that prison served as a "revolving door" for criminal activity, prompted new sentencing laws aimed at increasing sentences for repeat offenders. One of the most publicized new policies was habitual offender law, commonly called "Three-Strikes You're Out". This paper uses these laws to estimate how changes in sentencing policy impact the distribution of crimes. Three-Strikes impacts the penalty structure in two ways: first, it shifts up the expected penalty for all crimes (intercept shift), and second it flattens the penalty gradient by severity of felony crimes (slope shift). I develop a model in which the increase in the intercept has the expected effect of decreasing crime levels, while the gradient shift has the unanticipated effect of encouraging a shift toward more serious crime. Given the potential effect on violent crime, it is important to know how large any change in distribution will be. In order to do this I use California's Three-Strikes law to estimate how the distribution of crimes changes in response to the new policy. The results suggest that the propensity to commit violent crime increased after the passage of the law nullifying the benefits of increased sentencing and highlighting a potential drawback to broad sentence enhancement policies.

I. Background on Three-Strikes

In 1993, Washington and Wisconsin were the first states to adopt Three-Strikes sentencing laws. By 1997, twenty-two other states and the Federal Government instituted similar statutes. The common underlying theme among these statutes was severe punishment for recidivist offenders. Although many states ignored their statute, two important components of California's law led it be strictly enforced. First, the broad coverage of the law offered highly enhanced sentencing for all felonies allowing wide application. Second, lack of judicial discretion prevented judges from circumventing the

---

* The opinions and conclusions are solely those of the author. I grateful to David Autor, Hank Farber, Cecilia Rouse, Orley Ashenfelter, Jeffery Kling, Alex Mas, Lawrence Katz, Steve Levitt and participants at the Industrial Relation's labor lunch for numerous insightful suggestions. I would also like to thank Frank

law in cases in which its application seemed unreasonable.[1] In California, over 40,000 offenders have been sentenced under Three-Strikes while no other state has even reached 1000 (Zimring, Hawkins, Kamin, 2001).[2]

Three-Strikes changed the entire sentencing structure for felonies. Individuals convicted of a "record aggravating" offense faced a doubling of the sentence for the second felony or the maximum of three times the sentence of the current felony or 25 years to life for their third felony conviction. As Table 1 shows, the aggravating offenses are very broad under California law, ranging from murder and rape to burglary. The important aspect of the legal structure was that California law invokes a second or third strike for *any felony*, so long as the previous offense was an aggravating offense. Thus, the sentences do not preserve the proportional punishment of non-violent offenses relative to violent crimes on the second and third offense.[3] Simple trend analysis shows dramatic changes in crime rates in California. Charts 1 and 2 compare California's rate to the overall US rate of crime decline. Moreover, there is some evidence that criminals substitute between crimes. Shepherd (2001) compared the rates of triggering and non-triggering offenses before and after Three-Strikes and found significant declines in triggering offenses supporting a deterrence effect from expected increased punishment. This evidence suggests that the effect of Three Strikes may be more complicated than the trend analysis suggests.

II. Theoretical Framework for Comparing Deterrence Regimes

To begin modeling the criminal's decision, consider a simple version of the rational criminal's decision-making process (based on Becker, 1968). An individual will choose crime only if the utility from crime, as defined by the difference between the revenue from crime and the expected cost of committing this crime, is greater than some reservation utility or:

$$U_{crime} \geq \overline{U} \tag{1}$$

Zimring and Amanda Packel for generous assistance with data sources.

[1] In California, only prosecutors had discretion as to whether to charge individuals with qualifying offenses until 1997, when the California Supreme Court reinstated judicial discretion.

[2] Several studies (National Institute of Justice, 1996; Dickey, 1996; Kessler and Levitt, 1998), as well as anecdotal observations by the media indicate that Three-Strikes statutes have rarely been invoked anywhere else.

[3] In fact, a prior prison sentence is not even required to trigger additional penalties, a unique feature of California law (Clark, Austin, and Henry, 1997).

In such a model, the high cost of crime, typically generated by expected cost of imprisonment, will cause many individuals not to commit crime at all. I will refer to the effect of sentencing on the participation decision as *average deterrence*. If there is more than one type of crime, then the expected cost of crime can also affect the distribution of crime types committed. Suppose an individual can choose between two crime types, call them violent (*v)* and non-violent (*nv)*. An individual will chose the crime with the highest utility. Thus an individual will choose violent crime if:

$$U_V - U_{NV} \geq 0 \quad (2)$$

I will call the effect of enhanced sentencing on the choice between crime types *marginal deterrence*.[4] Given these two crime types, there are 6 possible orderings of utilities, which generates 3 different outcomes. Individuals will choose violent crime if either (3) or (4) is true.

$$U_V > U_{NV} > \overline{U} \quad (3)$$

$$U_V > \overline{U} > U_{NV} \quad (4)$$

Individuals will choose non-violent crime if either (5) or (6) is true.

$$U_{NV} > U_V > \overline{U} \quad (5)$$

$$U_{NV} > \overline{U} > U_V \quad (6)$$

Finally, individuals will choose not to engage in criminal activity at all if (7) or (8) is true.

$$\overline{U} > U_V > U_{NV} \quad (7)$$

$$\overline{U} > U_{NV} > U_V \quad (8)$$

The sentence enhancements from Three-Strike changed two things. first, it increased to cost of crime, i.e. $U_V^{TS} < U_V$ and $U_{NV}^{TS} < U_{NV}$. Second, assuming that violent crime is more profitable than non-violent crime [5] and holding the profitability of both types of crime fixed

[4] This follows in the vein of Stigler (1970). Becker (1968) suggested that eliminating the penalty gradient might serve a pragmatic purpose. In his classical model, the efficient criminal punishment system applies maximal (ideally infinite) punishment to all crimes with low probability of enforcement. This system is efficient in the sense that it has the highest ratio of crimes deterred relative to cost. The marginal deterrence introduces inefficiency in the sense that it potentially lowers this ratio. Stigler's response suggested that the increased marginal cost of crimes was necessary to transfer the increased social cost of these crimes onto the individual imposing the costs on society.

[5] Violence/severity could increase the expected gains from crime if the payoff from crime, U(C), increases with more serious or violent crime, or if violence decreases the probability of conviction, pi, for that crime. This seems reasonable if force helps secure payoffs or discourages reporting.

of time, the increased expected cost of crime increase the relative gains from violent crime over non-violent crime, i.e. $U_V^{TS} - U_{NV}^{TS} > U_V - U_{NV}$. The first effect results in some individuals moving from the utility ranking in equation (4) to that of equation (7) or similarly from equation (6) to equation (8). The second effect results in some individual moving from the utility ranking in (5) to the ranking in equation (3). Thus, among those that choose criminal activity, more will find violent crime preferable to nonviolent crime. In that case, we would expect to observe fewer non-violent offenses. To summarize: *for non-violent crime the average and marginal deterrence effect move in the same direction and the model predicts an unambiguous decline in non-violent crime. For violent crime, the average and marginal effects move in different directions and the overall effect of the policy is ambiguous.*

Although I cannot observe the utility of individuals choosing to commit crime, I can observe the crime type they choose. Define the following variable:

$$V^* = U_V - U_{NV} \tag{9}$$

Next, suppose that V^* is a linear function an individual's strike eligibility, age-crime rate, prior criminal history, county characteristics, and individual characteristics, such as age, race/ethnicity, and sex.

$$\begin{aligned} V^* = {} & \beta_0 + \beta_1(2strikes) + \beta_2(3strikes) + \beta_3(after*2strikes) + \beta_4(after*3strikes) + \\ & \beta_5(PCH) + \beta_6(year\ and\ county\ fixed-effects) + \beta_7(individual\ controls) + \varepsilon \end{aligned} \tag{10}$$

In equation (10), *2strikes* is an indicator variable for second strike eligibility, *3strikes* is an indicator variable for third strike eligibility, *PCH* is a vector valued variable detailing an individual prior criminal history, and individual controls include age race, sex, and felony rate per criminal year. I include the felony rate per criminal year (FRCY) because only controlling for age or the number of crimes committed in a given time span can be misleading. Although, sociological evidence suggests individuals are much more likely to commit crime when they are younger, at a given age there are certain individuals who are more likely to be recidivist offenders. The FRCY provides a measure of the combination of effect from youth and being a "crime-prone" individual. Specifically, it is defined as:

$$FRCY = \frac{NumberofFeloniesCommited}{AgeofOffender - 18}$$

(9)

Because I cannot control for the amount of time spent in prison prior to this conviction, this control may not adequately capture the effect of being "crime prone". I also include offender age as a control variable, which allows both an age effect as well as a rate effect, conditional on age.

Although the latent variable, V^* is not observable, I can observe whether an individual chooses to commit a violent crime (call this variable *V*). The observed binary variable $V = 1$ if $V^* > 0$ and $V = 0$ if $V^* < 0$. Assuming that the error term has a logistic distribution, I can use a logit model to estimate the probability that an individual chooses violent crime before and after the law passage and use the difference as a measure of the laws effect on crime choice. By construction this estimation strategy will not identify the average deterrence effect, since I do not observe individuals who decide not to commit a crime. Nevertheless, taking a set of observations on crime choices, I can estimate the change in the distribution of crime types, i.e. the marginal deterrence effect.

I use the *PCH* variable as the source of identification.[6] Under Three strikes, individuals with the same criminal history, but different ordering of crimes have different sentencing eligibility. This mismatch between strikes and felonies arises because while all felony convictions count as strikes after the first strike, only certain felonies are covered as triggering offenses (to give an individual a record-enhancing strike and evoke the harsher penalties). For example, an individual previous convicted of a robbery and then a theft has the same *PCH* value as an individual convicted of a theft and then a robbery. However, these individuals will face different strike eligibility because robbery is a triggering offense while theft is not. Using this fact, I assume that individuals with the same *PCH* variable have a fixed difference across time in all respects except sentencing eligibility. Comparing individuals with similar histories but different Three-Strikes eligibility before and after Three-Strikes provides a means to measure the change in propensity for committing a given

[6] The prior criminal history (PCH) variable is a vector of indicator variables for the types of crimes committed prior to the current offense, where prior crime categories are murder, rape, assault, robbery, burglary, theft, drugs, and other miscellaneous felonies.. For example, an individual with two priors in burglary and theft would have non-zero values for burglary and theft and zero values for all other crime

crime associated with the law change. In general, it would be troublesome to use prior criminal history as a control variable for an individual's innate propensity to commit crime, as the prior history itself may be affected by the law change. That is, individuals may be deciding whether to commit crimes now based, in part, on their effect on sentencing for future crimes. However, because arrestees in the post period were sampled in the years immediately following Three-Strikes, the decision to commit prior offenses was made prior to the passage of the law change for both the pre- and post-Three-Strikes samples. In short, the retroactive nature of Three Strikes makes the variation in *PCH* independent of enhanced sentence eligibility in both the pre- and post-Three Strikes periods. Therefore, individuals are responding to a change in the expected cost of committing crime with a fixed eligibility for sentence enhancements given crime choices.

III. Data Sources

Estimations of the effect of Three-Strikes laws thus far have been based largely on aggregate level data that described crime trends at the state or county level. This is due to the lack of offender level data.[7] I use offender-level arrest records collected by Zimring, Kamin, and Hawkins (2001) that include samples from three cities, Los Angeles, San Francisco, and San Diego.[8] In 1993, 1994, and 1995 arrest and conviction records were provided for a random sampling of arrests for felonies in three cities. These records provide information of the past convictions and current crime as well as personal characteristics such as age and race. Individuals are partitioned into three groups: first strike eligible, second strike eligible, and third strike eligible. Within each group, offenders can have between zero and six prior felonies. The 1993 (pre-law change) sample involved 1352 arrestees, a subset of whom have felonies that would have qualified them for sentence enhancements. This pre-law group serves as a control group.

---

types.

[7] The only publicly available data includes information about offenders after conviction, which may introduce biases if probability of conviction is affected by the law change. For example, it is possible that offenders who commit violent crimes on the second or third offense do so to decrease the probability of conviction. If offenders are able to escape conviction by committing more violent crimes (for example threatening witnesses), then the sample of convicted Three-Strike individuals will be biased away from violent offenders.

[8] For more explicit specifications of the collection methodology, see Zimring, Hawkins, and Kamin (2001), *Punishment and Democracy*, pp. 31-40. All results reported are consistent with those reported by Zimring et al.

The 1994 and 1995 (post-law change) sample includes 1848 arrest defendants from the same three cities. For a portion of the total sample (1993-1995), follow-up records on the disposition of cases were also attached, including information about sentencing. The percentage of arrests leading to conviction is around 22 percent for first strike offenders, 34 percent for second strike offenders, and nearly 40 percent for third strike offenders.

In addition to the simple two crime-type model above, I also estimate the propensity to commit seven felonies as an alternative outcome measure. Table 2 provides definitions and included crimes in each category.[9] The violent offenses are murder, rape, robbery and assault. The non-violent offenses are burglary, theft, and drug crimes. Table 3 compares the sample of arrestees in 1994-1995 to the entire population of second and third-strike arrestees in California in the same years. The criminal population in the sample differs from the population on key demographics. The higher proportion of blacks in the sample is likely due to the sampling of cities. The higher proportion of "other" crimes is also likely due to crimes, which occur more often in cities than in rural or suburban areas. These differences imply that some caution should be taken in generalizing the results.

There are two important exclusions in these data that may produce bias. First, juvenile records were not included despite the fact that Three-Strikes provides that juvenile offenses may count as a strike if they meet the statutory criteria. Second, out-of-state felonies count as a strike but are not documented in California arrest records. Additionally, because of the timing of their data collection, the number of third-strike offenders was relatively low. Thus in this set of data, the sample of third strike offenders is 58 in the pre-period and 64 in the post-periods.[10] However, because of the highly enhanced sentencing for the second strike, second-strike offenders can also be used to test marginal deterrence. The number of repeat offenders (with one or more strikes) is over 400.

[9] Note that there is a discrepancy in the included offenses in Table 1 and Table 3 because Table 3 represents the FBI's included offenses whereas Table 1 uses the Three Strike statute's aggravating offenses for included offenses for sentencing eligibility.

[10] Because the data was collected immediately after the law change, few individuals were caught after committing their third offense. Therefore, the number of eligible arrestees is quite low after three strikes. More generally, because eligibility for Three Strikes was retroactive, the number of individuals eligible for Three Strikes prior to the law change should be approximately the same as the post-law eligible sample. This appears to be the case in the this data.

IV. Results

Before looking at the estimated effect, it is necessary to determine if the Three-Strikes law resulted in sentencing differences based on strike eligibility. Table 4 reports the sentencing statistics before and after Three-Strikes. Because the sentencing data was collected later and appended to the dataset there are a substantial number of missing observations. After dropping all missing observations, the total sample size drops to 631.[11] The numbers in Table 4 are presented largely to suggest that criminals indeed had reason to believe that they might face substantially harsher penalties on their second and third strike. It appears that Three-Strikes did in fact double sentences on the second strike and dramatically increase sentences on the third strike, as is required by law.

Table 5 presents the fraction of offenders who commit a violent crime, conditional on a simplified *PCH* and their strike eligibility. In Table 5, an offender's *PCH* can only take on one of four values: only violent priors (10), only non-violent priors (01), both violent and non-violent priors (11), or no priors (00). All offenders fall into one and only one of these categories. Conditional on this category, I then break offenders down by strike eligibility. As is illustrated by this table, offenders with the same *PCH* can face different strike eligibility. Column (3) reports the difference in the probability of committing a violent offense before and after Three Strikes. This can be seen as the increase (or lack thereof) in the propensity to commit violence in after Three Strikes, conditional on prior criminal history. However, individuals who have multiple priors are likely to be more violent on average and the before after difference may just indicate an increase in the number of innately violent individuals. In fact, individuals with no priors, who are largely unaffected by enhanced sentences were about 4 percentage points more likely to commit violent crime. Given this, I need to control for the changing nature of criminals. Assuming that the innate difference in the propensity to commit violent crime between individuals with prior offenses and individuals with no priors is fixed over time, I difference out the innate violence effect in Column (4). Column (4) is the difference between the before-after change in propensity to commit violence (from Column (3)) and the before after change in individuals who have no strikes and no priors. If there was no

[11] Although in general, there are not significant differences between individuals with updated information versus those without, there does appear to be a marginally significant difference between individuals with current offenses of assault or drugs who have updated sentencing information.

effect from Three-Strikes, then we should observe that the propensity to commit violence is unaffected by sentence eligibility, controlling for the changing violence level of the criminal population. This does not appear to be the case, consistent with a marginal deterrence effect from Three Strikes. For individuals with only violent priors, the enhanced penalty on the second strike deters violent crime. However, on the third strike, these individuals are significantly more likely to commit violent. For individuals with only non-violent and both violent and nonviolent priors, the marginal deterrence effect begins on the second strike. These individuals are significantly more likely to commit violence on the second strike and insignificantly more likely on the third strike. This effect indicates that individuals who would otherwise choose non-violent crime find the penalty gradient so flat on the second strike that they choose to commit violent offenses even when facing only the second-strike enhancements. In fact, it appears that for these offenders, much of the substitution to violence occurs on the second, and not the third strike.

In order to allow a more detailed prior criminal history variable as well as additional controls for location and offender characteristics, I run a binomial logit with the outcome as whether an individual committed a violent crime. In this regression the *PCH* variable includes eight offenses, murder, rape, robbery, burglary, larceny, theft, drugs, and other. Because the offenders are collected from three distinct areas, Los Angeles, San Francisco, and San Diego, I cluster the standard errors by location. Results are reported in Table 6.[12] The estimate of propensity to commit violent crime indicates that second strike eligible individuals who choose to commit a felony in a post Three-Strikes world are about 12 percentage points more likely to choose a violent crime over a nonviolent crime than their counterparts were prior to Three-Strikes. Similarly, third-strike eligible individuals are about 6 percentage points more likely to commit violent crime. Surprisingly, this reveals that the substitution effect is stronger on the second strike. Looking at the results from columns 2 through 8 in Table 6, reveals that relative to second strike eligible offenders, third strike eligible offenders are more likely to commit assault or robbery and less likely, although insignificantly so, to shift towards rape or murder. Among second strike eligible, the increased proportion of severe offenses may be attributable to the decline in the proportion of burglaries, which decreased by almost 7 percentage points. Because burglary is record aggravating offense despite being nonviolent, offenders who

[12] These results are similar to those of a linear probability and multinomial logit with some differences in

commit crime may be seeking a greater "bang for their buck" by committing higher payoff, and therefore more violent, crimes. There also appears to be fewer substitutions from burglary but more substitutions from theft among third strike eligible offenders. The significance of the drugs coefficient is difficult to interpret largely because of the complex interaction between state and federal drug laws.

Overall these results seem consistent with the theory that by eliminating marginal deterrence, Three-Strikes resulted in a crime distribution that is skewed towards more violent crimes. For example, the decrease in murder given Three-Strikes seems reasonable since premeditated murder, activates the death penalty, preserving marginal deterrence. Therefore, conditional on committing a violent crime, criminals should substitute away from murder to assault or robbery. The shift away from non-violent crime towards assault or robbery also seems consistent with the theory the marginal deterrence is relevant. The most compelling evidence appears in the increase in robbery and the decrease in burglary. Robbery and burglary are similar crimes in terms of goal, but differ in the element of force. Moreover, both offenses are record aggravating, which means they generate similar sentence eligibility. The most puzzling aspect of these results is the higher effect of second-strike enhancements relative to third strike enhancements. This may be due to a selection effect of offenders who chose to commit crime. It may be the case that the offenders who are most willing to substitute between violent and non-violent crime are not the same offenders who are willing to commit crime when facing an extremely high penalty. Therefore, selecting on a medium level penalty (as occurs on the second strike), we will observe a fair amount of substitution towards violent crime. However, conditioning on a very high level of penalty (as occurs on the third strike), we only observe criminals who are willing to commit crime but not criminals who view crime as highly substitutable. In this scenario, we should observe some substitution but not as much as in the second strike case, which is consistent with these results.

V. Implications of Results

Although Three Strikes does appear to have a significant effect on the types of crime chosen by offenders, it is unclear how substantial this effect is. In order to make more tangible the average deterrence effect, I construct difference-in-difference estimate of the average

---

magnitude of the murder coefficient.

deterrence effect. Taking Uniform Crime Reports data from 1960 to 2000, I difference the number of crimes in California before and after 1994, when Three Strikes was passed. Similarly, I difference the number of crimes in the United States as a whole before and after 1994. I then difference these to differences, the results of which are presented in Table 7. Column (1) of Table 7 shows that crime in California decreased more than it did in the US on average. If I attribute this entire decline in crime to Three Strikes, it suggests that Three Strikes resulted in a decrease of almost 2,000 incidents per 100,000 people. However, although non-violent crime rates declined more significantly in California after 1994, violent crime rates did not. In fact, violent crime changed negligibly and insignificantly by only 16 incidents.

This estimate is likely to overstate the average deterrence effect of Three Strikes because crime was declining rapidly after 1994 due to the improved economic conditions in California relative to the rest of the US.[13] In order to determine how much of the decline in crime after 1994 can be attributed to Three Strikes, I compare the fraction of crime committed by second and third strike populations. If Three Strikes was responsible for the decline in crime rates, I would expect to find that recidivist offenders were disproportionately deterred from committing crime. However, The fraction of total crime (violent and nonviolent combined) committed by second and third strike offenders is almost exactly the same before and after Three Strikes (43.3 percent before and 42.2 percent after). This suggests that some other factor, not Three Strikes, is responsible for the large decline in crime rates.

Given the evidence above, it becomes increasingly important to quantify the cost of the elimination of marginal deterrence. I use the marginal effects from Table 6 to estimate the amount of crime committed by recidivist criminals attributable to the substitution towards more violent crime. Then, I estimate the number of violent crimes that were committed by Three Strikes eligible offenders that would not have been committed had these individuals chosen to commit non-violent crimes instead. From 1994-2000, I estimate that there were nearly 200,000 more violent crimes and nearly 60,000 violent crimes in just 1994 and 1995.

It is important to note, that I am explicitly assuming that these offenders would still have committed crime, just not violent crime. This makes it difficult to quantify the societal gain (or loss) because crime is not simply deterred or encouraged, instead it is substituted between different crimes. In order to better illustrate the impact to societal welfare, I use

[13] For example, California's unemployment rate rose substantially faster than the rest of the US

monetized estimates of the costs of crime constructed by Miller, Cohen, and Wiersema.[14] Table 8 reports the estimated losses per victimization broken down by contributing categories. Reported in columns (7) and (9) are two estimates of the costs per victimization, the first with more objective measures of cost ("tangible costs") and the second which also adjusts for the effect on quality of life. Estimating the total cost from the violent crimes that were substituted towards in 1994 and 1995 yields a value of $198 million in tangible costs and nearly $600 million in total costs. Similarly, estimating the total cost of non-violent crimes that were substituted away from in 1994 and 1995 suggests that nearly $350 million in tangible costs and about $380 million in total costs of crime were avoided. On net it appears that, when considering only the marginal deterrence effect, Three Strikes imposed a nearly $300 million cost on California.

Table 9 reports the potential costs or benefits of Three Strikes. The rows show how the expected costs savings change as I change the assumption of how much aggregate crime prevention is attributable to Three Strikes. If more than 15 percent of the drop in crime in 1994 and 1995 is due to Three Strikes, then the policy is cost-efficient. However, if less than 15 percent of the decline is due to Three Strikes then the policy actually imposes costs on society. Given the evidence suggesting that Three Strikes is not responsible for the declining crime rates post-1994, it is appears that Three Strikes is on net a wash and may even impose a cost.

VI. Concluding Remarks

This paper presents evidence consistent with the theory that marginal deterrence is necessary to prevent substitution towards more serious or violent offenses. However, there are several caveats to these results. First, it is possible that police officers began charging individuals with more serious crimes after the passage of Three-Strikes law. If this is correct, then the type of the crimes committed before and after Three-Strikes are the same and instead police discretion about the crime with which an offender is charged resulted in more serious charges for Three-Strikes eligible arrestees. While the use of discretion for an arrest is plausible, it is checked in part by the need for a judicial arrest warrant. Because the charges for violent felonies, like murder, rape and robbery, are difficult to compare to any nonviolent or misdemeanor crime it is difficult to imagine that judges would sanction the substitution of

[14] These costs include direct costs of crime, medical and psychological costs, and more intangible costs

felony charges for lesser degree crimes. Discretion could apply in cases where individuals are arrested during the commission of a crime or during other exigent circumstances. However, in these cases it is unlikely that officers know the strike eligibility of a particular individual.[15] Moreover, it is not necessarily clear that officers would have an incentive to charge more serious crimes. They might charge less serious crimes after Three Strikes, which would bias against the results presented in this paper.

