Three Strikes and You're Out: A Triple Differences Approach to Estimating the Deterrent Effect of California's Three Strikes Law

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Abstract: I estimate the deterrent effect of California's Three Strikes Law, using data from before and after the law was passed in combination with data from states other than California to perform a difference-in-difference-in-differences analysis. Using this unique combination of data sets enables me to control for the effect of omitted factors that seem to have influenced the results of previous authors. While I find evidence suggesting that the law operates to deter all criminals in California, I do not find significant evidence to support the idea that the law's harsher sentences provide an additional deterrent effect for criminals with one or two strikes. I can rule out a deterrent effect greater than 6.9 percentage points, or 15.5%, for criminals with one strike, and 3.3 percentage points, or 6.9%, for criminals with two strikes. In addition, I find that the law's unorthodox penalty structure creates incentives that cause individuals with two strikes to be 9.2 percentage points more likely to commit a serious or violent felony, conditional on recidivating. On the other hand, I find that individuals with one strike reduce their likelihood of committing a serious or violent felony by 8.2 percentage points, conditional on recidivating. I determine that the law falls far short of being a cost-effective method of reducing crime through deterrence.

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I. Introduction

From 1960 until the early 1990s, crime rates across the United States skyrocketed (Figure 1).[1] By 1991, property crime rates were at 300% of their 1960 per capita level, and even more alarming was the fact that violent crime rates rose by 450% over this same time period. State, local, and the federal government felt compelled to do something to stop this rapid growth of crime. The response of many states and the federal government came in the form of harsher sentencing for previously convicted criminals. Many of these laws were termed "three strikes and you're out" because of the extremely harsh sentences imposed on criminals who were convicted of a third felony.

The purpose of these laws was two-fold: to incarcerate habitual criminals for long periods of time, thus preventing them from committing crimes through incapacitation, and to increase penalties enough so that it was no longer worthwhile for criminals with one or two strikes to recidivate, thus preventing crime through deterrence. While many states had passed these laws, they were rarely enforced in most states and were generally "designed to have maximum symbolic impact while not making major changes in case outcomes."[2] Since it went into effect on March 6, 1994, the harshest and most consistently enforced of these laws has been the California Three Strikes Law. Frank Zimring, in his landmark study of the law, states that the California Three Strikes Law "was a revolutionary assault on penal practices in the nation's largest state."[3] The most controversial part of the California Three Strikes Law is that its mandatory sentences enhancements are enforced regardless of the severity of the crime that an individual commits. This means that criminals with two strikes face the same 25 year to life penalty if they are convicted of burglary or murder. In addition, criminals with two strikes face a

[1] Figure 1 data is publicly available at http://bjsdata.ojp.usdoj.gov/dataonline.
[2] Zimring, Hawkins, and Kamin (2001), 27.
[3] Ibid., 28.

sentence that is, on average, over 8 times longer than criminals with one strike face for the same crime. This is controversial because the law mandates huge increases in incarceration for what are sometimes relatively minor felonies. California has used its three strikes law to jail far more offenders than all of the other states and the federal government combined.[4] Therefore, the California Three Strikes Law creates a perfect natural experiment which can be used to analyze the effects of harsher sentences on criminal recidivism.

The cost of enforcing a law that mandates steep increases in incarceration for so many offenders, as the California Three Strikes Law does, is very high. The increase in operating costs has been conservatively estimated by the California Legislature to be close to $500,000,000 annually.[5] It is necessary to determine how much crime this type of legislation actually reduces in order to determine whether or not it is an efficient use of state resources. It is clear that this law works through incapacitation, by imprisoning the worst recidivists and physically preventing them from committing crimes. What is unclear is whether or not this law operates through deterrence, by preventing criminals who are outside of prison from committing a crime. Determining whether or not there is a deterrent effect to such an expensive and controversial law is important because reducing crime through deterrence is more cost effective than reducing crime through incapacitation. This is because if a law deters an individual from ever committing a crime, the state does not have to pay to incarcerate that individual and the damage of the crime is avoided. Thus, through deterrence the state produces the reduction in crime at no cost to itself.

While previous research has addressed the question of whether or not the three strikes law reduces crime, most previous authors have only used data from within California and from

[4] As of 2004, twenty-three states had three strikes laws. California had jailed 42,322 offenders. Georgia had jailed the second most, with 7,631. Florida and Maryland were the third and fourth most, with 1,628 and 330 offenders, respectively. <http://www.libraryindex.com/pages/2552/Sentencing-THREE STRIKES-YOU-RE-OUT.html>.
[5] Iyengar 2008, 25.

after the law was passed. This paper improves on the previous research by using data for criminals in multiple states, as well as data for individuals from both before and after the law was passed. When looking only at individuals within California, it is possible to create treatment and control groups based on criminals with similar prior criminal behavior who face substantially different penalties for their next crime. Using data from different states makes it possible to also control for factors that are constant across states and affect the recidivism rates of individuals with one and two strikes differently. Similarly, using data from before and after the law was passed makes it possible to control for factors that affect the recidivism rates of individuals in California and are constant across time. It is important to control for factors that cause differences in recidivism rates between the types of criminals in the post-law treatment group and the post-law control group. If these factors are properly controlled for, any differences in recidivism between these two groups can be attributed to the deterrent effect of the three strikes law. By combining these cross-state and cross-time approaches, I perform a triple differences analysis, which enables me to obtain a more unbiased estimate of the law's true deterrent effect.

When limiting the sample to individuals with very similar criminal histories who face different penalties under the three strikes law, I find no evidence of a deterrent effect for the law. I can rule out any deterrent effect greater than 3.3 percentage points for individuals with two strikes, and 6.9 percentage points for individuals with one strike, which are smaller estimates than those found by many previous authors. My results suggest that, prior to the passing of the three strikes law, individuals with two strikes[6] in California may have already been less likely to recidivate than similarly situated individuals with one strike. Similarly, I find that individuals

[6] Throughout the paper, I will refer to individuals as having "strikes". In reality, only individuals in the post-law group in California can have strikes because they are the only ones at risk for the three strikes law. But when discussing individuals in the pre-law group or in other states, if I say that they have strikes that means that, given their criminal history, if they were in the post-law group in California they would have strikes.

with one strike in California were less likely to recidivate than comparable individuals with no strikes, even in the absence of the three strikes law. These results are sensitive to many specifications, including allowing the effect of the law to vary based on an individual's age. Therefore, previous authors who only analyze data from after the passing of the law fail to account for this source of omitted variable bias. I also find evidence that criminals alter their choice of crimes in response to the increased sentences of the three strikes law. In response to the law's penalty structure, individuals with two strikes are 9.2 percentage points more likely to commit a serious or violent felony, conditional on recidivating, than are comparable individuals with one strike. Individuals with one strike, however, are 8.2 percentage points less likely to commit a serious or violent felony than comparable individuals with no strikes. These changes can be attributed to the incentives created by the perverse structure of the law's penalty enhancements.

This paper will proceed as follows. Section II provides an overview of previous literature on crime deterrence. Section III provides a brief summary of how the three strikes law works, and Section IV continues this discussion while also explaining previous research on the deterrent effect of the three strikes law. Section V describes the data sets that I am using. Section VI lays out the identification procedure for the treatment and control group methodology. Section VII explains my methodology and presents my results. Section VIII provides various sensitivity analyses for these results. Section IX discusses these results. Section X describes a less restrictive methodology, which allows for much greater sample sizes, and presents the results of this analysis. Section XI provides various sensitivity analyses for these results, Section XII discusses these results in comparison to my previous results, Section XIII provides a cost-benefit analysis of the law, and Section XIV concludes.

II. Previous crime literature

a. Factors that affect crime rates

Whether or not governments can deter crime through policy is a hotly debated topic. Many scholars point to various policy measures, such as harsher criminal sanctions, increased police force, or legalized abortion as having large, crime-reducing effects. At the same time, many authors claim that these policies have little or no impact on crime rates. In their 2008 paper "What Do Economists Know About Crime?" Dills, Miron, and Summers examine the relationship between crime rates over extended periods of time and across countries, and compare them with factors that are often thought to be deterrents to crime, such as arrest rate per capita, the size of the police force, the incarceration rate, and capital punishment. They do not find any evidence that increases in these variables have an effect on the crime rate. They claim that their results "present a challenge to key aspects of the deterrence model of crime", specifically the "more X, less crime" reasoning behind much of government policy.[7] After performing a regression of crime rates on a large number of potential deterrent effects, they state that their results "are not consistent with the view that standard deterrence variables, as well as other factors recently addressed in the economics of crime literature, are robust determinants of crime."[8] Their findings suggest that any analysis which claims that any one factor plays a major role in crime reduction should be heavily scrutinized. Furthermore, their results seem to suggest that social scientists know relatively little about the causes of changes in crime rates as well as the deterrent effects of different government policy measures. One clear question that their methodology fails to address is the idea of reverse causality; an area with a high crime rate is probably likely to hire more police to address this issue, or to have higher abortion rates, or to

[7] Dills, et al. 2008, 12
[8] Ibid. 21.

have a higher incarceration rate. They acknowledge that this is a problem, but state that because none of the major variables that are commonly associated with reducing crime are correlated with lower crime rates in their simple one variable regressions, any research that claims to find such a relationship must be subjected to serious scrutiny. Because they fail to address the issue of two-way causality, their results cannot be taken to show anything more than that.

Steven Levitt's 2004 paper "Understanding Why Crime Fell in the 1990s: Four Factors That Explain the Decline and Six That Do Not" seeks to do exactly what Dill, et. al., are skeptical of and identify the factors that caused crime to drop rapidly during the 1990s. From 1991 through 20001, crime rates dropped at an unbelievable rate in every crime category and in every state across the nation. Levitt claims that "four factors . . . can account for virtually the entire observed decline in crime: increase in the number of police, the rising prison population, the waning crack epidemic and the legalization of abortion."[9] Three of these factors were explicitly rejected by Dill, et al. as having no major effects on crime rates, yet Levitt claims that they account for the majority of the 1990s unprecedented drop in crime rates. Levitt's analysis of the 1991-2001 period is not exactly analogous to Dill, et al.'s claims because Dill, et al. are looking at longer periods of time. When Levitt extends his analysis over the period 1973-1991, he finds that rising prison populations should have reduced crime rates by 20-30%, but this reduction in crime was mitigated by the rise of the crack epidemic.[10] He also finds that adding more police and legalized abortion worked to reduce crime during this period, but had a much weaker effect than during the 1990s. Levitt concludes that of the four factors mentioned above, "only rising numbers of police and legalized abortion are likely to be continuing contributors to

[9] Levitt 2004, 164
[10] Ibid. 184

declines in crime rates."[11] The debate between Dill, et al., and Levitt is typical of the debate in much of this literature and shows that it is unclear how much social scientists know about crime.

Another paper written by Levitt in 1996, "The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Litigation," has a reasonable approach to addressing the reverse causality problem discussed by Dill, et al. (2008). Levitt (1996) is attempting to estimate the effect of incarceration rates on crime rates, and must overcome the simultaneity between crime rates and incarceration rates. In other words, he must overcome the fact that it is difficult to determine the causal effect of changes in prison populations on crime rates because prison populations and crime rates increase simultaneously. In order to do this, he uses the status of prison overcrowding litigation within a state as an instrument for changes in the prison population. Prison overcrowding litigation refers to litigation that forces a state to address the problem of overcrowding in its prisons. This problem can be remedied by shifting prisoners between facilities, expanding existing facilities, sending prisoners to other states, or, in the most extreme cases, by releasing prisoners. Levitt focuses on states where the entire prison system has come under court supervision. Overcrowding in these states is so severe that they are being forced to release prisoners. To test the validity of prison overcrowding litigation as an instrument, Levitt shows that the status of overcrowding litigation is strongly correlated with decreases in prison population growth rate. He also shows that changes in crime rates do not predict future overcrowding litigation, thus mitigating the concern that his results are being driven by reverse causality. Therefore it seems likely that the status of prison overcrowding litigation is a valid instrument. He finds that the instrumenting produces estimates of the effect of changes in incarceration rates on crime rates that are almost four times larger than the same estimates without instrumenting. His overall finding is that incarcerating the marginal prisoner

[11] Ibid. 186

leads to an annual reduction of 5.5 reported Index I crimes[12] and 15 total Index I crimes, and the benefit of incarcerating this individual exceeds the cost to society of doing so.[13] A reduction of 15 crimes annually much larger than the estimates found by most previous research, and is clear evidence supporting the theory that incapacitation significantly reduces crime. His estimates are robust when the sample is expanded to include all states, as well as when looking at different types of crime.

These findings have important implications for the effect of the California Three Strikes Law, because they imply that even if the law is unable to deter criminals, it can still reduce crime rates through an incapacitation effect, by imprisoning individuals for longer periods of time.[14] Levitt states that "if the amount of incarceration is to be increased, keeping the current pool of prisoners behind bars for a longer period of time is likely to be a more advisable public policy approach" than incarcerating more prisoners.[15] The three strikes law operates by giving longer sentences to certain individuals who would still have been imprisoned in the absence of the law, and therefore operates in the manner that Levitt describes. Therefore, it seems likely that even if the three strikes law fails to deter criminals, it may still operate through an incapacitation effect.

[12] Index I crimes are felonies, including but not limited to murder, rape, assault, robbery, burglary, larceny, and autho theft.
[13] These estimates are evaluated at the 1993 sample mean, and are based on reporting rates from the National Crime Survey (US Department of Justice 1992).
[14] This paper is not concerned with the law's incapacitation effect. Presumably, by locking up the worst recidivist offenders for very long periods of time, the law reduces far more crime through incapacitation than it does through deterrence.
[15] Levitt (1996), 347.

b. Deterrence literature

There has also been much research done in order to determine whether or not harsher criminal sanctions operate to reduce crime through a deterrent effect. Lee and McCrary (2005) exploit the fact that individuals face much harsher penalties upon turning 18 in order to see if individuals who are just under 18 are more likely to commit a crime than individuals who are just over 18. They restrict their sample to juveniles who have previously been arrested, in order to make it more likely that the individuals understand the change in severity of penalties. Penalties for adults are three to six times longer than those faced by juveniles, so if criminals are acting rationally they would be less likely to commit a crime after they turn 18. This is a particularly good natural experiment for evaluating the effect of harsher sentences on criminal activity because there are virtually no observable and most likely no unobservable differences that affect recidivism between an individual who is 17 years and 364 days old and an individual who is 18 years and 1 day old. Therefore any difference in the likelihood of recidivating between these two individuals could likely be attributed to the deterrent effect of the harsher sentences that the 18 year old individual faces.

Lee and McCrary find that there is no significant change in the likelihood that an individual is arrested on or at any time around his 18th birthday. Therefore they conclude that the harsher penalties that young criminals face when they turn 18 do not deter any crime. These results are robust even when juvenile transfer to adult courts and expungement of juvenile records are considered. Lee and McCrary also calculate the discount rates that would be consistent with their results if a rational model applies, and find them to be very high. They believe this to be evidence of either hyperbolic discounting or extremely high exponential discount rates. As Lee and McCrary state, “under hyperbolic discounting, a tripling of

incarceration lengths can potentially have a smaller proportional response in the short-run than in the long run."[16] Because the average incarceration length that they are examining is relatively short, between 2.6 weeks and 6.4 weeks, "a certain degree of myopia, which allows short-run discount factors to be smaller than long-run discount factors, can explain small effects in short time horizons."[17] Therefore, they believe that the reason that they do not find evidence of a deterrent effect is because these young criminals have high exponential discount rates or are practicing hyperbolic discounting.

It is unclear whether Lee and McCrary's findings of a minimal deterrent effect will be applicable to the harsher penalty increases of the California Three Strikes Law. First of all, the three strikes law's sentences are much longer than those examined by Lee and McCrary. The average sentence increase for individuals with one strike is over 21 months, while the average sentence increase for individuals with two strikes is 16.6 years.[18] Secondly, the average criminal sentenced under the three strikes law is over 30 years old. It seems likely that these older criminals will respond differently to an increase in penalties than the young criminals that Lee and McCrary are examining.

Drago, Galbati, and Vertova (2009) examine the effect of the Collective Clemency Bill, which was an "exogenous manipulation of prison sentences" on the behavior of prisoners released from prison in Italy.[19] This law simultaneously released 22,000 inmates before their sentences were fully served on the condition that, if they were arrested within five years of their release, the remaining time on their current sentence would be served in addition to the sentence imposed for the new offense. Therefore, these individuals would face sentences between 1 month

[16] Lee and McCrary 2005, 24.
[17] Ibid., 25.
[18] See the Cost-Benefit Analysis at the end of this paper for an explanation of how these numbers were calculated.
[19] Drago, et al., 2009.

and 3 years longer on their subsequent arrests. Furthermore, these individuals are representative of the criminal population, so any changes in their recidivism relative to other criminals can be attributed to the effects of this exogenous sentence manipulation. They use a logit model and estimate the marginal effects at the mean, and find that an additional month that would be served on a future conviction reduced the probability of recidivating by 1.3%. Therefore their evidence supports the idea that harsher sentences can have a deterrent effect.

In order to look for a deterrent effect for harsher criminal sanctions, it is ideal to have a law that imposes harsh sentences on one group of criminals while sentencing a group of similar criminals much less harshly. Under the California Three Strikes Law, individuals with two strikes face a sentence that is over eight times longer, on average, than individuals with one strike would face for the same crime.[20] Therefore this law provides an excellent natural experiment for investigating the deterrent effect of harsher criminal sanctions.

III. California's Three Strikes Law

The California Three Strikes law was designed to target recidivists by sharply increasing the penalties that a criminal who has been convicted of a serious felony faces for all subsequent felony convictions. The law first defines certain "triggering offenses", which are made up of the most violent and serious felonies. Table 1 shows a list of these offenses.[21]

If convicted of a triggering offense, the individual is said to have one "strike" for the purpose of future convictions. If an individual has one strike, he faces a mandatory doubling of the minimum sentence for his next felony conviction.[22,23] Generally, a crime is considered a

[20] Brown and Jolivette, 2005.
[21] California Penal Code, Section 1192.7.
[22] This and much of the subsequent information on the Three Strikes Law comes from California Penal Code, Section 667.

felony if it results in at least 1 year of imprisonment, therefore a double sentence length results in a penalty increase of at least 1 year. If a defendant has two strikes, in other words has been convicted of a "triggering offense" on two separate occasions,[24] the minimum sentence that he faces is either three times the normal punishment or 25 years in prison upon his next conviction for any felony, whichever is greater. These sentence enhancements for offenders with one or two strikes are enforced if the individual commits *any* felony, not just a serious or violent felony. Furthermore, the law mandates consecutive sentencing for multiple offenses committed by individuals with strikes, so an individual with two strikes who commits two subsequent felonies at the same time will face a minimum term of 50 years in prison if convicted for both offenses.[25] Until 1997, judicial discretion was not allowed under this law, so the law was enforced very stringently.[26] The discussion of this law continues in more detail in the next section.

IV. Three Strikes Literature

In their 2005 paper "A Primer: Three Strikes—The Impact After More Than a Decade," Brown and Jolivette examine the effects of the three strikes law in the ten years after it was passed. They find that, as of January 31, 2004, California had sentenced over 7,500 third strike and 80,000 second strike offenders under the sentence enhancements of the three strikes law. At that time, there were 43,000 prisoners in California serving extended sentences under the three strikes law, 7,500 of whom were serving sentences of at least 25 years for a third strike conviction. In the ten years since the law was passed, the average sentence imposed on

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[23] See Figure 2 for a clear explanation of the way in which sentence enhancements are employed under the three strikes law.
[24] These convictions for triggering offense must be on two separate occasions. Individuals convicted for aggravated assault and robbery during the same trial, for example, only receive one strike.
[25] The number of consecutive sentences is not limited under the three strikes law. Brown and Jolivett, 2005.
[26] Iyengar 2008, 16.

individuals with one strike who committed a subsequent felony was 43 months. More importantly, however, is the fact that ten years after the law was passed no individuals with two strikes who were convicted of a subsequent felony had been released from prison. Furthermore, the *earliest that any of these prisoners would be eligible for release is 2014*.[27] In examining the effect of the law on crime rates, Brown and Jolivette state that, while the crime rate in California fell by 43% in the ten years following the law's passage, "there is little consensus among researchers about the impact of three strikes on public safety."[28] They find that, while crime rates fell dramatically during this time period, the three strikes law only accounted for little, if any, of the decrease in crime rates in California. They propose several alternative explanations for why the law may not have been as effective at deterring crime as initially thought. Some counties are less faithful in enforcing the laws, which lessens the impact that the law has on the state as a whole. Furthermore, crime rates plummeted across the entire country since the passing of the law, and therefore there were fewer individuals eligible for three strikes sentencing than initially projected. Finally, "strike" offenders only account for approximately 11% of crime in California, so even a complete negation of the crime targeted by the three strikes law would only reduce California crime rates by 11%. Because of these three factors, any research attempting to identify the crime reduction produced by the law will be looking for a very small reduction in crime relative to the overall crime rate in California.

Several economists use clever instruments in an attempt to get around the problems of obtaining an unbiased estimate of the deterrent effect of the three strikes law. One of these papers, by Iyengar (2008), has received a lot of attention recently, but is unfortunately based on an incorrect interpretation of the way in which the law operates. Iyengar's mistake is that she

[27] That is, any prisoner sentenced for a third strike in 1994 would become eligible for parole in 2019 because the sentence for a third strike is 25 years to life.
[28] Brown and Jolivette, 2005.

believes that the ordering of crimes matters when determining what types of crimes count as first and second strikes under the three strikes law. Her claim is that, once an individual commits a triggering offense, all subsequent felonies count as strikes. As she says:

> 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 individual was previously convicted of an aggravating offense. (10, author's emphasis)

This is a fundamental misunderstanding of how the law works. In reality, while all subsequent felonies after the first or second strike receive harsher sentencing under the law, only the serious and violent felonies, which I call "triggering" offenses, can count as first or second strikes. The motivation for her methodology is based on this incorrect interpretation of the law. She compares individuals who committed a strikeable offense followed by a non-strikeable felony, with individuals who committed the same two crimes in a reverse order. As she describes:

> This paper uses the unique structure of Three strikes law in which offenders with the same criminal history but different ordering of crime commission face different sentencing eligibility to identify the effect of Three strikes sentencing eligibility on criminal activity . . . This meant that individuals who committed a "triggering" offense followed by a felony faced different potential sentences than those who committed a felony and then a "triggering" offense.[29]

If her interpretation of the law were correct, she would have created valid one strike and two strike groups. But because not all felonies can be counted as as a first or second strike, her two strike group will contain some individuals that actually have one strike. Therefore, differences in recidivism between her two strike group and her one strike group will underestimate the true deterrent effect of the third strike.

[29] Iyengar 2008, 4.

Iyengar claims[30] to have taken her interpretation of the California law from the book Punishment and Democracy by Zimring, Hawkins, and Kamin, but it is clear that she is interpreting the law differently than they do. Figure 2 is copied from their book, and provides a very clear illustration of how the sentencing under the three strikes law works.[31] Iyengar's misinterpretation of the law is made clear when comparing Figure 2 with her explanation of individuals A and B on page 14 of her paper. Iyengar states:

> Consider the following example with two criminals both of whom have previously committed a theft and a burglary. Criminal A first committed a theft and then committed burglary. Criminal B first committed a burglary and then committed a theft. . . After the Three strikes law change, the ordering of the crimes committed matters. Because burglary is a triggering offense, it activates Three strikes sentencing. All felonies committed after the activation of Three strikes then count as strikes. Thus, if individual A commits a new offense, that offense will count as a second strike since he has committed no offenses after the burglary. In contrast, a new offense committed by individual B will count as a third strike because he committed a theft after committing a burglary. Thus in the post-period, individuals A and B are exposed to different penalties based on the ordering of their previously committed crimes.

These two criminals are identical to Offenders A and B from Figure 2, which is produced directly from Zimring, et al. (2001). Offender A in Figure 2 committed a theft, followed by a burglary, and Offender B committed a burglary followed by a theft. As Figure 2 clearly shows, when either of these offenders is convicted of a subsequent burglary, they both face the same penalty of double the burglary sentence. Under Iyengar's interpretation of the law, Offender A would receive double the sentence and Offender B would receive a sentence of 25 years to life in prison. In reality, both of these offenders have only one strike, and therefore they each receive double the penalty for their next crime. Figure 2 shows that ordering of crimes does matter for sentencing on the current offense, but not for determining future sentence enhancements. Offender A had no sentence enhancement for his theft because he had no strikes at the time,

[30] She told me this in my personal communications with her.
[31] My Figure 2 is Table 1.2 from Zimring 2001, 10.

Offender B received double the sentence for theft because he had one strike at the time, and Offender C received 25 years to life for his theft offense because he already had two strikes. It seems that Iyengar misinterpreted the role that the ordering of crimes plays in the three strikes law.