Second, an alternative explanation consistent with the results presented in this paper is that offenders for non-violent crime are disproportionately deterred from committing crime. Thus, no offender switches the type of crime they commit, rather some are simply deterred while others are not. This alternative hypothesis is in part answered above, where I find no significant decrease in violent offenses. This would be less likely to occur if offenders were simply deciding whether or not to commit crime. Moreover, this alternative explanation for behavior does not diminish the need for marginal deterrence. If offenders who commit violent crimes receive higher payoffs for these crimes, then harsher penalties are still required to deter these criminals. Thus coincident with this alternative theory is an alternative justification for marginal deterrence. Thus although I am unable to firmly rule out this alternative explanation, it does not seriously harm the ultimate conclusion of this paper that marginal deterrence is necessary to ensure that more violent crimes are deterred as much as non-violent crimes. However, if this explanation of behavior is true, then Three Strikes did not encourage any crime that would not occurred, it simply failed to deter violent crime at all.

Although findings of this paper suggest that Three-Strikes increased violent crime committed by second and third strike offenders, I present only rough estimates about the effect of Three-Strikes on overall crime level. Three-Strikes appears to have little affect on the total number of offenders willing to commit an additional crime while at the same time increasing the propensity to commit more violent crime for those that do commit another crime. At best, the marginal deterrence effect nullifies any benefits from Three Strikes on violent crime. At worst, Three Strikes imposes a social cost from the increased crime rates. This paper does not predict how changes in Three-Strikes that reinstate marginal deterrence will impact the average deterrent effect. That is, I cannot determine whether the reinstatement of enhanced penalties

---

such as loss of productivity. For an in depth discussion see Miller, Cohen, and Wiersema, 1996.

[15] This theory would be of greater concern with indictment or conviction level data, where charges often reflect both the nature of the crime and a bargaining position for plea negotiations. For more discussion on

for more violent crime will diminish the existing average deterrence effect. Future studies that could sample the proportion of criminals who decide to commit another crime or studies that could compare felony records to misdemeanor records may be useful in estimating the average deterrence effect. The interactive effects of average and marginal deterrence are not estimated in this paper and are also left as an area of future work. Nevertheless, it is clear that crime reduction policies that hope to relieve the societal burden induced by crime must carefully weigh the benefits of deterring crime with enhanced penalties against the potential increase in violent crime these penalties may induce.

how discretion and sentence enhancements interact see Kesseler and Piehl (1998)

REFERENCES

Becker, Gary (1968), "Crime and Punishment: And Economic Approach" *Journal of Political Economy* 76:169-217

Beres, Linda S., and Thomas D. Griffith (1998), "Did 'Three-Strikes' Cause the Recent Drop in California Crime" An Analysis of the California Attorney General's Repot". *Loyola of Los Angeles Law Review* 32:101

California Department of Corrections (2002), *Weekly Report Of Population,* Data Analysis Unit, State of California, March 24

Census Population Reports, State Estimates, 1986-1998

Clarke, John, James Austin, and D. Alan Henry (1997), *Three-Strikes and You're Out": A Review of State Legislation*, National Institute of Justice, Washington D.C.

Cushman, Robert. (1996), "The Effect of 'Three-Strikes and You're Out' on Corrections," in *Three-Strikes and You're Out: Vengeance as Public Policy*, David Schichor and Dale Sechrest eds., p. 155-174, Thousand Oaks, CA: SAGE Publications.

Dickey, Walter (1996),*The Impact of Three-Strikes You're Out" Laws: What Have We Learned* Washington D.C.: Campaign for An Effective Crime Policy

DiIulio, John and Anne Morrison Piehl (1991), "Does Prison Pay? The Stormy National Debate over the Cost-Effectiveness of Imprisonment", *The Brooking Review*, Fall

Ewing v. California (2003). Certiorari to the Court Of Appeal Of California, Second Appellate District, No.01 –6978. Argued November 5,2002 —Decided March 5,2003

Flynn, Edith E. et al. (1997), Three-Strikes Legislation: Prevalence and Definitions," in Critical Criminal Justice Issues: Task Force Reports from the American Society of Criminology to Attorney General Janet Reno (Washington, DC: US Department of Justice/Office of Justice Programs, NCJ 158837)

Greenwood, et al. (1994) Three-Strikes and You're Out: Estimated Benefits and Costs of California's New Mandatory-Sentencing Law" (Santa Monica, CA: RAND)

Hawken, Angela and Peter Greenwood (2001), "Three-Strikes and You're Out: A Review of the Research Evidence On Impacts" Background Report for Three-Strikes Roundtable at RAND

Jaimeson, Ross (1999), "Striking Out: The Failure of California's 'Three-Strikes and You're Out' Law", *Stanford Law and Policy Review,* Fall

Kessler, Daniel and Anne Morrison Piehl (1998), "The Role of Discretion in the Criminal Justice System" *Journal of Law, Economics and Organization*, 14(2):256-276

Macallair, Daniel and Michael Males (1999), "*Striking Out: The Failure of California of California's 'Three-Strikes You're Out' Law*." Sand Francisco, CA: The Justice Policy Institute

Males, Michael and Dan Macallair, and Khaled Taqi-Eddin. (1999). "California Three-Strikes Ineffective." *Overcrowded Times* 10:1 14-16

Marvell, Thomas B., and Carlisle E. Moody (2001), "The Lethal Effects of Three-Strikes Laws," *Journal of Legal Studies*, v. 30, p. 89-106.

Miller, Ted, Cohen, Mark A., Wiersema, Brian. (1996) "Victim Costs and Consequences: A New Look", *National Institute of Justice Research Report*

New York Times (1996), "'Three-Strikes' Rarely Invoked in Courtrooms" A1:1, September 10

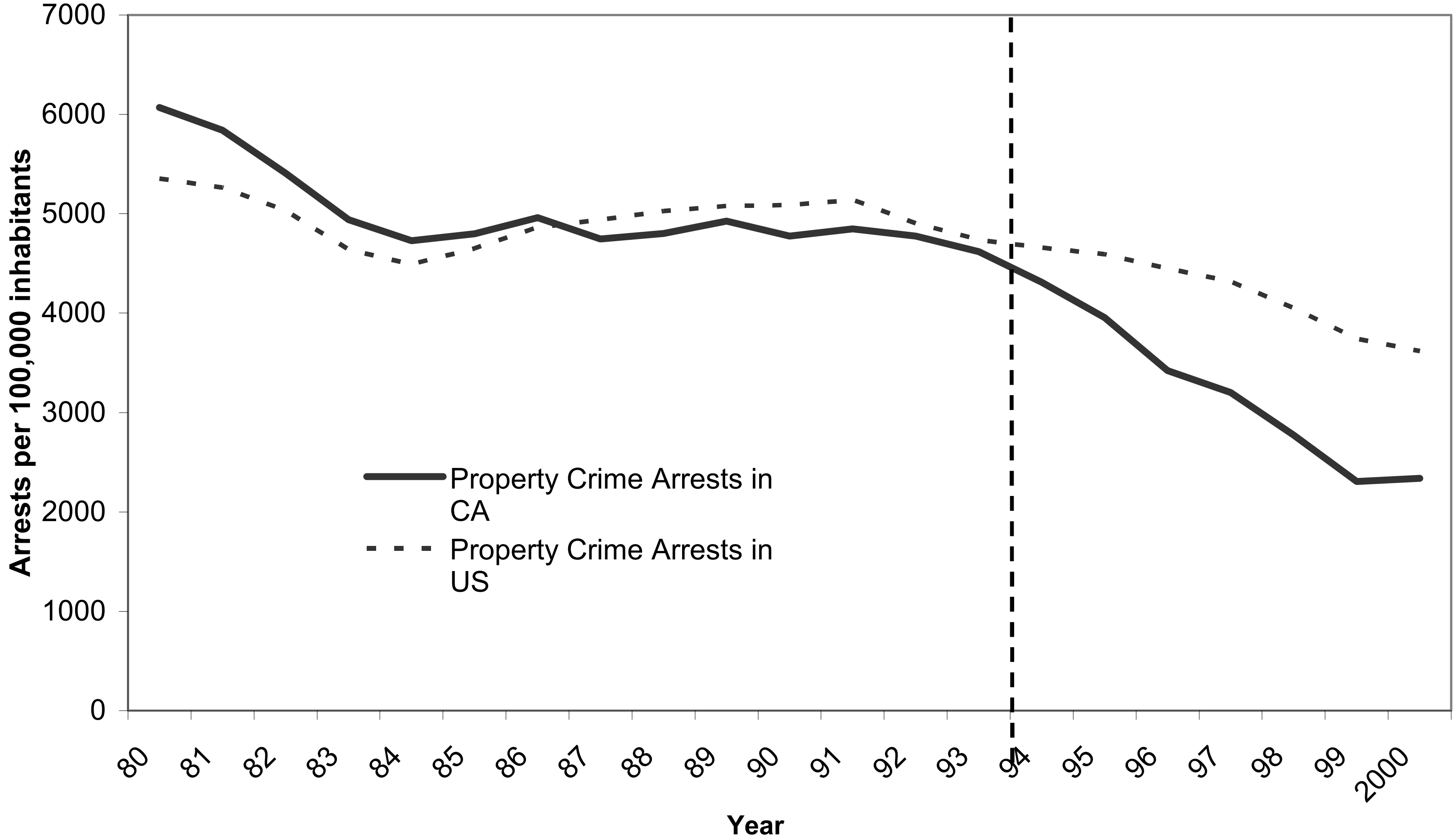
Schafer, John (1999), "The Deterrent Effect of Three-Strikes Law", *FBI Law Enforcement Bulletin*, April

Shepherd, Joanna M. (2002), "Fear of the First Strike: The Full Deterrent Effect of California's Two-and Three-Strikes Legislation." *Journal of Legal Studies* 31: 159-201.

George J. Stigler (1970), "The Optimum Enforcement of Laws." *Journal of Political Economy* 78:526-36

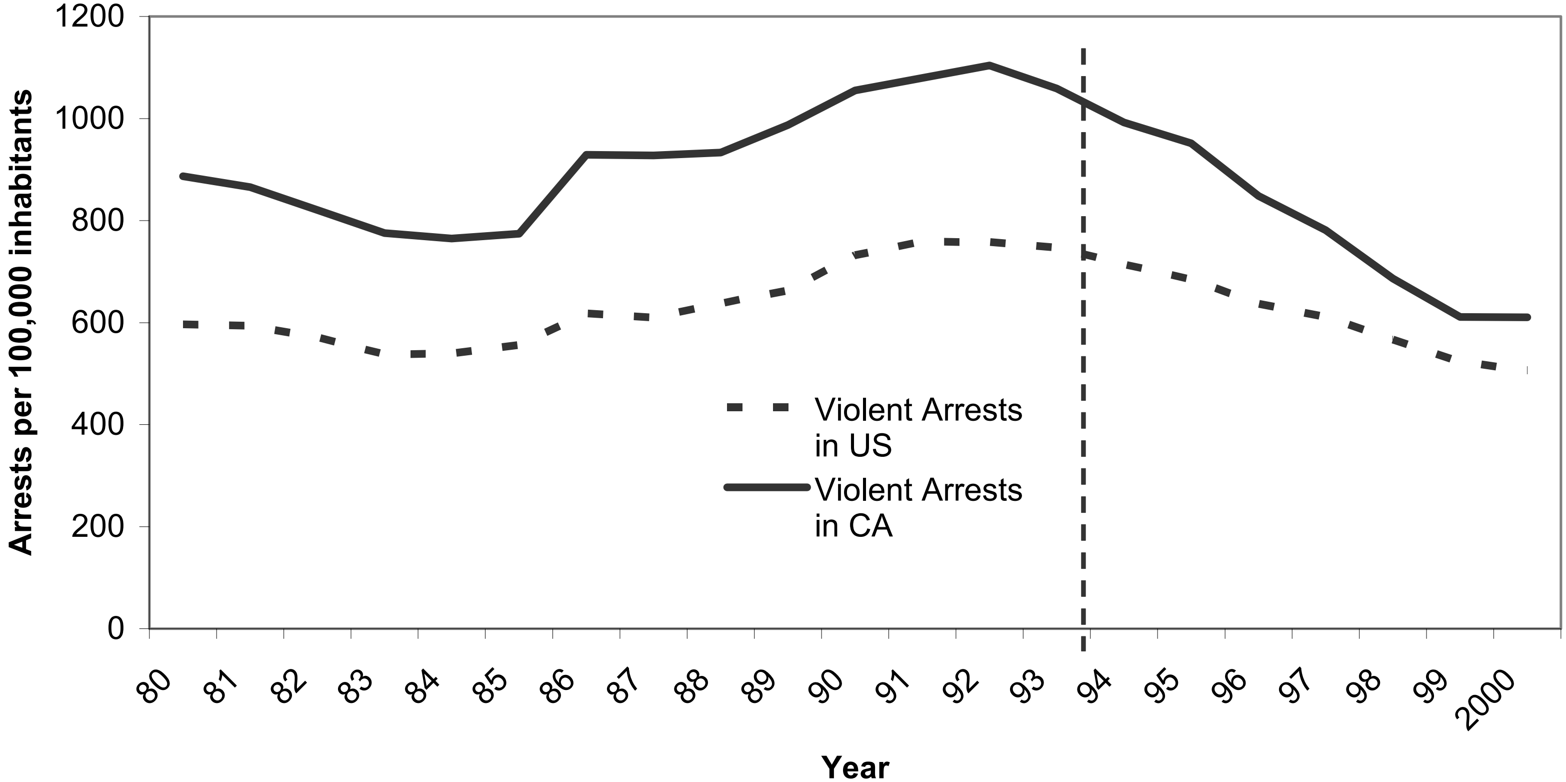
Zimring, Franklin E., Hawkins, Gordon, and Sam Kamin (2001) *Punishment and Democracy: Three-Strikes and You're Out in California"* Oxford ; New York : Oxford University Press

Chart 1: Property Crime Arrests in California and United States



Source: Criminal Justice Statistics Center, California Department of Justice and FBI Uniform Crime Reports 1980-2000. Property Crime is defined as burglary, larceny-theft, and vehicle theft.

Chart 2: Violent Crime Arrests in California and United States



Source: Criminal Justice Statistics Center, California Department of Justice and FBI Uniform Crime Reports. 1980-2000. Violent Crime is defined as murder, robbery, forcible rape, and assault.

Table 1: California Three Strikes Record Aggravating Offenses

Violent Felonies	*Murder*	*Murder* *voluntary manslaughter*
	Sex Offenses	Rape Sodomy by force, violence, duress, menace, or threat of injury Oral copulation by force, violence, duress, menace, or threat of injury Lewd acts on a child under 14 Continuous sexual abuse of a child
	Assault	Attempted murder Assault with the intent to commit mayhem, rape, sodomy, or oral copulation
	Robbery	Any Robbery
	Other Violent Crimes	Mayhem. Any felony in which the defendant inflicts great bodily injury on any person other than an accomplice Kidnapping Carjacking Arson which results in Bodily Harm Exploding device with intent to injure or kill
Serious Felonies (Non-Violent)	Property Crimes	Arson Burglary of a Home or Dwelling Grand Theft
	Drug Offenses	Drug Sales to Minors Drug Trafficking
	Other Felonies	Any felony in which the defendant uses a firearm Threats to victims or witnesses Extortion Any felony punishable by death or imprisonment for life.

Source: California Penal Code, Part 1. Title 16. General Provisions 667

Table 2: Crime Categories and Definitions

Crime	*Definition*	*Included Offenses (California Penal Code Sections)*
Murder	All willful (non-negligent) killing of one human being by another	Murder (§187) Voluntary Manslaughter (§192a) Involuntary Manslaughter (§192b) Gross Vehicular Manslaughter while intoxicated (§193.5)
Rape	Forcible sexual contact	Forcible rape, spousal rape (§261, §262) Forcible Sodomy or Oral Copulation (§286, 288a) Sexual assault with an object (§289) Lewd or Lascivious acts of continuous sex abuse of a child (§288, 288.5) Sexual battery (§243.4)
Assault	Unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily injury, usually accompanied by the use of a weapon or by means likely to produce death or great bodily harm.	Mayhem, Aggravated Mayhem (§203, 205) Torture (§206) Assault with intent to commit Mayhem or sex offenses (§220) Assault with Caustic Chemicals or Taser gun (§244, 244.5) Assault with deadly weapon or by force (§245) Infliction of injury on spouse, cohabitee or parent of child (§273.5)
Robbery	The taking or attempting to take anything of value from the care, custody or control of a person or persons by force or threat of force or violence and/or by putting the victim in fear.	Robbery (§211) First and Second Degree Robbery (§212.5) Train Robbery, Car Jacking (§214, 215)
Burglary	The unlawful entry of a structure to commit a felony or theft. The use of force to secure entry is often a part of burglary but is not required for a burglary charge.	Burglary (§459) Looting (§463)
Theft	The unlawful taking, carrying, leading or riding away of property from the possession or constructive possession of another in which no use of force, violence or fraud occurs.	Larceny (§484-502.9) Motor vehicle theft (§10851)
Drugs	The unlawful possession, sale, use, growing, manufacturing, and making of narcotic drugs. The relevant substances include: opium or cocaine and their derivatives (morphine, heroin, codeine); marijuana; synthetic narcotics (Demerol, methadone); and dangerous non-narcotic drugs (barbiturates)	Any individual subject to California Major Narcotic Vendors Prosecution Law (§13883) who is under arrest for violation of the Health and Safety Code Narcotics (§11350-11356.5) Controlled Substances formerly classified as restricted dangerous drugs (§11377-11382.5)

Note: Definitions from Uniform Crime Reporting Handbook. Not all potentially included offenses are included in the sample

Table 3: Summary statistics of in sample and total population

	2-Strikes		*3-Strikes*	
	Population	Sample	Population	Sample
N	15,230	401	1,477	122
Sex				
Male	94.9	95.4	98.5	94.8
Female	5.1	4.6	1.5	5.2
Age				
Under 20	7.3	5.1	7.1	8.2
20-29	46.7	48.3	43.1	39.7
30-39	34.1	32.8	35.3	32.8
40-49	10.1	11.2	11.5	12.7
50+	1.7	2.6	3.0	6.6
Race/Ethnicity				
Black	37.1	50	43.9	64
Hispanic	32.7	24	27.1	21
White	26.5	26	25.3	16
Current Offense				
Person	14.5	16.9	25.5	20.6
Property	41.1	35.2	38.8	37.9
Drugs	31.6	35.4	22.0	18.9
Other	12.8	12.5	13.8	22.6
Sentence Length				
Life	0.2	.5	0.5	.8
Other	99.8	99.5	99.5	99.2
Average Sentence	4 years, 11 months	4 years, 9 months	37 years, 3 months	30 years, 9 months

Note: Due to missing data on Current Offense cases for California, offenses do not add up to total intake values of 15,230 and 1,477 respectively. Source: California Department of Corrections, Admissions 1994-1995 and author's own calculations from Three City Sample, 1993, 1994, and 1995

Table 4: Median Sentences in Pre and Post Three Strikes Period, by Crime Type and Offender Strikes

	1993 (Pre-Three Strikes)	***1994-95 (Post-Three Strikes)***
First Strike Eligible		
Murder	20 years	20 years
Rape	4 years	3.8 years
Assault	1.5 years	6 months
Robbery	2 years	2 years
Burglary	8 months	1 year
Theft	1 year	11 months
Drugs	8 months	6 months
Second Strike Eligible		
Murder	20 years	23 years
Rape	--	4 years
Assault	1 year	2.5 years
Robbery	3.5 years	6.5 years
Burglary	2 years	3.7 years
Theft	2 years	2.7 years
Drugs	1 year	3 years
Third Strike Eligible		
Murder	20 years	Life
Rape	--	30 years
Assault	6.5 years	--
Robbery	4 years	21 years
Burglary	2 years	25 years
Theft	1.2	25 years
Drugs	2	10 years

Source: Three City Survey of Arrest Record in Los Angeles, San Diego, and San Francisco, 1993-1995. Offenders who committed "other" offenses are excluded from the sample. All sentences are truncated at 60 years. Offenders with missing sentencing data are omitted. Sample size is 631.

Table 5: Fraction of Offenders with a Current Violent Offense by Prior Criminal History and Strike eligibility

		Time Period		Difference-in-Difference Estimates	
		(1) 1993 (Pre-Three Strikes) (N=1113)	***(2) 1994-95 (Post-Three Strikes) (N=1425)***	(3) After—Before	(4) Column (3) — No prior, one strike eligible
Only violent priors	Two-strike eligible	0.4667 (115)	0.3500 (140)	-0.1167 (0.1536)	-0.1583*** (0.0096)
	Three-strike eligible	0.1667 (12)	0.4212 (19)	0.2544 (0.1618)	0.2128*** (0.0291)
Only Non-violent priors	One-strike eligible	0.3568 (199)	0.3174 (189)	-0.0393 (0.0481)	-0.0809*** (0.0025)
	Two-strike eligible	0.3636 (172)	0.4688 (182)	0.1351* (0.0780)	0.0935*** (0.0042)
	Three-strike eligible	.3333 (12)	0.4000 (16)	0.0667 (0.1236)	0.0251 (0.0233)
Both Violent and Non-violent Priors	Two-strike eligible	0.2667 (138)	0.5000 (215)	0.2333* (0.1229)	0.1917*** (0.0066)
	Three-strike eligible	0.0833 (33)	0.1429 (30)	0.0596 (0.1129)	0.0180 (0.0142)
No Priors	One-strike eligible	0.3934 (432)	0.3518 (634)	-0.0416* (0.0226)	--

Note: For columns (1) and (2), number of observations are reported in parentheses. For columns (3) and (4), standard errors are reported in parentheses. Results that are significant at .05 (0.1, 0.01) are reported with **, (*, ***). Statistics are means of an indicator variable that equals one if an individual commits murder, rape assault or robbery, and zero otherwise. Results are based on 1993-1995, Three City Sample of arrestees. Offenders with “violent priors” have at least one prior conviction for murder, rape, assault, or robbery. Offenders with “non-violent priors” have at least one prior conviction for burglary, theft, and drugs and have no prior convictions for violent offenses. Offenders who committed other offenses are omitted.

Table 6. Probability of Current Crime Type for Second and Third Strike Eligible Arrestees

	(1) Violent crime	(2) murder	(3) rape	(4) assault	(5) robbery	(6) burglary	(7) theft	(8) drugs
*after*2strikes*	0.1212***	-0.0048	0.0419**	0.0212*	0.0493**	-0.0679***	-0.0208	-0.0147
	(0.0205)	(0.0031)	(0.0083)	(0.0125)	(0.0237)	(0.0260)	(0.0296)	(0.0211)
*after*3strikes*	0.0599***	-0.0255	-0.0226	0.0451*	0.1014*	-0.0280*	-0.1094*	0.0613
	(0.0295)	(0.0218)	(0.0185)	(0.0236)	(0.0525)	(0.0168)	(0.0628)	(0.2448)
2 strikes	-0.1034	-0.0146	-0.0267	0.0947	-0.0772***	-0.0107	0.1147	-0.0461
(=1 if second strike eligible)	(0.0890)	(0.0179)	(0.0142)	(0.1016)	(0.0044)	(0.0158)	(0.0949)	(0.0714)
3 strikes	-0.3179	-0.0101	-0.0129	-0.0207	-0.2180***	-0.0012	0.1749	0.0160
(=1 if third strike eligible)	(0.2469)	(0.0037)	(0.0145)	(0.2181)	(0.0836)	(0.0361)	(0.1988)	(0.1802)
Constant	-0.0739	0.0023	0.0230**	-0.1013***	-0.1986***	-0.1446***	-0.2061***	-0.2962***
	(0.0468)	(0.0018)	(0.0026)	(0.0387)	(0.0086)	(0.0205)	(0.0252)	(0.0926)
Observations	2477							

Note: Results that are significant at .05 (0.1, 0.01) are reported with **, (*, ***). Reported values are marginal effects evaluated at the mean. Column (1) dependent variable is an indicator for whether the current offense is violent. Violent offenses are murder, sex offenses, assault and robbery. The dependent variables for columns (2)-(8) are indicator variables for whether an individual committed a given crime type (types are murder, sex offenses, assault, robbery, burglary, theft, drugs). Coefficients reported are an indicator variable for individuals who are second strike eligible, and an interaction term between the year indicator variables and strikes indicator variables. Also included but not reported are variables for age, race, ethnicity, sex, FRCY, PCH, and year and county fixed effects. The base category is first-strike eligible offenders in 1993. Standard errors, reported in parentheses, are clustered by county of arrest

Table 7. Linear Regression Estimates of the Effect of Three Strikes on Overall Crime Levels

	(1)	*(2)*	*(3)*
	Total Crime per 100,000 inhabitants	Violent Crime per 100,000 inhabitants	Property Crime per 100,000 inhabitants
after	-1,144.2***	15.1	-1,159.4***
(=1 after 1994)	(146.06)	(47.3)	(123.5)
California	1,912.4***	210.5***	1,701.9***
(=1 in California)	(94.2)	(23.8)	(106.2)
*after*California*	-1,878.0***	-16.6	-1,861.4***
	(218.7)	(49.3)	(187.7)
Constant	5,358.2***	545.8***	4,812.4***
	(82.5)	(13.4)	(76.1)
R-squared	0.8943	0.8329	0.8918
Observations		2048	

Note: Results that are significant at .05 (0.1, 0.01) are reported with **, (*, ***). Dependent variable is crimes reported per 100,000 inhabitants. Also included but not reported are variables are year and state fixed effects. Standard errors, reported in parentheses, are robust. Omitted year is 1980.

Table 8. Estimated Costs of Crime per Criminal Victimization

	(1) Productivity	*(2)* Medical Care	*(3)* Mental Health Care	*(4)* Police Services	*(5)* Social/ Victim Services	*(6)* Property Loss/ Damage	*(7)* Subtotal: Tangible Losses	*(8)* Quality of Life	*(9)* Total
Rape	2200	500	2200	37	27	100	5,100	81,400	87,000
Assault	950	425	76	60	16	26	1,550	7,800	9,400
Robbery	950	370	66	130	15	750	2,300	5,700	8,000
Burglary	12	0	5	130	5	970	1,100	300	1,400
Larceny	8	0	6	80	1	270	370	0	370
Motor Vehicle Theft	45	0	5	140	0	3300	3,500	300	3700

Notes: All estimates are in 1993 dollars value. Quality of life estimates are based on analysis of jury awards for compensatory damages.
Source: Miller, Cohen, and Wiersema (1996) Table 2

Table 9. Cost-Benefit Analysis of Three Strikes

		Gain from Average Deterrence Effect	*Loss from Marginal Deterrence Effect*	*Total Cost/Benefit*
Fraction of Crime decline between 1994 and 1995 attributed to Three Strikes	50%	$1 billion	$300 million	$1.9 billion
	25%	$500 million		$700 million
	10%	$215 million		$200 million
	0%	$0		-$85 million

Note: Estimates for average and marginal deterrence effects are based on author's own calculations. Total number of crimes based on Uniform Crime Report data. Cost of crimes are based on Miller, Cohen, and Wierserma (1996).

Chapter 2
Does the Certainty of Arrest Reduce Domestic Violence? Theory and Evidence from Mandatory Arrest laws[*]

Women are more likely to be beaten, raped, or killed by a current or former male partner than by anyone else (Epstein, 1999). Despite two decades of increased public awareness, domestic violence remains a serious public policy issue in the United States. From the late-80s through the mid-90s, states faced with increased liability for police inaction, passed laws requiring the warrentless arrests of individuals police believe to be responsible for misdemeanor assault of an intimate partner. Many of these policies were justified by results from a randomized experiment that demonstrated that arrests were effective at deterring future violence. This experiment was logically extended to support mandating arrest in all cases of domestic violence. However, the experiment provided no evidence on the effectiveness of a public policy *requiring* arrest. Policies which mandate arrest (i.e. make arrests certain, conditional on reporting) may have a different result from experiments which probabilistically apply arrest. Indeed the empirical analysis presented in this paper demonstrates that mandatory arrest laws increase intimate partner homicides. One reason for this is that a known policy of arrest may affect the decision by victims to seek police intervention making the application of experimental results inappropriate and potentially deleterious [1] In particular, it appears that the certainty of arrest dissuades victims from reporting abuse to the police resulting in higher rates of intimate partner abuse.

In this study I develop a model that illustrates how repeat interaction between abusers and victims can generate different results in an experimental versus policy setting. These results may be due to changes in the reporting behavior of victims in response to the certainty of arrest. Using a difference-in-difference framework, I tested to see if mandatory arrest laws affected the level of domestic violence. I found that

[*] I am grateful to Hank Farber, David Autor, Jesse Rothstein, Francine Blau, and participants at the Princeton University Industrial Relations Section labor lunch and Law and Public Affairs seminar for numerous comments and insightful suggestions. Financial support from Princeton University Industrial Relations Section and the Woodrow Wilson Society of Fellows is gratefully acknowledged.

[1] There is considerable controversy over the use of "victim" to describe people who have experienced domestic violence. In this study, I will be dealing with people in the immediate aftermath of a violent event and as such will be using the term "victim" because the transformation into "survivor" may or may not have occurred and in deference to those victims who in fact do not survive their violent experiences.

intimate partner homicides increased by about 60 percent in states with mandatory arrest laws. Because police intervention may decrease the risk of escalation and thus the risk of homicides this rise in homicide rates is consistent with a decline in reporting for intimate partner homicides. Results from a similar analysis of non-intimate partner family member homicides show declines in these homicides in response to mandatory arrest laws. These results are also consistent with the reporting explanation. In most cases of child abuse, the reporting of abuse comes from a third party (such as a teacher or doctor). In such cases the certainty of arrest does not shift the incentives of the third party to report, and as such we would expect to see a deterrence effect from arrest. Unrelated, non-familial homicides were unaffected

This study has two main objectives: First, it evaluates a public policy that currently enjoys both popular and financial support. [2] Identifying the problems of mandating arrests is critical to developing a more effective criminal justice response to domestic violence. Second, this study attempts to contribute to the ongoing discussion about the applicability of random experiments in determining social policy. For two decades there has been considerable debate about the validity of quasi-experimental approaches.[3] While in many cases randomized experiments can precisely isolate the effect of a specific intervention, this is not necessarily the same as measuring the effect of a policy that distributes potentially effective programs to the general population. This study provides a cautionary tale about extending experiments to the construction of social policy. Because of the responsiveness of individuals to incentives generated by known policies, the most straightforward applications of experiment-based evaluations may generate perverse outcomes when translated into public policy. In such cases, government programs may become counterproductive, harming the very people they seek to help.

[2] Currently, the Violence Against Women Office at the Department of Justice spends between $30 to 50 million each year on grants to encourage these mandatory arrest laws.
[3] Meyers (1992) provides a useful overview. For additional details see for example Ashenfelter and Card (1985), Lalonde (1995), Heckman and Robb (1985), Friedlander and Robins (1995).

I. The Emergence and Effectiveness of Mandatory Arrest Laws

Policies that encourage or require arrest of domestic abuses play a prominent role in the government's attempt to combat domestic violence. This is in part because, historically, law enforcement has been reluctant to arrest or even intervene in cases of domestic violence. For example, in the 1970s, the American Bar Association (1973) urged police to use conflict resolution, not arrests, when intervening marital disputes. Currently, twelve states and the District of Columbia have passed mandatory laws. A "mandatory arrest law" requires police to arrest a suspect, despite the lack of a warrant, if there is probable cause to suspect that an individual has committed some form of assault (either misdemeanor or felonious) against an intimate partner or family member. An additional ten states have recommended arrest laws, which specify arrest as a recommended but not required when confronted with probable cause that an intimidate partner or familial assault has occurred.

States enacted mandatory and recommended arrest laws for several reasons. After Thurman v. City of Torrington (1984) established the right to police protection from domestic violence, states faced potential lawsuits from police inaction.[4] Since the late seventies there has been increasing political pressure for states to offer more protection for victims of domestic violence. Indeed the domestic violence movement in part emerged as a collaborative effort by survivors and service providers to develop a more appropriate criminal justice response to domestic abuse.[5] In the eighties there emerged a growing consensus that mediation was neither a safe nor successful response to domestic abuse. The American Medical Association began to advise its member that counseling was dangerous and increased its effects to educate its member about the harms of domestic violence. Several states endorsed criminal sanctions, in the form of mandated arrests, in an effort to generate a more appropriate response. Due in large part to this

---

[4] Several other states found police departments liable for failing to protect battered women. For a review of these cases see Wanless (1996)

[5] For a detailed discussion of the emergence of mandatory arrest laws see Stark (1993)

these factors as well as federal incentives (in the form of grants), twenty-two states have passed either mandatory or recommended arrest laws. [6]

I.A Experimental Evidence Supporting Arrest Laws

The use of mandatory arrest laws is in many respects predicated on the results of the Minnesota Domestic Violence Experiment (MDVE).[7] This experiment, funded by the Minnesota Police Department, the Police Foundation, and the Department of Justice, was run by randomly assigning a police response to domestic violence calls (Wanless, 1996). Police applied one of three possible treatments: (1) advising and counseling the couple, (2) separating the individuals, or (3) arresting the suspect. Researchers then interviewed the victims shortly after police involvement and then followed up every two weeks for six months. The original results found that arresting the suspect resulted in substantially less future violence than did either advising or counseling (Sherman, 1992).[8] An in depth evaluation of the results by Tauchen and Witte (1995) found that arrest resulted in significantly more deterrence than either advising or separating the couple, consistent with the original findings of the experiment. However, unlike the original findings, Tauchen and Witte use a dynamic setting which found that most of the deterrent effect of arrest occurs within two weeks of the initial arrest. Thus if there is any deterrent effect it appears to be temporary.

While this experiment provided support for the contention that arrest deters abuse, the scope of applicability of its findings is uncertain. The public in general and battered women in particular were not informed of this experiment. Thus, the experiment actually tested the effect of a probabilistic arrest rather than a deterministic policy which requires arrest. This difference is significant because of behavioral differences that may arise in an ongoing nature of the relationship between the battered women and their abusers. A noteworthy feature of a mandatory arrest policy is the potential response by battered women to the certainty of an arrest of their abuser. The response by batterers relative to

---

[6] In most cases the assaults are committed by individuals against current or former intimate partners. These laws typically only apply to co-habiting, married, legally separated or divorced intimate partners. These laws are also typically applicable to the abuse of residential family members.
[7] Evidence that MDVE was discussed when passing these laws can be found in Wanless (1992).
[8] Further replications of the MDVE in Milwaukee, Omaha, Colorado Springs and Charlotte have produced mixed results. For a comparison of these experiments, see Symposium on Domestic Violence, 1992.

the response by victims to the increased costs of abuse may be relevant when determining the efficacy of an intervention such as mandatory arrest laws.

I. B Quasi-experimental Estimates on the Effectiveness of Mandating Arrest

In order to test the effectiveness of mandatory arrest laws, I sought to estimate the effect of these laws on intimate partner abuse. Note that it is important to consider the total number of incidents not just reported incidents because the fraction of incidents that are reported to the policy is potentially affected by this policy. However, because I cannot observe unreported incidents, changes in the number of reported incidents and change in the total number of incidents (both reported and unreported) are observationally equivalent.[9] In part because I can observe victim-offender relationship and in part because these crimes are almost perfectly reported, I use measure of intimate partner homicides as a way to measure intimate partner abuse. Assuming that police intervention can reduce the probability of violence, changes in the intimate partner homicide measure may provide insight into the impact of mandatory arrest laws on intimate partner violence.[10]

To construct a dataset of intimate partner homicides, I use the FBI Uniform Crime Reports, Supplementary Homicide Reports which provide data for all homicides that took place in the year 1980 to 1999 in all 50 states and the District of Columbia with additional descriptive variables about the victim, offender, and the nature of the crime. An intimate partner homicide is includes a homicide committed against a husband, wife, common-law husband, common-law wife, ex-husband, or ex-wife.[11] Although the specific coverage varies by state, the general categories and their proportion of the overall number of homicides are listed in Table 2. The data is constructed at the incident level with about 8 percent of the sample (42,000 observations) being intimate partner

[9] An added difficulty is that domestic violence is difficult to observe since there is not usually a "domestic violence" charge. Individuals are typically report some form of assault. Within these reports I cannot distinguish the relationship between the victim and the offender

[10] The linkage between misdemeanor assault prevalence and intimate partner homicide is well established. See for example Gwinn and O'Dell (1993). Moreover the underlying causes are linked see Mercy and Saltzman (1989)

[11] The specific coverage of each law is reported in the data appendix.

homicides.[12] I constructed a count of the number of relevant homicides by aggregating the incidents of intimate partner homicide, as defined above, in a given year in a given state. I also aggregated the number of intimate partner homicides by the race of the victim and offender and by sex of the victim and offender. I scaled all of these by population, using census estimates for state population.

Mandatory arrest laws appear to have had a significant impact on intimate partner abuse, Figure 1 shows the rate of intimate partner and family homicide rates as a function of time since the arrest law change. There appears to be a discrete increase of about 0.4 intimate partner homicides per 100,000. In contrast there is a small decline in the number of family violence homicides. If mandatory arrest laws reduce reporting, we would expect to see more intimate partner homicides. In contrast, Figure 2 shows that recommended arrest laws have relatively little effect on either intimate partner or familial violence.

Comparing intimate partner homicides in states with and without mandatory arrest laws before and after the passage of these laws, I estimate a linear regression of the number of intimate partner homicides per 100,000 inhabitants on an indicator for mandatory arrest laws, an indicator for recommended arrest laws, and state and year fixed effects. Column (1) of Table 3 reports some coefficients from this regression. The *mandatory arrest effect* variable is defined as 1 in states that passed mandatory arrest laws in the years after the law was passed. Similarly, *recommended arrest effect* variable equals 1 in states that passed recommended arrest laws in the years after the law was passed. The results suggest that mandatory arrest laws are responsible for an additional 1.4 murders per 100,000 people. This corresponds to a 62 percent increase in intimate partner homicides. There does not appear to be a significant effect in recommended arrest law states, although the coefficient is negative. Recommended arrest laws appear to have no significant effect on homicide rates.

I next estimated a specification controlling for the number of years since the law change. Combined with the year fixed effects this both controls for any differences at a

[12] There is some measurement error in victim-offender relationship variable. About 1.25% of female victims reported as having a relationship to their offender that would imply she's a man and about .43% of male victims reported as having a relationship to their offender that would imply he's a woman. Together these account for about 200 observations and less than 1 percent of the total sample. These cases are excluded from analysis.

given point in time (year fixed effect) as well as differences generated from the duration of the law (years since law change). Column (2) of Table 3 reports these results. The main effect of mandatory arrest laws is in the initial year of passage. This effect is slightly smaller than the previously estimated effect. When I combine the duration and the main effect the estimated effect of mandatory arrest laws varies from 1 to about 1.5 additional homicides per 100,000 people. The effect increases over the first two years after the law change and flattens after about 3 years. It is likely this corresponds to an effect from information diffusion—i.e. it takes some time for all parities to become fully aware of these mandatory arrest policies.

In order to control for other state characteristics, I included the unemployment rate and the average violent crime rate. To measure unemployment I used the average annual state unemployment rate derived from the Current Population Survey from 1980 to 1999. To measure the violent crime rate, I used the number of violent crime reports per 100,000 people from the FBI's Uniform Crime Reports. Column (3) of Table 3 reports these results. The coefficient on the effect of mandatory arrest laws on intimate partner homicides is nearly the same as in the previous specification. It is noteworthy that an increase in the unemployment rate increases the intimate partner homicide level by more than about .35 per 100,000 people, or about 15 percent. This is consistent with a body of literature that relates unemployment and economic distress to heightened levels of intimate partner abuse.[13] Based on the R-squared statistic, this seems to be the best fitting model and so I used it in subsequent analysis of subgroups.

Thus far I have given little attention to fault in these counts. This is relevant because this count included some homicides which are eventually (but not initially) classified as self-defense. While I could not identify "self-defense" killings from murders, the two subgroups may be affected in different ways. Given the gendered nature of domestic violence, a reasonable approximation to the murder count (as opposed to self-defense) is to count homicides committed against female intimate partners, especially by male intimate partners. In Table 3 column (4), I restricted my estimate to intimate partner homicides with only female victims and homicides of females by males. These effects are similar in magnitude to the previous the full set of intimate partner

[13] See for example Kryriacou et al. (1999), Tauchen and Witte (1992), or Farmer and Tiefenthaler (1997)

homicides but correspond to about a 70 percent increase. Overall, this suggests that most of the reporting effect is concentrated among women failing to report abuse by their male intimate partners. It is likely that abuse of males by their female partners is both infrequent and rarely reported thus largely unaffected by the law change. Moreover, evidence suggests that battered women who kill their husbands do so more often when their partner incapacitated (e.g. sleeping, passed out, etc.) rather than mid-fight.[14] As a result, these homicides may be less preventable through police intervention and consequently may be less responsive to arrest policy.

Columns (5) and (6) restrict analysis to intimate partner homicides with African-American victims and African-American victims and perpetrators respectively. In column (6) in particular, the effect of mandatory arrest laws is even larger than for the population on average. This is consistent with an explanation that depends on reporting. Studies have shown that many minority groups are reluctant to report crimes to the police, preferring instead to handle instances within their own communities.[15] There is no similar effect of Asians or for Native Americans (not reported).

The results from Table 3 demonstrate that mandatory arrest laws significantly increase intimate partner homicides. The same is not true for recommended arrest laws. A possible explanation for this difference is that allowing police discretion fails to change the police response and so it is comparable to having no arrest policy at all—hence the lack of significant effect. Table 3 also shows a consistent effect of unemployment on increasing intimate partner homicides. This effect appears to be smaller in the African American population, suggesting that unemployment may put more stress on white families.

II. THE DYNAMICS OF DOMESTIC VIOLENCE

The standard rational criminal model predicts that an increase in the penalty for a crime results in an unambiguous decline in the commission of that crime. Implicit in this prediction is the assumption that an increase in the penalty does not change the

[14] See for example O'Keefe (1997) This is also consistent with evidence that finds female perpetrated abuse is affected not by criminal justice options but by outside extra-legal resources (e.g. shelters) (Browne and Williams (1989).

[15] Evidence from sexual assault victims is consistent with this hypothesis see Neville and Pugh (1997). Evidence associated to disclosure of abuse to clinicians see Rodriguez, et al (2001)

probability of detection. This assumption may not be valid for intimate partner violence crimes because there is a potential for the criminal to shift the burden of the higher penalty onto his or her victim. If this occurs then arrest laws actually impose a cost on the individuals who must report the crime, and thereby reducing the probability of police detection of the abuser. Thus the effect of an increased penalty on the abuser is ambiguous.

Victims may bear the costs of an increased penalty to the abusers in several ways:[16] 1) There is a psychological and emotional component of intimate partner abuse that often generates victims who remain committed to their abuser and do not wish to send him to prison. Thus, guilt effectively transfers the cost from abusers to victims. 2) If abusers are arrested but no further legal action is taken, they may return home within a day of their arrest and further terrorize their victim. In a non-experimental evaluation of mandatory arrest as a policy, Lyon (1999) used a logistic model to compare the likelihood of arrest under mandatory arrest laws versus pro-arrest laws in two cities in Michigan. She found that once a victim calls the police to report an incident, she is significantly less likely to call again. This was likely because police intervention in the form of an arrest resulted in retribution by the abuser deterring future reporting.[17] 3) In many cases arrests laws resulted in the victim also being arrested if there was evidence that she (or he) physically assaulted her (or his) partner. In many areas, women constitute nearly 20 percent of domestic violence arrests, a far higher percentage than the estimated proportion of female abusers.[18] Over half of these female arrestees can be identified as previous victims of intimate partner violence (Martin, 1997). Anecdotal evidence from some battered women advocates suggests that these "dual arrests" are the

[16] In general, domestic violence is a heavily underreported. It is estimated that only about 1 in 7 domestic assaults are reported to police. See Rennison (2002).

[17] The Department of Justice (2000) found that fear of reprisal from abuser the most commonly cited cause for not reporting a domestic violence incident. This is hotly contested claim. Mills (1998) based on research by Sherman and Berk (1984) claims that arrests actually increase re-assaults. More recent work by Maxwell, Garner and Fagan (2002) find that there is no significant change in the risk of assault.

[18] For example, in Phoenix, AZ, 18 percent of domestic violence arrests are women (AZCASA). Women are thought to be abusers in less than 5 percent of intimate partner violence cases (Dobash, R.P., Dobash, E.E., Wilson, M. & Daly, M. 1992). Though some work suggests there is a surprisingly high rate of female on male abuse (see Strauss and Geller (1980)) however this work is problematic and for the most part ignores the severity and nature of the violence (see Blau, 1998).

most serious problem with mandatory arrest.[19] Dual arrests have serious implications for victims who are immigrants and may be deported if convicted of assault. In addition, those who have children face potential loss of custody during the arrest period. All of these costs may result in an increased unwillingness to report abuse to the police.

II. A Establishing the Relationship between Reporting and Arrest

In order to more systematically demonstrate the difference in the experimental evaluation of arrest and the policy of mandatory arrest consider a model in which an abuser and a victim interact in an infinitely repeated setting. In this model, each period has two-stages. In stage one, the abuser chooses whether to be violent (V) or non-violent (NV). If the abuser chooses non-violence, then the victim gets 1 and the abuser gets zero and the game (situation)n moves to stage 2. If the abuser chooses violence, then the victim chooses to either report (R) the crime or to not report (NR). If the victim does not report the crime, the abuser gets 1 and the victim gets 0 and the situation moves to stage 2. If the victim chooses to report the crime, then the abuser gets β and the victim gets α, where β and α incorporates the utility or disutility of the action as well as the penalties or rewards to each party from reporting, and the situation moves to stage 2.

The structure of this model and the fact that it is infinitely repeated allows for many potential equilibria.[20] In order to illustrate the effect of mandatory arrest laws on reporting, I make several simplifying assumptions.

Assumption 1: β is strictly less than 1 and finite
(A.1)

The first part of this assumption is intended to capture that batterers get some disutility from being reported (i.e. this assumption is equivalent to U(not report) > U(report)). The finiteness assumption is akin to a limited liability assumption and imposes that the government can only impose finite penalties on an individual for domestic abuse.

Assumption 2: $\alpha < 0$
(A.2)

This assumption focuses us on relationships in which victims do not always report nor do batterers always choose to be non-violent.[21] .Under A2 α can be considered as a

[19] This statement is based on conversations with individuals at battered women's coalitions in NJ, AZ, NY, CA, CT and IL.

[20] This model is analogous to the "chain store paradox" model with a sequentially infinite number of potential entrants. For a discussion of classes of equilibria see Selten (1978) or Rosenthal (1981)

“punishment” strategy in which the victim wishes to induce the batterer to choose non-violence more often and uses reporting with some probability to induce the batter to choose nonviolence with some positive probability. [22]

Assumption 3. There is some positive probability that the abuser will choose violence. (A.3)

This assumption can be thought of as an “unpredictability” assumption in which even well intentioned partners will abuse. This assumption allows us to narrow the class of dynamic equilibria to ones in which violence is chosen with positive probability, making the threat of reporting important.

Assumption 4. Single period path dependence (A.4)

This assumption is in some sense arbitrary as I could also specify an N period path dependence. However, in part because a “period” is itself arbitrary and in part because it greatly simplifies the set of equilibria strategies, I assume that the punishment strategy will depend only on choices from the previous period.

Assumption 5. The Transition Rule (A.5)

		State Properties		Strategies		
		Prob. Remain in current state	Payoffs to Current State	Non-violence	Violence, No Report	Violence, Report
Current State	Good	(x_1, y_1)	(A_g, V_g)	Good	Bad	Good
	Bad	(x_2, y_2)	(A_b, V_b)	Good	Bad	Good

efine two states: “good” in which the abuser chooses non-violence and “bad” in which the abuser chooses violence. Also, let, (x_1, y_1) represent the probability of remaining in the current state given that the players are currently in the good state and let (A_g, V_g) be the payoffs in that state . Similarly, let the probability that, once in the bad state the players will remain in the bad state be (x_2, y_2) and let the payoffs in that state be (A_b, V_b).

An intuitive explanation of A.5 is that x_1 represents the probability of violence if the victim reports this period and x_2 represents probability of violence if the victim does not report. Similarly, y_1 represents the probability of reporting given that the abuser is

[21] Without this, batterers would either always choose violence (if $\beta>0$) or always choose nonviolence (if $\beta<0$).

[22] A more intuitive argument for a negative α is that women face high expected future costs from reporting in terms of psychological trauma and the threat of heightened future abuse. From their perspective, they do not desire to report, but rather they use the threat of reporting as a way to deter violence from their partners. This is consistent with bargaining models used to describe abusive relationships. See for example Pollak (2004)

violent and y_2 represents the probability of reporting given the abuser is non-violent.[23] If the abuser chooses to remain non-violent, both players remain in the "good" state. If the abuser chooses to become violent and the victim chooses not to report, the players will enter the "bad" state. If the abuser chooses to become violent and the victim chooses to report, the players will enter the "good" state.