This excerpt from D'Addessa (2003) makes it clear that Iyengar's interpretation of the law is incorrect:

> To qualify as a first and second strike under the Three Strikes law, a defendant must be convicted of a serious or violent felony. Serious felonies are enumerated by statute and include such offenses as carjacking, robbery, grand theft involving a firearm, and first degree burglary. Violent felonies are also enumerated by statute and include such offenses as arson, kidnapping, robbery, and voluntary manslaughter. Many of the specified offenses overlap and can be characterized as both serious and violent felonies. Convictions for any of these offenses are called prior felony convictions for purposes of the Three Strikes law and are counted as strikes regardless of whether the accompanying sentence was suspended or stayed, or whether imprisonment was served in a mental health facility rather than a prison. A defendant's third strike, which triggers the harshest penalties under the Three Strikes law, can be any felony conviction under state law. Put simply, the conviction does not have to be for a serious or violent felony to constitute a third strike.

This clearly shows that only serious or violent felonies can be counted as strikes. Therefore, Iyengar's estimation strategy is not valid, as she is incorrectly sorting individuals in her sample into the one strike and two strike groups. She mistook the wording of the law, which says that strikes are "prior felony convictions" to mean any prior felony convictions, whereas a closer reading of the law makes it clear that it is referring only to the serious and violent felonies that are listed in Section 1192.7.[32]

Helland and Tabarrok (2006) interpret the law as I have defined it in Section II. In describing how they interpret the law to function, they say:

> A "strike" is a conviction for a serious or violent felony as these are laid out in the California Penal Code . . . A criminal with one strike who is convicted of *any*

[32] California Penal Code section 667

> subsequent felony (not necessarily a strike) faces an automatic doubling of the sentence length on that conviction and cannot be released prior to serving 80 percent of the sentence length. A criminal with two strikes who is convicted of any subsequent felony faces a prison sentence of 25 years to life and cannot be released prior to serving at least 80 percent of the 25 year term.[33]

They recognize that, while "*any* felony" can receive a sentence enhancement under the three strikes law, only convictions for strikeable offenses, which are "serious or violent felon[ies] as these are laid out in the California Penal Code," can count as first or second strikes. Therefore, Iyengar's individuals A and B have each only committed one strikeable offense, and therefore only face double the sentence for their next felony convictions. Iyengar, on the other hand, believes that after the first "strike", all felony convictions count as record enhancing "strikes". Therefore, she believes that individual B, who was convicted of burglary, which is a strikeable offense, and then theft, which is a felony but is not a strikeable offense, has two strikes and faces the penalty of 25 to life upon his next felony conviction. In reality, the theft conviction cannot act as a first or second strike because it is not defined as a serious or violent felony by the California Penal Code. I shall go on to describe Iyengar's strategy and findings because her paper has recently received a lot of attention, as well as because her article has other interesting ideas that inspired parts of my econometric analysis.

Iyengar's data tracks criminals in San Francisco, San Diego, and Los Angeles from 1990-1999.[34] She performs a difference-in-differences analysis for before and after the law went into effect in March of 1994, using a two stage least squares regression. She limits her dataset to all individuals who committed an offense after 1990, and compares how their recidivism rates changed from before 1994 to after 1994 for individuals with one-strike and two-strikes, relative to individuals with no strikes. She controls for an individual's prior criminal history by including

[33] Helland and Tabarrok 2005, 309-310.
[34] She ends up limiting this sample to individuals released no later than 1997, because she requires two years of tracking for each individual to determine whether or not they recidivated within 2 years.

variables for the number of times that an individual has been convicted of each of seven crime categories. The seven offense categories are murder, rape, assault, robbery, burglary, theft, drug crimes, and other crimes. She also controls for the individual's Felony Rate per Criminal Year, which is the number of crimes that the individual has committed per year that he has been out of prison since turning 18. Equation (1) shows the equation that Iyengar estimates:[35]

$$R = \beta_0 + \beta_1(1Strike) + \beta_2(2Strikes) + \beta_3(1Strike * AfterLaw) + \beta_4(2Strikes * AfterLaw) + Prior\ Criminal\ History + Felony\ Rate\ per\ Criminal\ Year + Individual\ Controls \quad (1)$$

In this equation, *R* is a dummy which is 1 if the individual recidivates within 2 years of his release from prison and 0 if he does not.[36] *1Strike* is a dummy variable which takes a value of 1 if the individual has one strike and 0 otherwise. *2Strikes* is a dummy variable which takes a value of 1 if the individual has two strikes and 0 otherwise. $1Strike * AfterLaw$ is a dummy variable which takes a value of 1 if the individual has one strike and is being tracked after the three strikes law went into effect. $2Strikes * AfterLaw$ is a dummy variable which takes a value of 1 if the individual has two strikes and is being tracked after the three strikes law went into effect. The coefficients of interest are β_3 and β_4, which show the change in recidivism for individuals with one strike and two strikes that can be attributed to the increased sentences that they face under the three strikes law. If the law is effectively deterring criminals with one and two strikes, β_3 and β_4 would be negative. Additionally, if the 25 year to life penalty faced by criminals with two strikes has more of a deterrent effect than the double sentence faced by criminals with one strike, β_4 will be more negative than β_3.

[35]This is taken from Iyengar's equation 7, on page 17 of her paper. The variables have been renamed to make it more clear what they represent.

[36] I believe that Iyengar lets R take a value of 1 if the individual is arrested for any crime during the 2 year tracking period and 0 if not, although she does not state so explicitly.

Because Iyengar is allowing individuals to enter the one strike and two strike groups after the three strikes law was passed, she must address the problem that the number of strikes that an individual commits after 1994 may endogenous. In other words, after March 1994, the number of strikes that an individual has at the beginning of his two year tracking period may have been affected by the three strikes law, as criminals may have tailored their criminal behavior to avoid committing strikes. She uses an individual's number of strikes based on his prior criminal history before the law change as an instrument for an individual's "true" number of strikes to address this problem. To do this, she constructs four variables based on an individual's criminal history prior to 1994, excluding the individual's most recent offense: *1Strike_pch, 2Strikes_pch, 1Strike_pch*AfterLaw,* and *2Strikes_pch*AfterLaw*. *1Strike_pch* and *2Strikes_pch* are dummy variables which take a value of 1 if the individual has 1 or 2 strikes based on the crimes that he committed prior to 1994, excluding the most recent offense. *1Strike_pch*AfterLaw* and *2Strikes-_pch*AfterLaw* are dummy variables which take a value of 1 if the individual has either 1 or 2 strikes based on the crimes that he committed prior to 1994, excluding his most recent offense, and the individual is being tracked after the three strikes law was passed. In other words, what she is doing is constructing equivalents of the explanatory variables in equation (1), using only crimes that were committed before 1994 and before the current arrest. She uses each of these variables to instrument for their respective non-pch equivalent—*1Strike, 2Strikes, 1Strike*AfterLaw*, and *2Strikes*AfterLaw*—and then uses the predicted values of *1Strike, 2Strikes, 1Strike*AfterLaw,* and *2Strikes*AfterLaw* to estimate the probability that an individual recidivates within 2 years of his release, using equation (1).

Iyengar finds that the California Three Strikes Law reduces arrest rates for individuals with one strike by 9 percentage points, or 18%, and reduces arrest rates for individuals with two

strikes by 14 percentage points, or 28%. In addition, she finds that the non-instrumented OLS estimates of recidivism are upwards biased, which is consistent with the idea that criminals were tailoring their behavior to avoid committing strikes after the law was passed. The additional deterrent effect of the third strike[37] should actually be interpreted as the difference between the reduction in recidivism for individuals with one strike and individuals with two strikes. Thus, the deterrent effect of the third strike is 5 percentage points, or 10%. Her results are highly significant and are robust to changes in specifications. She concludes that, on average, 222,000 crimes were deterred annually by the law, and determines that the law is cost-ineffective.

Using the same methodology, Iyengar also investigates the unintended consequences of the three strikes law. She finds that the law increases the likelihood that an individual with one strike leaves California to commit a crime in another state, conditional on recidivating, by 6%, and increases it for an individual with two strikes by 8.5%. Furthermore, she finds that individuals with one strike and two strikes are 4% and 10% more likely to commit a violent crime, respectively, conditional on recidivating. Once again, this means that the threat of the third strike increases the likelihood of recidivating out of state by 2.5%, and makes individuals with two strikes 6% more likely to commit a crime. Overall, she interprets her results to mean that the law leads to 21,000 additional violent crimes and 50,000 criminals migrating out of California annually.

Iyengar's misinterpretation of the law probably biases her results downwards because she is including one strike individuals in with the two-strike group. Her estimate of the additional deterrent effect of the third strike is 5 percentage points, which is lower than estimates in other papers, such as Helland and Tabarrok (2006). Because she believes that the ordering of crimes

[37] Here I mean the additional deterrent effect that can be attributed to the 25 year to life sentenced faced by individuals with two strikes relative to the double sentence faced by individuals with one strike.

matters when determining whether an individual has one or two strikes, she is mistakenly identifying some individuals who should actually have one strike as having two strikes. Individuals with one strike do not face the same harsh penalty as individuals with two strikes, and therefore including individuals with one strike in the two strikes group leads to an underestimation of the deterrent effect of the third strike. I cannot say what the magnitude of this bias is because I do not know how many individuals she is incorrectly assigning to the two strikes group. I expect that if she had properly interpreted the three strikes law, she would have found a larger difference between the coefficients on *2Strikes*AfterLaw* and *1Strike*AfterLaw*, and thus a larger deterrent effect for the threat of the third strike.

Helland and Tabarrok (2006) exploit the "fortuitous randomization of trial" in an effort to produce an unbiased estimation of the deterrent effect of California's law on individuals with two strikes.[38] They compare the recidivism rate of individuals with two trials and two convictions for strikeable offenses with the recidivism rate of individuals with two trials for strikeable offenses, but only one conviction for a strikeable offense and one conviction for a non-strikeable offense. In other words, for the individual to be placed in the one strike control group, he must have been tried for a strikeable offense on two separate occasions. On one of those occasions, he must have been convicted of a strikeable offense, and on the other occasion he must have been convicted only of a non-strikeable offense. Therefore, both individuals in the treatment group and individuals in the control group will all have two trials for strikeable offenses, and will have been convicted of some crime in each trial. The two groups have been arrested and tried for basically the same crimes, yet they face very different penalties for their next felony conviction because they have a different number of strikes. [39]

[38] Helland and Tabarrok 2006, 312.
[39] This will be explored in much greater depth later on.

Helland and Tabarrok look at individuals released from prison in California in 1994, and find that the criminals with two strikes were far less likely to recidivate than those with one strike and a similar criminal history. They compare the recidivism rate between these same treatment and control groups in other states that do not have widely enforced three strikes laws and find no such difference. They find that the threat of a third strike reduces arrest rates for individuals with two strikes by 8.3 percentage points, which is 17.2%. This finding is significant at the 1% level and is robust to many specifications.[40] They go on to perform a cost-benefit analysis, and conclude that the California law deters approximately 31,000 total crimes per year, at a cost of $148,000 per crime.[41] The cost to society per average crime is $34,000,[42] and therefore they conclude that the California law is not cost effective.

This paper begins by using Helland and Tabarrok's identification procedure, but improves on their methodology by formally creating a counterfactual out of a large group of states. I also use data from before the law was passed to compare the recidivism rates of similarly situated individuals both before and after the law was passed. This provides another counterfactual that Helland and Tabarrok did not have. The uses of these two additional counterfactuals should make it possible to obtain a more unbiased estimate of the law's actual deterrent effect.

I perform a subsequent analysis based on Iyengar's methodology, but improve on her methods by using a correct interpretation of the three strikes law. In order to obtain a more

[40] Among other robustness checks, they eliminate juveniles from the sample, stricter and looser definitions of strikes, and still find a significant deterrent effect to the third strike.
[41] Estimates are based on the Bureau of Justice Statistics' crime victimization surveys and the FBI index of reported crimes.
[42] They assume a constant rate of 8,000 third strike offenders per year, and use $35,000 as the average cost of incarceration per prisoner. Therefore, it costs an additional $583,333 per third strike prisoner (when compared to the average two strike prisoner, who is sentenced to 43 months on average, instead of 25 years). See page 328 of their paper for a more in depth explanation of their estimates.

unbiased estimate of the law's deterrent effect than her, I also formally construct a counterfactual by using data on recidivism from several states other than California.

V. Data

The data for this paper comes from two different data sets: the United States Department of Justice: Bureau of Justice Statistics Recidivism of Prisoners Released in 1983[43] and the United States Department of Justice: Bureau of Justice Statistics Recidivism of Prisoners Released in 1994.[44] The 1983 data set was composed by first selecting a representative 10% sample of all the prisoners released in each of the states sampled, and then adjusting the number of individuals selected to account for the percent of expected missing RAP sheets.[45] The 1994 data set, which is the same data set that Helland and Tabarrok use in their paper "Does Three Strikes Deter: A Non-Parametric Approach", was composed by selecting a representative sample of prisoners from each state, based on the most recent offenses that they were sentenced to prison for.[46] The 1983 and 1994 data sets are composed solely of prisoners who faced a maximum sentence of at least one year for their most recent offense. Both of these data sets contain information on criminals who were released from prison in either 1983 or 1994 in a group of states and follow each of these individuals for three years after that release. These data sets contain full criminal histories for individuals up until the year of the data set, and then contain information on all subsequent arrests, convictions, and incarcerations for three years after the prisoner's release date. They contain information about individual characteristics, such as birth date, gender, and

[43] US DOJ Bureau of Justice Statistics, 1988.
[44] This data is publicly available from the ICPSR, at www.icpsr.umich.edu.
[45] RAP sheets contain an individual's criminal history.
[46] A representative sample was composed by selecting a certain number of criminals who had each committed at least one of a series of thirteen crimes. This ensured that the criminal histories of the individuals within each sample were representative of each state. In addition, the percent of criminals selected within each state varies.

race, as well as very detailed criminal history records. For each time that the individual is arrested, the data set contains the arrest date, what crimes he was arrested for, the adjudication date, what crimes, if any, he was charged with, whether or not he was convicted, and the length of any prison sentences imposed.

These data sets have some important differences. The 1983 data set contains data on 16,355 individuals released in 11 different states,[47] 1,584 of whom are from California. The 1994 data set contains information on 38,624 individuals who were released from 15 states,[48] 7,500 of whom were from California. For each arrest, the 1983 data set contains information on up to 6 different offenses, whereas the 1994 data set only contains information on the three most serious offenses for each arrest cycle.[49]An arrest cycle is initiated each time that the individual is arrested, and ends either when charges are dropped, when the individual is acquitted, or when he is released from prison for all offenses during that cycle. Each cycle contains information on trials, convictions, and incarceration resulting from that arrest. In addition, the 1983 data set contains information on the dates of any "status changes" that occur while the individual is in prison, such as transfers within the prison system, escape from prison, and release from prison. Therefore, the 1983 data set provides information on the date of release from prison for all arrest cycles, unless the individual was in prison after the three year tracking period ended. On the other hand, the 1994 data set only contains information on the confinement length imposed on the individual for each offense, and therefore if individuals are released early or escape from prison, it is impossible to determine their exact release date from prison. The only date of release

[47] The states contained in the 1983 data set are California, Florida, Illinois, Michigan, Minnesota, North Carolina, Ohio, Oregon, New York, New Jersey, and Texas.
[48] In addition to the states in the 1983 data set, the 1994 data set contains information on criminals released in Arizona, Delaware, Maryland, and Virginia.
[49] I am controlling for year fixed effects, which should control for the different number of offenses recorded in the two data sets because this changed in the same way over time for all individuals.

that the 1994 data set gives with certainty is the 1994 release date, which is the beginning of the three year tracking period.

The 1994 data set, which is the post-law data set, provides many advantages over other available data sets for investigating the effects of California's three strikes law. First of all, because all of the criminals in this data set are being released in 1994, any reduction in recidivism rates for these offenders will be identifying a deterrent effect and not an incapacitation effect. All of the criminals in this sample are out of prison and are able to commit crimes because they are not incapacitated at the onset of the sample. Another advantage is that, because all of these prisoners were released from prison during 1994, this data set makes it possible to limit the sample to individuals who were out of prison and already had one or two strikes when the three strikes law went into effect. This mitigates concerns that criminals may have been tailoring their previous behavior in order to avoid committing a second, or even a first strike.[50] Also, because the data set contains information on prisoners from several states that do not have three strikes laws, it is possible to perform a difference-in-differences analysis across states.

Combining these two data sets is advantageous because it makes it possible to perform an analysis both across states and across time periods. Because the law did not go into effect until 1994, the individuals in the 1983 data set who were in California did not face the same harsh penalty enhancements that California criminals faced in 1994. Therefore if there are differences in recidivism rates for similarly situated individuals across these two time periods, then they may be the effect of the three strikes law.

[50] This is the problem that Iyengar had to address through instrumenting, because her sample contained individuals who were being admitted to and released from prison after 1994.

I had to greatly limit the data sets in order to perform this analysis. First of all, I eliminated juveniles from the sample because their arrest records may be sealed, and the three strikes law is enforced differently for them than it is for adults. There were very few juveniles included in the data set anyway, so this was not a very big problem. I eliminated Texas from the sample because it has its own form of a recidivist statute that is widely enforced but is structured differently than California's, and therefore including Texas in the sample would bias the results. The Texas law provides for a life sentence for any individual who is convicted of any three felonies. Including Texas would bias the estimated deterrent effect of the California law downwards because the purpose of including individuals from other states is to see how individuals in other states with similar criminal histories to those with two strikes in California behave in the absence of an increase in severity for their next offense.[51] Including individuals who face an increase in severity for their next crime in the other states' two strike group would bias the estimate of the deterrent effect of the California law downwards because it would seem as though individuals with criminal histories similar to those with individuals with two strikes are less likely to recidivate than individuals with one strike, even in the absence of the penalty increases of the three strikes law. I also eliminated Maryland and Ohio from the sample because I believe that there must have been some sort of data entry or prisoner tracking error for those states. They showed recidivism rates of 6% for all prisoners in the sample, which is unreasonably low, and therefore there must have been some type of mistake made with their data. The smallest value of recidivism for any other state is 24%, and therefore I believe that Maryland and Ohio

[51] Helland and Tabarrok (2006) perform an analysis of Texas using their survival rate methodology, and find that the Texas law deters crime even when the specifications of the model are not tailored to fit to the way that the Texas law functions (321). They find that criminals with two strike convictions and two trials for strikes in Texas are 50% less likely to be rearrested than criminals with two trials for strikes, one strikeable conviction, and one non-strikeable conviction, an estimate which is much larger (over 5 times larger) than the estimates that they find for individuals in California.

may not provide a good counterfactual. After limiting the 1983 data set as such, there are 12,642 individuals from 9 states, 1,644 of whom are from California. After limiting the 1994 data set, there are 27,945 observations in 12 states. 7,048 of these observations are in California.

VI. Identification Procedure

In order to measure the actual deterrent effect of the California Three Strikes Law, it would be necessary to observe how the same individuals would act in two different states of the world: one in which the law went into effect and one in which the law never went into effect. The reduction in crime in the state when the law is in effect compared to when it is not in effect would thus be the actual deterrent effect of the law. In reality, it is impossible to observe the same individual in a counterfactual state of the world, so the best that one can do is to "replace counterfactuals with 'comparables.'"[52] Simply comparing individuals with two strikes to individuals with one strike would not provide an accurate assessment of the deterrent effect of the law because these criminals are not comparable with respect to their criminal history. Individuals with two strikes likely have a more severe, violent criminal past than individuals with one strike, and therefore in the absence of the three strikes law would be more likely to recidivate. Therefore it is necessary to create "treatment" and "control" groups that are nearly identical to each other in all aspects except for the fact that the individuals in the treatment group face much harsher sentences than the individuals in the control group.

In order to create these treatment and control groups, I begin by using Helland and Tabarrok's identification strategy, and then build on it by using additional control groups from other states and from before the law was passed. This enables me to control for more potential sources of omitted variable bias. Helland and Tabarrok use the "fortuitous randomization of

[52] Helland and Tabarrok 2005, 312.

trial" as a quasi-experiment to create groups of individuals with two strikes and individuals with one strike that are very similar to each other. They explain their identification procedure as such:

> Consider two individuals both of whom are tried for a strikeable offense but only one of whom is convicted of that offense while the other is convicted of a lesser, nonstrikeable offense, perhaps due to a plea-bargain. The former individual is subject to the three strikes law but the latter is not. Our identification assumption is that these individuals are comparable because the outcome of the trial is to a considerable degree stochastic . . . [Several factors which help to determine the outcome of a trial] can be considered random with respect to other variables that might affect criminal disposition—thus providing us with a good quasi-experiment.[53]

The treatment and control groups are based on this "quasi-experiment". The treatment group is composed of individuals who have been adjudicated for a strikeable offense on exactly two separate occasions and who were convicted of a strikeable offense on each of those occasions. The control group is composed of individuals who were adjudicated for strikeable offenses on exactly two occasions, but convicted for a strikeable offense on exactly one of those occasions and convicted for a non-strikeable offense on the other. These two groups of criminals will be similar with respect to their criminal history because they both have exactly two strikeable adjudications and exactly two convictions for serious, even if non-strikeable, crimes. The only systematic difference between the two groups' criminal histories is that the criminals in the treatment group happened to be convicted of a strikeable offense twice, while criminals in the control group were tried for a strikeable offense twice but, for whatever reason, avoided conviction once. If Helland and Tabarrok's assumption of the somewhat random nature of trial outcomes is correct, then individuals with very similar criminal histories are assigned to the treatment or control group somewhat randomly. Therefore it seems reasonable to believe that these two groups are very similar with respect to their observable, and therefore their unobservable, characteristics that influence recidivism. I test this assumption in Table 2.

[53] Helland and Tabarrok 312

Table 2[54] compares the summary statistics of individuals in the treatment group with individuals in the control group in California in the 1994 data set. There are no significant differences between the age of individuals in each group, their age at first arrest, or the percent of each group that is black or Hispanic. "Number of Arrests" refers to the number of times that an individual was arrested prior to his 1994 release. Criminals in the one strike control group were, on average, arrested 1.2 times more frequently prior to their 1994 release than individuals in the two strike treatment group. This difference is statistically significant at the 1% level. If, in the absence of the three strikes law, criminals in the control group are more likely to be arrested than criminals in the treatment group, this may be a potential source of omitted variable bias. If individuals in the control group are more likely to be arrested in the absence of the law, it would bias the estimated deterrent effect of the law upwards because it would make it seem like the law caused individuals in the treatment group to decrease their recidivism more than it actually did. But Helland and Tabarrok find an identical difference in the number of arrests between these two groups, so I believe that it this difference is small enough that it should not bias the results. It seems likely that this difference is at least partially attributable to the fact that criminals in the treatment group have spent more time in prison because of the additional conviction for a serious crime. Therefore, as a sensitivity analysis I will include variables to control for frequency of crimes committed in the past and total prior incarceration time. Additionally, using data from before and after the law was passed will control for any differences in recidivism rates between the types of individuals in the treatment and control groups that are constant over time. Table 2

[54] Tables 2 and 3 are similar to Helland and Tabarrok's tables 1 and 2, but not identical. My descriptive statistics differ slightly from theirs because California Penal Code's offense definitions do not align perfectly with the offense descriptions provided in the data sets. Thus there will naturally be some subjectivity in determining the number of strikes that an individual actually has. I have done the best that I can to match their data based on the description of their procedures given in their paper as well as my personal communications with Tabarrok, in which he provided more details, for example, on which crimes they counted as strikeable offenses.

also shows the percent of individuals within each group who were arrested for each of a number of serious crimes before their 1994 release. The only statistically significant difference is that individuals in the control group are more likely to be arrested for robbery than are individuals in the treatment group. Therefore these treatment and control groups do a good job of creating two groups which are similar with respect to their observable characteristics. This is reassuring because it suggests that the two groups might also be somewhat similar with respect to their unobservable characteristics. But because of the limited number of observable characteristics, there is still a potential threat of omitted variable bias that will cause the two groups to recidivate differently.

Helland and Tabarrok only use data on individuals in California after the law was passed. In addition to this data, I use data from different states as well as from before the law was passed. Because individuals in states other than California, as well as all individuals in the 1983 data set, face no increase in severity of sentence for their next crime whether they are in the treatment or the control group, using data from other states and from 1983 will provide additional "comparable" groups. By using data from across states and time periods, I am able to control for any unobservable variables that affect recidivism and differ across the types of people in the treatment group and the control group over time. Helland and Tabarrok do examine two other states and find that they show no difference in the likelihood of recidivating between treatment and control, but they do not formally compare these other states with California, nor do they include the differences between treatment and control in other states as control variables in their econometric specifications. In this paper I formally construct a counterfactual out of a large group of states. In addition, I use data from 1983 to construct a counterfactual based on the

difference in recidivism rates between treatment and control groups in California well before the passage of the three strikes law.