I solve this game for an equilibrium in which have all players mixed strategy every period. The solution for the probabilities of violence given reporting and non-reporting (x_1 and x_2 respectively) is implicitly defined by:

$$x_1 = 1 - \alpha \frac{(1-\delta)}{\delta(1-\delta x_2)} - \frac{(1-\delta)(1-x_2)}{(1-\delta x_2)} \tag{1}$$

In equation (2), δ represents the period discount rate. Note that the abuser has flexibility in how he decides to distribute punishment and reward. The slack in determining x_1 and x_2 captures the slack the abuser has in determining how to induce the victim to behave appropriately. In addition to the indeterminacy of x_1 and x_2, note that x_1 is decreasing in α (the victim's payoff), while x_2 is increasing in α, for δ small enough.[24] These results illustrate the dual effect of changes in the victim's payoffs. Although an increase in the victim's utility from reporting will decrease the probability of violence in the next period if the victim reports, it will actually increase the probability of violence in the next period if she does not report.

Similarly, solving for the victim's probability of reporting yields:

$$y_1 = \frac{1}{1-\beta} \tag{2}$$

Notice that the probability of reporting decreases as the cost of reporting to the abuser increases. This result captures the idea that as the cost to the abuser increases, the punishment (i.e. reporting) becomes more severe. Thus victims are less willing to apply this punishment and thus reporting declines. In an intuitive sense, y_1 as a function of β illustrates the ability of the abuser to transfer changes in his (or her) utility to his (or her) victim.

---

[23] I do not discuss further the parameter y_2 because I implicitly assume the probability of reporting is 0 if no abuse occurred. This assumption seems reasonable given there is little evidence of false reporting.

[24] For this solution, $\delta > ½$ yields x_2 increasing in α, while $\delta<½$ yields x_2 decreasing in α. For all of this analysis, δ is bounded away from 0 and 1, and for the problem to be well defined, $\delta \neq ½$.

II. B The Difference between the Arrest Experiment and Mandatory Arrest Laws

To see how mandatory arrest laws change interactions between abusers and victims, consider how they change the components of the above game. Most obviously, mandatory arrest laws increase the cost of choosing violence (i.e. arrest laws decrease β). This effect is largely the reason why mandatory arrest laws were originally advocated. Ideally, this increase in cost would result in an equilibrium where violence is never chosen. However, it is not possible to sustain the equilibrium where the batterer chooses non-violence with probability 1 and if he deviates, the victim chooses to report with probability 1.[25] So we will observe some level of violence. Arrest laws also change α. If α increases, then the abuser can adjust x_2 (the probability of violence if the victim does not report) to increase the probability of non-reporting and similarly if α decreases the abuser can adjust x_1 (the probability of violence if the victim reports.) Because as demonstrated in equation (1), the batters have more degrees of freedom to determine the outcomes, they are better able to shift the burden of arrest onto the victims, deterring reporting rather than deterring abuse.

Next consider the results suggesting that mandatory arrest laws increased homicides. In order to illustrate how changes in the level of homicides can be linked to reporting, consider a model where with some small probability, p, domestic abuse escalates to murder. For n intimate partner incidents, the probability of a homicide is then pn. Suppose that y_1, the probability of reporting given violence, decreases as the model above predicts. This failure to report to the police can increase the rate of intimate partner homicide in two ways. First, police presence, regardless of the police response, can disrupt a violent incident keeping the violence level below a certain threshold. Thus, failing to notify the police increases p. Second, if arrest, conditional on reporting, deters violence, then the reduction in reporting also reduces the number of arrests which reduces the deterrence effect. Thus failing to notify the police increases n. In these two ways a

[25] To see this, consider the case where the batterer deviates to choose violence in period 1 and then returns to the equilibrium strategy. The victim will receive the same stream of payments with the exception of the first period where she compares α and 0. Given the assumption that $\alpha<0$, she will choose not to report. Since this is true for every period, this pair of strategies is ruled out as an equilibrium. This is because of A.2, restricting α.

decrease in y_1 (the probability of reporting given violence) generated from an increase in β (the cost of reporting to abusers) will increase intimate partner homicides.

In contrast, the MDVE held constant the second stage of the game—that is holding y_1 fixed, the MDVE estimates the effect of a decrease in β on the probability of choosing violence. By construction then this cannot estimate the effect on y_1 of a change in β, which is particularly salient given their explicit relationship illustrated in equation 2. The experiment tested the impact of arrest in the static model which ignores the interaction between victims and abusers. Because the experiment estimates the conditional expectation of arrest, it provides no insight into the unconditional effect of arrest laws, which remain theoretically ambiguous.

III. Arrest Laws and the Reporting Effect

III.A Family Violence

In order to test the plausibility of the reporting story I considered the effect of mandatory arrest laws on homicides committed against members of the immediate family. Because mandatory arrest laws allow arrest of an abuser in a domestic situation, familial abuse is also covered by these laws. However, unlike for adults, children typically do not report their own physical abuse to police. Instead, abuse is usually detected by an outside adult (such as a teacher or a doctor).[26] In this case reporting would not be a function of the cost of reporting to the abuser.[27] Unlike in intimate partner abuse settings, the escalation of violence in child abuse situations could *increase* the probability of another adult noticing and therefore reporting the abuse. Given this situation, mandatory arrest laws should reduce the probability of severe violence to children by family members. I therefore defined "family homicides" as homicides committed against a father, mother, step-father, step-mother, son, daughter, step-son, step-daughter, brother,

---

[26] More specifically, of the nearly 2.8 million child abuse cases reported to child protective services agencies in 2000, 56.1 percent of all reports were from law enforcement, educators, medical and mental health professionals, social services personnel, child care providers and other mandated reporters. U.S. Department of Health and Human Services, Administration on Children, Youth and Families, Child Maltreatment 2000 (Washington, DC: U.S. Government Printing Office, 2002).

[27] Actually, many professionals have legal requirements to report suspected abuse which can compensate for any potential costs they might incur from reporting abusers in their community.

or sister.[28] I constructed a count of these homicides by state by year and defined a count of family homicides per 100,000 inhabitants. This was intended to capture the other set of homicides affected by mandatory arrest laws

Table 4 reports the results from a regression of family homicides per 100,000 inhabitants on an indicator for the mandatory arrest laws, an indicator for recommended arrest laws controlling for state and year fixed effects. Column (2) adds controls for unemployment, violent crime rate, and duration of law fixed effects. The results indicate the family homicides decreased by between .35 and .52 per 100,000, corresponding to a 46 to 70 percent decline.[29] Recommended arrest laws have a similar effect in the second specification. These results are consistent with the model suggesting that once the reporting effect is eliminated, arrest laws appear to function as predicted, reducing harm to the protected individuals.

III.B "Other" Non-Familial Homicides

In order to check of the difference-in-difference framework, I also tested the effect of mandatory arrest laws on uncovered homicides. I therefore defined a class of homicides called "other homicides" which include homicides committed against employees, employers, friends, other known individuals, and strangers.[30] These homicides should be unaffected by mandatory arrest laws. I used the same two specifications as above, one with only state and year fixed effects, and one with controls for unemployment rate and the non-homicide violent crime rate.[31]

The results from these regressions are reported in Table 5, columns (3) and (4). In both specifications, neither mandatory arrest laws nor recommended arrest laws have a significant effect on the homicide level of uncovered homicides. Unemployment appears to have no significant effect on other homicides. This is in part because I am also controlled for the violent crime level per 100,000 inhabitants. Indeed the violent crime

[28] For specific coverage by state law, see data appendix.
[29] This substantial decline in familial homicides has been the subject of much discussion. See Durose, et al. (2005)
[30] I have excluded homicides committed by individuals of "unknown relationship." While it is likely that these homicides were not committed by immediate family members or intimate partners, it is was not possible to estimate the subset of these homicides that would be covered and thus all are excluded.
[31] The non-homicide violent crime rate is a count of the number of robberies, assaults, and rapes reported per 100,000 inhabitants.

effect is both large and significant, as would be expected since non-familial homicides are likely related to other criminal activities.

III.C Arrest Rates

Another way to get insight into the causal mechanism of the increase in homicides is to evaluate the impact of mandatory and recommended arrest laws on arrest rates. If arrest rates decrease, mandatory arrest laws may increase violence by deterring reports from victims. In order to estimate the impact on arrest rates, I use FBI Arrest data by state from 1990 to 1999.[32] Because there is no specific intimate partner violence category, I create a measure of intimate partner violence by combining the categories of crimes against family, aggravated assault, rape and other assaults.[33] Although these categories contain assaults that are not between intimate partners, if arrests for intimate partner violence change dramatically around the time of the law passage then we should observe an increase in arrest levels for some or all of these crime types. I estimated a difference-in-difference model of the effect of mandatory arrest laws using the constructed "intimate partner violence" variables as the dependant variable.

These results are reported in Table 5. They show a marginally significant increase in arrests for intimate partner violence related crimes in states with mandatory arrest laws. They also show a large and significant increase in arrests in recommended arrest law states. Including the unemployment rate and the reporting rates for violent and non-violent crimes reduces the magnitude of the effect but the results remain significant. Breaking this down by subcategory in columns (3) through (6), there is a significant decrease in arrests for simple assaults.[34] It therefore appears that there may be a stronger reporting effect in mandatory arrest states relative to recommended arrest states. Recommended arrest laws appear to have had no significant effect.

---

[32] This removes any useful identification from states that passed laws prior to 1990. These states are CT, IA, and NV.

[33] Offenses against the family and children include nonsupport, neglect, desertion, as well as abuse of family and children. Other assaults (simple) includes all assaults and attempted assaults where no weapon is used and which do not result in serious or aggravated injury to the victim. Aggravated assault includes any unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily injury. This type of assault usually is accompanied by the use of a weapon or by means likely to produce death or great bodily harm. Simple assaults are excluded. (FBI Data Dictionary)

[34] It is estimated that a large fraction of simple assaults are incidents between intimate partners (Reiss and Roth, 1993)

The arrest results suggest that mandatory arrest laws may not have actually increased the arrest of domestic abusers. I marshal two pieces of evidence to suggest this decline in arrests is not consistent with a failure to enforce the law in mandatory arrests states. First, I construct a rough measure of the ratio of arrests to reports for assaults in mandatory arrest states.[35] I find that this ratio significantly increased in mandatory arrest states after the law change but was unchanged in recommended arrest and no-arrest states. This suggests hat conditional on reporting, arrests increased in mandatory arrest states, consistent with enforcement of the law. Second, the change in homicide levels in mandatory arrest law states relative to others suggests some enforcement is occurring as the threat of arrest is sufficiently credible to elicit a significant response. Thus it appears that police did indeed increase the fraction of a reports in which they arrested someone, but this resulted in fewer arrests overall.

IV. Conclusions

Based on evidence from the Minnesota Domestic Violence Experiment (MDVE) that arresting abusers deterred future violence, many states passed laws requiring the warrentless arrest of individuals believed to be responsible for intimate partner abuse. Using data from the FBI Supplementary Homicide Reports from 1976-1999, I find that the level of intimate partner homicide increased in states with these mandatory arrest laws. This study provides both a theoretical explanation and empirical evidence as to why the effects of the policy differ from the results of the randomized experiment. Because arrest policies impose costs that may be transferred from the abuser to the victim, abuse victims may be less likely to contact the police in the face of a mandatory arrest law. This failure to contact the police results in fewer interventions risking an increased probability of escalating violence. Indeed these results are more pronounced among African-American, which may be because this community has a greater mistrust of police intervention especially in the fact of guaranteed arrest.

To support this interpretation, I investigated the effect of these laws on different types of homicides as well as estimated the change in arrest rates. I estimated the change

[35] Results not shown are available upon request. Estimates use FBI uniform crime arrests for assaults/ FBI uniform crime reports for assaults in states with mandatory arrest law.

in familial homicides in response to mandatory arrest laws. These crimes are covered by mandatory arrest laws but reporting of abuse typically is not performed by the victim. Thus the reporting effect should be reduced or eliminated and the results should more closely resemble the results from the MDVE. A difference-in-difference analysis reveals that familial homicides declined in response to mandatory arrest laws. I next estimated the effect of mandatory arrest laws on non-familial homicides, which are not covered under mandatory arrest laws. These homicides appear unaffected by mandatory arrest laws. Lastly, I investigated the change in arrest rates for intimate partner related crimes. It appears that arrests for intimate partner related crime declined in mandatory arrest states and increased in states with in recommended arrest laws . This suggests that reporting is actually reduced in mandatory arrest law states, nullifying the effectiveness of mandating arrests.

The analysis in this study leaves open several issues. First, while intimate partner homicides may have increased, it is not certain that this corresponds to increased levels of intimate partner abuse. If the intimate partner homicides and intimate partner abuse are negatively correlated, then arrest laws may decrease abuse while increasing homicides. The affect of mandatory arrest laws on less severe abuse therefore remains an open question. Second, the reasons mandatory arrest laws fail is also uncertain. If abusers penalize victims with harsher abuse after arrests, then arrests are an insufficient response to domestic violence. In this scenario, stronger sentences and aggressive prosecution policies, which will incapacitate abusers, are necessary to ensure the safety of victims. On the other hand, if mandatory arrest laws fail because of the psychological component of abuse that is based on the emotional bonds between the abuser and the victim that makes victims unwilling to inflict harsh penalties on their abusers then an alternative approach which does not depend on victims reporting is needed. If the problem is a misapplication of the law (for example, through dual arrests) then preceding the enforcement of arrest laws, comprehensive police training is required. Finally, it is well known in the sociological and psychological literature that arrests are not sufficient to induce victims to leave their abusers.[36] If the objective of arrest laws is to promote a decline in the level and prevalence of intimate partner violence then policy efforts

[36] See Mills (1998) for a comprehensive discussion of the problem of arrest policy

focused on providing victims the opportunities and resources to leave abusive situations are also required.

The irony that a mandatory arrest law intended to deter abuse actually increases domestic homicides is not lost on this author. The results from this study add to a growing literature on the unintended consequences of government policies intended to protect disadvantaged individuals. Indeed, if the entity the government wishes to penalize can adjust its behavior to deflect the cost of regulation it may shift these costs to more vulnerable members of society, and in particular onto the very class of people the government seeks to protect. That has been the case for several forms of labor regulations, including the Americans with Disabilities Act and restrictions on employment at will. These regulations intended to help workers and restrict firms appear to have worsened situations for the very workers they sought to help. [37] In this case, mandatory arrest laws appear to have worsened the situation for the victims it was intended to protect. To the extent that decisionmakers fail to appreciate the limited insights experiments can provide on the efficacy of the policies, experiments may inadvertently provide support for counterproductive policies.

Given the dangerous and pervasive nature of domestic violence, there is little doubt that state intervention, in some form, is required. Determining what shape that intervention takes is of vital importance. The results from this study suggest that the threat of arrest is insufficient to deter abusers from killing their victims. Finding that abusers are not deterred by their arrests but victims are provides valuable insight into the intricacies facing governmental attempts to decrease intimate partner violence. While it appears that mandatory arrest laws are not sufficient to deter abuse, the set of policies that can effectively prevent abuse and protect victims remains an issue for future research

[37] For example there is evidence of perverse or unintended consequences of labor market protections. See for example, Acemoglu and Angrist (2001), Oyer and Schaefer (2000, 2002), Autor (2003)

REFERENCES

American Bar Association (1973). Standards for the Urban Police Functions, Project on Standards for Criminal Justice, 106-108.

American Psychological Association, "Violence and the Family: Report of the American Psychological Association", Presidential Task Force on Violence and the Family (1996), p. 10.

Bertrand, M., Duflo, E. and Mullainathan, S. (2004) "How Much Should We Trust Differences-in-Differences Estimates?", *Quarterly Journal of Economics*

Francine D. Blau (1998). "Trends in the Well-Being of American Women, 1970-1995" *Journal of Economic Literature*, Vol. 36, No. 1. (Mar., 1998), pp. 112-165.

Browne, Angela and Kirk Williams (1989). "Exploring the Effect of Resource Availability and the Likelihood of Female-Perpetrated Homicides" *Law and Society Review* Vol. 23 (1) 75-94

Dobash, R.P., Dobash, E.E., Wilson, M. & Daly, M. 1992 "The myth of sexual symmetry in marital violence", Social Problems, 391

Durose, M, Harlow, C, Langan, P, Motivans, M Rantala, R and E. Smith (2005) " Family Violence Statistics: Including Statistics on Strangers and Acquaintances" Bureau of Justice Statistics (NCJ 207846)

Farmer, Amy and Jill Tienfenthaler(1997). "An Economic Analysis of Domestic Violence," Review of Social Economy, Vol. 55, No. 3, Fall, pp. 337-358

FBI Uniform Crime Reports, Supplementary Homicides Reports 1976-1999

Greenfeld, L., Rand, M., Craven, D., Klaus, P., Perkins, C., Ringel, C., Warchol, G., Maston, C., & Fox, J. (1998). "Violence by intimates - Analysis of data on crimes by current or former spouses, boyfriends, and girlfriends." *Bureau of Justice Statistics Factbook* (#NCJ167237).

Gwinn, Casey and Anne O'Dell (1993). "Stopping the Violence: The Role of the Police Officer and the Prosecutor", Western State Law Review, 1501-1521

Kirsch (2001) "Problems in Domestic Violence: Should Victims be Forced to Participate in the Prosecution of their Abusers", *William and Mary Journal of Women and Law*

Kryriacou, DN, Anglin, D, Taliaferro, E, Stone, S, Tubb, Toni, Linden, J, Muelleman, R, Barton, E, and J. Kraus (1999). "Risk Factors for Injury to Women from Domestic Violence" *The New England Journal of Medicine*, Vol. 341 (25) 1892-1898

Lalonde, Robert (1986) "Evaluating the Econometric Evaluations of Training Programs with Experimental Data" *American Economic Review* Vol. 76 (4) 604-20

Lyon, Andrea D. (1999). "Be Careful What You Wish For: An Examination of Arrest and Prosecution Patterns of Domestic Violence Cases in Two Cities in Michigan", *Michigan Journal of Gender and Law*, pp. 272-297

Martin, Margaret (1997). Double Your Trouble: Dual Arrest in Family Violence", *Journal of Family Violence* Volume 12, Number 2

Maxwell, Garner & Fagan "The Effects of Arrest on Intimate Partner Violence: New Evidence
From the Spouse Assault Replication Program" *Criminology & Public Policy* Vol. 2, No. 1

Mercy, JA and LE Saltzman (1989) "Fatal Violence Among Spouses in the United States" *American Journal of Public Health* Vol. 79(5) 595-9

Meyers, Bruce (1992). "Natural and Quasi-Experimental in Economics" *Journal of Business and Economic Statistics* Vol. 13(2)

Mills, L. G. (1998). Mandatory arrest and prosecution policies for domestic violence: A critical
literature review and the case for more research to test victim empowerment approaches. *Criminal Justice and Behavior,* 25, 306-318.

Neville, HA and AO Pugh (1997). "General and Culture-Specific Factors Influencing African American women's reporting patterns and perceived social support following sexual assault. An exploratory investigation" *Violence Against Women* Vol. 3 (4) 361-381

O'Keefe, Maura (1997) "Incarcerated Battered Women: A Comparison of Battered Women who Killed Their Abusers and Those Incarcerated for Other Offenses" *Journal of Family Violence* Vol. 12 (1) 1-19

Pollak, Robert A. (2004). "An Intergenerational Model of Domestic Violence", *Journal of Population Economics*

Rennison, Callie (2002). Rape and Sexual Assault: Reporting to Police and Medical Attention 1992 – 2000 Department of Justice, Bureau of Justice Statistics

Rosenthal, RW (1981) "Games of Perfect Information, Predatory Pricing, and the Chain Store Paradox" *Journal of Economic Theory* Vol. 25 (1) 92-100

Sherman, Lawrence (1992) *Policing domestic violence* New York: Free Press, 1992

Sherman, Lawrence W., and Richard A. Berk, (1984) "The Specific Deterrent Effects of Arrest for Domestic Assault," *American Sociological Review* 49 (1) 261-72.

Selten, R. (1978) "The Chain Store Paradox" *Theory and Decision* Vol. 9(2) 127-159

Stark, E. (1993), "Mandatory Arrest of Batterers", American Behavioral Scientist, Vol. 36, No 5

Strauss and Gelles (1980) *Behind Closed Doors: A Survey of Family Violence in America* New York: Double Day

Symposium on Domestic Violence (1992), 83 U. Crim. L. & Criminology. Spring

Tauchen, Helen and Ann Dryden Witte (1995). "The Dynamics of Domestic Violence", American Economic Review, Vol. 85, No. 2, May, pp. 414-418

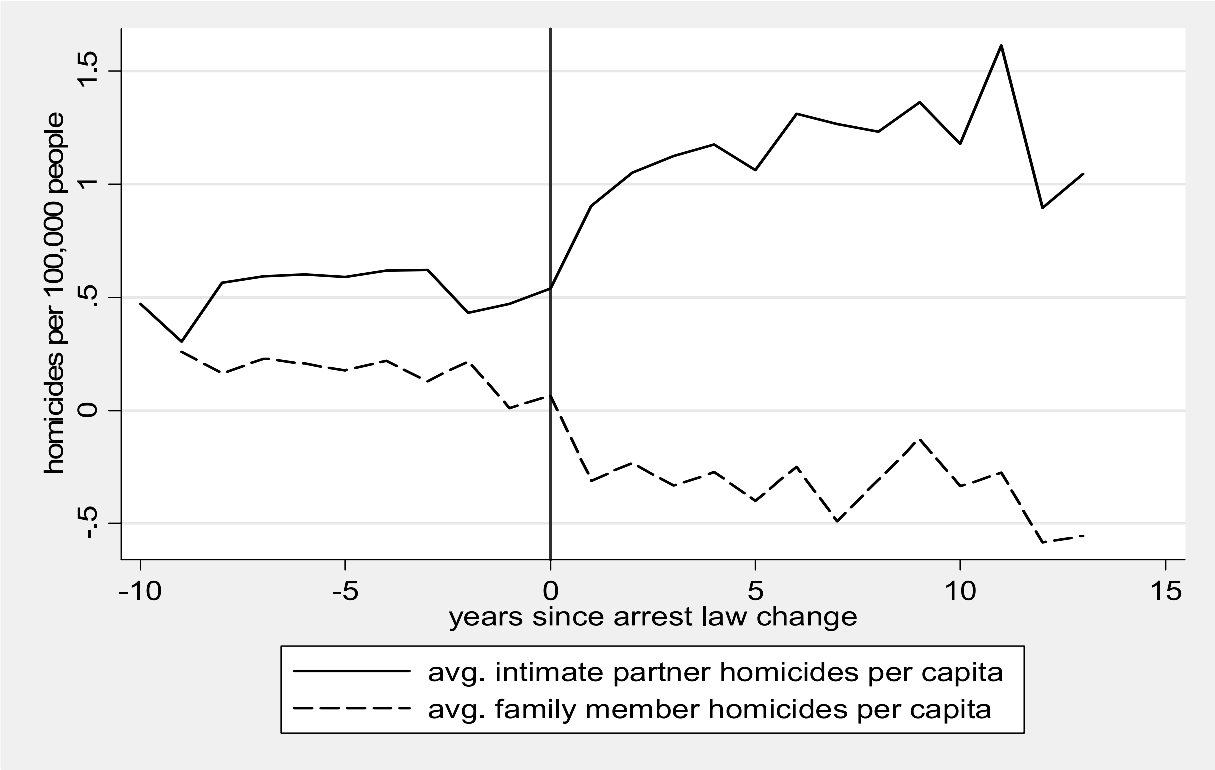
Thurman v. City of Torrington, 595 F. Supp. 1521 (D. Conn. 1984)

Wanless, Marion (1996): Mandatory Arrest: A Step Toward Eradicating Domestic Violence, But is it Enough", 1996 *U. Ill. L. Rev*. 533

West (2003). Westlaw Campus accessed via Princeton University license

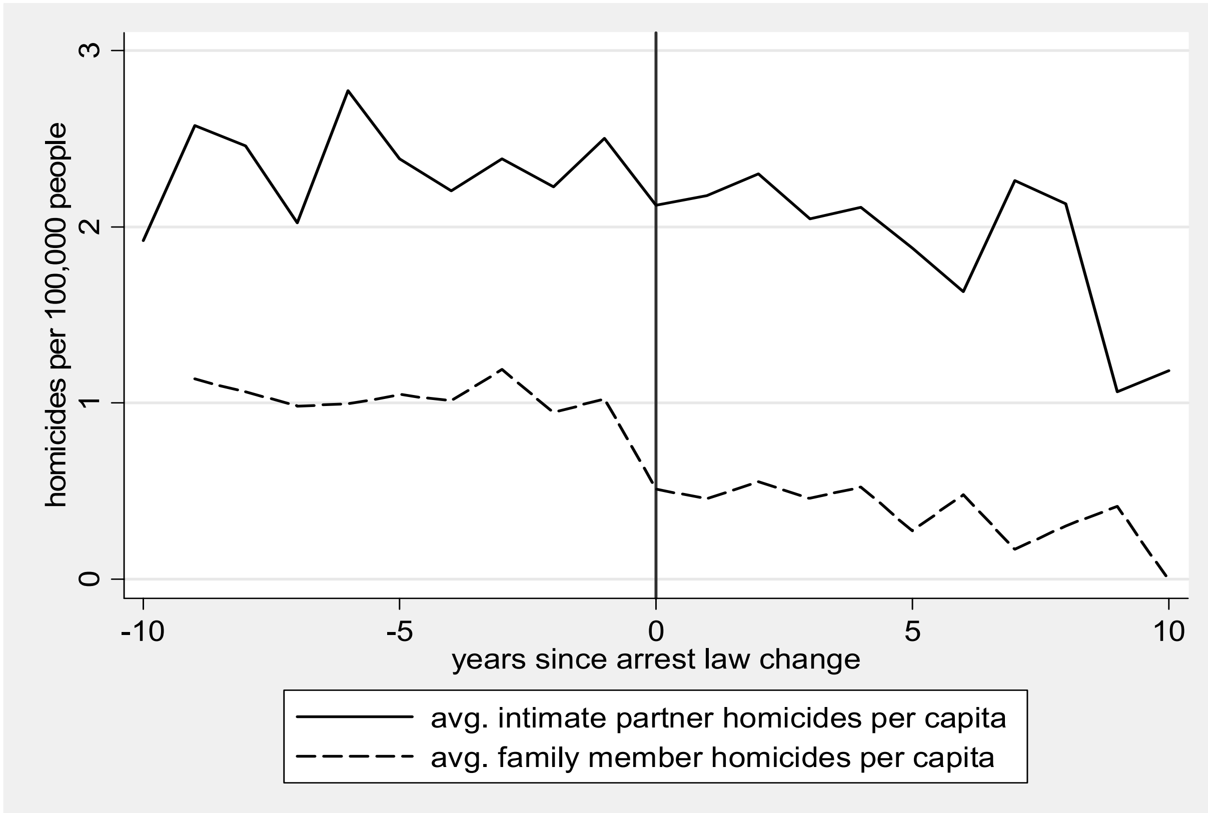
Zorza, Joan (1994). "Must We Stop Arresting Batterers?: Analysis and Policy Implications of New Police Domestic Violence Studies, 28 *New Eng. L. Rev*. 929

Figure 1. Intimate Partner and Familial Homicide Rates in Mandatory Arrest Law States



Notes: Means based on author's own calculations using Supplementary Homicide Reports 1980-1999. Intimate partner homicides include homicides of husbands, wives, ex-husbands, ex-wives, common-law husbands and common-law wives. Mandatory arrest states are defined as states where officers have no discretion as to whether to make a warrentless arrest when an intimate partner offense is reported.

Figure 2. Intimate Partner and Familial Homicide Rates in Mandatory Arrest Law States



Notes: Means based on author's own calculations using Supplementary Homicide Reports 1980-1999. Intimate partner homicides include homicides of husbands, wives, ex-husbands, ex-wives, common-law husbands and common-law wives. Mandatory Recommended arrest states are defined as states where officers are instructed but not required to make a warrentless arrest when an intimate partner offense is reported.

Table 1. Mandatory Arrest Laws by State

	State	*Year Passed*	*Code/Statute*
Recommended Arrest States	AZ	1991	Ariz. Rev. Stat. Ann. §13-3601(B)
	CA	1993	Cal. Penal Code §836(c)(1)
	MS	1995	Miss. Code Ann. §99-3-7(3)(a)
	MO	1989	Mo. Ann. Stat. §455.085(1)
	NY	1994	N.Y. Crim. Proc. Law §140.10(4)
	OH	1994	Ohio Rev. Code Ann. §2935.032(A)(1)(a)
Mandatory Arrest States	AK	1996	Alaska Stat. §18.65.530(a)
	CO	1994	Colo. Rev. Stat. Ann. §18-6-803.6(1)
	CT	1987	Conn. Gen. Stat. §46b- 38b(a)
	DC	1991	D.C. Code Ann. §16-1031(a)
	IA	1990	Iowa Code §236.12(3)
	ME	1995	Me. Rev. Stat. Ann. tit. 19-A, §4012(6)(D)
	NV	1989	Nev. Rev. Stat. Ann. §171.137(1)
	NJ	1991	N.J. Stat. Ann. §2C:25-21(a)
	SD	1998	S.D. Codified Laws §23A-3-2.1
	WI	1996	Wis. Stat. Ann. §968.075(2)(a)
	WA	1999	Wash. Rev. Code Ann. §10.31.100(2)

Source: West, 2003. Mandatory arrest states are defined as states where officers have no discretion as to whether to make a warrentless arrest when an intimate partner offense is reported. Recommended arrest states are defined as states where officers are instructed but not required to make a warrentless arrest when an intimate partner offense is reported.

Table 2. Definition and Summary Statistics for Homicide Categories

		Intimate Partner Homicide	Familial Homicide	"Other" Homicide	Excluded Groups
N		34,462	25,603	223,865	169,035
Total Percent of Sample		7.6	5.67	49.4	37.33
Fraction of Category Homicides committed Against	Husband	0.28	--	--	--
	Wife	0.52	--	--	--
	Common-law Husband	0.07	--	--	--
	Common-law Wife	0.07	--	--	--
	Ex-husband	0.02	--	--	--
	Ex-wife	0.04	--	--	--
		--		--	--
	Mother	--	0.11	--	--
	Father	--	0.13	--	--
	Son	--	0.26	--	--
	Daughter	--	0.19	--	--
	Brother	--	0.17	--	--
	Sister	--	0.04	--	--
	Stepfather	--	0.05	--	--
	Stepmother	--	0.01	--	--
	Stepson	--	0.04	--	--
	Stepdaughter	--	0.02	--	--
	In-law	--	--	0.02	--
	Neighbor	--	--	0.03	--
	Acquaintance	--	--	0.48	--
	Employee	--	--	0.01	--
	Employer	--	--	0.00	--
	Friend	--	--	0.08	--
	Other known	--	--	0.08	--
	Stranger	--	--	0.28	--
	Other family	--	--	--	0.01
	Homosexual relation	--	--	--	0.01
	Boyfriend	--	--	--	0.02
	Girlfriend	--	--	--	0.06
	Unknown relationship	--	--	--	0.90

Notes: Fractions based on FBI Supplementary Homicide Reports, 1980-1999. Numbers in sub-categories may not sum to one due to rounding errors.

Table 3: Difference-in-Difference Estimates of Mandatory and Recommended Arrest Laws

	(1)	(2)	(3)	(4)	(5)	(6)
	All Intimate Partner Homicides per 100,000 inhabitants			Homicides of female by males intimate partners	Intimate partner homicides with African-American victims	Intimate partner homicides with African-American victims and perpetrators
Dependant Variable Mean	2.26			1.37	0.99	0.96
Mandatory Arrest Law Effect	1.4139***	1.0111**	1.0340**	0.9906***	0.9830***	1.1450***
(=1 in MA law states after law change)	(0.4672)	(0.3930)	(0.4425)	(0.2718)	(0.2461)	(0.2453)
Recommended Arrest Law Effect	0.2520	-0.1058	-0.0606	-0.1047	0.0049	0.0276
(=1 in RA law states after law change)	(0.6226)	(0.4959)	(0.4716)	(0.2695)	(0.2982)	(0.2980)
1 year after law change		0.1381	0.0095	-0.1067	-0.0391	-0.0426
(=1 in arrest law states 1 year after law change)		(0.2879)	(0.2681)	(0.1681)	(0.0854)	(0.0839)
2 year after law change		0.3676	0.2549*	0.0420**	0.0850**	0.0776**
(=1 in arrest law states 2 years after law change)		(0.3126)	(0.1400)	(0.0169)	(0.0346)	(0.0345)
1 year after law change		0.5526	0.4461**	0.0746*	0.1690**	0.1513***
(=1 in arrest law states 3 years after law change)		(0.3719)	(0.1821)	(0.0407)	(0.0541)	(0.0531)
unemployment rate			0.3465*	0.1988	0.1556*	0.1519*
			(0.2066)	(0.1277)	(0.0836)	(0.0812)
Violent Crime Rate			-0.0003	-0.0002	-0.0001	-0.0002
(per 100,000)			(0.0009)	(0.0006)	(0.0004)	(0.0004)
Years since Law Change Fixed Effects	N	N	Y	Y	Y	Y
State Fixed Effects	Y	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y	Y
R-squared	0.7061	0.7063	0.7134	0.7135	0.6781	0.6745

Notes: All regressions include 992 observations. The dependant variable for each column is the column title per 100,000 inhabitants. Robust standard errors, clustered by state, are reported in parentheses. Coefficients that are significant at the .05 (.01, .1) percent level are marked with ** (***, *). Intimate partner homicides include homicides of husbands, wives, ex-husbands, ex-wives, common-law husbands and common-law wives.

Table 4. Difference-in-Difference Estimates of Familial and non-familial homicide Rates

	(1)	(2)	(3)	(4)
	Family homicides per 100,000 inhabitants		"Other Homicides" per 100,000 inhabitants	
Dependant Variable Mean				
Mandatory Arrest Law Effect	-0.3541** (0.1610)	-0.5230*** (0.1214)	1.6615 (1.9453)	-0.8855 (2.1812)
Recommended Arrest Law Effect	-0.3477 (0.2308)	-0.4652*** (0.1523)	-1.8728 (3.3064)	-3.3374 (2.6653)
unemployment rate		0.1262* (0.0716)		1.4644 (1.0106)
Violent Crime Rate (per 100,000)		-0.0001 (0.0003)		0.7126** (0.0154)
Years since Law Change Fixed Effects	N	Y	N	Y
State Fixed Effect	Y	Y	Y	Y
Year Fixed Effect	Y	Y	Y	Y
R-Squared	0.6891	0.6990	0.7098	0.7171

Notes: All regressions include 992 observations. The dependant variable for each column is the column title per 100,000 inhabitants. Robust standard errors, clustered by state, are reported in parentheses. Coefficients that are significant at the .05 (.01, .1) percent level are marked with ** (***, *). Family homicides include homicides of fathers, mothers, step-fathers, step-mothers, sons, daughters, step-sons, step-daughters, brothers and sisters. "Other homicides" include homicides committed against employees, employers, other (non-immediate) family, friends, other known individuals, and strangers. Mandatory Recommended arrest states are defined as states where officers are instructed but not required to make a warrentless arrest when an intimate partner offense is reported.

Table 5: Linear Estimates of Arrest Rates for Intimate Partner Violence Related Crimes, per 100,000 inhabitants

	(1)	(2)	(3)	(4)	(5)	(6)
	"Intimate partner violence" related crime arrest rates		*Crimes against Family*	*Simple assault*	*Aggravated assault*	*Rape*
Dependant Variable Mean	630.6		49.57	417.10	151.93	11.99
Mandatory Arrest Law Effect	-152.4607***	-130.2180**	-3.2718	-81.3409**	-47.5841***	1.9789
	(51.2462)	(53.4481)	(12.8070)	(34.1316)	(15.7344)	(1.5203)
Recommended Arrest Law Effect	52.2542	100.3018	35.5663	33.8386	28.1486	2.7482**
	(44.0653)	(68.0719)	(22.2864)	(36.2109)	(19.9058)	(1.3953)
unemployment rate		5.9783	-3.0967	4.9684	4.1617	-0.0551
		(12.8753)	(2.8161)	(9.7813)	(3.0259)	(0.3063)
Violent Crime Reports (per 100,000 people)		0.1021	-0.0718	0.1162	0.0506	0.0070
		(0.2256)	(0.0568)	(0.1503)	(0.0627)	(0.0046)
Property Crime Reports (per 100,000 people)		0.0345	0.0007	0.0299	0.0032	0.0007
		(0.0388)	(0.0055)	(0.0291)	(0.0101)	(0.0007)
Duration of Law fixed Effects	N	Y	Y	Y	Y	Y
State Fixed Effects	Y	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y	Y
R-squared	0.7565	0.7622	0.7891	0.7972	0.8265	0.7357

Notes: The dependant variable in columns 1-3 is the number of arrests for crimes included intimate partner violence index per 100,000 people . The intimate partner crime index includes crimes against family, simple assault, aggravated assault, and rape. The dependant variable in columns 4-6 is the number of arrests for robbery per 100,000 people . All regressions control for state and year fixed effects. Robust standard errors, clustered by state, are reported in parentheses. Coefficients that are significant at the .05 (.01, .1) are marked with ** (***, *).

Data Appendix

A.1 Coverage of Mandatory and Recommended Arrest laws by Relationship of Victim to Offender

State	Statute	Intimate Partner Violence						Family Violence					Ambiguous Classification		
		husband	wife	common -law husband	common -law wife	Ex-husband	Ex-wife	Parent	Step-parent	Child	Siblin g	Other Family	Boy-friend	Girl-friend	Homo-sexual Relation
AK†	§18.65.530(a)	X	X	X	X	X	X	X	X	X	X		X	X	X
AZ	§13-3601(B)	X	X			X	X	X	X	X	X	X			
CA	§836(c)(1)	X	X	X	X	X	X	X	X	X	X		X	X	X
CO†	. §18-6-803.6(1)	X	X	X	X	X	X								
CT‡	§46b- 38b(a)	X	X			X	X	X	X	X	X				
DC‡	. §16-1031(a)	X	X	X	X			X	X	X	X				
IA	§236.12(3)	X	X	X	X	X	X	X	X	X	X	X	X	X	X
ME‡	Ann. tit. 19-A, §4012(6)(D)	X	X	X	X	X	X	X	X	X	X				
MO[a]	§455.085(1)	X	X	X	X	X	X	X	X	X	X				
MS†	§99-3-7(3)(a)	X	X	X	X	X	X	X	X	X	X		X	X	X
NJ	§2C:25-21(a)	X	X	X	X	X	X	X	X	X	X		X	X	X
NV	§171.137(1)	X	X	X	X	X	X	X	X	X	X	X	X	X	X
NY	§140.10(4)	X	X			X	X	X	X	X	X	X			
OH‡b	§2935.032(A)(1)(a)	X	X	X	X	X	X	X	X	X	X				
SD	§23A-3-2.1	X	X	X	X	X	X		X	X	X				
WA	§10.31.100(2)	X	X	X	X	X	X	X	X	X	X	X			
WI	§968.075(2)(a)	X	X	X	X	X	X								

Notes: Coverage based on legal definitions in statutes at time of initial passage (some alterations have been made). Statutory text retrieved from Westlaw (West, 2005). Relationship of victims to offenders based on FBI Uniform Crime Reports, Supplementary Homicide categories. Matching of legal terms to FBI coverage based on author's own interpretations. Other family is assumed to be blood relatives of an level of relation.

† Signifies states in which degree of blood relation is specified. In these states, I assume other family members are not covered although some family members may be covered by arrest laws.

‡ Signifies states in which residential family is included. I exclude "other family" because residency is not observable

a Missouri law requires arrest if police are called more than once in any 24 hour period

b Ohio specifies officers *must* separate the victim and abuser if they fail to arrest the abuser. Separate statements must be taken from each individual as well as the officer who declined the arrest.

Chapter Three

An Analysis of the Performance of Federal Indigent Defense Counsel*

1. INTRODUCTION

The Sixth Amendment guarantees that "in all criminal prosecutions, the accused shall…have the assistance of counsel for his [sic] defense." The Warren court, through *Gideon v. Wainwright*, implemented this right by requiring the state to provide lawyers to criminal defendants who face imprisonment. In 1964, the passage of the Criminal Justice Act (CJA) heralded the establishment of a federal indigent defense system intended to ensure that everyone, regardless of wealth, had representation to ensure a fair trial. The federal indigent defense system relies on both salaried government workers (public defenders) and hourly-wage earning court-appointed private attorneys (CJA panel attorneys). Over fifty years after the passage of the CJA, there is still a great deal of variation in the quality of services that is provided to the poor potentially related to this appointment of private attorneys.[1] Given that federal funds support both types of attorneys, the variation in performance raises questions of whether the current system meets its legal obligations of fairness as well as whether it is an efficient way to use funds to provide effective counsel.

* I am extraordinarily grateful to Henry Farber for continued advice and support. I would like to thank Josh Palazola for research assistance and Manda Wilson for programming assistance. I would also like to thank Mark Motivans at the Bureau of Justice Statistics and George Drakulich at the Administrative Office of the US Courts for generous assistance with data. Orley Ashenfelter, Jeffrey Kling, Jesse Rothstein, Cecilia Rouse, Courtney Stoddard, James Wilson, and participants at the Industrial Relation Section labor lunch provided numerous insightful suggestions. The opinions and conclusions are solely those of the author. Any remaining errors are entirely my own.

[1] For detailed criticisms of the current system see American Bar Association (2004), Butcher and Moore (2000). For detailed analysis regarding the appointment of private counsel in different districts see Wool, Howell, Yedid (2003)

This study analyzes the performance of attorneys in the federal indigent defense system. After identifying districts in which randomizations appears to be effective, I estimate the difference in probability of guilt and sentence length between CJA panel attorneys and Federal Public Defenders. Defendants with CJA panel attorneys are more likely to be found guilty and on average to receive longer sentences. Overall, the expected sentence for defendants with CJA panel attorneys is nearly 8 months longer. Decomposing these differences suggests they are largely due to differences in attorney performance when negotiating a plea and the selection of which cases to plead rather than to take to trial.

To explore what generates these differences, I compare the characteristics of the two groups of attorneys. CJA panel attorneys, on average, have less experience and attended lower "quality" law schools. This difference in experience and law school quality, combined with the effect of wages and caseload explain over half of the overall difference in expected sentence. Procedurally, the difference in outcomes appears to operate through plea bargaining as higher experience levels and wage rates encourages higher plea rates and lower negotiated sentences. The lower plea rates by CJA panel attorneys overwhelm any cost-saving generated by paying them lower wages. My estimates suggest that using CJA attorneys imposes a cost of approximately $61 million per year due to higher court costs.

The difference in outcomes between CJA panel attorneys and public defenders is especially troubling because the frequency of the use of CJA attorneys is correlated with the race of the defendants. Specifically, districts with a higher fraction of caseload assigned to CJA panel attorneys are also districts with more minority defendants.

Additionally, in non-randomizing districts, blacks are more likely to be assigned a CJA panel attorney than are whites. As a result, poor representation in the federal indigent defense system disproportionately impacts minorities. Because there does not appear to be an invidious purpose behind the creation of the current indigent defense system, the systematic provision of poor quality counsel likely does not violate any constitutional rights.[2] However, because disparate impact can be considered discrimination, the differences in outcomes may violate the Civil Rights Act.[3] Thus, the seemingly neutral system intended to provide counsel to financially needy defendants results in *de facto* discrimination against minority defendants.

2. Institutional Details and Data Description

2.1 The Federal Indigent Defense System

In order to qualify for representation in the federal indigent defense system, an individual must be charged with an imprisonable federal offense. In most cases these offenses are felonies or Class A misdemeanors. If an individual is arrested for a federal offense, that charge may be pursued through five stages:

1. Issuance of a charging document: Involves the formal filing of charges on which the defendant will be tried in a court of law. The defendant is not party to this proceeding. At least one of these offenses must be sufficiently serious to invoke an individual's right to federally funded counsel.

[2] This standard of discrimination is based on Personnel Administrator Of Massachusetts v. Feeney 442 U.S. 256 (1979) and Washington v. Davis, 426 US 229 (1976).
[3] Title VI of the Civil Rights Act of 1964 42 U.S.C § 2000d expressly prohibits discrimination in any program which uses federal money.

2. Arraignment: Formally informs the defendant of the charges upon which he or she will be tried and assigns counsel. If the defendant can establish that he or she is financially unable to provide counsel, one of the two types of indigent defense counsels will be appointed.

3. Detention Hearing: Determines bail and the nature of any pre-trial detention imposed on the defendant

4. Guilt determination: Establishes whether the defendant is guilty, beyond a reasonable doubt, of at least one of the charges for which he or she is charge. It either involves a negotiated agreement in which the defendant pleads guilty to a charge in exchanged for a sentence recommendation or a trial in which evidence is presented to a judge or jury, who then determine the defendant's guilt.

5. Sentencing Hearing: Only occurs if the defendant is convicted of a crime. In such a case, this hearing imposes a sentence on the defendant.

If the defendant can establish that he or she cannot afford the necessities of life for him/herself and any dependents in addition to the cost of counsel, then the counsel is appointed. In the federal system, this results in one of two types of attorneys representing the defendant.

- Federal Public Defender: These are salaried federal workers who represent indigent defendants as their full-time job.
- Criminal Justice Act (CJA) Panel Attorney: These are private attorneys who are selected to be on a panel of qualified individuals and contacted by

the federal government on a case-by-case basis.[4] While the criteria required to apply to be considered vary by district they typically involve a minimum number of years of experience and good standing in the state bar association.

These two types of counsel split the indigent caseload for the district in a predetermined ratio. Appointment of cases to one of the two types of attorneys is done either through the court clerk's office or through the federal public defenders' office. In either system, cases are randomly assigned to either the panel or the public defender pool and then a specific attorney is also randomly assigned. Attorney assignment typically occurs in a rotational manner to ensure equitable distribution of cases. Except in very rare cases, it is not possible to request a specific attorney.[5]

2.2 Court Case Dataset

Measuring attorney performance requires data on all of the cases filed in various districts with identifying details about type of attorney used, type of case, and information about the defendant. Using data from the Administrative Office of the U.S. Courts (AOUSC) Criminal Docket, I was able to observe the type of crime committed at the initial filing, the type of attorney assigned at the initial filing, as well as the disposition of the case for all criminal cases from 1997-2002. The AOUSC court data does not report defendant characteristics, such as age, race, marital status, or citizenship. To track defendant characteristics, the Bureau of Justice Statistics created a special linkage that set up a non-identifying case and defendant code which matched individuals

[4] These individuals are typically judges and defense attorneys
[5] For a more detailed description about criminal procedures, eligibility, or attorney assignment see Appendix A.

from arrest records maintained by the Federal Bureau of Investigation (FBI), US Marshall's Service (USMS), and Drug Enforcement Agency (DEA).[6] These data also track the defendant through the process, so it is possible to verify attorney assignment and charging offenses at different stages.

2.3 Defendant Characteristics and Usage of Indigent Defense Counsel

Table 1 shows the distribution of offenses and characteristics of defendants represented by different types of attorneys for all 96 federal districts from 1997-2001.[7] This table includes two types of non-indigent counsel, privately retained attorneys and pro se counsel (where individuals defend themselves). It appears that indigent defense cases account for a majority of federal criminal cases. There also appear to be differences in the demographic characteristics of defendants by type of counsel.

Defendants who retain private attorneys are much less likely to be minorities, are more likely to be married and are slightly older. In general, these characteristics are also correlated with the distribution of types of crimes covered by private attorneys. Private attorneys tend to represent individuals charged with public order offenses, which are largely white collar and federal financial crimes. In contrast, individuals charged with drug crimes are much more likely to be represented by indigent defense counsel. In part these differences may be correlated to the differences in the distribution of race by attorney type.

---

[6] The data used in this paper is a subset of the Cases Terminated files, maintained by the AOUSC. However, not all cases could be matched to defendant records. As such, for the time period, this data constitutes between 90-95 percent of the cases in any given year.

[7] Crosswalk showing the classification of filing offenses into BJS classified subcategories and main categories available upon request. Tables 1 uses the main category classification. Later analysis is done using subcategories.