VII. Methodology and Results

a. Person-Year Arrest Rates

Helland and Tabarrok estimate the deterrent effect of the three strikes law by looking at the differences in person-year arrest rates between the treatment and control groups after their 1994 release. These person year arrests rates are created by dividing the number of times an individual was arrested for any crime in the three years after his 1994 release by the total time, in years, that he spent out of prison during those three years. For example, consider an individual who was arrested twice during the three years after his 1994 release, and sentenced to 6 months for each arrest. The formula for calculating his person-year arrest rate is shown by equation (2).

$$(2)\ \frac{2\ arrests}{3\ years\ \ after\ \ release\ -2(6\ mont\ hs\ in\ jail\ \ per\ \ arrest\ )} = 1\ arrest\ per\ person\ year\ out\ of\ prison$$

This method is advantageous because Helland and Tabarrok can be certain that they are identifying a deterrent effect and not an incapacitation effect because they are only counting time out of prison as the time that individuals are at risk. Therefore their methodology is not threatened by the fact that individuals in the treatment group will be sentenced more harshly, and therefore be able to commit fewer crimes than individuals in the control group.

Helland and Tabarrok find that person-year arrest rates are lower for individuals in the treatment group for every year after the 1994 release. They use a survival model to estimate hazard rates, comparing individuals with two strikes to individuals with one strike and a similar criminal history. Overall, they find that survival rates for individuals in the treatment group were 8.3 percentage points higher, meaning that their person-year arrest rates were lower. This result

is significant at the 1% level. They therefore conclude that the threat of the third strike has a strong deterrent effect for individuals with two strikes. Table 3 shows my reproduction of Helland and Tabarrok's Person-Year Arrest Rates.[55] I am able to reproduce their results quite closely, and I find that using their methods the threat of a third strike reduced recidivism by 7.1%. Because I have been able to closely match their results, any differences that I find in the deterrent effect of the law will not be driven by differences in the treatment and control groups that we are using.

b. Linear Probability Model

Calculating person-year arrest rates with these data sets is problematic for several reasons. First of all, the 1994 data set only provides the "confinement length" for up to three offenses for each arrest cycle. If criminals are sentenced to prison for a fourth or fifth offense, this would lead to problems in calculating the denominator of the person-year arrest rates. Additionally, the information on confinement length is actually reporting the "length of the incarceration sentence" imposed on the individual for each offense.[56] If individuals are released early from prison, it would lead to a miscalculation of the denominator of their person-year arrest rates. These criminals would be out of prison and eligible to commit crimes, but the person-year arrest rates, which can only take into account the "incarceration sentence" would count them as being in prison. This would lead to an under-estimation of the time spent out of prison, and thus an over-estimation the true person-year arrest rates for any criminals who were released early. This is particularly problematic for estimating the deterrent effect of the third strike because criminals with two strikes who are convicted of a subsequent felony will not be eligible for early

[55] These are analogous to their Table 2 results.
[56] United States Department of Justice: Bureau of Justice Statistics (2002), 5.

release within the three year period, whereas criminals with one strike who are convicted of a subsequent felony may be. Therefore, Helland and Tabarrok may have systematically over-estimated the person-year arrest rates of criminals with one strike relative to criminals with two strikes. If this is the case, an over-estimation of person year arrest rates for criminals with one strike may have been driving some of their results.

Because of the clear problems with using the person-year arrest rates for this data set, I have chosen to use a linear probability model. While there are reasons for using a non-linear probability model, mainly the fact that the linear probability model violates the assumptions that errors are normally distributed and have a constant variance, I believe that the linear probability model is a better choice. A major advantage of using the linear probability model is that it does not entail any assumptions about how errors are distributed. Probit and logit functions will only give a correct estimate if the assumptions that one makes about distribution of errors are correct. In addition, the results produced by the linear probability model are easy to interpret and are not dependent on where the derivative is evaluated. Angrist and Pischke (2009) show that probit marginal effects are indistinguishable from linear estimates for the impact of a dummy variable, even for probabilities close to 1.[57] To test this, I will include probit and logit marginal effect estimates as a sensitivity analysis for all of my regressions. I will focus the majority of my discussion on the linear probability model because of ease of interpretation as well as because of the reasons mentioned above.

[57] They go on to quote their former teacher, who stated with regard to these non-linear models, "we should ask how it has come about that we have to deal with such an awkward, difficult, and non-robust object." Angrist and Pischke, 2009.

c. Measuring Recidivism

The model that I will be estimating will be a linear probability model where the dependent variable is a dummy variable for whether or not an individual committed a felony within three years after his release from prison. However there are many ways to define recidivism, all of which have benefits and drawbacks. Ideally I would like to observe all crimes, specifically all felonies, committed whether or not they are detected by police, but this is obviously impossible. Therefore it is necessary to choose a measure of recidivism that takes into account two facts. The first of these is that the three strikes law only provides for penalty enhancements when an individual with either one or two previous strikes is convicted of a felony. Therefore, if there is solely a reduction in the number of misdemeanors committed, it is unlikely that it is due to the deterrent effect of the three strikes law. Second, as Iyengar observes, "because of the nature of discretion in the criminal justice system, the further along the data is collected the more affected by discretion is the data."[58] Her point is that arrest rates should be a more accurate measure of the actual crime because they are affected only by police discretion. Adjudication records would be affected by prosecutorial discretion, and conviction[59] records would be affected by prosecutorial, judicial, and jury discretion.

This discretion could bias the results if, after 1994, there is a systematic difference in adjudication rates or conviction rates between individuals with two strikes and similar individuals with one strike. Prosecutors may be less willing to convict an individual of a felony if he already has two strikes, since that would send him to jail for life, compared to an individual with one strike, who would receive a much shorter sentence for the same crime. For the same reason, juries may feel less inclined to convict two strike individuals who are being charged with

[58] Iyengar 2008, 12.
[59] Convicted here can mean plead guilty or was tried and found guilty. Both of these possibilities are problematic.

felonies. This discretion could work in the opposite direction, also, with the "two strike" tag engendering negative connotations in jurors and prosecutors, and therefore two strike individuals may be more likely to be prosecuted for and convicted of felonies when compared to one strike individuals with a similar criminal history. Another concern along these lines is the way that plea bargaining changed for individuals with two strikes in California after the three strikes law was passed. Iyengar states that because individuals with two strikes faced a minimum sentence of twenty five years to life for any felony conviction, they no longer had any incentive to plea to a lesser charge, so long as they were facing a felony conviction. Furthermore, under the three strikes law, prosecutors were restricted in the plea bargains that they could offer individuals with two strikes. Therefore, this reduction in plea bargains for individuals with two strikes would undoubtedly change their conviction rates, as they would be far more likely to take their chances before a jury than individuals with one strike, who could still receive some reduced sentence by plea bargaining. For these reasons, I prefer to avoid using adjudication or conviction records to define recidivism.

There are several alternative definitions of failure that can be used, all of which are prospective measures of recidivism. Helland and Tabarrok define a failure as an arrest for any crime. This is a necessity of their methodology, as failing to include all arrests would lead to a mis-estimation of the person-year arrest rates.[60] This has a drawback, though, because the three strikes law only imposes sentence enhancements for felonies, and therefore one would not expect to see much of a deterrent effect for non-felony crimes. Therefore the decrease in arrest rates that

[60] Failing to include all arrests would be problematic in the survival model because they use person-year arrest rates, and if they are not including all arrests, then they would be under-estimating person-year arrest rates by taking time out of the denominator while failing to account for some arrests into the numerator. Therefore, if there is a systematic difference in propensity to commit misdemeanors between the treatment and control groups, this would lead to an incorrect estimate of the deterrent effect of the third strike. It seems reasonable that there could be such a systematic difference because individuals with two strikes may alter their behavior to commit more misdemeanors and fewer felonies.

Helland and Tabarrok find may not be a direct effect of the law if it is actually measuring a decrease in the number of non-felony arrests. They fail to distinguish between the types of crimes being committed, so it is difficult to know what result they are actually measuring.

To avoid this problem, I define recidivism as an arrest for any felony. The three strikes law only provides penalty enhancements for felony convictions, therefore I expect the strongest deterrent effect to be seen in how individuals with two strikes change their likelihood of committing a felony, relative to similar individuals with one strike. Using felony arrests as a definition of recidivism avoids the problematic issue of prosecutorial discretion, but there is the possibility that police discretion leads to discretion in the charges that individuals are arrested for. I do not believe this to be the case because it is the responsibility of prosecutors to research an individual's criminal history and determine the number of strikes that he has. It seems unlikely that police would know how many strikes somebody has when arresting them, so I do not think that this form of discretion is a concern. But to make sure that this discretion is not biasing the results, along with this definition of recidivism, I will try alternative definitions as a sensitivity analysis.[61]

One final issue when measuring is recidivism is whether to focus on all recidivism or only in-state recidivism. Obviously, the California three strikes law only applies to crimes committed within California, so there may be a migration effect, where individuals with one or two strikes are more likely to re-offend out of state to avoid the harsher penalties that they face in California. This is particularly problematic for my analysis because the 1983 data set contains information on arrests regardless of whether they occurred in the same state where the person was released from prison in 1983, but does not enable me to determine which state the individual

[61] All of the alternative recidivism definitions have merits and drawbacks. For the purposes of the initial results I will be using the last definition discussed.

was arrested in. Furthermore, because the 1983 data set was made before all of the arrest records were computerized, out of state arrest records are likely to be underreported in this data set. The 1994 data set, on the other hand, contains more reliable data on out of state recidivism than the 1983 data set, and contains information on which state each arrest occurred in.

In my preferred specifications, I define recidivism as any felony arrests,[62] regardless of what state they are committed in. This makes sense for two reasons. First of all, this makes it possible to define the dependent variable more consistently across time periods. Secondly, the goal of the three strikes law is to reduce crime, not to push it out of state. But as sensitivity analyses I include both a cross-state analysis of the 1994 data set when considering only in-state recidivism, as well as a regression combining data from both 1983 and 1994 where I define recidivism as only in-state recidivism in the 1994 data set and all recidivism in the 1983 data set.[63]

d. Single Year Regression

The initial regression that I perform compares the difference between the recidivism rates of individuals in the treatment and control groups in California to that same difference in other states, after the California Three Strikes Law was passed. This is essentially performing a simple difference-in-differences analysis which can be summarized as follows:

(3) $\beta_2 = (Recidivism\ Rate\ of\ California\ Treatment - Rec.Rate\ of\ California\ Control)$

$$-(Rec.Rate\ of\ Other\ States'Treatment - Rec.Rate\ of\ Other\ Control)$$

[62] Because the definition of felony varies by state and depends on the specifics of the offense, I use the California Penal Code's definition of a felony.

[63] In this second sensitivity analysis, I am looking at all recidivism for individuals in the 1983 data set because I cannot discern between in state and out of state recidivism in that data set.

As is made clear by equation (3), the coefficient β_2 is measuring the deterrent effect of the three strikes law on individuals with two strikes relative to individuals with one strike. By formally creating a counterfactual group out of other states, I am able to control for any differences between the treatment and control groups that are constant across states. In the 1994 data set, I would expect a negative value for β_2 because it is expected that the increased penalties of the three strikes law would make individuals with two strikes less likely to commit a felony. In 1983 there should be no difference in the likelihood of recidivating, so long as the treatment and control types of criminals have similar unobservable characteristics that influence recidivism, because individuals in the treatment and control group face the same penalties.

In the form of a linear probability model, the equation that I am estimating for the post law data set is:

$$(4) R_i = \beta_1(Treatment) + \beta_2(Cal * Treatment) + Individual\ Characteristics + State\ Fixed\ Effects + Age\ Spline$$

The dependent variable in this equation, R_i, takes a value of 0 if the individual does not recidivate[64] after his release from prison,[65] and a value of 1 if he does. The variable of interest is $Cal * Treatment$, which is an interaction term between California and treatment. This variable takes a value of 1 for individuals in the treatment group in California, and 0 for everyone else. The coefficient of this variable is the estimate of the deterrent effect of the third strike. The variable *Treatment* is a dummy variable which is equal to one if the individual is in the two strikes "treatment" group, and 0 if he is in the one strike "control" group. This controls for any factors that affect recidivism and differ between the treatment and control group, but are constant

[64] In all of these regressions, recidivism is defined as an arrest for any felony.
[65] "Release from prison" refers to the individual's release in either 1983 or 1994 (depending on which data set he is in). Each individual is tracked for three years after this release.

across all states. The treatment and control group are the only individuals in the sample, therefore β_1 is a measure of the difference in the likelihood of recidivating between these two groups, in the absence of the three strikes law. If the treatment and control groups contain people with similar unobservable characteristics that influence recidivism, β_1 should not be significantly different from zero. State fixed effects control for any factors which affect recidivism differently in different states, but which are similar across the treatment and control group. The age spline allows the effect of age to vary within each age category that I am controlling for, as well as across these categories. The coefficient on the age spline should be interpreted as the estimated change in expected recidivism likelihood per year. Therefore if the coefficient on the age spline from 40 to 45 is -.01, that means that between 40 and 45 an individual becomes 5 percentage points less likely to recidivate.

Table 4 shows the results of this regression for the 1994 data set. Standard errors are robust with clustering by state and number of strikes.[66] Column 1 shows the results of this regression when no control variables are included and when only individuals in California are included in the sample. This is similar to what Helland and Tabarrok do, except for the fact that this regression is only examining the possibility that an individual recidivates within three years, instead of survival rates. In column 1, I use Helland and Tabarrok's definition of recidivism, which is an arrest for any crime. The estimate of the deterrent effect of the law is -.108, and is statistically significant at the 1% level. This is larger than the estimate of -.08 that Helland and Tabarrok find, but the 95% Confidence Interval, (-.162, -.054) includes their estimate.

Column 2 shows the results of this same regression when recidivism is defined as an arrest for a felony, instead of any crime. The estimate of the deterrent effect of the law is

[66] Each cluster is made up of individuals in either the treatment or control group within a particular state. This solves the problem that Moulton (1990) finds, which is that when looking at the effect of public policy variables on individual units, OLS standard errors can be biased downwards.

-.043, and is statistically insignificant. This shows that the definition of recidivism that one chooses has a large effect on their estimate of the deterrent effect of the three strikes law. The differences between the way that I define recidivism and the way that Helland and Tabarrok define recidivism will therefore explain some portion of the difference between our estimates. However, the 95% Confidence Interval for the estimates in column 2 (-.10, .013) includes Helland and Tabarrok's estimate of .08.

Column 3 shows the results of the same regression when including individual controls. When including individual controls, the estimate of the deterrent effect is still negative and statistically insignificant. This result is nearly identical to the result found in column 2.

Column 4 shows the results of the regression in equation (4) when all states are included. In this regression, California is the omitted category, so the coefficients for the state variables show the difference in recidivism between each state and California. When other states are included, the deterrent effect of the third strike is reduced to 1.2 percentage points and is still statistically insignificant. The coefficient on the *Treatment* variable shows the reason for this decrease in the estimated deterrent effect, as the treatment group is less likely to recidivate in all states. Even though the estimate of -2.8 percentage points is insignificant, it accounts almost perfectly for the difference between the coefficient on *Cal*Treatment* in column 4 and the coefficient on *Treatment* in Column 3. It seems likely that there are unobserved characteristics of individuals in the two strike treatment group that make them less likely to recidivate than individuals in the control group even in the absence of the increased penalties of the three strikes law. The 95% Confidence Interval for the deterrent effect of the third strike (-.045, .021) enables me to rule out Helland and Tabarrok's estimate of -.08.

In this regression, the age splines tell an interesting story. Because they are all negative, they show that the older an individual gets, the less likely he is to recidivate. Almost all of these effects are statistically significant and have a large impact on the predicted recidivism of an individual. In The coefficient on the 18 to 24 year old spline of -.0185 means that for every year an individual ages between 18 and 24, his estimated probability of recidivating drops by 1.85 percentage points. Therefore, between the ages of 18 and 24, an individual becomes 11.1 percentage points less likely to recidivate. This is a hugely significant difference, and as criminals age they continue to become less likely to recidivate. This model estimates that, between the ages of 18 and 45, an individual becomes 37.75 percentage points less likely to recidivate. This is a hugely important factor considering the fact that criminals that face three-strikes sentence enhancements are, on average, older than other criminals, and may already be less likely to recidivate, even in the absence of three-strikes penalties.[67]

e. Difference-in-difference-in-differences

In order to determine the differences in recidivism between the treatment and control group both across states and across time periods, I must control for factors that vary across time as well as across states. By "differencing out" these factors, I will be able to obtain a more accurate estimate of the law's deterrent effect. This can be conceptualized as a difference-in-difference-in-differences[68] analysis which is composed of two parts, which are the two difference-in-differences equations illustrated by equations (5) and (6):

$$\beta_2 = \begin{bmatrix} (Recidivism\ RateTreatmentAfterLaw\ in\ CA - RecidivismRateofControlAfterLawinCA) \\ -(RecidivismRateofTreatmentBeforeLawin\ CA - RecidivismRateofControlBeforeLawinCA) \end{bmatrix} \quad (5)$$

$$\beta_3 = \begin{bmatrix} (Rec.RateofTreatmentAfterLawinOtherStates - Rec.RateofControlAfterLawinOtherStates) \\ -(Rec.RateofTreatmentBeforeLawinOtherStates - Rec.RateofControlBeforeLawinOtherStates) \end{bmatrix} \quad (6)$$

[67] This will be addressed in more detail in the discussion section.
[68] Gruber, Jonathan. *Public Finance and Public Policy*, (New York: Worth Publishers, 2007), 411-12.

Equation (5) shows the difference between the treatment and control groups in California before and after the law went into effect. The first part of equation (5) is the difference between the recidivism rates of the treatment and control groups within California after the law was passed, and it should be negative if the third strike is effectively deterring individuals with two strikes. The second part of the equation is an estimate of the difference in recidivism between the types of people in the treatment and control groups in California in the absence of the three strikes law. This will control for factors that influence recidivism between the treatment and control groups differently and are constant over time within California. Overall, equation (5) will produce a negative result if the three strikes law was effectively deterring crime.

Equation (6) shows the difference between the treatment and control groups in states other than California before and after the three strikes law went into effect. The first part of the equation controls for any factors that cause individuals in the treatment and control groups in other states to recidivate differently in the post-law period. The second part of equation (6) controls for factors that cause individuals in the treatment and control groups in other states to recidivate differently in the pre-law period. Overall, equation (6) makes it possible to control for omitted factors that cause the difference in recidivism rates between individuals in the treatment and control groups to change over time.

Subtracting equation (6) from equation (5) produces the simple version of the triple differences analysis. This is a long equation, which can best be explained in words. The first part of the triple differences analysis is the difference in recidivism rates between the treatment and control groups in California after the law was passed, minus the difference in recidivism between the treatment and control groups in California before the law was passed. This is what is shown by equation (5), and is a difference-in-differences analysis of the law, looking across time but

only at California. This enables me to remove any unobserved factors that affect recidivism and are different for individuals in the treatment and control groups, but are constant over time. The second part of the triple differences analysis, shown by equation (6), is the difference in recidivism rates between the treatment and control groups in other states after the law was passed, minus the difference between treatment and control groups in other states before the law was passed. By subtracting equation (6) from equation (5), I can remove any unobserved factors that cause differences in the recidivism rates for individuals in the treatment and control groups to change over time.

A difference-in-differences analysis controls for any omitted factors that influence recidivism rates differently for the treatment and control groups and that are constant across time. The important benefit of the triple differences analysis is that, in addition to controlling for those factors, it will also remove any omitted factors that influence recidivism differently across time for individuals in the treatment and control groups. This will help me to obtain a more unbiased estimate of the true deterrent effect of the three strikes law. The linear probability model that I am estimating is structured as follows:

$$\begin{aligned} R = {} & \beta_1(\text{Treatment} * Cal * Yr1994) + \beta_2(\text{Treatment} * Cal) + \beta_3(Cal * Yr1994) \\ & + \beta_4(\text{Treatment} * Yr1994) + \beta_5(\text{Treatment}) + Individual\ Characteristics \\ & + State\ Fixed\ Effects + Yr1994 + Age\ Spline \end{aligned} \qquad (7)$$

β_1 is the estimated deterrent effect of the California Three Strikes Law for the individuals in the treatment group. The interaction term $Treatment * Cal * Yr1994$ will take a value of 1 for individuals in the treatment group in California in 1994 and 0 for everybody else in the sample. These are the only individuals in the sample who face the severe third strike penalties under the law, and therefore it is their response to the law that determines the deterrent effect of a third strike.

*Treatment*Yr1994* controls for any factors that affect recidivism differently across time for individuals in the treatment and control groups but are constant across states. The *Cal*Yr1994* interaction term controls for anything that affects recidivism and changes similarly over time for all individuals, or for individuals in the treatment and control group, in California. The *Cal*Treatment* interaction term controls for any factors that affect recidivism, are different between criminals in the treatment and control groups within California, and are constant over time. If there is some omitted variable that is unique to California and causes individuals in the treatment group to recidivate less frequently than individuals in the control group, or vice versa, then including this last interaction term will remove this potential source of bias.

Treatment controls for anything that affects recidivism, differs between individuals in the treatment and control groups similarly in all states, and is constant over time. The *Yr1994* dummy variable controls for anything that affects recidivism and is changing in the same way over time in all states. The state fixed effects control for anything that affects recidivism and is constant over time within a state.[69]

Table 5 shows the results of this regression. Standard errors are robust and are clustered by state and number of strikes.[70] All of the results in the table include data from all states in both 1983 and 1994. Column 1 shows the simple difference-in-difference-in-differences analysis, with no individual control or state control variables. The estimate of the deterrent effect of the law is the coefficient on *Treatment*Cal*1994*, and it is not statistically distinguishable from zero. The standard errors are large, but the 95% Confidence Interval (-.039, .126) enables me to rule out both Helland and Tabarrok's estimate of -.08 and Iyengar's estimate of -.05. The statistically

[69] As a sensitivity analysis, I also allow state-effects to vary by year. This has no significant effect on the results.
[70] Bertrand, Duflo, and Mullainathan (2004) find that in regressions where there are multiple years of data and serially correlated outcomes, standard errors tend to be biased downwards unless one adjusts for clusters by states. I follow their advice for all of the linear probability models.

significant and positive coefficient on the 1994 dummy variable shows that recidivism rates increased for both the treatment and control group between 1983 and 1994. This is consistent with the fact that crime rates across the country were higher in 1994 than they were in 1983.[71]

Column 2 shows the results of the triple differences regression, including individual controls and state fixed effects. The coefficient on *Treatment*Cal*1994* is positive and not statistically significant, which suggests that the threat of a third strike does not reduce recidivism. In Columns 2, 3, and 4, California is the omitted category.[72] Therefore all state coefficients can be interpreted as the difference in recidivism for the control groups of each state when compared to California. Column 3 includes state-year effects (not shown), allowing the differences in factors that affect recidivism to vary across time periods for all states.[73] The coefficient on *Treatment*Cal*Yr1994,* .0326 (.0264), is statistically insignificant. The 95% Confidence Interval for this estimate (-.019, .08), enables me to rule out both Helland and Tabarrok's and Iyengar's estimates. This means that the increased penalties provided for the third strike reduce recidivism at most by 1.9 percentage points.

The coefficient on the *Treatment*Cal* interaction term is -.045 and is significant at the 5% level, which shows that even in the absence of the three strikes law, individuals in the treatment group in California were 4.5 percentage points less likely to recidivate than control group individuals in California. This means that recidivism rates always differ between treatment and control groups in California. This suggests that there may be a California-specific, time

[71] See Figure 1.
[72] In all of my regressions, if I am controlling for state fixed effects, then California is the omitted category. This makes the results easier to interpret. It also leads to significantly lower R^2 values because Stata does not include the portion of variation explained by the overall intercept in its calculation of R^2.
[73] Columns 3 and 4 include state-year effects, and it was therefore necessary to omit the Yr1994 dummy variable because I am effectively letting the year-effects be different for every state, instead of constraining it to be constant for all states.

invariant omitted variable that is influencing the results of other authors, such as Helland and Tabarrok, whose data is limited to post-law individuals.

The coefficient on *Treatment* is -.033 and is significant at the 5% level, suggesting that the treatment group is less likely to recidivate even in the absence of the three-strikes law in states other than California. The coefficient on *California*1994* is -.094 and is significant at the 5% level, which shows that recidivism rates for individuals in the control group in California decreased by 9.4 percentage points more between 1983 and 1994 than the recidivism rates for the control groups in other states. This indicates the possibility that the three strikes law deters criminals before their second strike. Forward looking criminals may be deterred by a future increases in penalties, and therefore may avoid committing a first strike or a second strike. Therefore, by looking only at the deterrent effect of the third strike, I may be failing to capture the true deterrent effect of the law.

Column 4 shows the results of this same regression as equation (4), with the addition of prior criminal history variables, which control for some systematic differences in criminal histories between individuals in the treatment and control groups. Because individuals in the treatment group necessarily have a worse criminal history, having been convicted of two strikeable offenses, it is possible that they are more likely to recidivate in the absence of increased penalties than individuals in the control group. Therefore, controlling for criminal history variables enables me to ensure that differences in criminal history are not driving the results.