3. RESULTS

3.1 VERIFICATION OF RANDOM ASSIGNMENT

In order to evaluate attorney performance, I focus my analysis on indigent defense counsel and restrict the analysis to districts that appear to randomly assign cases. In order to do this, I exclude districts-years in which 85 percent or more of cases are covered by CJA panel attorneys as these districts are not required to randomly assign (this excludes 18 districts and 8 percent of cases).[8] In addition, I restrict my analysis to district-years with a sufficiently large number of cases per year, which I set at 30.[9] This criterion does not reduce the dataset substantially. While 11 percent of the district-years have too few cases, when these district-years are excluded only about 3 percent of the sample of cases are excluded. This leaves 338 district-years for analysis. Since the probability with which a defendant receives a type of counsel is dependent on his or her defendant number within the case, I limit my analysis to the "first" defendant.[10] If defendants are randomly assigned a number in their given case, as most courts claim they are, and then selecting

[8] Districts with no cases covered by public defenders are: Eastern District of Wisconsin , Southern District of Georgia, Northern District of Alabama , Eastern District of Kentucky, Maine, Northern District of Mississippi, Southern District of Mississippi , Western District of North Carolina, North Dakota, Western District of Virginia, Northern District of West Virginia,. Districts with very few cases covered by public defenders are: Western District of Wisconsin (.92), Rhode Island (.99), Vermont (.88), Eastern District of Virginia (.97), Middle District of Georgia (.99), Northern District of Indiana (.94), Northern Marina Islands (.98), and South Dakota (.89).

[9] While this decision is arbitrary, evidence from the consistency literature suggests 30 is the minimum size needed for asymptotic properties to apply.

[10] For cases with multiple defendants the process is more complicated. In a case with multiple defendants, the defendants are randomly assigned an order. Then defendant 1 is assigned either a public defender or a CJA panel attorney as described above. If defendant 1 is assigned a public defender, defendants 2 through n are assigned different CJA panel attorneys. If defendant 1 is assigned a CJA panel attorney, then defendant 2 is assigned either a public defender or a CJA panel attorney. If defendant 2 is assigned a public defender, defendants 3 through n are assigned CJA panel attorneys. If defendant 2 is assigned a CJA panel attorney, the process moves to defendant 3. In this case, although defendant 2 may be assigned either type of attorney, the probability that he or she would be assigned a CJA panel attorney is going to be higher than if he or she was the first defendant.

the first defendant should not create a bias. However, if the process by which defendants are assigned a position within the case is non-random, this restriction may bias the sample in the direction of the failure of randomization. Approximately 11 percent of cases have 2 or more defendants.

In order to assess whether randomization of case assignment occurred, I tested how well a set of observable characteristics, race, age, and sex of defendant, predict the type of attorney assigned. If randomization of assignment was truly achieved, then defendant characteristics and crime type should not influence the type of attorney a defendant is assigned. To formally test this, I estimate a probit of the probability of being assigned a *CJA* panel attorney on defendant characteristics and type of crime for each district-year. [11] . Each of these categories of variables, race, sex, marital status, citizenship, offense category, and age are represented as a vector of indicator variables. Thus, the regression includes a full set of dummy variables for race (*black*, *Native American*, *Asian*), sex (*female*), marital status (*divorced, widowed, separated*), U.S. citizenship (*citizen*), offense category (60 BJS offense sub-categories), as well as a continuous variable for defendant age. Under the null hypothesis of randomization, I would expect that the vector of variables for defendant race, defendant sex, defendant marital status, age, defendant citizenship and offense category for the defendant's crime should each be insignificantly different that zero. In addition, these vector-variables should be jointly insignificant. I therefore define the failure to randomly assign for a district-year as having either:

---

[11] This procedure is equivalent to fitting a discriminant function but instead of determining the best predictor I verify that the observable are poor predictors.

- The set of variables comprising one of the vector variables(race, sex, marital status, citizenship, offense category, or age) are jointly significant at or below the 0.05 level

or

- Joint significance of all variables in the regression is at or below the .05 level

Even under the null of random assignment, using a .05 level cutoff rule would result in 5 percent of the districts appearing not to randomly assign. I nevertheless remove these districts from the data because I cannot identify the districts that randomly assign but fall into this *p*-value range from districts that do not randomly assign. This process eliminates just over one-third of the remaining district-years leaving 51 districts, 225 district-years and about 40,000 cases for analysis.[12]

3.3 Differences in Performance

3.3.1 Defining Outcome Measures

Restricting my attention to the set of districts that appear to randomly assign cases between two groups of lawyers, I next evaluate their relative effectiveness in representing indigent clients. I consider two outcomes:

- Fraction of cases resulting in a guilty verdict: This is the fraction of total cases in which the defendant either pleads guilty or is convicted at trial.
- Average sentence for all cases: This is defined as the average prison term for all cases regardless of outcome. Sentences for acquittals and dismissals are defined as zero.

[12] Appendix B provides summary statistics for excluded districts.

These outcomes use the entire universe of cases that appear to randomly assign and as such I would expect there to be little difference in the outcome between types of attorneys. Moreover, differences in either outcome can be attributed to differences in the quality of representation provided and not to case quality. This is true because, on average, within a district year CJA panel attorneys and public defenders should have the same underlying distribution of guilt in the cases they represent and thus are equally likely to lose at trial.

3.3.2 Differences in Guilty Rates

To determine if there exists a difference in guilty rates, I first estimate a simple probit regression of the probability of guilt on an indicator for the type of attorney. Table 2, Panel A, column 1 reports this unrestricted difference in means. It appears that defendants with CJA panel attorneys are more likely to be found guilty. Next controlling for district, year and crime effects, I estimated a parsimoniously specified probit:

$$\Pr(guilty = 1) = \Phi\left[\beta_0 + \beta_1 CJA + district\ FE + year\ FE + crime\ FE\right] \tag{1}$$

In equation (1), *CJA* is an indicator variable for whether the case was handled by a CJA panel attorney or a Federal Public Defender. The variables *district FE*, *year FE*, and *crime FE* are the fixed effects for the district in which the case was filed, the year of initial case filing and the crime category respectively. Table 2, Panel A, Column 2 reports the results of this model. Defendants assigned to CJA panel attorneys appear slightly more likely to be found guilty. While the magnitude of this effect is small, the overall probability of being found guilty is nearly 97 percent. As such, the increase in probability of guilt attributed to having a CJA attorney covers 10 percent of the remaining 3 percent probability of being found not guilty.

To further control for defendant characteristics, I estimate:

$$\begin{aligned}\Pr(guilty = 1) = \Phi[&\beta_0 + \beta_1 CJA + \beta_2 black + \beta_3 NA + \beta_4 asian + \beta_5 female + \beta_6 age \\ &+ district\ FE + year\ FE + crime\ FE]\end{aligned} \tag{2}$$

In equation (2), *black, NA,* and *asian* are indicator variables for whether the defendant is black, Native American, or Asian, respectively. The variable *female* is an indicator for whether the defendant is female. The variable *age* is the age of the defendant at the time of initial case filing. Under random case assignment, the gap in attorney performance should be unaffected by defendant demographic controls. As reported in Table 2, Panel A, Column 3, the coefficient on the CJA indicators changes insignificantly after including demographic controls. Because the unit of randomization is a district in a given year, I next estimate a specification with district-year fixed effects. Specifically, I estimate:

$$\begin{aligned}\Pr(guilty = 1) = \Phi[&\beta_0 + \beta_1 CJA + \beta_2 black + \beta_3 NA + \beta_4 asian + \beta_5 female + \beta_6 age \\ &+ district\ \ year\ FE + crime\ FE]\end{aligned} \tag{3}$$

The results from equation 3 are reported in Table 2, Panel A, column 4. This analysis shows that there is little difference between using district-year fixed effects and using district and year fixed effects. The gap appears quite robust to specification. These estimates suggest that defendants assigned CJA panel attorneys are three-tenths of a percentage point more likely to be convicted.

For each type of attorney, I next tested whether the difference in case outcomes varies across the type of crime with which the defendant was charged. To do this, I estimated a probit with district and year fixed effects and included interaction terms between major crime categories and the CJA indicator variable. Specifically, I estimated:

$$\begin{aligned}\Pr(guilty=1) = \Phi[&\beta_0 + \beta_1 CJA*violent + \beta_2 CJA*property + \beta_3 CJA*drugs + \beta_4 CJA*p.o.\\ &+ \beta_5 CJA*weapons + \beta_6 CJA*immigration + \beta_7 violent + \beta_8 property\\ &+ \beta_9 drugs + \beta_{10} p.o + \beta_{11} weapons + \beta_{12} black + \beta_{13} NA + \beta_{14} asian\\ &+ \beta_{15} female + \beta_{16} age + districtFE + yearFE + crimeFE]\end{aligned} \quad (4)$$

In equation (4), I include interaction terms between the major crime categories and the *CJA* indicator variable. The major crime variables are *violent,* which includes all violent crimes, *property,* which includes property crimes such as thefts, *drugs,* which includes all drug offenses including possession, sales and trafficking, *p.o.,* which includes all public order offenses including most white-collar financial crimes, *weapons,* which include all weapons offenses including possession and sales, and *immigration*, which includes all immigration related offenses.[13] I also include crime subcategory fixed effects to control for the specific type of crime committed within these broad categories. I repeat this analysis using district-year fixed effects. The results from both of these specifications are reported in Panel B of Table 2. It appears that much of the difference in performance before public defenders and CJA attorneys in cases involving weapons and drugs offenses. In part, this may be due to the high fraction of cases in these categories, allowing better identification of differences in these categories. This may also be due to the structure of randomization. In some districts, cases are placed into severity tiers (randomization occurs within these tiers), based largely on the major crime category. Most drugs and weapons offenses are considered less severe (typical charges are for low-level distribution or personal possession). Violent offenses are considered very severe.

[13] The classification of offenses into these categories is based on the Bureau of Justice Statistics classification of primary offense categories.

Public order and property offenses fall somewhere in between. Cases are randomly assigned to a type of attorney (e.g. CJA or public defender), but the specific attorney assigned will have the requisite experience deemed necessary to defend against the type of charge. In this situation, drugs and weapons charges will be handled by the least experienced attorneys, while violent offenses will be handled by the most experienced attorneys.[14] In this light it appears that highly experienced attorneys, regardless of type, perform similarly while the lesser experienced public defenders perform better than the lesser experienced CJA panel attorneys.

3.3.3 Differences in Sentence Length

To estimate the difference in sentence length, I begin by estimating a simple difference in means using an unconditional linear regression. The results from this simple difference, reported in Table 2, Panel A, Column 5 suggests that there is a about a 3 month difference in sentence length. Interestingly, when controlling for district, year, and offense type the difference in sentence length increases to over 5 months (Table 2, Panel A, Column 6). Table 2, Panel A, Column 7 reports results after including defendant characteristic controls. This verifies random assignment as the inclusion of demographic controls does not affect the difference in sentence length between the two types of attorneys. The results from including district-year fixed effects are reported in Table 2, Panel A, Column 8. There is little change in the difference in sentence length with using district-year fixed effects, suggesting that the 5 month difference is relatively

[14] Evidence for the difference in experience level of attorneys handling various major offense categories comes from conversations with district clerks regarding the administration of the assignment of cases to indigent counsel as well as from the district specific CJA Plans guiding the implementation of an indigent defense system.

robust to specification. It is worth noting that conditional on district, year, and type of offense, black defendants receive substantially longer sentences than comparable white defendants.

In the bottom panel of Table 2 (Panel B), I test whether the difference in sentence length varies across type of crime. I estimated a linear regression with district and year fixed effects and included interaction terms between major crime categories and the CJA indicator variable. I repeat the analysis using district-year fixed effects. The biggest differences are again concentrated in weapons and drugs offenses. In part, this may be due to mandatory sentencing for weapons and drugs offenses. In these categories, increased probability of conviction will have much larger impact on sentence length as judges have no discretion to adjust sentences based on case-specific characteristics.[15] Moreover, while some convictions (for example for property crimes) could result in only probation, mandatory sentencing requires prison time for most drugs and weapons offenses.

3.4 THE ROLE OF PLEA BARGAINING

3.4.1 The Use of Plea Bargaining In Determining Case Outcomes

Because most cases are disposed of using plea bargains, understanding performance differences requires an analysis of the relative plea rates of the two types of attorneys. This paper will uses a notion of efficient plea bargains to measure attorney

[15] According to Freed (1992, p. 1690), there are approximately 100 federal mandatory minimum penalties, contained in 60 different criminal statutes most of which involve drugs and/or the use of a gun.. An analysis of sentence length conditional on conviction shows much lower variance in sentence length for offenders convicted of the same crime in drug and weapons cases relative to those convicted of other offenses. This supports the idea that the specific crime, not judicial discretion, generates differences in sentences in these cases.

performance. Because trials are costly both in terms of time and monetary expenditures, from an efficiency standpoint, plea bargaining is a lower cost way to resolve criminal disputes. Plea bargains shorten the duration of a trial and save the cost of running a trial, and therefore a higher fraction of guilty cases that plead guilty (rather than go to trial) is a measure of efficient attorney performance.

Moreover, plea bargains have the potential to be a pareto improvement. If defendants are sufficiently risk averse then a negotiated shorter sentence is preferable to the risk of a higher sentence at trial.[16] Consider the extreme example of a guilty defendant who will be convicted at trial with probability one. In this case, the plea serves only to shorten the defendant's sentence and reduce the administrative costs of the case. The question of undue pressure for the innocent to plead guilty is moot as the injustice of the system (should any exist) is not generated by the decision to plead guilty. [17] Lastly, if prosecutors are averse to losses, then they will be willing to lessen the severity of charges in exchange for a guilty plea. Thus, all parties in this system may be made better off by plead bargaining.

3.4.2 The Difference in Plea Rates

To study plea rates, I first define plea cases as cases in which the defendant pleads guilty or *no lo contendre* to either the top charge or a lesser included charge and waives his or her right to a trial or future appeal. I estimate several specifications, reported in Table 3

[16] These situations, there are serious fairness concerns when risk-aversion (especially aversion that may be due to perceived racial or class-based biases in the system) rather than true guilt determines who is found guilty in court. For a discussion of the relationship between plea bargaining and the distribution of risk aversion, see Kobayashi and Lott (1996)

[17] The question of whether plea bargains generate excess pressure for innocent defendants to plead guilty is outside the scope of this paper. Chin and Holmes (2002) discuss the relationship between ineffective counsel and guilty pleas. However, several papers suggest that plea bargaining can be structured to ensure truthful revelation. See for example Grossman and Katz (1983).

Panel A. Regardless of specification, it appears that defendants with CJA panel attorneys are 2.5 percentage points less likely to plead guilty. Repeating this analysis by type of crime, it appears that again, much of the effect is concentrated in weapons and drugs cases.

Pleading guilty in part works because defendants are able to plead guilty to less severe crimes (and therefore receive shorter sentences) in exchange for saving the government the cost of a trial.[18] The most effective form of plea bargaining then is pleading guilty to a lesser charge (typically included in the indictment). Therefore, I also estimate the relationship between attorney type and the probability of pleading guilty to a lesser included charge. It appears that CJA panel attorneys are over 8 percentage points less likely to negotiate pleas for lesser included charges. This difference again is especially pronounced in drugs and weapons offenses. This highlights the importance of plea bargaining in determining expected sentence length. Pleading to lesser included offenses allows defendants to either receive lower mandatory sentences or avail themselves of judicial discretion. In these cases, plea bargaining is the only way to negotiate lower sentences for defendants, as the sentences imposed at the sentencing hearing are highly constrained by federal guidelines.

The analysis of plea rates sheds some light on what is generating the overall difference in guilty rates and sentence length. The difference in the probability of being found guilty combined with the lower plea rates by CJA panel attorneys suggests that: 1) CJA attorneys are performing significantly worse at trial and/or 2) CJA panel attorneys are not taking the "right" cases to trial. I cannot determine whether CJA panel attorneys

[18] Evidence suggests that the sentencing guidelines shifted prosecutors from sentence bargaining to bargaining over the charges or guideline factors regarding mitigating or aggravating circumstances (Nagel and Schulhofer, 1997)

are only pleading only a proper subset of the cases that public defenders are pleading or if they are pleading an intersection set of cases. Given the high probability of plea bargaining among both groups of attorneys, it is likely that there is significant overlap in the cases which each type of attorney decides to plead guilty. Nevertheless, it appears that in some way, be it in the decision of what cases to plead or the quality of negotiations during the plea bargaining stage, the use of guilty pleas plays an important part in explaining the difference in attorney performance.

3.4.3 Decomposing the Difference in Outcomes

Analysis of case outcome and sentencing rates reveals differences but it is unclear if the overall difference between public defenders and CJA panel attorneys is due to performance at trial or incorrect decisions about which cases to take to trial in the first place. Moreover, because of the differing rates at which the lawyers plead as the differing sentence length, it is unclear how to attribute raw differences in outcomes to differences in performance at the various stages of criminal proceeding, and how much to attribute to the single decision of whether to plead guilty or not.

In order to identify the stage in which there is a difference in performance, I constructed a set of overall measures to be used in decomposition analysis. To estimate the overall difference in performance, I estimated the expected sentence for defendants with each type of lawyer. I defined the expected sentence as:

$$E(sentence) = \Pr(plea = 1) * (sentence \mid plea = 1) + \Pr(plea = 0) * (sentence \mid plea = 0)$$

(5)

I then estimated these outcomes by type of attorney. From these estimates, I constructed eight predicted values:

$$\hat{P}_{CJA} = \Pr(plea = 1 \mid CJA = 1) = \Phi[X^{P}_{CJA}'\beta_{CJA}] \tag{6}$$

$$\hat{P}_{PD} = \Pr(plea = 1 \mid CJA = 0) = \Phi[X^{P}_{PD}'\beta_{PD}] \tag{7}$$

$$\hat{S}^{P}_{CJA} = E(sentence \mid plea = 1 \,\&\, CJA = 1) = X^{SP}_{CJA}'\gamma_{CJA} \tag{8}$$

$$\hat{S}^{P}_{PD} = E(sentence \mid plea = 1 \,\&\, CJA = 0) = X^{SP}_{PD}'\gamma_{PD} \tag{9}$$

$$\hat{S}^{C}_{CJA} = E(sentence \mid plea = 0 \,\&\, CJA = 1) = X^{SC}_{CJA}'\lambda_{CJA} \tag{10}$$

$$\hat{S}^{C}_{PD} = E(sentence \mid plea = 0 \,\&\, CJA = 0) = X^{SC}_{PD}'\lambda_{PD} \tag{11}$$

In equations (6) through (11) X_{CJA} is the vector of case characteristics variables (district, year, crime sub-category, and defendant characteristics) for the cases assigned to CJA attorneys. Similarly, X_{PD} is a vector of the case characteristics variables (district, year, crime sub-category, and defendant characteristics) for the cases assigned to public defenders. The superscript in each case refers to the stage of inclusion. P refers to the plea bargaining stage, SP refers to the plea sentencing stage, C refers to the trial stage, and SC refers to the sentencing after trial conviction stage. Therefore X_i^P includes all cases, X_i^{SP}, includes only cases in which the defendant pleads guilty, and X_i^{SC} include only cases which continue to trial. β_{CJA} and β_{PD} are the parameters from the probit regressions of the probability of pleading guilty for CJA attorneys and public defenders respectively. Likewise, γ_{CJA} and γ_{PD} (λ_{CJA} and λ_{PD}) are the parameters from the linear model of sentence length, conditional on pleading guilty (sentence length, conditional on conviction at trial) for CJA attorneys and public defenders respectively. Using these predicted values, I then construct two measures of expected sentence length:

$$E(sentence \mid CJA = 1) = J_{CJA} = \hat{P}_{CJA} * \hat{S}^{P}_{CJA} + (1 - \hat{P}_{CJA}) * \hat{S}^{C}_{CJA} \tag{12}$$

$$E(sentence \mid CJA = 0) = J_{PD} = \hat{P}_{PD} * \hat{S}^{P}_{PD} + (1 - \hat{P}_{PD}) * \hat{S}^{C}_{PD} \tag{13}$$

To determine the proportional difference, the first column of Table 4 reports the difference in these two measures, i.e. $J_{CJA} - J_{PD}$. Overall, defendants with CJA attorneys have nearly eight months of additional jail time. Repeating the above analysis by primary offense type, it appears that the effect of having a CJA panel attorney ranges from a difference of about 5 months for violent offenses to a difference of nearly a year and a half for weapons offenses. Immigration offenses move in the opposite direction, so that defendants with CJA panel attorneys have about 2.5 month shorter sentences.

In order to better determine the source of the differences between attorney types, I next decompose the overall effect into six components, three of which are due to attorney performance holding the distribution of case characteristics fixed and three of which are due to selection of cases into the given stage, holding attorney performance fixed. [19] In order to do this, I define an estimate of expected sentence length with Public Defender case characteristics but CJA parameters. Define this predicted expected sentence length as:

$$\tilde{J} = \tilde{P} * \tilde{S}^{P} + (1 - \tilde{P}) * \tilde{S}^{C} \tag{14}$$

These variables in equation (14) $\tilde{P}, \tilde{S}^{P}$, and $\tilde{S}^{C}$ correspond to the predicted expected probability of plea bargaining for CJA panel attorney cases at public defender parameter values, the predicted sentence length in pleaded cases for public defender cases at CJA

[19] Standard errors for these estimates are constructed by bootstrapping. The process involves drawing from the sample, with replacement, then constructing the estimates of J_{CJA} and J_{PD}, as well as a J_{CJA} and J_{PD} for each primary offense category. I repeated this process 1000 times and then constructed the standard error of the mean from these estimates.

panel attorney parameter values, and the predicted sentence in trial cases for public defender cases at CJA panel attorney parameter values, respectively. Taking the difference between equations (12) and (13), I add and subtract $\tilde{J}$ from equation (14). After some algebra, this yields:

$$\begin{aligned} J_{CJA} - J_{PD} = {} & (\hat{P}_{CJA} - \tilde{P})(\tilde{S}^{P} - \tilde{S}^{C}) + \hat{P}_{CJA}(\hat{S}^{P}_{CJA} - \tilde{S}^{P}) + (1 - \hat{P}_{CJA})(\hat{S}^{C}_{CJA} - \tilde{S}^{C}) \\ & + (\tilde{P} - \hat{P}_{PD})(\hat{S}^{P}_{PD} - \hat{S}^{C}_{PD}) + \tilde{P}(\tilde{S}^{P} - \hat{S}^{P}_{PD}) + (1 - \hat{P}_{PD})(\tilde{S}^{C} - \hat{S}^{C}_{PD}) \end{aligned} \qquad (15)$$

This decomposition is similar to a Oaxaca decomposition, decomposing the effects into "procedural performance" and "selection of cases to plead." In equation (15) the first line of the equation contains terms which measure attorney procedural performance holding case characteristics fixed. This measures how well the attorney advocates in a given procedure (e.g. trial or plea negotiations) holding fixed the offense type and defendant characteristics as well as district and year fixed effects. The second line contains terms which measure the effect of selecting certain cases to plead guilty, holding attorney procedural performance fixed. This measures the effect of the decision to plead guilty on outcomes, assuming that attorneys perform equally well once a given case is in a specific procedural stage. The stages are the decision-to-plea stage, the plea-sentencing stage and the trial-sentence stage. The decision to plea stage measures the effect of pleading guilty or not, regardless of the outcome of the plea. The plea sentencing stage measures the quality of plea, defining higher quality pleas as those with shorter sentences (conditional on type of crime). The trial-sentence stage includes acquittals and dismissals, treating these cases as being assigned no prison sentence.

Category	Term	Expanded Form	Description
Difference in Attorney Procedural Performance holding Case Characteristics Fixed	$(P_{CJA} - \widetilde{P})(\widetilde{S}^{P} - \widetilde{S}^{C})$	$(\Phi[X^{P}_{CJA}{}'\beta_{CJA}] - \Phi[X^{P}_{CJA}{}'\beta_{PD}])\Delta\widetilde{S}$	difference in probability of plea bargaining
	$P_{CJA}(S^{P}_{CJA} - \widetilde{S}^{P})$	$\hat{P}_{CJA} X^{SP}_{CJA}(\gamma_{CJA} - \gamma_{PD})$	difference in plea bargained sentences
	$(1 - P_{CJA})(S^{C}_{CJA} - \widetilde{S}^{C})$	$(1 - \hat{P}_{CJA}) X^{SC}_{CJA}(\lambda_{CJA} - \lambda_{PD})$	difference in sentences after trial
Difference in Selection of Cases to Plead holding Attorney Procedural Performance Fixed	$(\widetilde{P} - P_{PD})(S^{P}_{PS} - S^{C}_{PD})$	$(\Phi[X^{P}_{CJA}{}'\beta_{PD}] - \Phi[X^{P}_{PD}{}'\beta_{PD}])\Delta\hat{S}_{PL}$	difference in case characteristic prior to the plea stage
	$\widetilde{P}(\widetilde{S}^{P} - S^{P}_{PD})$	$\widetilde{P}(X^{SP}_{CJA} - X^{SP}_{PD})\gamma_{PD}$	difference in case characteristics at sentencing for plead case
	$P_{PD}(\widetilde{S}^{C} - S^{C}_{PD})$	$(1 - \hat{P}_{PD})(X^{SC}_{CJA} - X^{SC}_{PD})\lambda_{PD}$	difference in case characteristics at sentencing for tried cases

Of the 7.76 months difference in sentence length, over half of the difference is due to attorney procedural performance related measures. 3.63 months (or about 48 percent) are due to difference in the selection of cases to plead guilty. I next analyze at which stage these differences arise using the terms from equation (15). It appears a little more than half of the difference in expected sentences is due to attorney performance when plea bargaining and negotiating sentences and a little less than half is due to the selection of cases which are plead versus those which go to trial. This decomposition also provides a check of random assignment. If cases are randomly assigned then term four should be zero since there should be no difference in case characteristics between the two types of attorney at the beginning of criminal proceedings. In all cases, there appears to be no significant difference in the case characteristics between CJA attorneys and public defenders at the plea stage.

Table 4 also reports the decomposition by major crime type. The relative importance of different measures of attorney performance is similar across major crime category. For violent, property, and public order offenses, nearly half of the expected sentence length is due to the difference in attorney performance during plea bargaining. Between a quarter and a third of the difference is explained by the difference in case characteristics for cases which are in the plea sentencing stage. For weapons offenses, nearly 85 percent of the 17 month difference in expected sentence length is due to difference in attorney performance when plea bargaining. For drug offenses, on the other hand, over half of the difference in expected sentence length is due to case characteristics. The last major crime category considered is immigration offenses, where defendants with CJA attorneys receive shorter sentences. Consistent with the other case categories, CJA attorneys perform worse during plea bargaining. In contrast with the other case categories, the case characteristics of trial cases explain the shorter sentences for CJA panel attorneys relative to the public defenders. Overall, it appears that attorney performance is responsible for a large fraction of the overall difference in expected sentence length. Although there is some variation across the type of crime committed, these results are robust across most crime types. These differences also do not appear due to case characteristics pre-case assignment, confirming random assignment.

3.5 The Impact of Wages, Experience, and Law School Quality on Attorney Performance

3.5.1 Linking Case Outcomes and Attorney Characteristics: A Subsample from 3 Districts

In order to collect the data necessary to compare attorney characteristics between these two groups, I restricted my sample to three districts. Taking the AOUSC Criminal Master File, I used the case docket numbers to identify the cases. Then using PACER, the Federal Court on-line case management system, it was possible to find the case records, which identify the lawyer. This collection was done for three districts (all of which passed the randomization tests): California Southern District, California Central District, and Arizona. These districts were chosen in part because their court records are currently on-line. The PACER system for District level dockets is not fully implemented and so not all districts have their court dockets available on line. In addition, these districts are in states that have on-line publicly accessible attorney information available through their State Bar Associations. Using this look-up service, I linked attorneys to the date they passed the bar as well as the law school they attended as a measure of their experience and ability respectively.

3.5.2 Defining Measures of Attorney Characteristics, Wages, and Caseload

To examine whether there are differences in the attorneys in the two tiers of the indigent defense system, I define several variables to measure differences in outside opportunities and attorney characteristics. One characteristic which might be important for performance is the legal experience of the attorney. I defined experience as number of years the attorney has practiced law in the district where he or she was assigned a case, and therefore construct it as year the case was filed minus year the attorney passed the bar in that state. While in most cases this measure will accurately represent years of practice, some attorneys may have practiced for many years in other states and only passed the bar after moving to their current state. For these attorneys this experience measure will understate their experience. Similarly, for attorneys who passed the bar and then took time off from practice to engage in other activities, this measure will overstate their experience.

In order to estimate the quality of training an attorney received, I use the law school each lawyer attended. I rank the law schools using the *U.S. News and World Reports* ranking from 2005. I break the differences down by tiers. Tier 1 includes law schools ranked 1 through 10. Tier 2 includes law schools ranked 11 through 25. Tier 3 includes law schools ranked 26 through 50. Tier 4 includes law schools ranked 51-100. Tier 5 includes law schools ranked 101 through 134 (this is the *U.S. News and World Reports* "tier 2" schools). Tier 6 includes law schools ranked 135 through 177 (this is *U.S. News and World Reports* "tier 3" schools).[20]

In addition to lawyer-specific characteristics, I also look at some market variables. The variation in outside opportunity wages is likely to result in different types of attorneys selecting to be CJA panel attorneys and public defenders and as such could influence the performance of the attorneys in criminal proceedings. I develop a variable called the attorney wage gap, which is defined as the wage the federal government pays minus the outside opportunity wage. For CJA panel attorneys, the wage gap is defined as the wage the attorney receives for courtroom work minus the average wage for an

---

[20] These law school rankings likely only roughly approximate the "quality" of education these schools provide and may not be an entirely accurate predictor of the quality of the schools or of the lawyers who graduate from them.

attorney in that area. Similarly, for public defenders the wage gap is defined as the difference between the wage for public defenders in that district-year and the average wage for attorneys in that area. In order to measure market wages, I used the Occupation Employment Statistics from the Bureau of Labor Statistics data, which lists wages by industry.[21] In order to estimate federal government wages I use the Criminal Justice Expenditure report, which includes wages for federal government legal establishments. These estimates include U.S. Attorney's Offices (the prosecuting attorneys in federal cases) and Federal Public Defender Offices. Since Federal Public Defenders and U.S. Attorney's Offices have the same pay scale, I assumed that the average wage per employee is the same. For CJA panel attorneys, I use the established federal wage rate for CJA panel attorneys as set by Congressional Approval and appropriated through the Administrative Office of the US Courts.

Other factors affecting attorney performance may be their caseload or the frequency with which attorneys interact with prosecutors and judges in the criminal system and the number of cases an attorney handles in the federal criminal system. However, I do not observe the total caseload for either type of attorney. In order to capture this effect, I calculate the average indigent caseload for an attorney in a district in a year. To do this I use the number of public defenders (L_{PD}), the number of CJA panel attorneys (L_{CJA}) and the number of cases handled by each (N_{CJA}, N_{PD}). I estimate L_{PD} and L_{CJA}, by contacting the districts and asking them the number of people on the panel and in the public defender's office in each year from 1997-2001. I can observe N_{CJA} and

[21] I use wages for lawyers from 1997-2001 from the following Metropolitan Statistical Areas (MSA): Flagstaff, Phoenix-Mesa, Tucson, Yuma, Los-Angeles-Long Beach, Orange County, Riverside-San Bernardino, San Louis Obispo-Atascadero-Paso-Robles, Santa Barbara-Santa Konica-Lompoc, Ventura, and San Diego.

N_{PD} from the AOUSC data. I then define average indigent caseload as: N_i / L_i for i=*CJA*, *PD*.

3.5.3 Differences in Attorney Characteristics and Wages

Some summary statistics on these lawyers are shown in Table 5. The CJA panel attorney wage is on average lower than the average wage in the county in which the attorney resides. However, this varies a great deal depending on the county location. In some counties in Arizona, for example, the CJA wage is greater than the market wage. The experience level varies greatly between attorney types. CJA panel attorneys, on average, have very low experience. Figure 1 shows the distribution of experience. It appears that public defenders on average have higher experience and a wider distribution. Many panel attorneys have less than 10 years experience, but there is a cluster of attorneys with about 15 years experience and another cluster with about 25 years of experience. These are the attorneys that frequently handle the more difficult or highly technical cases and are, in some cases, former public defenders or well established criminal defense attorneys.[22] In addition, public defenders appear to be from higher ranked law schools. Relative to the overall population of lawyers, CJA attorneys are less experienced and attended lower quality law schools while Federal Public Defenders are more experienced and attended higher quality law schools.

3.5.4 Explaining the Performance Gap Using Attorney Characteristics, Wages, and Caseload

---

[22] The recruitment and appointment of these highly experienced lawyers was described in detail to me by several public defenders offices including the Federal Defenders of San Diego, Inc.

While it appears that the attorneys in the two groups are observably different and there appears to be significant difference in their outcomes due to attorney performance, the analysis thus far has not explored the relationship between these two facts. I next consider the importance of differences in wages, experience, and education quality on generating the observed difference in attorney performance. Because this analysis is restricted to the three districts for which I have attorney level data, I estimate equation (1) (parsimonious specification with district, year, and crime fixed effects) restricting my analysis to these districts.

I first conduct this analysis for the outcome Pr(*Guilty* = 1). The difference in probability of being found guilty is about 2.6 percentage points greater for CJA panel attorneys. This is much larger than the overall difference across all districts. I next estimated a probit of the probability of being found guilty on attorney type, experience, education quality, expected repeat interaction frequency and two wage gap variables. These regressions are of the following specification:

$$\begin{aligned}\Pr(guilty = 1) = \Phi[&\beta_0 + \beta_1 CJA + \beta_2 (w_l - \overline{w}) + \beta_3 caseload + \beta_4 \exp + \beta_5 LS + \\ &district\ FE + year\ FE + crime\ FE]\end{aligned} \tag{16}$$

The results from equation (16) are reported in column (2) Table 6. Comparing the CJA-public defender performance gap using this new specification, it appears that the wage gap, experience, caseload, and law school quality variables explain the entire difference in guilty rates. Experience appears to be very important, reducing the probability of being found guilty by about 1.2 percentage points.[23] Higher quality law schools also appear quite important. A 1 percentage point change in the wage gap reduces the probability of being found guilty by about 4 percentage points. Put another way, districts in which

[23] Specifications with a quadratic experience term found this term insignificant.

indigent defense counsel wages are closer to the market wage are associated with better performing attorney.

A potential criticism of the specification in equation 16 is that both the wage gap and the caseload measures have systematic measurement error. The wage gap for CJA attorneys compares their courtroom wage to the average market wage. If wages are positively correlated with experience, and then the wage gap measure will overstate the size of the true wage gap for CJA panel attorneys. This is because CJA attorneys are less experienced and thus command a lower market wage. The caseload measure cannot account for the non-indigent work of CJA panel attorneys and as such may not accurately estimate the relationship between either workload or system interaction and attorney performance. Because these variables are market variables and not at the individual case level I cannot include them in a specification with district-year fixed effects. However, these fixed effects may allow me to isolate the impact of experience and law school quality without the potential contamination of these arguably mis-measured variables. As such, I estimate a specification

$$\Pr(guilty = 1) = \Phi[\beta_0 + \beta_1 CJA + \beta_3 \exp + \beta_4 LS + district - year\ FE + crime\ FE] \quad (17)$$

The results from equation (17) are reported in Table 6. Including district-year fixed effects along with experience and law school quality yields qualitatively similar results to the specification that included wages and caseload. The performance gap between attorneys remains insignificant and is not significantly different than the coefficient in the previous specification. The importance of law school appears to be virtually identical across specification. Overall, it appears that these variables can explain all of the difference in probability of guilt between the two types of indigent defense counsel.

Repeating this analysis for sentence length, I estimate the difference in expected sentence length between the two groups. On average, defendants with CJA panel attorneys will receive an additional sentence of nearly 7 months. This difference shrinks to about 2.6 months when including the wage gap, caseload, experience, and law school measures (as well as district and year fixed effects). Wages are only marginally significant but a 1 percentage point increase in the wage gap (higher indigent defense wage relative to the market wage) reduces sentence length by over 5 months. An additional year of experience also reduces sentence length, by about 5 months. Attorneys who attended higher-tier law schools (Tiers 1 and 2) secure 8 month shorter sentences for their clients. Again because of concerns about the mis-measurement of wages and caseload, I estimate a specification with district-year fixed effects. The difference between attorneys in this specification is about 3.3 months, slightly larger than the difference in the previous specification. The effects of experience and law school quality are almost identical across specifications. Thus it appears that attorney characteristics (along with wages and caseload) explain over half of the difference in sentence length between attorneys.

Finally, I estimate the effect of these variables on the propensity of these attorneys to engage in plea bargains. There is a marginally significant difference in the probability of plea bargaining between the types of attorneys in these three districts. These results are reported in column (7) of Table 6. It appears that CJA panel attorneys plead less often than public defenders (consistent with the analysis across all districts). The difference between plea rates is insignificant after including wages, experience, caseload, and law school quality measures, as reported in Table 6 column (8). Moreover, it appears that higher levels of experience increase the probability of plea bargaining by about 2 percentage points, as do higher wages. Attending higher "quality" law schools increase the probability of plea bargaining by about 5 percentage points. The specification that includes district-year fixed effects is reported in column (9) of Table 6. The difference between types of attorneys shrinks even further and the effect of experience increases to 3 percentage points. Overall it appears that experience and law school quality (along with caseload and wages) fully explain any differences in plea rates.

3.5.5 The Effect of Wages, Attorney Characteristics, and Caseload by Attorney Type

I next estimate whether the effects of wages, experience, law school quality, and caseload have different effects for the two types of lawyers. I constructed a separate set of interaction terms between lawyer characteristics and attorney type.

The results using the dependent variable Pr(*Guilty* = 1) are reported in columns (1) and (2) of Table 7. It appears that wages explain more of the performance of CJA panel attorneys than public defenders. As indigent defense lawyers wages move 1 percentage point closer to the market wage, the probability that a defendant will be found guilty decreases by 3.7 percentage points if they have a public defender and 5.5 percentage points if they have a CJA panel attorney. The effect of experience and law school quality is virtually identical across the two types of attorneys. A higher expected caseload increases the probability a defendant will be found guilty by about 6 percentage points for defendants with public defenders and has no significant effect on the probability of being found guilty for CJA panel attorneys.

I similarly estimated a regression of sentence length on the full set of interaction terms. These results are reported in columns (3) and (4) of Table 6. Again wages impact the performance of CJA panel attorneys more than that of public defenders. A 1 percentage point change in the wage gap reduces the sentence received by 4 months for defendants with public defenders and nearly 6 months for defendants with CJA panel attorneys. An additional year of experience reduces sentence length by about 5 months both for defendants with public defenders and defendants with CJA panel attorneys. A higher expected caseload increases sentence length by 3 months for defendants with public defenders and reduces sentence length by 6.75 months for defendants with CJA panel attorneys.

In terms of plea rates (reported in columns (5) and (6) of Table 6), a change in the wage gap increases the probability that a defendant will plead guilty by nearly 3 percentage points if they have a public defender and nearly 5 percentage points if they have a CJA panel attorney. Again, the effect of experience is around 2 percentage points and is almost identical between the two types of attorneys. A higher expected caseload increases the probability that a defendant will plead guilty by about 6 percentage points if they are represented by a public defender and about 2.6 percentage points if they are represented by a CJA panel attorney.

The differential effects of caseload may be due to the positive effect of repeat interactions with prosecuting attorneys (U.S. Attorneys). Assuming there are diminishing returns to the positive effect of repeat interactions, the marginal effect of increasing the likelihood of repeat interactions for public defenders with prosecuting attorneys may be very small. If plea bargains allow public defenders to reduce the marginal cost of additional work from a higher caseload then it is reasonable to observe little significant effect on the negotiated sentence length. On the other hand, CJA panel attorneys may have little or no interaction with prosecutors outside of their assigned indigent defense caseload. Given the low experience level of many of these attorneys, it is possible that CJA panel attorneys are attorneys beginning their career and may have a high marginal benefit from improved relationships with US attorneys. However, because this caseload measure is likely to have error, it is difficult to develop a full explanation of these effects.

4 Summary and Conclusions

4.1 Key findings

This study has analyzed the differences in performance between CJA panel attorneys and public defenders. It appears that public defenders outperform CJA panel attorneys in all outcomes that were considered. Defendants represented by CJA panel attorneys are more likely to be found guilty and receive longer sentences. These differences appear to be related to the ability of attorneys to determine which cases to plead guilty as well as their ability to negotiating plea bargains. These differences appear to be due to differences in the training and experience levels between the attorneys in the CJA attorney panel and attorneys in the public defenders offices.

Analyzing the attorneys in the two groups reveals that public defenders on average have more experience and are more likely to have attended a top tier law school as defined by the *U.S. News and World Reports* ranking. Given the significant effect of experience on outcomes, this difference in attorney characteristic explains some of the differences in the performance gap. Wages too have an effect: attorneys in geographical areas where the wage paid to CJA panel attorneys is close to the average market wage in that area perform better. The expected caseload of an attorney appears to have different effects for the two types of attorneys. Public defenders perform worse when the number of cases they handle increases while CJA panel attorneys perform better. While this observation may seem contradictory to the overall findings in this study in reality it may not be so. This effect may be due to competing effects of increased caseload, which not only increases the workload/effort required by an attorney but also increases an attorney's exposure to the system through repeat interactions, trial experience and the development of general institutional knowledge. The results in this study suggest that this type of experience would preferentially benefit the CJA attorneys since as a group they are less experienced in the court system than public defenders. Taken together these observations suggest that the lower level of experience of the CJA attorneys and the ability of CJA attorneys to decide which cases to take to trial may combine to produce a situation where the decision to take a case to trial may be based not only on the facts related to the case but also on the desire of the CJA attorney to obtain trial experience.

4.2 Implications for Indigent Clients: Not Getting Their Due Process

Since the poor in the U.S. are disproportionately from minority communities, inequities in systems that disadvantage them have the unintended consequence of perpetuating discriminatory practices on the basis of race. The use of lower-performing CJA panel attorneys impacts minority communities in several ways. First, as Table 1 illustrates, over 30 percent of indigent defendants are of African-American descent while they constitute only 13 percent of the U.S. population. Furthermore, only 19 percent of defendants who can afford to retain their own counsel are African-Americans. About 4000 cases per year involve minority defendants who are randomly assigned CJA panel attorney. Given the large fraction of defendants of African-American descent, it becomes obvious that poor quality representation may disproportionately affect them. Second, districts with high minority and immigrant populations have a higher fraction of their cases covered by CJA panel attorneys. A simple correlation between the fraction of cases covered by CJA panel attorneys and the fraction black defendants yields a correlation factor of 0.77. This correlation may be due to district specific factors such as cases per year, prevalence of urban centers, and other factors related to local geography and culture. Third, in districts that do not randomly assign, blacks are significantly more likely to be assigned a CJA panel attorney than whites. Immigrants are also slightly more likely to be assigned CJA attorneys (although this difference is only significant at the 0.10 level).[24] In part this difference is due to selection of cases based on crime type (the inclusion of crime fixed effects explains about 1/3 of the difference in the probability of assignment to a CJA panel attorney between blacks and whites). The performance gap between CJA panel attorneys and public defenders is larger among non-randomly assigning districts than among randomly assigning districts. This could be due to case selection decisions on the part of the attorneys (i.e. CJA panel attorneys are assigned cases which are more likely to end in conviction). However, because it is unclear how much of the gap is due to performance, the higher fraction of blacks assigned to CJA panel attorneys raises questions about whether race affects the quality of the representation indigent defendants are assigned. Thus, an initial decision to create a two-

[24] These statements are based on the marginal effects of a probit of Pr(CJA = 1) on defendant demographics and district, year and offense fixed effects. Results from this analysis are available upon request.

tiered system without racial consideration can percolate through the system to have racially-linked negative consequences.

Indeed the differences isolated in this study may legally constitute a case of discrimination under Title VI of the Civil Rights Act (1964). Though the procedures implemented to assign counsel are facially neutral, the difference in performance and the disproportionate impact this difference has on minorities may support a case for discrimination based on disparate impact.[25] There is some evidence that the mere provision of inferior services or benefits to a protected group is sufficient cause to establish discrimination, regardless of the cost of the impact. [26] Moreover, under disparate impact theory, if an organization which uses federal funds uses a "neutral procedure or practice that has disparate impact on protected individuals, and such practice lacks a substantial legitimate justification," then this constitutes a violation of civil and perhaps due process rights.[27]

Given the potential discriminatory impact of this institutional structure, it is unclear why the federal government does not simply hire more public defenders. One reason might be because it would be too costly. As shown in Table 5, wages paid to public defenders are on average higher than wages paid to CJA panel attorneys. To quantify the cost effectiveness of using CJA panel attorneys, I consider the potential costs and savings to using panel attorneys. In terms of benefits, CJA panel attorneys earn a lower hourly wage than public defenders. Thus for the same hours worked, CJA panel attorneys will provide cheaper services. However, CJA panel attorneys take longer than

[25] The elements of a Title VI disparate impact claim derive from cases decided under Title VII disparate impact law. See for example *New York Urban League v. New York,* 71 F.3d 1031, 1036 (2nd Cir. 1995).
[26] *Larry P. v. Riles*, 793 F. 2d 969 (9th Cir. 1984)
[27] This definition of disparate impact is based on the US Department of Justice usage in its Legal Manual (1998)

public defenders for observably similar cases. This will impose costs in the form of additional hourly wages. Because CJA panel attorneys' plea-bargain less frequently, they impose additional costs through the administrative and personnel court costs.[28]

$$\text{Cost of Public Defenders} = \mathrm{C}_{\mathrm{PD}} = w_{PD}h_{PD} + (1 - \Pr(\mathit{Plea} = 1)_{PD}) * \mathit{trial}\cos t \tag{18}$$

$$\text{Cost of CJA panel attorney} = \mathrm{C}_{\mathrm{CJA}} = w_{CJA}h_{CJA} + (1 - \Pr(\mathit{Plea} = 1)_{CJA}) * \mathit{trial}\cos t \tag{19}$$

Assuming that $h_{CJA} = h_{PD} + \delta$, then after some algebra, the difference in cost between the two types of attorneys is:

$$C_{PD} - C_{CJA} = (w_{PD} - w_{CJA})h_{PD} + w_{CJA}\delta + [\Pr(\mathit{Plea} = 1)_{CJA} - \Pr(\mathit{Plea} = 1)_{PD}] * \mathit{trial}\cos t \tag{20}$$

Calculating these components it appears that using CJA panel attorneys imposes a \$5800 per case cost on the federal system or a cost of \$61.1 million per year.[29]

4.3 Future Research

The results of this study also have some implications for improving the quality and efficiency of federally funded service for indigent defendants. It appears that while wages may affect performance, the effect is not large enough to account for the overall difference in attorney performance. Moreover, it appears that CJA panel attorneys are more affected by wages than public defenders. This difference may be due to the

[28] Administrative and personnel costs per case include judge, court monitor, deputy clerk, bailiff as well as charge to the jury and time for clerical processing. Estimates of these costs are based on the Ostrom and Hall (2005).

[29] These estimates compare average hours per case * (wage_{PD} – wage_{CJA}) to the difference in hours per case * wage_{CJA} + (difference in Pr(plea))*cost of trial. Average values based on AOUSC data, BJS wage and hours data, and estimates for the National Center for States Courts on trial costs.

underlying reasons that motivate workers to enter indigent defense service. For example, if CJA panel attorneys choose to apply for the CJA panel not solely for monetary reasons but because they receive trial experience which they could not find in the open market, then wages alone may not be sufficient to induce an improvement in performance. This is an area requiring further evaluation. If altruism is a major component in selecting jobs in public defenders offices, then this factor may influence the forms of compensation that can effectively reward high performance. It may be useful to combine psychological and economic analyses to address how altruism might affect employment decisions and in turn allow for the development and structuring of effective compensation packages.

While this study analyzes the effect of attorney characteristics on performance, there remain several areas that require additional research. It would be informative to have a more detailed analysis of the relationship between caseload and performance. Additional research on the relationship between different types of wages, organizational structures, and lawyer performance would help estimate the potential returns as measured by effective representation to higher attorney salaries. Specifically, evaluation of how attorney wages in civil cases affects the case outcome might also help isolate the benefits of effective legal representation. An additional area of study is the potential interactive effects of defendant race, victim race, and attorney race on case outcome and sentence length.

References

American Bar Association (2004). "Gideon's Broken Promise: American's Continuing Quest for Equal Justice" A report on the American Bar Association's Hearing on the Right to Counsel in Criminal Proceedings

Anderson, James M. , Kling,, Jeffery R., Kate Stith (1999) "Measuring Interjudge Sentencing Disparity: Before and after the Federal Sentencing Guidelines" *Journal of Law and Economics* Vol. 42, No.1 Part 2 pp271-307

Bardhan, Pranab, "Labor Tying in a Poor Agrarian Economy: A Theoretical and Empirical. Analysis," *Quarterly Journal of Economics*, August 1983, 501-514.

Butcher, Alan K. and Michael K. Moore (2000) "The Crisis in Criminal Defense in Texas" A Report Received by the State Bar of Texas From the Committee on Legal Services to the Poor in Criminal Matters

Chin, Gabriel J. and Holmes, Jr., Richard W., "Effective Assistance of Counsel and the Consequences of Guilty Pleas" . Cornell Law Review, Vol. 87, No. 3, 2002

Clarke, Judy, Testimony of the NACDL President '96-97 before the Senate Appropriations Sub Committee on Department of Commerce, Justice, and State, and the Judiciary

Employment and Earnings (2004), Division of Current Employment Statistics Bureau of Labor Statistics, U.S. Department of Labor

Freed, D.J. "Federal Sentencing in the Wake of Guidelines: Unacceptable Limits on the Discretion of Sentencers." *The Yale Law Journal*, Vol. 101 (1992), pp. 1681-1754.