I use the same prior criminal history (PCH) variables that Iyengar describes in her paper. The PCH variables count the number of convictions that an individual had prior to his release[74] for each of eight crime categories: murder, rape, assault, burglary, robbery, theft, drug crimes,

[74] This refers to the 1983 or 1994 release, depending on which data set the individual is in.

and other crimes.[75] The PCH variables are not perfectly collinear with the treatment and control groups for two reasons. First of all, the eight categories of PCH do not align perfectly with what is counted as a strikeable offense. For example, simple assault fits into the assault category, but is not a strikeable offense. Secondly, individuals in the treatment or control group may have been convicted of multiple offenses during their trial. I do not allow this to exclude them from the sample, however, as it would result in a very small sample size. Furthermore, multiple convictions for strikeable offenses during the same trial only count as one strike, therefore PCH variables, regardless of how detailed they are, are not perfectly collinear with the number of strikes that an individual has.

When controlling for an individual's criminal history, the coefficient on *Treatment*Cal*Yr1994*, .0212, is still slightly positive and statistically insignificant like in column 3, which shows that differences in criminal history are not driving the results.[76] The 95% confidence interval for *Treatment*Cal*1994* (-.033, .075) allows me to rule out deterrent effects greater than 3.3 percentage points, which is less than half of what Helland and Tabarrok find. Therefore, if there is a deterrent effect to the law's third strike, it is not nearly as large as what previous authors have found.[77]

[75] All previous convictions fall in to one of these categories, therefore PCH variables account for an individual's entire criminal history.

[76] I include alternative PCH vectors (more detailed) as a sensitivity analysis, and find that this has no significant effect on the results.

[77] In order to ensure that the linear probability model is not biased, I also include logit and probit estimations of the same regression in Tables 11 columns 1 and 2, respectively. The estimated marginal effects calculated at the mean values of the explanatory variables are similar in magnitude for all of the estimates, thus showing that the linear probability model is an appropriate model to use for these regressions.

f. Triple differences analysis for individuals with one strike

While I find no statistically significant evidence that individuals with two strikes are deterred by the threat of the third strike, it does not mean that the law as a whole is not effective. It is possible that criminals are equally deterred by the double penalties that individuals with one strike face and the twenty five to life penalty that individuals with two strikes face. This finding would be consistent with my findings for two reasons. First of all, it would mean that there is no statistically significant difference in deterrence for individuals with two strikes and individuals with one strike and a similar criminal history. Secondly, the coefficient on *Cal*Yr1994* is consistently negative and statistically significant in all columns of Table 6. This shows that between 1983 and 1994, recidivism rates went down more for the control group in California than for the control group in other states. This would be consistent with individuals with one strike being deterred by the law.

To test whether individuals with one strike are deterred by the double penalties that they face for their next subsequent felony, I perform the same analysis as in the previous section, but focus on individuals with one strike and individuals with no strikes but a similar criminal history. In this analysis, the treatment group is composed of individuals whose criminal history consists of exactly one trial for a strikeable offense, on which they were convicted of a strikeable offense. These individuals therefore have one strike. The control group is composed of individuals whose criminal history consists of exactly one trial for a strikeable offense, but during that trial they were not convicted of a strikeable offense and were convicted of a non-strikeable offense. Both groups of individuals have been tried once for a strikeable offense and on that occasion were convicted of some crime, but individuals in the treatment group will have one strike and will

therefore face a double sentence for their next felony conviction, while individuals in the control group have no strikes and therefore face no three strikes penalty enhancement.

The regression equation is still represented as equation (7) above, but the composition of the treatment and control groups have changed. Column 1 of Table 6 shows the results of the triple differences analysis for individuals with one strike and no strikes, with no individual or state control variables included. Column 2 includes individual controls and state-fixed effects. Column 3 adds state-year effects. The coefficient on *Treatment*Cal*1994*, .0389 (.0291), is statistically insignificant, suggesting that the doubled sentence length faced by individuals with one strike does not have a significant deterrent effect, when compared to individuals with no strikes and a comparable criminal history. The 95% confidence interval (-.018, .096) enables me to rule out a deterrent effect greater than 1.8 percentage points, which is far smaller than Iyengar's estimated deterrent effect of 9 percentage points.

Column 4 includes PCH variables, to ensure that difference in criminal history between the treatment and control groups are not masking the deterrent effect of the law. When PCH variables are included, the coefficient on *Treatment*Cal*1994*, -.015 (.028), is slightly negative but is statistically insignificant. The 95% Confidence Interval (-.069, .040) enables me to rule out a deterrent effect greater than 6.9 percentage points for individuals with one strike, which is still smaller than what Iyengar found.

In Table 6, the coefficient on *Cal*Treatment* is significant and negative in all four columns, suggesting that individuals in the treatment group are less likely to recidivate than individuals in the control group in California in 1983. Authors who do not use data that looks at recidivism both before and after the law went into effect will fail to observe the effects of time-

invariant variables that apparently cause recidivism to be lower for the one strike treatment group than for the no strikes control group, even in the absence of the three strikes law.

The coefficient on *Cal*1994* is negative and statistically significant in all four columns, which means that, between 1983 and 1994, recidivism rates for the control group in California decreased more than recidivism rates for the control groups in other states. Therefore, authors who fail to use data that looks at multiple states will overestimate the deterrent effect of the three strikes law. Alternatively, it is possible that the negative, significant coefficient on *Cal*1994* is evidence that the California three strikes law is actually deterring all criminals, not just those with strikes.

g. Effect of Three Strikes Law on Individuals of Different Ages

I also examine the possibility that the effect of the three strikes law has different effects on individuals of different age groups. To test this possibility, I allow the effect of being in the treatment group in California after the law was passed to vary by the individual's age. I construct dummy variables for 6 age categories, 18 to 24, 25 to 29, 30 to 34, 35 to 39, 40 to 44, and 45 or older, and interact each of these dummy variables with the *Treatment*Cal*1994* variable from previous regressions. The coefficient on each of these variables is the deterrent effect of the three strikes law for individuals within each age category.

The results of this regression when looking at the deterrent effect of the third strike are presented in Table 7. Here, the treatment group is composed of individuals with two strikes, and the control group is composed of individuals with one strike and a similar criminal history. Column 1 includes age dummy variables to control for age, and column 2 contains age splines. Both columns contain PCH, state, state-year, and individual controls. The coefficients on the

*Treatment*Cal*94*Age* dummies measure the deterrent effect of the third strike for individuals within each age category. Unfortunately, because of relatively small sample sizes in each age category, standard errors are higher than in the other regressions. Still, it appears that until age 34, individuals with two strikes appear more likely to recidivate than individuals with one strike and a comparable criminal history. After age 34, the coefficients on the *Treat*Cal*94*Age* interactions in column 2 are all negative, which is consistent with a deterrent effect. The coefficient on *Treatment*Cal*94*Age18to24*, .056 (.036), means that the enhanced penalties of the third strike actually makes individuals in the treatment group who are between the age of 18 and 24 5.6 percentage points more likely to recidivate than comparable individuals in the control group. There may be some omitted variables between the treatment and control group that causes individuals between 18 and 24 in the treatment group to be more likely to recidivate, even in the face of the 25 to life penalty that they face. This result is statistically insignificant, however, and should not be given much weight.

I perform the same analysis for individuals with one strike and individuals with no strikes and a similar criminal history. Table 8 shows the results of this regression. Once again, column 1 includes age dummy variables to control for age, and column 2 contains age splines. Both columns contain PCH, state, state-year, and individual controls. Once again, standard errors are high because of the smaller sample sizes within each age category. The coefficient on *Treat*Cal*94*Age18to24*, .076 (.028), is significant at the 5% level. This suggests that individuals between the ages of 18 and 24 who have one strike are 7.6 percentage points more likely to recidivate than comparable individuals with no strikes. There must be some omitted variables between the treatment and the control group that make individuals in the treatment group more likely to recidivate, even in the face of the harsher penalties that they face. The

coefficients on the *Treatment*Cal*94*Age* variables for ages 30 and over are all negative, suggesting that criminals with one strike become less likely to recidivate after the age of 30.

h. Altered Criminal Behavior for Individuals with Two Strikes

Thus far I have failed to address the issue of whether or not criminals alter the choices of type of crime in response to the increased penalties of the three strikes law. Because the three strikes law provides sentence enhancements for all felonies, regardless of their severity, it is possible that individuals with one and two strikes would be more likely to choose a severe crime over a less severe crime. This is because, as Iyengar states, the three strikes law "increased the expected penalty for all crimes (intercept shift), and flattened the penalty gradient with respect to severity of crime (slope shift)."[78] Therefore, given that an individual with two strikes recidivates, he may be more likely to choose a violent crime or a more serious crime than an individual with one strike and a similar criminal history because the two strike individual faces a much harsher sentence for his next felony conviction, regardless of the actual severity of the crime.

To test the possibility that the three strikes law makes individuals with two strikes more likely to commit a violent crime than individuals with one strike and a similar criminal history, I use the same basic model as before to see if there is a change in the likelihood that an individual with two strikes is arrested for a violent crime, given that he was arrested for a felony at least once during the three year period following his release. I use equation (7), but replace the dependent variable with a dummy variable which is 1 if the individual is arrested for a violent crime within three years being released from prison, and 0 if he is not.[79] I limit the sample to

[78] Iyengar 2008, 3.
[79] I define violent crime as the violent crimes listed as strikeable offenses. See Table X for the full list of violent crimes.

individuals who were arrested for a felony at some point in the three years after their release, so that I am not capturing any of the reduction in overall recidivism that the law may have.

Table 9 shows the results of this regression for the two strike treatment group and the one strike control group.[80] Column 1 contains individual, state, and state-year controls. The coefficient on *Treatment*Cal*1994*, .069 (.031), is significant at the 5% level. This suggests that individuals with two strikes are 6.9 percentage points more likely to commit a violent crime, given that they recidivate, than individuals with one strike and a similar criminal history.

Column 2 adds PCH variables to the regression in column 1. When an individual's criminal history is controlled for, the coefficient on *Treatment*Cal*1994*, .0297 (.027), is no longer statistically significant. The 95% Confidence Interval (-.023, .083) includes potentially large increases in propensity to commit violent crime, and I cannot rule out Iyengar's estimate of .054.[81]

The coefficients on the PCH variables are what I expected. Violent crimes in an individual's past, such as murder, rape, assault, and robbery, are all positively correlated with the likelihood than an individual commits a violent crime after his release. Nonviolent crimes in an individual's past, such as burglary, theft, and drug crimes, are all negatively correlated with the likelihood that an individual commits a violent crime after his release.

Interestingly, the coefficient on the age spline is positive and statistically significant from the ages of 18 to 24, meaning that between these ages, a criminal becomes more likely to commit a violent crime. The value of .0159 means that from the ages of 18 to 24, a criminal becomes 9.5 percentage points more likely to commit a violent crime, conditional on recidivating. This is the

[80] These are the same treatment and control groups that I used in the first set of regressions.
[81] .054 is the difference between the after*2strikes and the after*3strikes coefficients in Iyengar's Table 6.

opposite of what I found when looking at overall recidivism, as individuals became less likely to recidivate between the ages of 18 and 24.

It is still possible that individuals with two strikes increase the severity of their criminal action in response to the three strikes law, but not solely through committing violent crimes. In order to test this hypothesis, I perform the same analysis as above, but replace the dependent variable with a dummy variable for whether or not the individual was arrested for a strikeable offense within three years of his release from prison.[82] Once again, the sample is limited to individuals who were arrested for a felony within three years of their release from prison. Strikeable offenses are a good way to define the dependent variable because they are defined by California's Penal Code as the most serious or violent felonies.

The results of this regression are displayed in Table 10. Column 1 shows the results of this regression when including state, state-year, and individual controls. Once again, California is the omitted category for ease of interpretation. The coefficient on *Treatment*Cal*1994*, .084 (.029), is positive and significant at the 1% level, implying that individuals with two strikes are 8.4 percentage points more likely to commit a serious or violent felony, given that they recidivate, when compared to individuals with one strike and a similar criminal history.

But it is possible that these results can actually be attributed to differences in an individual's criminal history, because individuals in the treatment group have two convictions for a strikeable offense in their criminal history whereas individuals in the control group only have one. Column 2 includes PCH variables to ensure that criminal history is not driving these results. When PCH variables are included, the results are made slightly stronger. The coefficient on *Treatment*Cal*1994*, .092 (.03), is statistically significant at the 1% level, and means that

[82] See Table 1 for a list of strikeable offenses, as defined by the California Penal Code. These include both the violent offenses and the serious offenses.

individuals with two strikes are 9.2 percentage points more likely to commit a serious or violent felony, conditional on recidivating, than comparable individuals with one strike. The harsher penalties of the three strikes law make individuals with two strikes more likely to commit a serious or violent felony because they face the same punishment regardless of the severity of the felony that they commit. This is an unintended consequence of the sentence enhancement system under the three strikes law.

i. Altered Criminal Behavior for Individuals with One Strike

In this section, I perform the same analysis as in the previous section, but looking at individuals with one strike, compared to individuals with no strikes but a similar criminal history. The treatment group is composed of individuals with one trial and one conviction for a strikeable offense, and the control group is composed of individuals with one trial for a strikeable offense, and on that occasion they were not convicted for a strikeable offense and were convicted of a non-strikeable offense.[83]

First I look at whether or not individuals with one strike were more likely to commit a violent crime, given that they recidivated during the three years after their release. Table 11 shows the results of this regression. Column 1 includes state, state-year, and individual controls. The coefficient on *Treatment*Cal*1994* is negative but insignificant. When PCH controls are added in column 2, the coefficient on *Treatment*Cal*1994*, -.0869 (.0371), is significant at the 5% level. This means that individuals with one strike are 8.7 percentage points less likely to commit a violent crime than individuals with no strikes and a similar criminal history. These

[83] These are the same treatment and control groups that I used previously in the one strike and nearly one strike regressions.

results directly contradict Iyengar, who found that individuals with one strike were 4.1 percentage points more likely to commit a violent crime than individuals with no strikes.

Next I look at whether or not individuals with one strike were more likely to commit a serious or violent felony, given that they recidivated during the three years after their release. The results of this regression can be seen in Table 12. Column 1 includes state, state-year, and individual controls, and column 2 adds PCH variables. The coefficient on *Treatment*Cal*1994*, -.082 (.047), is significant at the 10% level, and means that individuals with one strike are 8.2 percentage points less likely to commit a strikeable offense, given that they commit a felony in the three years following their release.

The results of the previous two regressions may at first seem puzzling, but in reality they make sense. Unlike individuals with two strikes, the sentence that individuals with one strike face for their next felony conviction depends upon the severity of the crime that they commit. Double the sentence for a lesser felony results in less of an increase in sentence length than double the sentence for a serious or violent felony, and therefore individuals with one strike still have incentives to avoid committing serious or violent felonies. Individuals with two strikes face the same penalty regardless of the severity of their crime, so they have an incentive to commit more severe crimes.

Furthermore, individuals with one strike may be altering their criminal behavior to avoid committing a second strike. All of the serious or violent felonies are strikeable offenses, and therefore forward looking criminals with one strike may want to avoid committing these offenses. If this is the case, given that an individual with one strike recidivates, he would be more likely to choose a crime that cannot be counted as a strikeable offense, which is consistent with my results. Individuals with two strikes, however, receive a third strike for any felony, regardless

of its severity, so they may be more likely to commit a serious or violent crime, to ensure that they "get their money's worth." This is also consistent with my findings.

VIII. Sensitivity Analyses

a. Linear Probability Model v. Probit and Logit Models

As mentioned previously, some economists may be skeptical about my use of the linear probability model. To ensure that the linear probability model is producing an unbiased estimate of the deterrent effect of the three strikes law, I include logit and probit estimations of several of the previous tables.

Columns 1 and 2 of Table 13 show the probit and logit estimates, respectively, of the deterrent effect of the third strike when looking only at the 1994 data set. Standard errors are robust and clustered by state and treatment group. Estimates are marginal effects reported at the mean of the data.[84] This is analogous to the linear probability model estimates in column 4 of Table 4, which was looking at 1994 data only and contained a full set of control variables. The probit model estimate, -.010 (.018), the logit model estimate, -.011 (.019), and the linear estimate, -.012 (.017), are all nearly identical, showing that the linear probability model is an acceptable choice.

Columns 1 and 2 of Table 14 show the probit and logit estimates, respectively, of the deterrent effect of the third strike when looking at the combined 1983 and 1994 data. These estimates correspond to the linear probability model estimates in column 3 of Table 5, which contain state, state-year, and individual controls. Once again, the estimates for *Treatment*Cal*1994* are nearly identical across the three models, and the signs and magnitude

[84] All probit and logit estimates in this paper are reported as marginal effects at the mean of the data.

of the estimates are consistent across all of the variables. Therefore the linear probability model is not biasing the results.[85]

b. Alternative Age Specifications

To ensure that my use of an age spline is not biasing the results, I include several regressions with alternative age specifications. Table 15 shows the results of this analysis for the two strike treatment and one strike control group. The regressions in this table include PCH, state, state-year, and individual controls, and are analogous the results in column 4 of Table 5 which includes an age spline and is comparing the two strike treatment group with the one strike control group. Column 1 of Table 15 contains an age variable in place of the age spline. Column 2 contains age and age^2, and Column 3 contains age, age^2, and age^3. The results are nearly identical to those in column 4 of Table 5, which is estimating the deterrent effect of the law on individuals with two strikes and contains a full set of controls. showing that use of the age spline does not bias the results.

I performed the same sensitivity analysis for individuals with one strike compared to individuals with no strike and a similar criminal history (not shown). These results correspond to Column 4 of Table 6, which contains the estimates of the deterrent effect of the law on individuals with one strike and contains the full set of controls, and are nearly identical to the results when including the age spline variable.

c. In State Versus Out of State Recidivism

As discussed above, there is no way to discern between in state and out of state recidivism in the 1983 data set. Therefore, to this point, I have defined recidivism as both in state

[85] I performed logit and probit analyses for the violent crime regressions and found the same results (not shown).

and out of state recidivism. To ensure that individuals in the two strikes treatment group in California are not more likely to recidivate out of state, I limit the dependent variable to only count in state recidivism for the 1994 data set. Table 16 shows the results of this regression, looking at 1994 only, for the two strike treatment and one strike control group. Columns 1 and 2 both contain individual and state controls. Column 1 contains the results of the regression when including all recidivism, both in state and out of state, and provides an estimate for the *Treatment*Cal* coefficient of -.012(.017). Column 2 restricts the definition of recidivism to only include in state arrests, and provides an estimate for the *Treatment*Cal* coefficient of -.0056 (.018). These results are statistically indistinguishable from the column 1 model that includes all recidivism, and therefore it seems that counting all recidivism is not biasing the results.

I perform the same analysis for the combined 1983 and 1994 data, restricting recidivism in the 1994 data set to only in state recidivism. I include all recidivism in the 1983 data set, as I have no way of distinguishing between in state and out of state recidivism for individuals in the 1983 data set. The results of this regression are presented in Table 17. Both columns 1 and 2 contain state, individual, and PCH controls. In column 1, recidivism is defined as all recidivism, both in state and out of state. In column 2, recidivism is restricted to only in state recidivism for the 1994 data set, but still defined as all recidivism for the 1983 data set. There is no statistically significant difference in the two estimates, therefore using overall recidivism as the dependent variable is not biasing my results.[86]

[86] I perform these same analyses looking at the one strike treatment group, as well as at propensity to commit violent crime. I find no differences when restricting recidivism or violent crimes to only those committed in state for the 1994 data set.

d. Alternative Measures of Recidivism

As discussed previously, there are several different ways to define recidivism. To this point, I have defined recidivism as an arrest for a felony as defined under California law. To see if this measure of recidivism is problematic, I perform the same analyses as before, using different definitions of recidivism.

I try two alternative definitions of recidivism. The first of these, arrest for any crime, is the same definition of recidivism that Helland and Tabarrok use. I perform the same regression as specified by equation (7), but now define recidivism as an arrest for any crime. The result of this regression for the treatment group with two strikes is presented in column 1 of Table 18. The results in this table, which contain PCH, individual, state, and state-year controls, are analogous to column 4 of Table 5, which is the combined 1983 and 1994 data regression looking at the deterrent effect of the third strike. The coefficient on *Treatment*Cal*1994*, -.0212 (.0231), is not statistically significant. The 95 % Confidence Interval (-.082, .040) does not enable me to rule out Helland and Tabarrok's estimate of .08. Therefore, it is possible that the differences between our results are being driven by the differences between our measures of recidivism. But the coefficient on *Treatment*Cal*, -.039 (.0175), is negative and is statistically significant at the 5% level. This means that there is a time-invariant omitted variable that makes individuals in the treatment group in California nearly 4 percentage points less likely to recidivate than individuals in the control group in California, even in the absence of the increased penalties of the three strikes law. Because Helland and Tabarrok's data only examines California after the law was passed, they cannot account for this source of omitted variable bias.

The second alternative definition of recidivism that I use is more complicated than the previous definitions that I have discussed. For this specification, I define recidivism as any time

that an individual is arrested for a felony and convicted of some crime on that same arrest.[87] Adding the restriction that an individual must be convicted of some crime serves several purposes. First of all, it addresses the concern that individuals with two strikes may be more likely to be arrested than individuals with one strike because of the nature of their criminal history. The fact that an individual was convicted for some offense on an arrest implies a stronger case for guilt than the fact that an individual was simply arrested. Secondly, this definition of recidivism will enable me to see if there is some type of prosecutorial, judicial, or jury bias, or if there is a change in the likelihood that an individual with two strikes accepts a plea bargain, relative to an individual with one strike and a similar criminal history. If the results change significantly between my original definition of recidivism, which was an arrest for any felony, and this definition of recidivism, than one of these factors may be influencing the results.

The results of this regression are presented in Column 2 of Table 18, and correspond to column 4 of Table 5, which provides a triple differences estimate of the deterrent effect of the third strike of .0212 (.0276). The coefficient on *Treatment*Cal*1994*, .046 (.02), is significant at the 5% level. This means that individuals in the treatment group in California after the three strikes law was passed were 4.6 percentage points more likely to be arrested for a felony and convicted of some crime than individuals in the control group. This estimate is similar in magnitude to the estimate in column 4 of Table 5, .0212, where recidivism is defined as a felony arrest.

I perform this same analysis for the one strike treatment group and no strike control group,[88] and present the results in Table 19. These results correspond to those in Column 4 of

[87] The individual need not have been convicted of a felony, just some crime.
[88] The treatment and control groups are the same ones used previously. The treatment group is composed of individuals with one trial and one conviction for a strikeable offense. The control group is composed of individuals

Table 6, which is a triple differences estimate of the deterrent effect of the law on individuals with one strike that estimates the deterrent effect of the law to be -.0145 (.0276). Column 1 of Table 19 shows that defining failure as any arrest does not produce results that are significantly different from defining failure as a felony arrest. Defining recidivism as a felony arrest and a conviction for some crime, however, has a very large effect on the estimate of the deterrent effect for individuals with one strike. The coefficient on *Treatment*Cal*1994*, -.11 (.027), is statistically significant at the 1% level, and means that individuals with one strike are 11 percentage points less likely to be arrested for a felony and convicted of some crime than individuals with no strikes and a similar criminal history. This estimate is nearly 8 times larger than the estimate of -.015 (.028), which was obtained by defining recidivism as any felony arrest.

Overall, these sensitivity analyses show that the particular measures of recidivism that one chooses to use can have a large impact on the measured effect of the deterrent effect of the three-strikes law. I still maintain that defining recidivism as any felony arrest is the most accurate way to measure the deterrent effect of the three strikes law because it ensures that I am measuring only changes in the rate at which felonies are committed. This is important because felonies are the only crimes that lead to penalty increases under the three strikes law. Additionally, defining recidivism as a felony arrest mitigates the problem of discretion by individuals in farther along the law enforcement chain, such as prosecutors or judges, which affects trial and conviction records.

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with one trial for a strikeable offense, but no conviction for a strikeable offense and one conviction for a non-strikeable offense.

e. Differences in incarceration time and criminal tendencies

Even though individuals in the treatment and control groups are very similar with respect to their criminal histories, it is possible that differences in criminal tendencies or incarceration time are masking the law's deterrent effect. Individuals with more strike convictions have likely spent more time incarcerated, and it seems reasonable that this could affect their likelihood of recidivating in the post-law period. Furthermore, because they have spent more time in jail, they have had less of an opportunity to commit crimes. Therefore it is possible that prior criminal history is not accurately controlling for the unobserved factors that influence an individual's propensity to commit crimes.

I add two control variables to ensure that these factors are not influencing the result. The first is simply total incarceration time, prior to release from prison. The second is felony rate per criminal year (FRCY),[89] which is the number of felonies that the individual has committed, divided by the number of years that the individual has spent out of prison or jail since turning 18.