Gideon v. Wainwright, 372 U.S at 339-40 (The Sixth Amendment interpreted as requiring counsel be provided for defendants)

Grossman, G.M. and Katz, M.L. (1983). "Plea Bargaining and Social Welfare." *American Economic Review*, Vol. 73, pp. 749-757.

Harlow, Caroline Wolf (2000) "Defense Counsel in Criminal Cases" *Bureau of Justice Statistics Special Report* Office of Justice Programs U.S. Department of Justice

Larry P. v. Riles, 793 F. 2d 969 (9th Cir. 1984)

Lohey, Arnold H. (2003) *Criminal Law: In a Nutshell* West Group, St Paul, Minnesota

Lott, John R. and Bruce H. Kobayashi (1996) " In Defense of Criminal Defense Expenditures and Plea Bargaining" International Review of Law and Economics, Vol. 16, No. 4

Meng, Xin and Junsen Zhang (2001) "The Two-Tier Labor Market in Urban China: Occupational Segregation and Wage Differentials between Urban Residents and Rural Migrants in Shanghai" *Journal of Comparative Economics*

National Center for State Courts (2005) "Estimating Court Costs" [FULL CITE NEEDED]

Nagel, I.H. and Schulhofer, S.J. "Plea Negotiations Under the Federal Sentencing Guidelines: Guideline Circumvention and Its Dynamics in the Post-Mistretta Period." *Northwestern University Law Review,* Vol. 91 (1997), pp. 1284-1316.

New York Urban League v. New York, 71 F.3d 1031, 1036 (2nd Cir. 1995).

Oaxaca, R. (1973) "Male-female wage differentials in urban labor markets", *International Economic Review*, Vol 14 (3) , pp 693-709

Office of the Federal Public Defenders, District of Utah (2003) "Introduction to the Utah Federal Defender Office and CJA Panel Attorneys: Outline of the Federal Criminal Process"

Ostrom, Brian and Daniel J. Hall (2005) "Trial Court Performance Measures" *CourTools*, National Center for State Courts

Reed, Stacey (2003) "A Look Back at Gideon v. Wainwright After Forty Years: An examination of the Illusory Sixth Amendment Right to Assistance of Counsel" *52 Drake L. Rev. 47*

Reinganum, J.F "Plea Bargaining and Prosecutorial Discretion." *American Economic Review*, Vol. 78 (1988), pp. 713-728.

Sherwood, Mark (1994) "Difficulties in the measurement of service outputs", *March 1994, Vol. 117, No. 3*

Voorhees, Donald S. (2001) *Manual on Recurring Problems in Criminal Trials* Fifth Edition, Federal Judicial Center

US Department of Justice (1998) *Title VI Legal Manual* Civil Rights Division

Wall Street Journal (2005) "Boomtown Creates Plenty of Jobs and a Two-Tiered Labor Market", March 20.

Wool, Jon, Howell, K. Babe, Yedid, Lisa. (2003) "Improving Public Defense Systems: Good Practices for Federal Panel Attorney Programs" Vera Institute of Justice

Wright, Charles Alan (1999) *Federal Practice and Procedure: Criminal* Vol 1. West Group, St. Paul Minnesota

Table 1. Characteristics of Cases Assigned to Different Types Attorneys

	Privately Retained Attorney	Pro Se	Indigent Defense Counsel	Percent of Indigent Defense Cases covered by: Public Defenders	CJA Panel Attorneys
Fraction of Sample (N =158,253)	0.25	0.02	0.73	0.55	0.45
Defendant Demographic					
African-American	11.73	1.25	87.03	45.94	54.06
Native American	8.58	0.08	91.34	46.30	53.70
Asian	19.68	0.48	79.84	50.05	49.95
White	52.99	0.54	46.47	55.71	44.29
Female	45.07	1.08	53.85	47.56	52.44
Male	20.26	0.61	79.12	45.22	54.78
US Citizens	50.26	1.01	48.73	48.86	51.14
Age of Defendant	35.86	31.81	31.81	--	--
	(11.47)	(10.99)	(9.22)	--	--
Primary Filing Offense Type					
Violent	11.74	0.13	88.13	73.01	26.99
Property	29.58	1.16	69.26	64.94	35.06
Drug	24.92	0.37	74.71	65.80	34.20
Public Order	43.08	3.09	53.84	69.30	30.70
Weapon	22.27	0.14	77.59	69.26	30.74
Immigration	6.52	0.33	93.14	68.05	31.95

Notes: Estimates based on author's own calculations using Administrative Office of the US Courts (AOUSC) Criminal Master File. Federal Public Defender category includes Community Defender Organizations recognized by the AOUSC as the indigent defense provider in that federal district. Pro se refers to cases in which the defendant represents him or herself.

Table 2. Estimates of Differences in Guilty Rate and Sentence Length between Indigent Defense Counsel

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Dependent Variable N = 46,167		E[Pr(*Guilty* = 1)]= 0.9676				E[*Sentence Length*] = *36.62*		
Panel A: Estimates over all Offense Types								
CJA	0.0061***	0.0030**	0.0028*	0.0034**	3.26***	5.39***	5.27***	5.69***
(=1 if CJA attorney)	(0.0016)	(0.0015)	(0.0015)	(0.0014)	(1.45)	(0.44)	(0.43)	(0.44)
Black			0.0082***	0.0061***			15.80***	15.84***
(=1 if client is black)			(0.0016)	(0.0019)			(0.65)	(0.65)
US Citizen			-0.0072	-0.0032**			5.57***	5.42***
(=1 if client is a Citizen)			(0.0050)	(0.0016)			(0.52)	(0.52)
Likelihood Ratio					--	--	--	--
R-squared	--	--	--	--				
District FE	N	Y	Y	N	N	Y	Y	N
Year FE	N	Y	Y	N	N	Y	Y	N
District-Year FE	N	N	N	Y	N	N	N	Y
Crime Category FE	N	Y	Y	Y	N	Y	Y	Y
Panel B: Estimates by Offense Type								
Violent Offenses			-0.0021	-0.0016			5.05	5.67
			(0.0051)	(0.0032)			(5.15)	(3.16)
Property Offense			0.0032	0.0023			3.75***	3.59**
			(0.0024)	(0.0031)			(0.63)	(0.71)
Drug Offense			0.0116***	0.0111**			7.07***	6.37**
			(0.0022)	(0.0026)			(1.09)	(1.22)
Public Order Offense			0.0057*	0.0031			2.95*	3.00*
			(0.0027)	(0.0081)			(1.79)	(1.41)
Weapons Offense			0.0190***	0.0212***			34.14***	31.11***
			(0.0054)	(0.0061)			(8.66)	(8.41)
Immigration Offense			-0.0091**	-0.0027			-0.13	-0.02
			(0.0042)	(0.0038)			(0.32)	(0.31)
Likelihood Ratio					--	--	--	--
R-squared	--	--	--	--				
District FE	N	Y	Y	N	N	Y	Y	N
Year FE	N	Y	Y	N	N	Y	Y	N
District-Year FE	N	N	N	Y	N	N	N	Y
Crime Category FE	N	Y	Y	Y	N	Y	Y	Y

Note: Columns (1) through (4) report marginal effects evaluated at the mean [$\beta_j * \varphi(X'\beta)$] with standard errors reported in parentheses. Columns (5) through (8) have robust standard errors reported in parentheses. Coefficients that are significant at the .05 (.1, .01) level are marked with ** (*, ***). Offender characteristics included but not reported are variables for Native American descent, Asian descent, female, marital status, age and country of birth. Crime categories are 60 detailed BJS detailed crime subcategories.

Table 3. Probit Estimates of Plea Rate for different types of attorneys representing defendants

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Dependent Variable N = 46,167		E[Pr(*Plea* = 1)] = 0.9492				E[Pr(*Plea to Lesser Included Charge* = 1)] = 0.8032		
Panel A: Estimates over all Offense Types								
CJA	-0.0349**	-0.0226***	-0.0233***	-0.0227***	-0.0420**	-0.0921***	-0.0921***	-0.0877***
(=1 if CJA attorney)	(0.0022)	(0.0011)	(0.0021)	(0.0017)	(0.0139)	(0.0128)	(0.0118)	(0.0124)
Black			-0.0234***	-0.0186***			-0.0150***	-0.0169***
(=1 if client is black)			(0.0022)	(0.0022)			(0.0023)	(0.0023)
US Citizen			-0.0052	-0.0066***			-0.0046**	-0.0055***
(=1 if client is a Citizen)			(0.0075)	(0.0020)			(0.0021)	(0.0020)
Likelihood Ratio								
District FE	N	Y	Y	N	N	Y	Y	N
Year FE	N	Y	Y	N	N	Y	Y	N
District-Year FE	N	N	N	Y	N	N	N	Y
Crime Category FE	N	Y	Y	Y	N	Y	Y	Y
Panel B: Estimates by Offense Type								
Violent Offenses			-0.0184**	-0.0181**			-0.0132**	-0.0134**
			(0.0069)	(0.0073)			(0.0064)	(0.0063)
Property Offense			-0.0125***	-0.0119*			-0.0068*	-0.0080**
			(0.0050)	(0.0063)			(0.0041)	(0.0041)
Drug Offense			-0.0180***	-0.0172***			-0.0141***	-0.0137***
			(0.0030)	(0.0041)			(0.0027)	(0.0027)
Public Order Offense			-0.0151*	-0.0144*			-0.0081	-0.0072
			(0.0075)	(0.0073)			(0.0077)	(0.0077)
Weapons Offense			-0.0240***	-0.0239***			-0.0122**	-0.0135**
			(0.0063)	(0.0062)			(0.0061)	(0.0061)
Immigration Offense			-0.0164**	-0.0155*			-0.0031	-0.0007
			(0.0052)	(0.0071)			(0.0035)	(0.0035)
Likelihood Ratio								
District FE	N	Y	Y	N	N	Y	Y	N
Year FE	N	Y	Y	N	N	Y	Y	N
District-Year FE	N	N	N	Y	N	N	N	Y
Crime Category FE	N	Y	Y	Y	N	Y	Y	Y

Note: All columns report marginal effects evaluated at the mean. Standard errors are reported in parentheses. Coefficients that are significant at the .05 (.1, .01) level are marked with ** (*, ***). Offender characteristics included but not reported are variables for Native American descent, Asian descent, female, marital status, age and country of birth. Crime categories are 60 detailed BJS detailed crime subcategories.

Table 4. Decomposition of Difference in Expected Sentence into Performance and Selection Effects by Type of Crime

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Difference in Expected Sentence Decomposition Categories					
			Attorney performance (holding case characteristics fixed)			*Case characteristic (holding attorney performance fixed)*		
	Sample Size	*Difference in Expected Sentence Length*	*Difference in Probability of Plea Bargaining*	*Difference in Plea Bargained Sentences*	*Difference in Sentences after Trial*	*Difference in Probability of Plea Bargaining*	*Difference in Plea Bargained Sentences*	*Difference in Sentences after Trial*
For all Offense Types	46,167	7.76***	2.48**	0.42***	1.79**	-0.14	3.63***	-0.42***
		(1.29)	(1.10)	(0.18)	(0.70)	(0.23)	(0.06)	(0.03)
Violent Offenses	3,198	5.52**	2.45**	0.07***	0.70*	0.76	1.30***	0.24
		(1.26)	(1.16)	(0.03)	(0.42)	(0.71)	(0.23)	(0.16)
Property Offenses	2,340	3.71**	1.92**	-0.01	0.53***	-0.06	1.25***	0.08***
		(1.44)	(0.81)	(0.01)	(0.13)	(0.33)	(0.19)	0.06
Drug Offenses	16,880	12.03**	1.04***	0.11***	2.14***	0.70	3.35***	4.69***
		(1.38)	(0.32)	(0.01)	(0.09)	(0.45)	(0.28)	(0.11)
Public-order Offenses	2,746	6.18**	3.52***	0.11	0.68	-0.22*	3.28***	-1.19*
		(2.96)	(1.42)	(0.13)	(0.50)	(0.13)	(1.34)	(0.67)
Weapon Offenses	7,612	16.97***	14.29***	-0.10	5.98**	-0.72	-2.56*	0.08
		(1.78)	(1.35)	(0.12)	(2.54)	(0.53)	(1.31)	(0.10)
Immigration Offenses	13,391	-2.52	2.16	-0.03	0.26	-0.35	-0.71	-3.85
		(1.20)	(1.06)	(0.00)	(0.03)	(0.28)	(0.76)	(1.18)

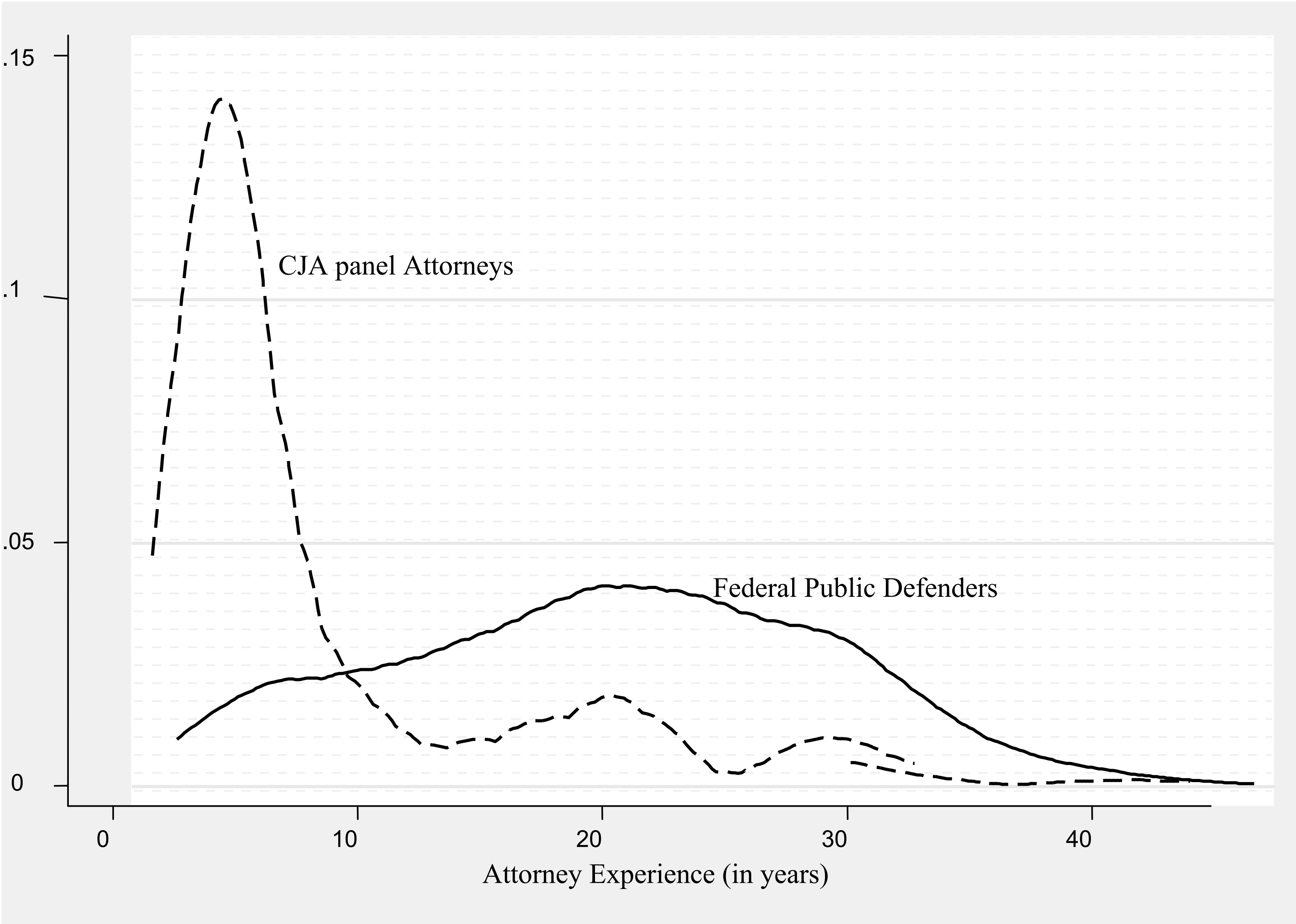
Notes: Bootstrapped standard errors are reported in parentheses. Estimates in columns (3) – (8) do not add up to column (2) due to rounding errors. Coefficients that are significant at the .05 (.1, .01) level are marked with ** (*, ***). All estimates control for state, year, and crime subcategory as well as for offender characteristics.

Table 5. Statistics and Estimates of the Relationship between Lawyer Characteristics and Lawyer Type

	(1)	*(2)*	*(3)*
Panel A: Summary Statistics for Attorney Characteristics, by Attorney Type			
	CJA Panel	*Public Defender*	*All Lawyers*
Avg. Wage (1997-2001)	71.54	76.63	77.93
	(2.31)	(16.11)	(17.54)
Experience for Lawyers	9.29	20.79	--
(Case Filing year – Year passed State Bar)	(6.59)	(9.71)	--
Law School "Quality"			
% in Tier 1 (ranked 1-10 in *US News & World Reports*)	4.22	9.63	--
% in Tier 2 (ranked 11-25 in *US News & World Reports*)	13.97	22.1	--
% in Tier 3 (ranked 26-50 in *US News & World Reports*)	18.34	16.99	--
% in Tier 4 (ranked 51-100 in *US News & World Reports*)	30.54	27.33	--
% in Tier 5 (ranked 101 – 134 in *US News & World Reports*)	7.60	9.85	--
% in Tier 6 (ranked 135 – 177 in *US News & World Reports*)	25.33	14.09	--
Attorneys in Sample	103	613	

Notes: Panel A reports standard deviations are reported in parentheses. Panel B reports marginal effects evaluated at the mean and standard errors in parentheses. Coefficients marked with ** (*,***) are significant at the .05 (.1, .01) level. Districts included are the Southern District of California, Central District of California, and Arizona.

Figure 1. Kernel Density Estimates of Indigent Attorney Experience for Lawyers Assigned to Cases 1997-2001



Notes: Districts included are the Southern District of California, the Central District of California, and the Federal District of Arizona. Experience is defined as years between case filing and bar admission. Estimates use optimal bandwidth and Epanechnikov kernel.

Table 6. Regression Estimates of the Effect of Attorney Characteristics on Case

Dependent Variable (N=2907)	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*
	E[Pr(*Guilty* = 1)] = 0.9812			E[*Sentence Length*] = 20.99			E[*Pr(Plea=1*] = 0.9744		
CJA	0.026	0.0064	0.0085	6.77*	2.61**	3.34***	- 0.024	-0.0114	-0.0034
(=1 if CJA attorney)	(0.013	(0.0148)	(0.0157)	(1.22	(0.88)	(1.06)	(0.014	(0.0180)	(0.0199)
Log Wage Gap		-0.0383**	--		-5.66*	--		0.0225*	--
$\log(wage_l) - \log(wage_{market})$		(0.0152)			(2.96)			(0.0113)	
Experience		-0.0115***	-0.0208***		-4.82***	-5.02***		0.0216**	0.0312***
(Year of case filing – Year attorney passed the bar)		(0.0034)	(0.0074)		(0.87)	(0.82)		(0.0086)	(0.0093)
Avg. indigent caseload		0.0131	--		0.06**	--		0.0085	--
(Average # cases assigned to attorney type in a district-year)		(0.0146)			(0.02)			(0.0138)	
Attended Tier 1 Law School		-0.0443***	-0.0448***		-8.37**	-8.93**		0.0598***	0.0605***
(=1 if attorney attended Tier 1 LS)		(0.0148)	(0.0148)		(3.86)	(3.87)		(0.0188)	(0.0188)
Attended Tier 2 Law School		-0.0534***	-0.0540***		-7.54***	-7.67***		0.0594***	0.0598***
(=1 if attorney attended Tier 2 LS)		(0.0170)	(0.0169)		(2.89)	(2.88)		(0.0215)	(0.0215)
Attended Tier 3 Law School		-0.0194	-0.0233		-1.64	-1.75		0.0350	0.0396
(=1 if attorney attended Tier 3 LS)		(0.0198)	(0.0198)		(3.30)	(3.30)		(0.0251)	(0.0252)
Attended Tier 4 Law School		0.0221	0.0244		0.37	0.79		0.0240	0.0271
(=1 if attorney attended Tier 4 LS)		(0.0151)	(0.0151)		(2.94)	(2.94)		(0.0192)	(0.0192)
Attended Tier 5 Law School		0.0046	0.0065		1.28	1.56		0.0018	0.0040
(=1 if attorney attended Tier 5 LS)		(0.0180)	(0.0180)		(3.51)	(3.51)		(0.0229)	(0.0228)
District FE	Y	Y	N	Y	Y	N	Y	Y	N
Year FE	Y	Y	N	Y	Y	N	Y	Y	N
District-Year FE	N	N	Y	N	N	Y	N	N	Y

Notes: Marginal effects evaluated at the mean reported in Columns (1) through (6). Standard errors are reported in parentheses. Coefficients marked with ** (*,***) are significant at the .05 (.1, .01) level. . All columns include crime category fixed effects. Sample uses 2908 observations from the Southern District of California, Central District of California, and the Arizona District. Law School Tiers are based on the *U.S. News and World Reports* 2001 Law School Ranking. Average wage is based on the average lawyer wage from the Occupation Employment Survey from the Bureau of labor Statistics.

Table 7. Estimates of the Effect of Attorney Characteristics on Case Outcome and Sentence Length by Attorney Type

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Dependent Variable Mean (N=2907)	*E[Pr(guilty = 1)]* = 0.9812		*E[Sentence Length]* = 20.99		*E[Pr(plea = 1)]* = 0.9744	
	PD	*CJA*	*PD*	*CJA*	*PD*	*CJA*
Log Wage Gap	-0.0367**	-0.0560***	-4.22**	-5.86**	0.0291**	0.0512**
$\log(wage_i) - \log(wage_{market})$	(0.0175)	(0.0187)	(1.64)	(1.42)	(0.0122)	(0.0236)
Experience	-0.0115***	-0.0117***	-5.04***	-4.77***	0.0218**	0.0213*
(Year of case filing – Year attorney passed the bar)	(0.0015)	(0.0052)	(1.38)	(1.05)	(0.0111)	(0.0109)
Avg. indigent caseload	0.0611**	0.0085	-9.72**	-6.84**	0.0586**	0.0264**
(Average # cases assigned to attorney type in a district-year)	(0.0298)	(0.0138)	(2.65)	(2.31)	(0.0292)	(0.0103)
Attended Tier 1 Law School	-0.0512***	-0.0421	-7.90***	-7.10**	0.0484**	0.703**
(=1 if attorney attended Tier 1 LS)	(0.0173)	(0.0291)	(2.53)	(1.38)	(0.0219)	(0.0395)
Attended Tier 2 Law School	-0.0215	-0.0545*	-2.65	-1.17	0.0482	0.0531
(=1 if attorney attended Tier 2 LS)	(0.0221)	(0.0232)	(4.52)	(1.21)	(0.0279)	(0.0293)
Attended Tier 3 Law School	-0.0186	-0.0111	-1.82	-1.12	0.0343	0.0801***
(=1 if attorney attended Tier 3 LS)	(0.0216)	(0.0232)	(1.64)	(1.50)	(0.0273)	(0.0293)
Attended Tier 4 Law School	0.0211	0.0272	0.73	0.18	0.0315	0.0207
(=1 if attorney attended Tier 4 LS)	(0.0180)	(0.0238)	(0.87)	(2.17)	(0.0227)	(0.0300)
Attended Tier 5 Law School	0.0040	0.0084	1.80	1.15	0.0059	0.0016
(=1 if attorney attended Tier 1 LS)	(0.0213)	(0.0301)	(1.36)	(1.43)	(0.0270)	(0.0380)
District Fixed Effects	Y	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y	Y
Crime Category Fixed Effects	Y	Y	Y	Y	Y	Y

Notes: Parameters reported are marginal effects evaluated at the mean. Standard errors are reported in parentheses. Coefficients marked with ** (*,***) are significant at the .05 (.1, .01) level. All regressions include district, year and crime category fixed effects. Districts included are the Southern District of California, Central District of California, and Arizona. Law School Quality is the average rank of law schools, based on the *U.S. News and World Reports* 2001 Law School Ranking. Average wage is based on the average lawyer wage from the Occupation Employment Survey from the Bureau of labor Statistics