Table 20 shows the results when controlling for these factors, comparing individuals in the two strikes treatment group to individuals in the one strike control group. Column 1 does not control for either of these factors, but does control for prior criminal history. Column 2 controls for FRCY, Column 3 includes for total previous incarceration in place of FRCY, and Column 4 controls for both FRCY and total incarceration time. All four columns control for prior criminal history. The results are nearly identical across all four columns, showing that these factors are not biasing the results. Similarly, controlling for these factors when comparing the one strike treatment group with the no strike control group produces results that are consistent across all four columns (not shown).

[89] This variable will be discussed in more detail later on.

f. Alternative Prior Criminal History Specification

One final sensitivity analysis that I perform is to examine the effects of an alternative definition of criminal history on the results. Previously, I have been using 8 separate offense categories to define each individual's Prior Criminal History. To see if this is a source of omitted variable bias, I make a much more detailed set of PCH variables for the 1994 data set, which encompass 26 different possible offense categories. Because of the way that the 1983 data set is structured, constructing these same, more detailed PCH variables in the 1983 data set would require a huge amount of work. As I find that the more detailed PCH makes very little significant difference in the results, it seemed unnecessary to do so. This set of PCH variables is still not perfectly collinear with the treatment and control groups for the reasons discussed previously, but should better control for any differences in criminal history that affect recidivism.

The results of this regression when looking only at the 1994 data set and comparing the two strikes treatment group with the one strike control group can be seen in Table 21. Column 1 contains no PCH, column 2 contains the 8 category PCH, and column 3 contains the 26 category PCH. I only use data from 1994 for this table,[90] so this is a difference-in-differences analysis looking across states after the three strikes law was passed.

The coefficients on *Treatment*California* are nearly identical in all three columns, and are statistically indistinguishable from each other. Therefore it seems that the treatment and control groups are doing a good job of controlling for differences in criminal history, even in the absence of PCH variables. Furthermore, it seems that the 8 category PCH produces the same results as the 26 category PCH.

[90] Constructing these same PCH variables in the 1983 data set would require a lot of work, and as I find that the more detailed PCH makes no significant difference, it seemed unnecessary to do.

I perform this same analysis for the 1994 data set, comparing the one strike treatment group with the no strike control group, and present these results in Table 22. Column 1 contains no PCH, column 2 contains the 8 category PCH, and column 3 contains the 26 category PCH. The results in column 1 are different than those presented for individuals with one strike previously because this is only a difference-in-differences analysis, looking across states after the three strikes law went into effect. Once again, the coefficients on *Treatment*Cal* in the three columns are statistically indistinguishable. The estimated deterrent effect is slightly smaller when I use the 26 category PCH, but this difference is not statistically significant. Therefore my results are not being driven by differences in criminal history that affect recidivism differently for criminals in the treatment and control groups.

IX. Discussion

I can rule out a deterrent effect larger than -.033 for individuals with two strikes and -.069 for individuals with one strike. These estimates are smaller than those found by most previous authors. The major advantage to performing a triple differences analysis is that it makes it possible to remove sources of bias that cannot be dealt with when only looking at individuals within California after the law was passed. In Table 5, the coefficients on *Treatment*Cal* are consistently negative and are statistically significant. This implies that, even before the law, individuals with two strikes in California were less likely to recidivate than individuals with one strike and a very similar criminal history. There must be omitted California specific, time invariant factors that cause individuals with the same criminal history as the two strikes treatment group to recidivate less frequently than the control group, even in the absence of the 25 years to life penalty that they face under the three strikes law. Authors who only use data from

one time period cannot account for this source of omitted variable bias, and thus will overestimate the deterrent effect of the third strike.

Similarly, the coefficients on *Treatment*Cal* in Table 6 are consistently negative and statistically significant. This shows that individuals in the one strike treatment group in California were significantly less likely to recidivate than individuals with no strikes, even in the absence of the doubled sentence length that they faced under the three strikes law. Therefore, authors who fail to use data from before and after the law went into effect will miss this large source of omitted variable bias and overestimate the deterrent effect of the law.

In addition, the coefficients on *Cal*1994* in both Tables 5 and 6 are consistently negative and statistically significant. This means that recidivism for both treatment and control type individuals in California went down more than in other states. Thus, it is possible that the law is deterring all criminals in California, and not only those with strikes. This is consistent with the idea that forward looking criminals want to avoid committing a second, or even a first strike, because of the future penalty increases that they will face. In other words, by effectively raising the expected cost of engaging in a "life of crime", the California Three Strikes Law may be operating to deter all criminals, regardless of how many strikes they have. But this does not explain why the very harsh penalties faced by criminals with one or two strikes in the immediate future do not provide a strong, additional deterrent effect.

There are several explanations as to why the increased sentences of the three strikes law would operate minimally through a deterrent effect. One fundamental assumption for any deterrent theory of punishment is that criminals are engaging in some type of cost-benefit analysis when determining whether or not to commit a crime. Deterrence operates by increasing the cost, or the punishment, of committing a crime to a high enough level that committing the

crime is no longer worthwhile to the criminal. If criminals are not rational or do not understand the penalties that they face if caught, then a deterrent theory of punishment will not hold. Rational criminals who understand the punishments that they face should reduce, or at least alter, their criminal behavior in response to an increase in the sentence lengths. Irrational or ill-informed criminals will not do so.

If criminals are ill-informed, then they may not understand the severity of the penalty increases that they face under the law, and therefore would not be deterred by it. Because I am looking at criminals' response to the law immediately after it was passed, it seems reasonable that they may not have known about how the law worked. Zimring, Hawkins, and Kamin (2001) chronicle the passing of the law, and find that it was passed quickly, without much debate in either of the legislative houses of government. Furthermore, the wording of the law is not particularly clear, and even highly intelligent economists can misinterpret how strike eligibility works. Therefore it seems perfectly reasonable that uneducated criminals may not understand how the law works, or may not know how many strikes they have. The fact that I was unable to find a statistically significant deterrent effect to the three strikes law is consistent with the hypothesis that, in the immediate aftermath of the law's passing, criminals with one and two strikes either did not know that they faced harsher penalties or did not understand how these harsher penalties would be implemented. Prisoner survey data could be useful to test this hypothesis, but I am unaware of any such data for the three strikes law. Proponents of the three strikes law cite "anecdotal evidence that prisoners inquire about their strike status" as evidence of the fact that criminals are aware of the way in which the law operates and understand the penalties that they face.[91] But I have found no empirical evidence to support this claim.

[91] Prison Policy Institute (1999), 12.

An alternative explanation for the possible lack of a deterrent effect is that criminals understand the penalty increases that they face, but fail to respond to them for other reasons. Lee and McCrary (2005) examine the possibility that criminals are exponential discounters or are hyperbolic discounters, both of which could lead them to fail to respond to the harsher sentences of the three strikes law. Both of these types of individuals value their present utility much higher than utility in the future, and therefore future penalty increases may fail to deter them. Because the individuals with one strike and two strikes are both facing long sentences, then the apparent lack of a deterrent effect could be due to high discount rates, or dynamically time-inconsistent behavior such as hyperbolic discounting. Given my findings, however, I cannot distinguish between either of these possible explanations.

Another possibility which could account for the law operating only minimally through deterrence is that California criminals feel that the law will not be strictly enforced. Brown and Jolivette (2005) find that prior strikes may be dismissed in 25-45% of third strike cases. While their research was done after the introduction of judicial discretion in 1997, it is possible that prosecutorial discretion played a similar role during the years covered by the post-law data set. If criminals felt that prosecutors were unlikely to enforce three strikes sentences, then they would be less likely to alter their criminal behavior in response to the law.

In order to determine whether a significant number of strikes are being dismissed, I performed some brief, back of the envelope calculations, based on my data set. The 1994 data set contains 7,048 criminals released within California, 685 of whom were offenders with two strikes who were convicted of a subsequent felony within three years of their release. This means that I would expect 9.7% of the criminals released in 1994 to have been convicted of a third

strike. In 1994, there were a total of 105,000 offenders released from California prisons.[92] Based on my estimates, this means that if no strikes were dismissed I would expect the total number of criminals who were convicted of a third strike by the end of 1997 to be 10,185. In reality, there were 4,368 third strike offenders jailed as of June 1998,[93] so I will estimate that 4,000 third strikers were jailed by the end of 1997. This means that I have overestimated the number of actual third strike convictions by 255%, Based on these numbers, I estimate that at least one prior strike is dismissed in 65% of cases in which an individual would be facing a third strike conviction.

My estimate of the percent of strikes dismissed is most likely inflated. I am using a rather inclusive definition of "strikes" that was used by Helland and Tabarrok. Therefore it is likely that I am overestimating the true number of people with two strikes. Unfortunately, based on the information in the data set I cannot identify strikes more accurately than this. The offense definitions in the data set do not match up exactly with the California penal code, so it was necessary to accept some amount of error in identifying strikeable offenses. Another reason for the over-estimation is the way in which the data sets were composed. The data set only contains individuals who faced a maximum sentence of at least 1 year for their most recent offense.[94] It seems likely, therefore, that the proportion of criminals with two strikes is greater in the data set than it is in the general population, because the sample is already limited to people who have committed some crime serious enough to face a sentence of at least one year. But even though my estimate of the frequency with which strikes are dismissed is inflated, it still provides strong evidence for the idea that California prosecutors dismiss strikes in a meaningful proportion of

[92] United States Department of Justice: Bureau of Justice Statistics (2002).
[93] Males, Macallair, and Taui-Eddin (1999).
[94] This refers to whatever offense they were being released from prison for in 1994 (or 1983).

third strike cases. If criminals do believe that prosecutors are likely to ignore strikes, then the deterrent effect of the law would be greatly mitigated.

Another interesting possibility investigated by Tonry (1996) is the effect that a criminal's age has on his likelihood of recidivating. He finds that a criminal's propensity to commit property crimes greatly decreases beginning at age 18, and the propensity to commit violent crimes falls off sharply beginning at age 22.[95] The coefficients on my age splines in Tables 4 and 6 tell a similar story, with likelihood of recidivism decreasing sharply after age 18. The average age of a criminal in both my two strike treatment and one strike control groups is 33, and the average age of a criminal in my one strike treatment and no strike control groups is 32. Therefore, these individuals have aged substantially past their criminal "prime". If Tonry's findings are applicable to these criminals, then it is possible that they were unlikely to reoffend anyway. I attempt to test this hypothesis in Tables 7 and 8, by creating an interaction term between *Treatment*Cal*1994* and each of six age categories. If Tonry's findings are applicable to the three strikes law, I would have expected to see a significant negative coefficient for the younger age categories, and smaller coefficients for the older age categories. This is because older individuals are less likely to recidivate even in the absence of the three strikes law, while young criminals who are likely to recidivate in the absence of a penalty increase will be significantly deterred, relative to their normal recidivism rate. Tables 7 and 8 directly contradict this idea, with younger individuals who face harsher penalties under the three strikes law actually being more likely to recidivate than individuals in their same age group who face less harsh penalties. Therefore, it seems likely that there is some omitted variable that varies between the treatment and control groups and causes young criminals in the treatment group to recidivate more frequently than young criminals in the control group.

[95] Tonry 1996, 8.

My results are consistent with the idea that criminals in California have changed their selection of the types of crimes that they commit in response to the incentives that they face under the three strikes law. Individuals with two strikes are 9.2 percentage points more likely to commit a serious or a violent felony, given that they commit some felony, than are comparable individuals with one strike. This makes sense because they face the same penalty regardless of what type of felony they commit, so they benefit more from committing more serious, high rewarding crimes. Individuals with one strike, on the other hand, are 8.2 percentage points less likely to commit a serious or violent felony, given that they commit some felony, than comparable individuals with no strike. This is because, as long as they do not commit another serious or violent felony, they can avoid facing the 25 years to life penalty that comes with the third strike. In addition, because they face a double sentence for their next felony conviction, the severity of the crimes that they commit matters. Therefore they have incentives to commit less serious crimes.

Up to this point, my results are consistent with the idea that the three strikes law, for whatever reason, does not provide an additional deterrent effect for individuals with one and two strikes. But to this point I have been greatly restricting the samples, to only include individuals with very similar criminal histories. While this contains several advantages, such as greatly reducing potential sources of omitted variable bias, it also has drawbacks, mainly in the form of small sample sizes and high standard errors. In this next section, I move away from this very limited treatment and control approach and use a much broader approach, based on Iyengar's methodology.

X. Overall Deterrent Effect of the Three Strikes Law

a. Methodology

In this section, I include all of the individuals who were released from prison in my sample, and attempt to control for as many sources of omitted variable bias as possible. This approach is based on Iyengar's methodology. By comparing individuals with two and one strikes with individuals with no strikes, and controlling as strictly as possible for differences in criminal history, I should be able to produce an unbiased estimate of the overall deterrent effect of the three-strikes law. I will still be using data from across states and time periods, and performing a triple differences approach.

The regression that I will be estimating is very similar to equation (7), but I must account for effectively having two treatment groups: one strike and two strikes. Therefore, I will be estimating equation (8):[96]

$$(8) \quad R_i = \beta_1(\text{TwoStrikes} * Cal * 1994) + \beta_2(\text{OneStrike} * Cal * 1994) + \beta_3(\text{TwoStrikes} * Cal) + \beta_4(\text{OneStrike} * \text{Cal}) + \beta_5(Cal * 1994) + \beta_6(\text{TwoStrikes} * 1994) + \beta_7(\text{OneStrike} * 1994) + \beta_8(\text{TwoStrikes}) + \beta_6(\text{OneStrike}) + Prior\ Criminal\ History\ Vector + Felony\ Rate\ Per\ Criminal\ Year + Individual\ Characteristics + State\ Fixed\ Effects + State * Year\ Effects + Age\ Spline$$

R_i still measures recidivism, and takes a value of 1 if the individual is arrested for a felony within three years of his release and 0 if he is not. Equation (8) contains all of the same interaction terms as equation (7) for both the two strikes treatment group and the one strike treatment group. All of these interactions function similarly to the way that they did in equation (7). β_1 is the estimate of the overall deterrent effect of the law on individuals with two strikes, and β_2 is the same measurement for individuals with one strike. If there is a statistically significant difference

[96] This is based on Iyengar's Equation 7, which I have reproduced as my Equation 1.

between β_2 and β_1, it means that individuals with two strikes are being deterred at a different rate compared to individuals with one strike. If the twenty five year to life penalties for the third strike actually deter individuals with two strikes, then β_1 should be significantly more negative than β_2.

The control group in this regression is individuals with no strikes. Because the data sets are composed of people who were released from prison in a certain year, everyone in the sample has been arrested for some crime and has spent some time in prison. In these regressions it will be more important to control for criminal histories is because, unlike my previous regressions where I carefully constructed treatment and control groups that were very similar to each other, the treatment and control groups in this regression will have very different criminal histories. Controlling for criminal history will help to reduce any potential sources of omitted variable bias that affect recidivism and vary based on an individual's criminal history. I will use the same 8 category PCH variables that I used in the previous sections because these are the PCH variables that Iyengar uses.

I also follow Iyengar's methodology and create a Felony Rate per Criminal Year (FRCY) variable. FRCY is calculated as follows:

FRCY = (Felony Convictions Prior to Release) /(Age – 18 – Total Incarceration Time) (8)

FRCY is the number of felonies that an individual committed per year out of prison since turning 18. This will take into account the fact that individuals with different criminal histories and individuals of different ages may be more "crime prone" than others. Iyengar states that controlling for both age and FRCY allows for "both an age effect as well as a rate effect, conditional on age."[97]

[97] Iyengar 2008, 16.

b. Overall Deterrence Results

The results of this regression are presented in Table 23. Column 1 includes state, state-year, and individual controls. Column 2 adds PCH variables to the regression, to control for the substantial differences in criminal history between individuals with no strikes, one strike, and two strikes. The coefficient on *TwoStrikes*Cal*1994*, -.39 (.022), is significant at the 1% level and means that the threat of the third strike reduces recidivism by individuals with two strikes by 39 percentage points. The coefficient for *OneStrike*Cal*1994*, -.394 (.0206), is significant at the 1% level and means that the three strikes law reduces recidivism by 39.4 percentage points for criminals with one strike. These are huge deterrent effects which are far larger than any that I have found in the previous literature.

But it seems likely that there are omitted variables influencing these results, as individuals in the no-strike control group are very different from individuals in two treatment groups. Therefore, in table 24, I have limited the sample to individuals with at least one previous felony arrest.[98] This eliminates criminals from the sample who have only been arrested for misdemeanor crimes, and are therefore probably very different from criminals with one or two strikes.

Column 4 of this table controls for both FRCY and total incarceration time. Both the coefficient on *TwoStrikes*Cal*1994*, -.02 (.018), and the coefficient on *OneStrike*Cal*1994*, .0115 (.0172), are statistically insignificant, providing no clear evidence of a deterrent effect to the law. But the difference between these two coefficients, -.031, is statistically significant at the 10% level, implying that individuals with two strikes are 3.1 percentage points less likely to recidivate than individuals with one strike. This supports the idea that the 25 year to life penalty that individuals with two strikes face has more of a deterrent effect than the double sentence

[98] For the rest of the paper, the samples are limited to only include those with at least one prior felony arrest.

faced by criminals with one strike. These results are more likely to be biased by omitted variables than are my treatment and control regressions because the criminals being compared have vastly different criminal histories. However, this estimate is within my 95% confidence interval for the deterrent effect of the third strike (-.033, .075) from my earlier treatment and control regressions. Therefore I cannot rule out a small deterrent effect to the large penalty increases that criminals with two strikes face.

The coefficient on *OneStrike*Cal,* -.034 (.-012) is significant at the 1% level and means that individuals with one strike in California were 3.4 percentage points less likely to recidivate, even without the increased penalties of the three strikes law. This means that using data from only after the law was passed would lead to an overestimation of the law's deterrent effect on criminals with one strike.

The coefficient on *TwoStrikes*Cal,* .024 (.13), is significant at the 10% level, and means that, criminals with two strikes in California were more likely to recidivate than those with no strikes in 1983. This makes sense, as there are probably omitted variables that vary between criminals with the serious history of two strikers and criminals with no strikes that influence their recidivism rates.

The coefficient on *Cal*1994*, -.054 (.0056), is statistically significant at the 1% level, and helps to explain the differences between my results and Iyengar's results. This coefficient means that, between 1983 and 1994, recidivism in California went down 5.4 percentage points more for individuals with no strikes and at least one prior felony arrest between than it did for similar individuals in other states. Iyengar, who only used data on California, could not see that recidivism rates for criminals with no strikes decreased more in California than in other states. This would lead her to calculate a larger deterrent effect of the law for criminals with one and

two strikes, as it would seem that the law Therefore, she may have over-estimated the law's deterrent effect for both individuals with one and two strikes.

The coefficient on *FRCY*, .051 (.012), is significant at the 1% level. This means that for each additional felony per year out of prison that one has committed in his past, he is 5.1 percentage points more likely to recidivate in the future. The coefficients on the PCH variables, except for *Rape*, are all significant and positive. This means that each additional conviction for one of these crimes in an individual's past makes him more likely to recidivate. This is what I expected because it means that the more crimes an individual has committed in his past, the more likely he is to commit a crime in the future.

c. Altered Criminal Behavior in Response to the Three Strikes Law

Next, I examine the effects that the three strikes law has on the probability that an individual commits a violent crime, given that he commits a felony during the three years after his release. Again, the sample is limited to those with at least one felony arrest prior to their 1983 or 1994 release from prison. The results of this analysis are included in Table 25. Column 1 includes PCH variables, column 2 adds the FRCY controls, and column 3 adds a variable to control for total incarceration time. The coefficient on *TwoStrikes*Cal*1994*, -.053 (.032), is statistically insignificant. It implies that having two strikes makes an individual choose violent crime 5.3 percentage points less frequently, but I cannot rule out a zero effect. This same estimate for individuals with one strike, -.011 (.032), is also statistically insignificant. The difference between these two coefficients, -.042, means that individuals with two strikes are 4.2 percentage points less likely to choose violent crime than individuals with one strike, but these results are statistically insignificant.

In addition, individuals who have committed violent crimes in their past, such as murder, rape, assault, and robbery, are significantly more likely to commit a violent crime after their release from prison. For example, each murder conviction in an individual's criminal history means that he is an estimated 8 percentage points more likely to commit a violent crime in the future. Individuals who have committed non-violent crimes, such as burglary, theft, and drug crimes, are significantly less likely to choose a violent crime, given that they commit a crime within three years of their release.

I also examine the possibility that individuals changed the rate at which they commit strikeable offenses, which are serious or violent felonies, after the three strikes law. The results of this regression are presented in Table 26. Column 1 controls for state, state-year, and individual controls, and column 2 includes prior criminal history variables. Individuals with two strikes were 1.7 percentage points more likely to commit a serious or violent felony, conditional on recidivating, than were individuals with no strikes, but this result is statistically insignificant. Individuals with one strike were 2.3 percentage points less likely to commit a serious or violent crime. The difference between these two estimates, .04, is statistically significant at the 10% level, and means that individuals with two strikes were 4 percentage points more likely to commit a strikeable felony, conditional on recidivating, than were individuals with one strike.

XI. Sensitivity Analyses

a. Linear Probability Model v. Probit and Logit Models

Table 27 includes the regression from equation (8), but using the probit and logit models instead of the linear probability model. The sample is one again limited to only include individuals with at least one felony arrest prior to their 1983 or 1994 release. Column 1 shows

probit marginal effects, reported at the mean of the data, and column 2 shows the logit marginal effects. The estimates in this table are analogous to the linear probability model estimates of the law's overall deterrent effect in column 2 of Table 24, in which the sample is limited to criminals with at least one prior felony arrest. The logit and probit estimates are nearly identical to those produced by the linear probability model, which shows that the linear probability model is not providing biased estimates.

Table 28 shows the estimates produced by the logit and probit model when looking at the law's effect on the probability that an individual commits a violent crime, conditional on reoffending. The sample is limited to only include individuals with at least one felony arrest prior to their 1983 or 1994 release and one felony arrest after their release. Column 1 reports the estimates of the probit model, and column 2 reports the estimates of the logit model, both of which are reported as marginal effects at the mean. These results use the same specifications as those in column 2 of Table 25, which provides the linear probability model estimates for the effect of the law on the propensity of criminals to choose violent crime. The probit and logit models produce estimates for the deterrent effect of the law on individuals with one and two strikes that are much larger than the estimates produced by the linear probability model. This means that the linear probability model may be biased when looking at the rate of violent crime in the aggregate sample, and therefore the results should looked at with scrutiny.

b. Alternative definitions of recidivism

Table 29 shows the results of the regression in equation (8) when I use alternative definitions of recidivism. These results include PCH variables, individual controls, state-year effects, and state-fixed effects. They correspond to the estimates in column 3 of Table 24, where

the sample was limited to criminals with at least one prior felony arrest and recidivism was defined as an arrest for any felony. In column 1, recidivism is defined as an arrest for any crime, which is the definition of recidivism used by Helland and Tabarrok. This produces a significantly larger deterrent effect for both individuals with one and two strikes. The coefficient on *TwoStrikes*Cal*1994*, -.072 (.017), is statistically significant at the 1% level, and means that the law reduces the probability that an individual with two strikes is arrested for any crime by 7.2 percentage points. The coefficient on *OneStrike*Cal*1994*, -.035 (.018), is significant at the 10% level, and means that individuals with two strikes are 3.5 percentage points less likely to be arrested for any crime after their release. This is consistent with the idea that the double sentences faced by individuals with one strike provide a small deterrent effect. Furthermore, this provides strong evidence of an additional deterrent effect for the 25 year to life penalties faced by individuals with two strikes. The 7.2 percentage point reduction in recidivism is very close to Helland and Tabarrok's estimate of 8 percentage points.

Column 2 defines recidivism as an arrest for a felony and a conviction for some crime on that arrest. The coefficients on *TwoStrikes*Cal*1994*, -.064 (.0159), and *OneStrike*Cal*1994*, -.052 (.013), are both significant at the 1% level and are statistically indistinguishable from each other. This means that the law deters both individuals with one strike and two strikes equally, and that the additional penalty faced by criminals with two strikes provides no more of a deterrent effect than the double sentences faced by criminals with one strike.

The fact that this definition of recidivism provides strong evidence of reduced recidivism for individuals with one and two strikes confirms my earlier suspicions that there may be some sort of prosecutorial, judicial, or jury bias influencing these results. The only difference between this definition of recidivism and my previous definition of recidivism (any felony arrest) is that

this definition adds the requirement that the individual must have been convicted of some crime. This means that the prosecutor must press charges, and the individual must either accept a plea bargain or be convicted at trial. For some reason, individuals with one or two strikes were equally likely to be arrested for a felony when compared to individuals with no strikes, but were significantly less likely to be convicted of some crime on those arrests. This can be explained in several ways. If individuals with strikes were less likely to accept plea bargains and preferred to take their chances at trial, it would reduce their conviction rates. Similarly, if prosecutors were hesitant to press charges against criminals with previous strikes, or juries were hesitant to convict them, it would also reduce their conviction rates. Another possibility is that, in the aftermath of the three strikes law, police became more likely to arrest criminals with one or two strikes, especially when they was little evidence against them or when they were not guilty. The three strikes law was popular when it was passed, so it is possible that the police wanted to show the people of California that they were stringently enforcing the by arresting more one strike and two strike offenders.

These two alternative definitions of recidivism show that my results are very sensitive to the particular definition of recidivism that I use. In addition, these sensitivity analyses confirm Iyengar's suspicion that, the further along criminal data is collected, the more it is affected by discretion. Therefore, using a definition of recidivism that involves conviction records may bias estimates of the law's deterrent effect.

XII. Discussion

There are huge differences between the results in Table 23, which included all criminals in the data set, and Table 24, which limited the data set to only include criminals with at least

one prior felony arrest. This huge difference is attributable to the fact that criminals with no prior felony do not provide an appropriate counterfactual for criminals with one or two strikes. The coefficient on *Cal*1994* when including all criminals (column 4 of Table 23), .36 (.013), means that recidivism rates increased by 36 percentage points between 1983 and 1994 for criminals with no strikes in California. On the other hand, the coefficient on *Cal*1994* when looking only at criminals with at least one prior felony arrest (column 4 of Table 24), -.054 (.0056), means that recidivism rates decreased by 5.4 percentage points in California for criminals with no strikes and at least one prior felony arrest. Therefore, when including all criminals in the sample, the counterfactual is that recidivism in California would have skyrocketed for individuals with one strike and two strikes if it were not for the three strikes law. This explains the huge estimates that I find in Table 23. But the individuals that compose the no strike group in this regression are too different from criminals with one and two strikes and therefore do not provide a good counterfactual.

When limiting the sample to only include criminals with at least one prior felony arrest, the recidivism rate for the no strikes group actually decreased slightly between 1983 and 1994. The counterfactual in this regression is that recidivism rates for individuals in California with one or two strikes would have decreased between 1983 and 1994, even in the absence of the three strikes law. The fact that recidivism for criminals with one or two strikes in California would have decreased even in the absence of the three strikes law helps to explain why, when limiting the sample to only include criminals with a prior felony arrest, I find so little evidence of a deterrent effect for criminals with one or two strikes.

This still leaves an interesting question unanswered: why did the recidivism rate for people without a prior felony increase so much in California between 1983 and 1994? It may

have been an effect of the crack cocaine epidemic, or the crime epidemic in general. The answer to this question is, however, beyond the scope of this paper. Whatever the reason for this huge increase in recidivism may be, I believe that limiting the sample to individuals with a prior felony conviction makes the most sense for two reasons. First of all, I am using Iyengar's methodology as a basis for these regressions, and she limits the sample to criminals with at least one prior felony. Secondly, it seems pretty clear that criminals with no prior felony arrest are very different from criminals with one or two strikes, and thus do not provide a very useful counterfactual group. While criminals with no strikes are always going to be very different from those with one or two strikes, at least limiting the sample to include only those with a felony arrest ensures that the individuals in the counterfactual group have been engaged in some sort of serious criminal activity.

The most consistent finding across this entire paper is that the threat of the third strike provides no, or very little, additional deterrent effect for individuals with two strikes. It seems that the double penalties faced by individuals with one strike and the 25 year to life penalty faced by individuals with two strikes both fail to reduce crime significantly through deterrence. But my findings do not necessarily mean that the law completely fails to deter crime. The coefficient on *Cal*1994* in column 4 of Table 24, -.0536 (.005), shows that the law may have been operating to deter criminals with no strikes and at least one prior felony arrest. This estimate, which is significant at the 1% level, means that California criminals with no strikes and a prior felony arrest were 5.4 percentage points less likely to recidivate in 1994 than they were in 1983. As I discussed previously, the three strikes law could act to deter criminals with no strikes by increasing the cost of a life of crime for all criminals. Criminals with no strikes are generally early on in their criminal career relative to criminals with one and two strikes and may therefore

choose to abandon a life of crime in the face of the future penalty increases of the three strikes law. In other words, I do not find strong evidence of an additional deterrent effect for individuals with one and two strikes, but I do find evidence that the law may operate to reduce crime even before individuals actually face its harsher penalties.

Another consistent finding across this paper is that the estimated deterrent effect of the three strikes law varies greatly depending on how I measure recidivism. Even similar definitions of recidivism can produce very different estimates of the law's deterrent effect. For example, in column 2 of Table 24, when I define recidivism as any felony arrest and limit the sample to those with at least one prior felony arrest, I estimate the deterrent effect of the law to be 2 percentage points for criminals with two strikes. But, in column 2 of Table 29, when I add the restriction that the individual must have been convicted of some crime on that arrest, I estimate the deterrent effect of the law to be 6.4 percentage points for criminals with two strikes. And when I expand the definition of recidivism to include an arrest for any crime, I estimate the deterrent effect of the law to be 7.2 percentage points for criminals with two strikes. Thus, different reasonable definitions of failure can produce large variations in the estimate of the law's deterrent effect. I believe that the best way to define recidivism is an arrest for any felony because, under the three strikes law, criminals only face increased penalties for felonies. It does not seem unreasonable to assume that criminals who understand the penalty structure under the three strikes law can understand the difference between a felony and a misdemeanor.[99] Therefore, for the reasons discussed throughout this paper, I maintain that the best way to define recidivism for the purposes of this paper is as a felony arrest.

[99] I mean to imply here that criminals who are smart enough to alter their behavior because of the three strikes law probably understand that more serious crimes are felonies and less serious crimes are misdemeanors. This seems plausible, considering that all individuals in the data set have had previous interactions with the legal system and have spent time in prison.

One plausible scenario in light of these results is that, during the 1994 through 1997 post law period, criminals did not know about or did not understand the three strikes law. If criminals did not know about the sentence enhancements that they faced, they would not be deterred by the law. The law only went in to effect on March 6, 1994, and criminals who were released immediately after this date may not have known about the law's passing or may not have understood the details of the law. The law is complex and not clearly worded, so a lack of knowledge or understanding could be at least partially responsible for the law's failure to deter crime.

A similar, plausible explanation for these results is that criminals with one or two strikes did not believe that the three strikes law would be stringently enforced, and therefore were not deterred by the law. If they believed that prosecutors would ignore previous strikes or that juries would be less willing to convict knowing that it would send them to jail for 25 years, it would weaken the law's deterrent effect. Brown and Jolivette (2005) and Zimring (2001) both find evidence that prosecutors could use their discretion in determining the number of strikes that an individual had. Additionally, comparing Column 4 of Table 24, where recidivism is defined as a felony arrest, with Column 2 of Table 29, where recidivism is defined as a felony arrest followed by a conviction for any crime, shows that the estimated deterrent effect for individuals with one or two strikes is significantly greater when the definition of recidivism contains the requirement that the individual was convicted of some crime. This supports the idea that criminals with one or two strikes were, for whatever reason, less likely to be convicted of a crime than similar individuals with no strikes. If criminals with strikes felt that they were unlikely to be convicted because of sympathetic juries, or felt that prosecutors were likely to ignore previous strikes in their criminal records, the law would act as a much weaker deterrent.

If criminals are aware of the penalties that they face under the law and believe that these penalties will be consistently enforced, then there is another possible explanation for why they are not deterred. If criminals practice irrational, time- inconsistent behavior, such as hyperbolic discounting, or have very high discount rates, then they might fail to be significantly deterred by the penalty increases of the three strikes law. Individuals with these types of discount rates may be unable to refrain from committing pleasurable or rewarding actions in the present, even if these actions come with huge future costs. Criminals who practice this type of discounting would value their present day utility much more than their utility in future periods, and may not refrain from committing crimes in the future even if they know about the huge punishments that they face if caught. If criminals make decisions in this way, they would fail to be deterred by the three strikes law.

When analyzing the effect of the law on an individual's propensity to commit serious or violent crimes and including all individuals with at least one prior felony (Tables 25 and 26), I find no significant evidence that criminals altered their choice of crimes in response to the three strikes law. But in my earlier regressions, when comparing individuals with very similar criminal histories but different numbers of strikes (Tables 10 and 12), I find that, under the three strikes law, criminals with one strike are less likely to commit a serious or violent felony, conditional on recidivating, while criminals with two strikes are more likely to do so. This makes sense given the incentives of the three strikes law. Column 2 of Table 10 shows that criminals with two strikes and two trials for strikes are 9.2 percentage points more likely to commit a serious or violent felony, conditional on recidivating, than are criminals with two trials for strikeable offenses, but only one conviction for a strikeable offense and one conviction for a non-strikeable

offense. This is because criminals with two strikes do not face any increase in sentence length based on the severity of their crimes, so they have no incentive to commit less severe crimes.

Column 2 of Table 12 shows that criminals with one strike and exactly one trial for a strikeable offense are 8.2 percentage points less likely to commit a serious or violent felony, conditional on recidivating, than are criminals with one trial for a strike, no strikeable convictions, and one conviction for a non-strikeable offense. This is because criminals with one strike face a double penalty, and doubling the sentence of a serious felony is a greater absolute sentence increase than doubling the sentence of a non-serious felony. In addition, criminals with one strike may want to avoid committing serious or violent felonies so that they do not receive a second strike on their record. Rational, forward looking individuals with one strike would want to avoid committing a second strike so that, in the future, they do not have to face the severe 25 years to life penalties faced by individuals with two strikes.

The results in Tables 10 and 12 are difficult to reconcile with the idea that criminals are ill-informed, completely irrational, or have very high exponential discount rates. If criminals with one strike are altering their criminal behavior to avoid committing strikeable offenses, then they must have some understanding of how the law works. If, instead, they are avoiding committing serious felonies because of the longer double sentences that they will face, then it seems unlikely that they have high discount rates. Therefore, these results provide evidence that criminals with one and two strikes have at least some understanding of how the law works, and are not engaging in completely irrational behavior.

XIII. Cost-Benefit Analysis

In this section, I attempt to perform a simple cost-benefit analysis to determine if, based on my estimates for the deterrent effect of the law, the law is cost-effective. I will use numbers based on those that Helland and Tabarrok use for their cost-benefit analysis because their estimates are based on the same years that I am examining. If I take the largest possible deterrent effects[100] from my treatment and control group estimates, I find that the three strikes law reduces recidivism for criminal with one strike by 6.9 percentage points, and that the 25 year to life sentence imposed on individuals with two strikes reduces their recidivism rate overall by an additional 3.3 percentage points. I will use these numbers for my cost-benefit analysis, as they are the most generous that my results allow.

The felony-arrest rate for individuals in the no strike control[101] group was 44.5%, which means that a reduction in recidivism of 6.9 percentage points for comparable individuals with one strike is a 15.5 % reduction in crime rates for individuals with one strike. Zimring, Kamin, and Hawkins (2001) estimate that, in the year before the three strikes law was passed, 7.2% of all California felonies were committed by individuals with one strike. I assume that crimes in general are deterred at the same rate as arrests, and use data from the California Attorney General's Office, which states that there were 487,364 felony arrests were in California in 2002.[102] Therefore the three strikes law reduces the number of crimes committed by individuals with one strike by *487,364*.072*.155/(1-.155)* = 6,437 felony arrests per year.[103] Using Helland and Tabarrok's estimate that 1 in 2.5 crimes are actually reported to the police, I find that the law

[100] Calculated as the lower bound of my 95% Confidence Interval.
[101] Individuals with one trial for a strikeable offense and no strikeable conviction, but one conviction for a non-strikeable offense
[102] This data is publicly available at http://stats.doj.ca.gov/cjsc_stats/prof08/00/3A.htm.
[103] I have to adjust for the number of crimes that would have been committed in the absence of the three strikes law by multiplying the deterrence figure by *1/(1-.155)*.

reduces the number of crimes committed by individuals with one strike by 16,093 felonies per year.

Individuals with one strike face an average sentence of 43 months,[104] so in the absence of the double sentences under the three strikes law they would face an average sentence of 21.5 months, assuming that they still commit the same types of crimes.[105] Therefore, the second strike provision of the three strikes law increases the average sentence for individuals with one strike by 21.5 months, or 1.8 years. I use Helland and Tabarrok's estimate of the average cost of incarcerating an individual for one year, which is $35,000.[106] Therefore the additional prison time for these individuals costs the state *1.8 * $35,000* = $63,000.[107] Between 2001 and 2004, the number of individuals in prison under this provision of the law remained constant at 35,000. The increase in incarceration costs is *$63,000 * 35,000 offenders* = $2.2 billion. This means that the reduction in crime attributed to deterrence for criminals with one strike is *$2.2 billion / 16,093 felonies* = $137,016 per felony avoided. This is much higher than the average cost to society per crime, which is generally estimated to be around $34,000.[108]

The recidivism rate for individuals in the one strike control group was 47.9%.[109] Therefore a reduction in recidivism of 3.3 percentage points for comparable individuals with two strikes is a 6.9% reduction in crime rates for individuals with two strikes. Zimring, Kamin, and Hawkins find that individuals with two strikes are responsible for 3.2% of the total number of

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[104] Brown and Jolivette, 2005.
[105] This is confirmed by Brown and Jolivettte, who find that the average time served by felons before the law was passed was 21 months. Much of the information used for these calculations are based on their estimates.
[106] This estimate is the average annual cost of incarcerating an inmate, and does not take into account the aging of the criminal population under three strikes sentencing.
[107] Once again, this is confirmed by Brown and Jolivette, who estimate the additional cost to be $60,000.
[108] This estimate of the cost of crime was calculated by Helland and Tabarrok, by dividing the estimated total cost of crime in the United States by the total number of index crime committed (328). Index crimes are more serious crimes, and are therefore the cost per index crime should approximate the cost per felony.
[109] Individuals with two trials for strikeable offenses, one conviction for a strikeable felony, and one conviction for a non-strikeable offense

felonies committed in California each year. Therefore the reduction in overall crime attributed to the deterrent effect of the third strike is 0.2%. It seems unnecessary to proceed farther, given that Helland and Tabarrok find that the additional cost for imprisoning third strikers is $4.6 billion, and the aggregate reduction in crime is far less than I found for individuals with one strike.

It is also possible that the law reduces the cost of crime to society by affecting the types of crimes that individuals choose to commit. Here I will estimate the cost of the change in probability that individuals with one or two strikes choose serious or violent crime, assuming no change in their overall rate of recidivism. In other words, I am assuming that the overall crime rates are not changed by the three strikes law, but that only the types of crime committed by criminals with one and two strikes is changed. Table 12 shows that individuals with one trial and one conviction for a strikeable offense are 8.2 percentage points less likely to commit a serious or violent felony, conditional on recidivating, than are individuals with no strikes and a similar criminal history. 32% of the criminals released in the no strike control group were re-arrested for a serious or violent crime after their 1994 release. Assuming that the law does not reduce any crime, the probability that these one strike individuals recidivate within three years of their 1994 release is 44.5%. Therefore, the counterfactual is that, in the absence of the law, *.32 * .445* = 14.4% of the individuals with one strike would have committed a serious or violent felony within three years of their release.

But, in Table 12, I find that there was an 8.2 percentage point decrease in the likelihood that individuals with one strike commit a serious or violent felony, conditional on recidivating. This means that the law caused one strike offenders to reduce their likelihood of committing a serious or violent felony by *8.2 / 32* = 25.6%. Thus, the law reduced the percentage of serious or violent crimes that people with one strike commit by *.256 * .144* = 3.7% of their overall felonies.

If offenders with one strike are responsible for 7.2% of the 487,364 felony arrests committed in California each year, then they are responsible for 35,090 reported felonies per year in California. Once again, I multiply this number by 2.5 to adjust for the FBI's estimated ratio of the difference between reported and actual crime rates, to find that criminals with one strike are responsible for approximately 87,725 felonies each year in California.

This means that criminals with one strike committed *.037 * 87,725* = 3,246 less serious or violent felonies than they would have in the absence of the three strikes law, committing 3,246 more non-serious crimes in their place. The difference between the cost to society of a serious or violent felony, which I calculate in Appendix A, and a non-serious felony is $*104,886 - $34,000* = $70,886, meaning that this reduction in serious or violent crime reduced the annual cost of crime in California by approximately $230 million. While this reduction in the cost of crime is substantial, it is still far less than the additional costs of imprisoning third strike offenders, which I previously estimated to be $2.2 billion.

In Table 10 I find that criminals with two strikes are 9.2 percentage points more likely to commit a serious or violent felony, conditional on recidivating, when compared to similar individuals with one strike. Performing the same calculations as above, I estimate that in the absence of the law, 17.7% of individuals with two strikes would have committed a serious or violent felony within three years of their release. Because of the incentives of the three strikes law, however, I estimate that criminals with two strikes increased their likelihood of committing violent crime by 4.4%. This means that they committed an additional 1,715 serious or violent felonies in place of less serious crimes. At an additional cost of $70,886 per serious or violent crime, this means that the increase in severity of crime for individuals with two strikes costs the state of California an additional $122 million annually.

The large sentence increases mandated by the three strikes law are extremely expensive and seem to have little, if any, impact on deterring criminals with one or two strikes. But the law also operates through incapacitation, and one cannot deny that keeping the most serious, violent, recidivist offenders incarcerated for long periods of time will reduce crime. Examining the incapacitation effect of the law is beyond the scope of this paper. But if previous research on incapacitation, such as Levitt's 1996 paper "The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Litigation", are applicable to the California Three Strikes Law, then it seems likely that the law would greatly reduce crime through incapacitation. Levitt's findings indicate that "if the amount of incarceration is to be increased, keeping the current pool of prisoners behind bars for a longer period of time is likely to be a more advisable public policy approach" than incarcerating more prisoners.[110] The three strikes law operates in exactly this way, so it seems likely that it operates effectively through an incapacitation effect. Investigating the total amount of crime that the three strikes law reduces through incapacitation would be an important improvement for performing a more comprehensive cost-benefit analysis of the law. But when looking only at the law's deterrent effect, it is clear that the California Three Strikes Law is not a cost-effective method of reducing crime.

XIV. Conclusion

Using a triple differences analysis, I find weak, if any, statistical evidence that the third strike penalty of the California Three Strikes Law operates to reduce crime through deterrence. The 25 year to life sentence that individuals with two strikes face does not appear to deter criminals any more effectively than the double sentence faced by individuals with one strike, and I can rule out a deterrent effect larger than 3.3 percentage points, or 6.9%, for the threat of the

[110] Levitt 1996, 347.

third strike. Similarly, I find weak, if any, evidence that the double penalties faced by individuals with one strike reduce recidivism rates through a deterrent effect. For the double sentences, I can rule out a deterrent effect greater than 6.9 percentage points, or 15.5%. But this is not to say that the law does not deter any crime.

I find some evidence that supports the idea that the law may be acting to deter crime for individuals with no strikes and at least one prior felony arrest. After the three strikes law was passed, recidivism decreased by 5.4 percentage points more for these criminals than it did for similar criminals in other states, even though the no strike criminals in California do not face immediate penalty enhancements under the law. The three strikes law raises the cost of engaging in a life of crime for all California criminals. Therefore, if criminals are future oriented, then it is possible that the law would deter them regardless of the number of strikes that they have.

I also find evidence that criminals alter the type of crime that they commit in response to the differing incentives that they face under the three strikes law. Criminals with one strike are 8.2 percentage points less likely to commit a serious or violent felony, conditional on recidivating, when compared to individuals with no strikes and a similar criminal history. This can be viewed as a deterrent effect of sorts, as it leads criminals with one strike to commit less costly crimes. But because of the perverse structure of the law's penalty enhancements, criminals with two strike trials and convictions are 9.2 percentage points more likely to commit a serious or violent felony, conditional on recidivating, than are similar criminals with one strike. This is an unintended consequence of the law which arises because criminals with two strikes face the same penalty upon their next felony conviction, regardless of the severity of the felony that they commit.

The major contribution of my analysis to the previous literature on the deterrent effect of the three strikes law is the unique combination of data sets that I use, which enables me to compare differences in recidivism rates both across time and across states. Using this multi-dimensional data is important because I am able to control for omitted variables that affect recidivism and are constant either across time or across states. I do find statistically significant evidence that there is a source of omitted variable bias that is constant across time and affects the recidivism rates of individuals in California differently based on whether they have one strike or two strikes. This factor seems to reduce the recidivism rate of individuals with two strikes with respect to similarly situated individuals with one strike. The magnitude of this omitted variable bias is quite large relative to the estimates that some previous researchers have found for the deterrent effect of the three strikes law, and is robust to many specifications. Any researcher who only uses data from after the three strikes law went into effect will therefore run the risk of over-estimating the law's deterrent effect. In addition, using evidence from across states makes it possible to remove the influence of any omitted factors that cause the difference in recidivism between individuals in my treatment and control groups to change over time. Without looking across states, it would have appeared that the law had a much larger deterrent effect than it actually does. Overall, using multi-state, cross-time data enables me to remove these potential sources of omitted variable bias and conclude that the additional penalties faced by criminals with one and two strikes deter very little, if any, additional crime.

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Figures

Figure 1. Violent and Property Crime Rates in the United States: 1960-2008

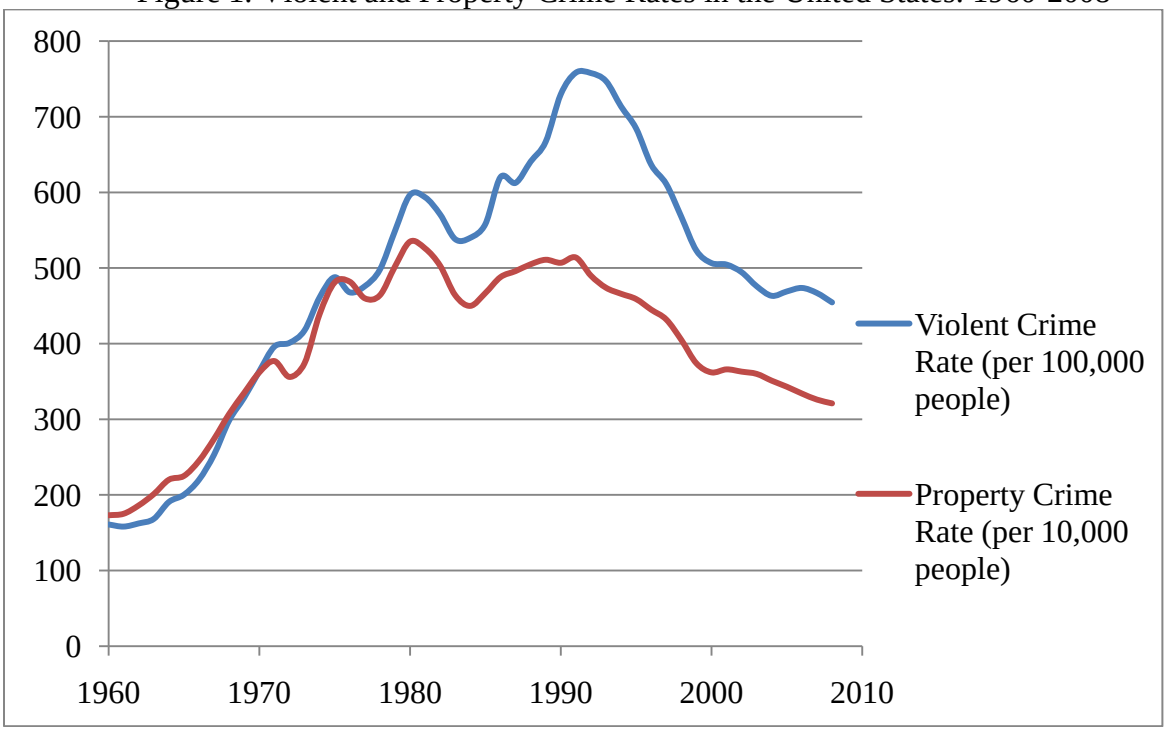

Figure 2. Functioning of Three Strikes Law[111]

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Table 1.2. Sequence and Penalty Under California Three Strikes Law

	Offender A	Offender B	Offender C
First conviction	Theft	Burglary	Burglary
Second conviction	Burglary	Theft	Burglary
Third conviction	Burglary	Burglary	Theft
Sentence on second conviction	No Three Strikes enhancement	Twice the penalty for theft	Twice the penalty for burglary
Sentence on third conviction	Twice the penalty for burglary	Twice the penalty for burglary	25-years-to-life

[111] This is Figure 1.2 from Zimring, Hawkins, and Kamin 2001, 10.

Tables[112]

Table 1. List of Triggering Offenses (List of Strikeable Offenses)[113]

"Strikeable offenses" under the three strikes law are broken down into two categories: Violent felonies and serious felonies. Whether an individual commits a serious felony or a violent felony does not matter for the purposes of future sentencing.

Violent Felonies

- Murder
- Attempted Murder
- Manslaughter
- Rape by force or violence
- Child molestation
- Any felony resulting in great bodily injury or in which a firearm was used
- Robbery of an inhabited dwelling in which a deadly or dangerous weapon was used
- Out-of-state kidnapping

Serious Felonies

- Aggravated assault
- Arson
- Exploding a destructive device with intent to do harm
- Burglary of an inhabited dwelling
- Robbery or bank robbery
- Kidnapping
- Any felony in which the defendant used a dangerous or deadly weapon selling or distributing heroin, cocaine, PCP, or other methamphetamines
- Grand theft involving a firearm
- Any attempt to commit a crime listed here other than assault

[112] For regression tables, I include full tables for most of the major regressions, but reduced tables for sensitivity analyses and less important tables.

[113] This is a reduced version of the list of strikeable offenses. Repeated offenses and very rare or obscure offenses were excluded from this list. The full list can be found at http://www.threestrikes.org/tscrimes.html.

Table 2. Summary Statistics of Treatment and Control Groups within California in 1994

Characteristic	Control	Treatment	Difference
Age at Law Passing	33	33.3	-0.3
	(.36)	(.273)	(.45)
Age at First Arrest	22.3	22.1	0.2
	(.28)	(.22)	(.36)
Black	0.27	0.27	0
	(.021)	(.015)	(.026)
Hispanic	0.31	0.31	0.01
	(.022)	(.015)	(.026)
Number of Arrests	9.6	7.8	1.2***
	(.31)	(.184)	(.356)
Murder	0.059	0.071	-0.012
	(.011)	(.008)	(.014)
Robbery	0.34	0.26	0.08***
	(.022)	(.014)	(.027)
Arson	0.023	0.017	0.006
	(.007)	(.004)	(.007)
Assault	0.48	0.44	0.04
	(.024)	(.016)	(.028)
Burglary	0.02	0.02	0
	(.023)	(.016)	(.029)
Weapon	0.29	0.28	0.01
	(.022)	(.015)	(.026)
Observatioms	473	974	

***Significant at the 1% level

Table 3. Person-Year Arrest Rates in Three Years For Individuals in California, Following 1994 Release

	Control	Treatment	Difference
Year 1	0.612	0.516	-0.096**
	(.041)	(.027)	(.049)
Year 2	0.432	0.401	-0.031
	(.037)	(.027)	(.046)
Year 3	0.417	0.331	-0.07
	(.038)	(.023)	(.044)
Overall	0.487	0.416	-0.071***
	(.026)	(.018)	(.032)

**Significant at the 5% level, ***Significant at the 1% level

Table 4:[114] Estimate of the Deterrent Effect of the Third Strike: 1994 Data Set Only
Column 1- California only, Recidivism defined as any arrest
Columns 2 and 3- California only, Recidivism defined as felony arrest
Column 4- All States, recidivism defined as felony arrest

VARIABLES	(1)	(2)	(3)	(4)
CalTreatment	-	-	-	-0.0121
				(0.0169)
Treatment	-0.108***	-0.0433	-0.0400	-0.0281
	(0.0276)	(0.0289)	(0.0281)	(0.0170)
Arizona	-	-	-	0.148***
				(0.0242)
Delaware	-	-	-	0.0566
				(0.0550)
Florida	-	-	-	-0.0322
				(0.0220)
Illinois	-	-	-	0.0940***
				(0.0178)
Michigan	-	-	-	-0.227***
				(0.0413)
Minnesota	-	-	-	-0.0153
				(0.0199)
NewYork	-	-	-	-0.00988
				(0.0149)
NorthCarolina	-	-	-	-0.145***
				(0.0154)
Oregon	-	-	-	-0.0637***
				(0.0201)
Virginia	-	-	-	-0.0387*
				(0.0224)
NewJersey	-	-	-	0.0540**
				(0.0224)
Hispanic	-	-	-0.0222	0.0101
			(0.0311)	(0.0186)
Black	-	-	0.0938***	0.121***
			(0.0321)	(0.0106)
Male	-	-	0.127**	0.0791**
			(0.0613)	(0.0368)
SplineAge18_24	-	-	-0.0253	-0.0185*
			(0.0293)	(0.0103)
SplineAge25_29	-	-	-0.0128	-0.0117**
			(0.0119)	(0.00486)
SplineAge30_34	-	-	-0.0217**	-0.0113
			(0.0111)	(0.00702)
SplineAge35_39	-	-	-0.00704	-0.0166**
			(0.0122)	(0.00685)
SplineAge40_44	-	-	-0.0274**	-0.0137*
			(0.0127)	(0.00692)
SplineAge45plus	-	-	-0.00812**	-0.00852***
			(0.00324)	(0.00158)
Constant	0.679***	0.477***	1.080	0.919***
	(0.0224)	(0.0239)	(0.685)	(0.237)
Observations	1,379	1,379	1,379	5,176
R-squared	0.011	0.002	0.076	0.101

[114] For all tables after this, robust clustered standard errors in parentheses:*** p<0.01, ** p<0.05, * p<0.1

Table 5: Triple Differences Analysis of the Deterrent Effect of the Third Strike

VARIABLES	(1)	(2)	(3)	(4)
Treatment*Cal*Yr1994	0.0437	0.0435	0.0326	0.0212
	(0.0420)	(0.0374)	(0.0264)	(0.0276)
Treatment*Cal	-0.0429	-0.0529**	-0.0448***	-0.0229*
	(0.0379)	(0.0224)	(0.0120)	(0.0128)
Cal*Yr1994	-0.220***	-0.169***	-0.0941***	-0.103***
	(0.0262)	(0.0237)	(0.00978)	(0.00917)
Treat*Yr1994	-0.0308	-0.00593	0.00418	0.0559**
	(0.0420)	(0.0383)	(0.0252)	(0.0255)
Treatment	-0.0145	-0.0258	-0.0332***	-0.0814***
	(0.0379)	(0.0235)	(0.0111)	(0.0120)
Yr1994	0.0531*	0.0741***	-	-
	(0.0262)	(0.0234)		
California	0.209***	-	-	-
	(0.0280)			
Murder	-	-	-	0.0643***
				(0.0211)
Rape	-	-	-	-0.00874
				(0.00524)
Assault	-	-	-	0.0228***
				(0.00794)
Burglary	-	-	-	0.0556***
				(0.00690)
Robbery	-	-	-	0.0565***
				(0.00673)
Theft	-	-	-	0.0167***
				(0.00292)
Drug Crimes	-	-	-	0.0125***
				(0.00270)
Other Crimes	-	-	-	0.0137***
				(0.00189)
Florida	-	-0.181***	-0.135***	-0.125***
		(0.0130)	(0.0111)	(0.0111)
Illinois	-	-0.113***	-0.157***	-0.153***
		(0.0122)	(0.0154)	(0.0198)
Minnesota	-	-0.194***	-0.206***	-0.183***
		(0.0129)	(0.0114)	(0.0117)
Virginia	-	-0.208***	-0.136***	-0.143***
		(0.0247)	(0.0229)	(0.0274)
NorthCarolina	-	-0.290***	-0.261***	-0.242***
		(0.0152)	(0.0121)	(0.0137)
Oregon	-	-0.195***	-0.119***	-0.111***
		(0.0134)	(0.0278)	(0.0312)
NewYork	-	-0.205***	-0.248***	-0.252***
		(0.0197)	(0.00715)	(0.0105)
Arizona	-	-0.0198	0.0535**	-0.00257
		(0.0308)	(0.0255)	(0.0147)
Delaware	-	-0.112*	-0.0397	-0.257***
		(0.0570)	(0.0551)	(0.0273)
NewJersey	-	-0.165***	-0.212***	-0.205***
		(0.0165)	(0.0127)	(0.0148)
Michigan	-	-0.361***	-0.318***	-0.302***
		(0.0186)	(0.0198)	(0.0202)
Black	-	0.126***	0.127***	0.125***

		(0.0115)	(0.0114)	(0.0118)
Hispanic	-	0.0153	0.0149	0.0259
		(0.0198)	(0.0193)	(0.0229)
Male	-	0.103***	0.0993***	0.0970***
		(0.0303)	(0.0297)	(0.0327)
State-Year Effects	N	N	Y	Y
Age Splines	N	Y	Y	Y
Constant	0.438***	1.088***	1.104***	0.949***
	(0.0280)	(0.136)	(0.125)	(0.133)
Observations	7,637	7,637	7,637	7,637
R-squared	0.005	0.091	0.095	0.141

Table 6: Triple Differences Analysis of the Deterrent Effect of the Three Strikes Law for Individuals with One Strike

VARIABLES	(1)	(2)	(3)	(4)
Treatment*Cal*1994	0.0269	0.0412	0.0389	-0.0145
	(0.0463)	(0.0301)	(0.0291)	(0.0276)
Treatment*Cal	-0.104***	-0.134***	-0.130***	-0.0885***
	(0.0193)	(0.0120)	(0.0133)	(0.0144)
Cal*1994	-0.204***	-0.173***	-0.0456***	-0.0666***
	(0.0241)	(0.0305)	(0.00886)	(0.0108)
Treatment*Yr1994	-0.0631	-0.0561*	-0.0538*	0.0474*
	(0.0463)	(0.0300)	(0.0290)	(0.0257)
Treatment	-0.0129	-0.0199*	-0.0247*	-0.0838***
	(0.0193)	(0.0113)	(0.0126)	(0.0201)
YR1994	0.0845***	0.128***	-	-
	(0.0241)	(0.0313)		
California	0.205***	-	-	-
	(.0160)			
Murder	-	-	-	0.0549**
				(0.0225)
Rape	-	-	-	0.0204***
				(0.00644)
Assault	-	-	-	0.0266*
				(0.0146)
Burglary	-	-	-	0.0767***
				(0.0178)
Robbery	-	-	-	0.0683***
				(0.0127)
Theft	-	-	-	0.0234***
				(0.00320)
Drug Crimes	-	-	-	0.0130***
				(0.00315)
Other Crimes	-	-	-	0.0176***
				(0.00205)
Florida	-	-0.236***	-0.221***	-0.184***
		(0.0102)	(0.0109)	(0.0107)
Illinois	-	-0.184***	-0.189***	-0.151***
		(0.0132)	(0.0104)	(0.0108)
Minnesota	-	-0.217***	-0.236***	-0.186***
		(0.0112)	(0.00862)	(0.00936)
Virginia	-	-0.269***	-0.140***	-0.173***
		(0.0288)	(0.0399)	(0.0345)
NorthCarolina	-	-0.363***	-0.334***	-0.281***
		(0.0187)	(0.0108)	(0.0114)
Oregon	-	-0.148***	-0.160***	-0.131***
		(0.00979)	(0.0107)	(0.0104)
NewYork	-	-0.268***	-0.274***	-0.236***
		(0.0115)	(0.00963)	(0.0110)
Arizona	-	-0.167***	-0.0376	-0.0955**
		(0.0524)	(0.0590)	(0.0380)
Delaware	-	-0.224***	-0.0941***	-0.262***
		(0.0151)	(0.0266)	(0.0235)
NewJersey	-	-0.220***	-0.228***	-0.190***
		(0.0138)	(0.0107)	(0.0110)
Michigan	-	-0.370***	-0.352***	-0.310***
		(0.0132)	(0.0113)	(0.0111)

Black	-	0.150***	0.150***	0.136***
		(0.0142)	(0.0142)	(0.0130)
Hispanic	-	0.0123	0.0118	0.0261
		(0.0166)	(0.0166)	(0.0173)
Male	-	0.0927***	0.0936***	0.0993***
		(0.0148)	(0.0145)	(0.0148)
SplineAge18_24	-	-0.0235***	-0.0236***	-0.0249***
		(0.00499)	(0.00495)	(0.00465)
SplineAge25_29	-	-0.0117**	-0.0117**	-0.0143***
		(0.00423)	(0.00424)	(0.00415)
SplineAge30_34	-	-0.00964*	-0.00958*	-0.0114**
		(0.00500)	(0.00504)	(0.00464)
SplineAge35_39	-	-0.0109**	-0.0108**	-0.0106**
		(0.00455)	(0.00455)	(0.00487)
SplineAge40_44	-	-0.0168***	-0.0170***	-0.0143***
		(0.00358)	(0.00357)	(0.00350)
SplineAge45plus	-	-0.00442***	-0.00440***	-0.00266**
		(0.00122)	(0.00123)	(0.00106)
State-Year Effects	N	N	Y	Y
Constant	0.360***	1.063***	1.065***	0.993***
	(0.0160)	(0.114)	(0.112)	(0.108)
Observations	14,601	14,601	14,601	14,601
R-squared	0.008	0.099	0.100	0.151

Table 7: Triple Differences Analysis of the Deterrent Effect of the Third Strike, Varying by Age

VARIABLES	(1)	(2)
Treat*Cal*94*Age18to24	0.0558	0.0424
	(0.0363)	(0.0308)
Treat*Cal*94*Age25to29	0.0325	0.0110
	(0.0389)	(0.0297)
Treat*Cal*94*Age30to34	0.0356	0.0170
	(0.0410)	(0.0308)
Treat*Cal*94*Age35to39	-0.0219	-0.0477
	(0.0388)	(0.0284)
Treat*Cal*94*Age40to44	0.00663	-0.00872
	(0.0341)	(0.0313)
Treat*Cal*94*Age45plus	0.0211	-0.0197
	(0.0434)	(0.0345)
Cal*Treatment	-0.0199	-0.00797
	(0.0188)	(0.0158)
Cal*1994	-0.138***	-0.0954***
	(0.0292)	(0.0167)
Treatment*1994	0.0518	0.0589**
	(0.0374)	(0.0256)
Treatment	-0.0801***	-0.0832***
	(0.0216)	(0.0117)
Murder	0.0594**	0.0634***
	(0.0231)	(0.0208)
Rape	-0.0131**	-0.00878
	(0.00488)	(0.00528)
Assault	0.0214**	0.0228***
	(0.00782)	(0.00799)
Burglary	0.0569***	0.0557***
	(0.00714)	(0.00692)
Robbery	0.0560***	0.0565***
	(0.00658)	(0.00674)
Theft	0.0167***	0.0167***
	(0.00288)	(0.00295)
Drug Crimes	0.0128***	0.0124***
	(0.00264)	(0.00271)
Other Crimes	0.0127***	0.0136***
	(0.00176)	(0.00187)
Black	0.124***	0.125***
	(0.0119)	(0.0118)
Hispanic	0.0256	0.0252
	(0.0219)	(0.0230)
Male	0.100***	0.0964***
	(0.0336)	(0.0327)
State-Fixed Effects	Y	Y
State-Year Effects	Y	Y
Age Category Dummies	Y	N
Age Splines	N	Y
Constant	1.112***	0.953***
	(0.139)	(0.134)
Observations	7,637	7,637
R-squared	0.138	0.141

Table 8: Triple Differences Analysis of the Deterrent Effect of the Three Strikes Law for individuals with One Strike, Varying by Age

VARIABLES	(1)	(2)
Treat*Cal*94*Age18to24	0.0755**	0.0725**
	(0.0275)	(0.0296)
Treat*Cal*94*Age25to29	0.00597	-0.00290
	(0.0299)	(0.0299)
Treat*Cal*94*Age30to34	-0.0259	-0.0274
	(0.0313)	(0.0316)
Treat*Cal*94*Age35to39	-0.0391	-0.0540
	(0.0300)	(0.0309)
Treat*Cal*94*Age40to44	-0.0375	-0.0447
	(0.0399)	(0.0357)
Treat*Cal*94*Age45plus	-0.0389	-0.0441
	(0.0356)	(0.0358)
Cal*Treatment	-0.0890***	-0.0869***
	(0.0127)	(0.0153)
Cal*1994	-0.102***	-0.0722***
	(0.0300)	(0.00973)
Treatment*1994	0.0470	0.0466
	(0.0284)	(0.0291)
Treatment	-0.0829***	-0.0838***
	(0.0157)	(0.0176)
Murder	0.0531**	0.0513**
	(0.0208)	(0.0210)
Rape	0.0173***	0.0197***
	(0.00354)	(0.00448)
Assault	0.0264*	0.0275*
	(0.0126)	(0.0126)
Burglary	0.0769***	0.0768***
	(0.0157)	(0.0154)
Robbery	0.0674***	0.0680***
	(0.00908)	(0.00917)
Theft	0.0230***	0.0233***
	(0.00265)	(0.00275)
Drug Crimes	0.0125***	0.0131***
	(0.00221)	(0.00194)
Other Crimes	0.0172***	0.0174***
	(0.00184)	(0.00180)
Black	0.136***	0.136***
	(0.0154)	(0.0155)
Hispanic	0.0224	0.0218
	(0.0191)	(0.0192)
Male	0.0986***	0.0999***
	(0.0147)	(0.0144)
State-Fixed Effects	Y	Y
State-Year Effects	Y	Y
Age Category Dummies	Y	N
Age Splines	N	Y
Constant	1.017***	1.000***
	(0.139)	(0.112)
Observations	14,601	14,601
R-squared	0.151	0.153

Table 9: Effect of Three Strikes Law on Probability that an Individual with Two Strikes commits a Violent Offense
Sample limited to those who committed a felony within three years of their release

VARIABLES	(1)	(2)
Treatment*Cal*1994	0.0690**	0.0297
	(0.0309)	(0.0270)
Treatment*Cal	0.0115	0.0197
	(0.0268)	(0.0232)
Cal*1994	-0.512***	-0.465***
	(0.0115)	(0.0173)
Treatment*1994	0.0417	0.0338
	(0.0304)	(0.0265)
Treatment	-0.0409	-0.0502**
	(0.0251)	(0.0221)
Murder	-	0.0189
		(0.0459)
Rape	-	0.0275**
		(0.0107)
Assault	-	0.0742***
		(0.00913)
Burglary	-	-0.0310***
		(0.00750)
Robbery	-	0.0395***
		(0.00717)
Theft	-	-0.00655***
		(0.00233)
Drug Crimes	-	-0.0131***
		(0.00442)
Other Crimes	-	0.00694**
		(0.00335)
Black	0.0792***	0.0596**
	(0.0268)	(0.0245)
Hispanic	0.0450	0.0425
	(0.0279)	(0.0260)
Male	0.165***	0.146***
	(0.0360)	(0.0351)
SplineAge18_24	0.0244***	0.0159**
	(0.00761)	(0.00630)
SplineAge25_29	0.00177	0.00195
	(0.00757)	(0.00691)
SplineAge30_34	-0.00722	-0.0106
	(0.00853)	(0.00784)
SplineAge35_39	0.000541	0.00320
	(0.00652)	(0.00682)
SplineAge40_44	-0.00594	-0.0164
	(0.0111)	(0.00981)
SplineAge45plus	0.00891*	0.00969**
	(0.00453)	(0.00418)
State-Year Effects	Y	Y
State-Fixed Effects	Y	Y
Constant	0.155	0.340**
	(0.167)	(0.141)
Observations	3,449	3,449
R-squared	0.102	0.169

Table 10: Effect of Three Strikes Law on Probability that an Individual with Two Strikes commits a Serious or Violent Felony
Sample limited to those who committed a felony within three years of their release

	(1)	(2)
VARIABLES		
Treatment*Cal*1994	0.0844***	0.0917***
	(0.0292)	(0.0304)
Treatment*Cal	-0.0397**	-0.0345**
	(0.0167)	(0.0166)
Cal*1994	-0.126***	-0.135***
	(0.0105)	(0.0127)
Treatment*1994	-0.0322	-0.0276
	(0.0278)	(0.0300)
Treatment	0.0191	-0.00819
	(0.0143)	(0.0155)
Murder	-	-0.0691*
		(0.0354)
Rape	-	-0.00744
		(0.0170)
Assault	-	0.0104
		(0.00772)
Burglary	-	0.0242***
		(0.00593)
Robbery	-	0.0242**
		(0.00871)
Theft	-	-0.00544*
		(0.00276)
Drug Crimes	-	0.00267
		(0.00184)
Other Crimes	-	-0.00325
		(0.00278)
Black	0.0639***	0.0652***
	(0.0183)	(0.0175)
Hispanic	0.0735***	0.0738***
	(0.0232)	(0.0234)
Male	0.113***	0.100***
	(0.0359)	(0.0342)
State-Year Effects	Y	Y
State-Fixed Effects	Y	Y
Age Splines	Y	Y
Constant	1.014***	0.928***
	(0.161)	(0.164)
Observations	3,449	3,449
R-squared	0.138	0.146

Table 11: Effect of Three Strikes Law on the Probability that Individuals with One Strike Commit a Violent Crime
Sample limited to those who committed a felony within three years of their release

VARIABLES	(1)	(2)
Treatment*Cal*1994	-0.0233	-0.0869**
	(0.0365)	(0.0371)
Treatment*Cal	0.0184	0.0341
	(0.0366)	(0.0338)
Cal*1994	-0.217***	-0.230***
	(0.00917)	(0.0114)
Treatment*1994	-0.0677*	-0.0504
	(0.0365)	(0.0359)
Treatment	0.0969**	0.0410
	(0.0361)	(0.0326)
Murder	-	0.123***
		(0.0431)
Rape	-	0.0467***
		(0.0122)
Assault	-	0.0843***
		(0.00985)
Burglary	-	-0.0614***
		(0.0160)
Robbery	-	0.0502***
		(0.0101)
Theft	-	-0.00569**
		(0.00236)
Drug Crimes	-	-0.0127***
		(0.00394)
Other Crimes	-	-0.00533*
		(0.00282)
Black	0.0693***	0.0457**
	(0.0240)	(0.0187)
Hispanic	0.0358	0.0160
	(0.0235)	(0.0230)
Male	0.102**	0.0813**
	(0.0365)	(0.0340)
SplineAge18_24	0.0184***	0.0152***
	(0.00551)	(0.00475)
SplineAge25_29	9.78e-06	0.000834
	(0.00481)	(0.00460)
SplineAge30_34	-0.00273	-0.00171
	(0.00766)	(0.00742)
SplineAge35_39	-0.00914	-0.00986
	(0.00707)	(0.00632)
SplineAge40_44	-0.00830	-0.00379
	(0.00877)	(0.00751)
SplineAge45plus	0.00951**	0.00299
	(0.00455)	(0.00451)
State-Year Effects	Y	Y
State-Fixed Effects	Y	Y
Constant	0.158	0.308**
	(0.128)	(0.114)
Observations	5,234	5,234
R-squared	0.072	0.136

Table 12: Effect of Three Strikes Law on the Probability that Individuals with One Strike Commit a Serious or Violent Felony (a strikeable felony): Sample limited to those who committed a felony within three years of their release

VARIABLES	(1)	(2)
Treatment*Cal*1994	-0.0844*	-0.0817*
	(0.0482)	(0.0467)
Treatment*Cal	0.0891***	0.0952***
	(0.0292)	(0.0287)
Cal*1994	0.0303***	0.0143
	(0.00782)	(0.0182)
Treatment*1994	-0.0232	-0.0173
	(0.0477)	(0.0476)
Treatment	0.0666**	0.0242
	(0.0290)	(0.0317)
Murder	-	0.0521
		(0.0355)
Rape	-	0.0232
		(0.0168)
Assault	-	0.0158*
		(0.00813)
Burglary	-	0.0320**
		(0.0125)
Robbery	-	0.0284***
		(0.00819)
Theft	-	-0.0109***
		(0.00308)
Drug Crimes	-	0.00911**
		(0.00373)
Other Crimes	-	-0.00408
		(0.00330)
Black	0.0977***	0.0935***
	(0.0181)	(0.0181)
Hispanic	0.0781**	0.0719**
	(0.0307)	(0.0309)
Male	0.167***	0.151***
	(0.0273)	(0.0280)
SplineAge18_24	-0.0140***	-0.0133**
	(0.00495)	(0.00507)
SplineAge25_29	-0.00297	-0.00280
	(0.00549)	(0.00567)
SplineAge30_34	-0.00329	-0.00248
	(0.00602)	(0.00610)
SplineAge35_39	-0.0178*	-0.0172*
	(0.0100)	(0.00995)
SplineAge40_44	0.00685	0.00748
	(0.0125)	(0.0126)
SplineAge45plus	-0.000294	-0.000824
	(0.00471)	(0.00464)
State-Fixed Effects	Y	Y
State-Year Effects	Y	Y
Constant	0.822***	0.825***
	(0.113)	(0.111)
Observations	5,234	5,234
R-squared	0.164	0.173

Table 13: Deterrent Effect of the Third Strike on Criminals with Two Strikes, Post-Law Data Only
Column 1- Probit Model, Column 2- Logit Model
Estimates are marginal effects, reported at the mean of the data

VARIABLES	(1)	(2)
Treatment*Cal	-0.0101	-0.0108
	(0.0183)	(0.0189)
Treatment	-0.0320*	-0.0314
	(0.0186)	(0.0192)
Black	0.130***	0.131***
	(0.0116)	(0.0119)
Hispanic	0.0130	0.0126
	(0.0198)	(0.0200)
Male	0.0830**	0.0826**
	(0.0390)	(0.0389)
State-Fixed Effects	Y	Y
Age Splines	Y	Y
Observations	5,176	5,176

Table 14: Deterrent Effect of the Third Strike on Criminals with Two Strikes, Combined Data
Column 1- Probit Model, Column 2- Logit Model
Estimates are marginal effects, reported at the mean of the data

VARIABLES	(1)	(2)
Treatment*Cal*1994	0.0365	0.0380
	(0.0286)	(0.0295)
Treatment*Cal	-0.0467***	-0.0484***
	(0.0124)	(0.0127)
Cal*1994	-0.0943***	-0.0960***
	(0.00988)	(0.0100)
Treatment*1994	0.00334	0.00475
	(0.0270)	(0.0276)
Treatment	-0.0361***	-0.0372***
	(0.0116)	(0.0118)
Black	0.136***	0.138***
	(0.0122)	(0.0125)
Hispanic	0.0172	0.0173
	(0.0206)	(0.0209)
Male	0.103***	0.105***
	(0.0308)	(0.0314)
State-Fixed Effects	Y	Y
State-Year Effects	Y	Y
Age Splines	Y	Y
Observations	7,637	7,637

Table 15: Deterrent Effect of the Three Strikes Law on Individuals with Two Strikes
Alternative Age Specificatoins
Column 1- Age, Column 2- Age and Age^2, Column 3- Age, Age^2, and Age^3

VARIABLES	(1)	(2)	(3)
Treatment*Cal*1994	0.0247	0.0232	0.0229
	(0.0269)	(0.0268)	(0.0271)
Treatment*Cal	-0.0263**	-0.0247*	-0.0244*
	(0.0119)	(0.0121)	(0.0122)
Cal*1994	-0.115***	-0.102***	-0.102***
	(0.00803)	(0.00897)	(0.00906)
Treatment*1994	0.0563**	0.0560**	0.0560**
	(0.0253)	(0.0251)	(0.0251)
Treatment	-0.0818***	-0.0820***	-0.0820***
	(0.0117)	(0.0118)	(0.0119)
Black	0.124***	0.125***	0.125***
	(0.0118)	(0.0119)	(0.0119)
Hispanic	0.0263	0.0260	0.0260
	(0.0224)	(0.0229)	(0.0229)
Male	0.102***	0.0969***	0.0967***
	(0.0325)	(0.0328)	(0.0328)
Age	-0.0121***	-0.0255***	-0.0285**
	(0.000775)	(0.00285)	(0.0118)
Age^2	-	0.000179***	0.000256
		(3.31e-05)	(0.000292)
Age^3	-	-	-6.18e-07
			(2.23e-06)
State-Fixed Effects	Y	Y	Y
State-Year Effects	Y	Y	Y
PCH Variables	Y	Y	Y
Constant	0.708***	0.933***	0.968***
	(0.0345)	(0.0665)	(0.159)
Observations	7,637	7,637	7,637
R-squared	0.138	0.140	0.140

Table 16: In State vs. Overall Recidivism: The Deterrent Effect of the Three Strikes Law for Individuals with Two Strikes, Post Law Data Set Only
Column 1- Overall Recidivism, Column 2- In State Recidivism Only

VARIABLES	(1)	(2)
Treatment*Cal	-0.0121	-0.00558
	(0.0169)	(0.0179)
Treatment	-0.0281	-0.0374*
	(0.0170)	(0.0182)
Hispanic	0.0101	0.00901
	(0.0186)	(0.0214)
Black	0.121***	0.129***
	(0.0106)	(0.0112)
Male	0.0791**	0.0575*
	(0.0368)	(0.0332)
State-Fixed Effects	Y	Y
Age Spline	Y	Y
In State Recidivism Only	N	Y
Constant	0.919***	0.829***
	(0.237)	(0.219)
Observations	5,176	5,176
R-squared	0.101	0.101

Table 17: In State vs. Overall Recidivism: The Deterrent Effect of the Three Strikes Law for Individuals with Two Strikes, Combined Data Set
Column 1- Overall recidivism, Column 2- Post law data set limited to in state recidivism

VARIABLES	(1)	(2)
Treatment*Cal*1994	0.0212	0.0278
	(0.0276)	(0.0281)
Treatment*Cal	-0.0229*	-0.0238*
	(0.0128)	(0.0125)
Cal*1994	-0.103***	-0.117***
	(0.00917)	(0.0104)
Treatment*1994	0.0559**	0.0430
	(0.0255)	(0.0264)
Treatment	-0.0814***	-0.0788***
	(0.0120)	(0.0119)
Black	0.125***	0.131***
	(0.0118)	(0.0123)
Hispanic	0.0259	0.0240
	(0.0229)	(0.0239)
Male	0.0970***	0.0839**
	(0.0327)	(0.0314)
State-Fixed Effects	Y	Y
Age Spline	Y	Y
PCH Variables	Y	Y
In State Recidivism Only	N	Y
Constant	0.949***	0.926***
	(0.133)	(0.126)
Observations	7,637	7,637
R-squared	0.141	0.136

Table 18: Deterrent Effect of the Three Strikes Law for Individuals with Two Strikes
Alternative Recidivism Definitions
Column 1- Failure is any arrest, Column 2- Failure is arrest for a felony and a conviction for some crime

VARIABLES	(1)	(2)
Treatment*Cal*1994	-0.0212	0.0456**
	(0.0310)	(0.0204)
Treatment*Cal	-0.0394**	-0.0741***
	(0.0175)	(0.00999)
Cal*1994	-0.0601***	-0.0957***
	(0.00905)	(0.0122)
Treat*1994	0.0489	0.0365*
	(0.0287)	(0.0183)
Treatment	-0.0663***	-0.0561***
	(0.0163)	(0.0108)
Murder	0.0450*	0.0417*
	(0.0238)	(0.0207)
Rape	-0.00890	-0.00779
	(0.00861)	(0.00829)
Assault	0.0118**	0.0237**
	(0.00502)	(0.00905)
Burglary	0.0417***	0.0565***
	(0.00605)	(0.00789)
Robbery	0.0436***	0.0494***
	(0.00634)	(0.00641)
Theft	0.0112***	0.0149***
	(0.00226)	(0.00222)
Drug Crimes	0.0173***	0.0102***
	(0.00204)	(0.00243)
Other Crimes	0.0181***	0.0108***
	(0.00181)	(0.00200)
Black	0.118***	0.0802***
	(0.0149)	(0.00858)
Hispanic	0.0561***	0.00865
	(0.0180)	(0.0109)
Male	0.0790**	0.0620**
	(0.0296)	(0.0268)
State fixed effects	Y	Y
State-Year effects	Y	Y
Age Splines	Y	Y
Observations	7,637	7,637
R-squared	0.150	0.110

Table 19: Deterrent Effect of the Three Strikes Law for Individuals with One Strike
Alternative Recidivism Definitions
Column 1- Failure is any arrest, Column 2- Failure is arrest for a felony and a conviction for some crime

	(1)	(2)
VARIABLES		
Treatment*Cal*1994	-0.000573	-0.109***
	(0.0245)	(0.0265)
Treatment*Cal	-0.135***	0.0273*
	(0.0150)	(0.0143)
Cal*1994	-0.0263***	0.00873
	(0.00858)	(0.0107)
Treat*1994	0.0287	0.0397
	(0.0234)	(0.0258)
Treatment	-0.0637***	-0.0902***
	(0.0182)	(0.0223)
Murder	-0.00523	0.0373
	(0.0216)	(0.0228)
Rape	0.0215***	0.0139**
	(0.00657)	(0.00645)
Assault	0.0196	0.0364**
	(0.0118)	(0.0147)
Burglary	0.0794***	0.0759***
	(0.0140)	(0.0200)
Robbery	0.0621***	0.0588***
	(0.00950)	(0.0129)
Theft	0.0178***	0.0195***
	(0.00283)	(0.00288)
Drug Crimes	0.0237***	0.0127***
	(0.00321)	(0.00372)
Other Crimes	0.0257***	0.0136***
	(0.00271)	(0.00221)
Black	0.139***	0.0800***
	(0.0156)	(0.0113)
Hispanic	0.0585***	0.0129
	(0.0164)	(0.0139)
State fixed effects	Y	Y
State-Year effects	Y	Y
Age Splines	Y	Y
Constant	1.097***	0.818***
	(0.108)	(0.0988)
Observations	14,601	14,601
R-squared	0.174	0.115

Table 20: Triple Differences Estimate of the Deterrent Effect of the Third Strike
Column 1- includes PCH, Column 2- includes FRCY, Column 3- includes incarceration time, Column 4- includes both FRCY and incarceration time

VARIABLES	(1)	(2)	(3)	(4)
Treatment*Cal*1994	0.0211	0.0233	0.0207	0.0228
	(0.0276)	(0.0279)	(0.0278)	(0.0281)
Treatment*Cal	-0.0227*	-0.0258*	-0.0227*	-0.0258*
	(0.0128)	(0.0131)	(0.0129)	(0.0131)
Cal*1994	-0.103***	-0.117***	-0.104***	-0.119***
	(0.00915)	(0.0118)	(0.00927)	(0.0119)
Treatment*1994	0.0565**	0.0563**	0.0574**	0.0571**
	(0.0255)	(0.0257)	(0.0257)	(0.0259)
Treatment	-0.0822***	-0.0807***	-0.0824***	-0.0809***
	(0.0121)	(0.0121)	(0.0121)	(0.0122)
FRCY	-	0.0728***	-	0.0729***
		(0.0251)		(0.0251)
Total Incarceration Time	-	-	-0.000280***	-0.000281***
			(9.34e-05)	(9.53e-05)
Prior Criminal History	Y	Y	Y	Y
Age Spline	Y	Y	Y	Y
State Fixed Effects	Y	Y	Y	Y
State-Year Effects	Y	Y	Y	Y
Constant	0.910***	0.657***	0.910***	0.656***
	(0.131)	(0.158)	(0.131)	(0.158)
Observations	7,629	7,629	7,629	7,629
R-squared	0.140	0.141	0.140	0.142

Table 21: Deterrent Effect of the Three Strikes Law for Individuals with Two Strikes: 1994 Data Set Only
Alternative Prior Criminal History Specifications
Column 1- No PCH, Column 2- 8 Category PCH, Column 3- 26 Category PCH

	(1)	(2)	(3)
VARIABLES			
Treatment*Cal	-0.0121	-0.000887	0.000328
	(0.0169)	(0.0179)	(0.0187)
Treatment	-0.0281	-0.0103	0.00404
	(0.0170)	(0.0175)	(0.0182)
Hispanic	0.0101	0.0138	0.00816
	(0.0186)	(0.0212)	(0.0280)
Black	0.121***	0.113***	0.100***
	(0.0106)	(0.0123)	(0.0124)
Male	0.0791**	0.0968**	0.0847**
	(0.0368)	(0.0411)	(0.0356)
8 Category PCH	N	Y	N
26 Category PCH	N	N	Y
State Fixed Effects	Y	Y	Y
State Year Effects	Y	Y	Y
Individual Controls	Y	Y	Y
Constant	0.919***	0.851***	0.837***
	(0.237)	(0.254)	(0.245)
Observations	5,176	5,176	5,176
R-squared	0.101	0.146	0.166

Table 22: Deterrent Effect of the Three Strikes Law for Individuals with One Strike: 1994 Data Set Only
Alternative Prior Criminal History Specifications
Column 1- No PCH, Column 2- 8 Category PCH, Column 3- 26 Category PCH

	(1)	(2)	(3)
VARIABLES			
Treatment*Cal	-0.0864**	-0.0920***	-0.0643**
	(0.0355)	(0.0251)	(0.0221)
Treatment	-0.0780*	-0.0196	-0.0124
	(0.0357)	(0.0270)	(0.0244)
Hispanic	-0.00287	0.00710	0.00712
	(0.0143)	(0.0172)	(0.0143)
Black	0.151***	0.132***	0.105***
	(0.0170)	(0.0159)	(0.0121)
Male	0.0694***	0.0957***	0.0817***
	(0.0115)	(0.0194)	(0.0186)
8 Category PCH	N	Y	N
26 Category PCH	N	N	Y
State Fixed Effects	Y	Y	Y
State Year Effects	Y	Y	Y
Individual Controls	Y	Y	Y
Constant	0.905***	0.862***	0.845***
	(0.131)	(0.126)	(0.115)
Observations	9,940	9,940	9,940
R-squared	0.110	0.159	0.184

Table 23. Overall Estimate of the Deterrent Effect of the Three Strikes Law
Column 2 includes PCH, Col 3 includes FRCY,
Col 4 includes FRCY and Total Previous Incarceration Time

VARIABLES	(1)	(2)	(3)	(4)
TwoStrikes*Cal*1994	-0.305***	-0.390***	-0.384***	-0.389***
	(0.0159)	(0.0218)	(0.0221)	(0.0228)
OneStrike*Cal*1994	-0.296***	-0.394***	-0.389***	-0.391***
	(0.0153)	(0.0206)	(0.0208)	(0.0211)
TwoStrikes*1994	-0.0414***	-0.0334**	-0.0368**	-0.0310*
	(0.0150)	(0.0150)	(0.0152)	(0.0156)
OneStrike*1994	-0.0639***	-0.0312**	-0.0340**	-0.0320**
	(0.0146)	(0.0137)	(0.0139)	(0.0141)
OneStrike*Cal	0.282***	0.363***	0.358***	0.360***
	(0.00782)	(0.0142)	(0.0145)	(0.0148)
TwoStrikes*Cal	0.317***	0.388***	0.382***	0.384***
	(0.00959)	(0.0159)	(0.0160)	(0.0164)
One Strike	0.0337***	-0.00133	-0.00230	-0.00299
	(0.00629)	(0.00823)	(0.00815)	(0.00832)
Two Strikes	0.141***	0.0276	0.0213	0.0190
	(0.00854)	(0.0179)	(0.0176)	(0.0180)
Cal*1994	0.276***	0.369***	0.358***	0.356***
	(0.00337)	(0.0121)	(0.0132)	(0.0132)
FRCY	-	-	0.0627***	0.0630***
			(0.0155)	(0.0155)
Total Incarceration Time	-	-	-	-0.000483***
				(0.000123)
Black	0.140***	0.125***	0.126***	0.126***
	(0.00894)	(0.00848)	(0.00846)	(0.00846)
Hispanic	0.00506	0.0149	0.0153	0.0153
	(0.0117)	(0.0126)	(0.0125)	(0.0125)
Male	0.0694***	0.0717***	0.0728***	0.0732***
	(0.0128)	(0.0123)	(0.0124)	(0.0124)
State Fixed Effects	Y	Y	Y	Y
State-Year Effects	Y	Y	Y	Y
Age Spline	Y	Y	Y	Y
PCH Variables	Y	Y	Y	Y
Constant	0.737***	0.657***	0.546***	0.544***
	(0.0596)	(0.0647)	(0.0762)	(0.0764)
Observations	40,579	40,579	40,519	40,519
R-squared	0.109	0.157	0.158	0.158

Table 24. Overall Estimate of the Deterrent Effect of the Three Strikes Law: Sample limited to those with at least one previous felony arrest
Column 2 includes PCH, Col 3 includes FRCY,
Col 4 includes FRCY and Total Previous Incarceration Time

VARIABLES	(1)	(2)	(3)	(4)
TwoStrikes*Cal*1994	-0.0181	-0.0165	-0.0157	-0.0201
	(0.0189)	(0.0179)	(0.0181)	(0.0183)
OneStrike*Cal*1994	0.0268	0.0129	0.0127	0.0115
	(0.0190)	(0.0169)	(0.0171)	(0.0172)
TwoStrikes*1994	-0.0308	-0.0146	-0.0186	-0.0127
	(0.0185)	(0.0180)	(0.0181)	(0.0185)
OneStrike*1994	-0.0566***	-0.0184	-0.0219	-0.0197
	(0.0189)	(0.0168)	(0.0170)	(0.0171)
OneStrike*Cal	-0.0402***	-0.0346***	-0.0346***	-0.0340***
	(0.0121)	(0.0116)	(0.0118)	(0.0118)
TwoStrikes*Cal	0.0331**	0.0236*	0.0228*	0.0241*
	(0.0125)	(0.0122)	(0.0124)	(0.0126)
One Strike	0.0129	-0.0198	-0.0191	-0.0201
	(0.0121)	(0.0117)	(0.0121)	(0.0121)
Two Strikes	0.111***	0.00355	4.92e-05	-0.00258
	(0.0129)	(0.0162)	(0.0166)	(0.0170)
Cal*1994	-0.0476***	-0.0478***	-0.0513***	-0.0536***
	(0.00607)	(0.00507)	(0.00541)	(0.00555)
FRCY	-	-	0.0507***	0.0512***
			(0.0125)	(0.0125)
Total Incarceration Time	-	-	-	-0.000479***
				(0.000122)
Murder	-	0.0365**	0.0353**	0.0410***
		(0.0143)	(0.0141)	(0.0135)
Rape	-	-0.00314	-0.00344	-0.00202
		(0.00553)	(0.00563)	(0.00563)
Assault	-	0.0138***	0.0120**	0.0124**
		(0.00457)	(0.00457)	(0.00458)
Burglary	-	0.0278***	0.0264***	0.0270***
		(0.00345)	(0.00335)	(0.00337)
Robbery	-	0.0317***	0.0311***	0.0322***
		(0.00418)	(0.00422)	(0.00428)
Theft	-	0.0180***	0.0161***	0.0162***
		(0.00182)	(0.00175)	(0.00177)
Drug Crimes	-	0.0105***	0.0102***	0.0103***
		(0.00133)	(0.00126)	(0.00128)
Other Crimes	-	0.0154***	0.0145***	0.0147***
		(0.00185)	(0.00190)	(0.00195)
Black	0.148***	0.132***	0.133***	0.133***
	(0.00855)	(0.00853)	(0.00856)	(0.00856)
Hispanic	0.00473	0.0141	0.0147	0.0148
	(0.0119)	(0.0125)	(0.0125)	(0.0125)
Male	0.0768***	0.0852***	0.0866***	0.0871***
	(0.0120)	(0.0117)	(0.0117)	(0.0117)
SplineAge18_24	-0.0233***	-0.0252***	-0.0196***	-0.0195***
	(0.00276)	(0.00297)	(0.00324)	(0.00324)
SplineAge25_29	-0.00780***	-0.0141***	-0.0128***	-0.0128***
	(0.00226)	(0.00223)	(0.00230)	(0.00229)
SplineAge30_34	-0.00890***	-0.0115***	-0.0105***	-0.0105***
	(0.00299)	(0.00285)	(0.00280)	(0.00282)

SplineAge35_39	-0.0111***	-0.0125***	-0.0120***	-0.0119***
	(0.00222)	(0.00245)	(0.00248)	(0.00247)
SplineAge40_44	-0.0174***	-0.0165***	-0.0160***	-0.0158***
	(0.00264)	(0.00262)	(0.00258)	(0.00260)
SplineAge45plus	-0.00828***	-0.00576***	-0.00573***	-0.00567***
	(0.00142)	(0.00121)	(0.00121)	(0.00120)
State-Fixed Effects	Y	Y	Y	Y
State-Year Effects	Y	Y	Y	Y
Constant	0.999***	0.999***	0.855***	0.853***
	(0.0654)	(0.0694)	(0.0767)	(0.0768)
Observations	32,607	32,607	32,607	32,607
R-squared	0.101	0.153	0.153	0.154

Table 25. Effect of the Three Strikes Law on the Probability that an Individual Commits a Violent Offense
Sample limited to those with at least one prior felony arrest and at least one felony arrest within three years of their release
Column 1- includes PCH, Column 2- includes PCH and FRCY,
Column 3- includes PCH, FRCY, and total incarceration time

VARIABLES	(1)	(2)	(3)
TwoStrikes*Cal*1994	-0.0465	-0.0470	-0.0526
	(0.0322)	(0.0321)	(0.0316)
OneStrike*Cal*1994	-0.00899	-0.00908	-0.0110
	(0.0325)	(0.0325)	(0.0323)
TwoStrikes*1994	-0.130***	-0.129***	-0.122***
	(0.0319)	(0.0317)	(0.0312)
OneStrike*1994	-0.131***	-0.131***	-0.128***
	(0.0324)	(0.0322)	(0.0320)
OneStrike*Cal	-0.0232	-0.0234	-0.0217
	(0.0313)	(0.0313)	(0.0312)
TwoStrikes*Cal	0.0161	0.0162	0.0182
	(0.0316)	(0.0316)	(0.0315)
One Strike	0.146***	0.146***	0.145***
	(0.0318)	(0.0317)	(0.0317)
Two Strikes	0.116***	0.117***	0.114***
	(0.0335)	(0.0336)	(0.0335)
Cal*1994	-0.280***	-0.279***	-0.281***
	(0.00663)	(0.00668)	(0.00685)
FRCY	-	-0.0119	-0.0109
		(0.0186)	(0.0185)
Total Incarceration Time	-	-	-0.000639***
			(0.000209)
Murder	0.0735***	0.0738***	0.0800***
	(0.0240)	(0.0239)	(0.0237)
Rape	0.0298***	0.0298***	0.0316***
	(0.00564)	(0.00570)	(0.00542)
Assault	0.0687***	0.0692***	0.0694***
	(0.00421)	(0.00436)	(0.00434)
Burglary	-0.0167***	-0.0164***	-0.0157***
	(0.00293)	(0.00320)	(0.00327)
Robbery	0.0281***	0.0282***	0.0293***
	(0.00394)	(0.00391)	(0.00390)
Theft	-0.00681***	-0.00637***	-0.00625***
	(0.00111)	(0.000926)	(0.000914)
Drug Crimes	-0.0121***	-0.0120***	-0.0119***
	(0.00138)	(0.00136)	(0.00134)
Other Crimes	0.00246	0.00271*	0.00302*
	(0.00157)	(0.00155)	(0.00153)
Black	0.0536***	0.0532***	0.0531***
	(0.0119)	(0.0121)	(0.0120)
Hispanic	0.0138	0.0135	0.0138
	(0.0187)	(0.0187)	(0.0188)
Male	0.154***	0.153***	0.154***
	(0.0201)	(0.0201)	(0.0200)
SplineAge18_24	0.00998*	0.00859	0.00882
	(0.00494)	(0.00617)	(0.00617)
SplineAge25_29	-0.00106	-0.00143	-0.00130
	(0.00331)	(0.00324)	(0.00323)
SplineAge30_34	2.22e-06	-0.000311	-0.000296

	(0.00316)	(0.00314)	(0.00315)
SplineAge35_39	-0.0118***	-0.0120***	-0.0117***
	(0.00330)	(0.00332)	(0.00329)
SplineAge40_44	-0.00604	-0.00620	-0.00561
	(0.00479)	(0.00474)	(0.00480)
SplineAge45plus	0.00197	0.00194	0.00216
	(0.00240)	(0.00240)	(0.00232)
State-Fixed Effects	Y	Y	Y
State-Year Effects	Y	Y	Y
Constant	0.365***	0.401***	0.396***
	(0.106)	(0.140)	(0.140)
Observations	14,509	14,509	14,509
R-squared	0.129	0.129	0.129

Table 26. Effect of the Three Strikes Law on the Probability that an Individual Commits a Serious or Violent Felony (a strikeable offense)
Sample limited to those with at least one prior felony arrest and at least one arrest for a felony within three years of their release
Column 2- includes PCH

VARIABLES	(1)	(2)
TwoStrikes*Cal*1994	0.0262	0.0166
	(0.0231)	(0.0218)
OneStrike*Cal*1994	-0.0198	-0.0234
	(0.0229)	(0.0215)
TwoStrikes*1994	-0.0803***	-0.0924***
	(0.0236)	(0.0209)
OneStrike*1994	-0.0742***	-0.0774***
	(0.0230)	(0.0214)
OneStrike*Cal	0.0171	0.0229
	(0.0200)	(0.0197)
TwoStrikes*Cal	-0.0470**	-0.0370*
	(0.0194)	(0.0197)
One Strike	0.140***	0.113***
	(0.0200)	(0.0207)
Two Strikes	0.186***	0.135***
	(0.0201)	(0.0220)
Cal*1994	0.0289***	0.0264***
	(0.00454)	(0.00699)
Murder	-	0.0186
		(0.0187)
Rape	-	0.0129**
		(0.00474)
Assault	-	0.0259***
		(0.00360)
Burglary	-	0.0147***
		(0.00234)
Robbery	-	0.0115***
		(0.00288)
Theft	-	-0.0109***
		(0.000999)
Drug Crimes	-	0.00663***
		(0.000902)
Other Crimes	-	-0.00283
		(0.00171)
Black	0.0647***	0.0631***
	(0.00730)	(0.00709)
Hispanic	0.0367***	0.0303***
	(0.0104)	(0.0105)
Male	0.223***	0.197***
	(0.0232)	(0.0204)
State-Fixed Effects	Y	Y
State-Year Effects	Y	Y
Age Spline	Y	Y
Constant	0.665***	0.684***
	(0.0904)	(0.0848)
Observations	14,509	14,509
R-squared	0.059	0.080

Table 27. Overall Deterrent Effect of the Three Strikes Law
Sample limited to those with a previous felony arrest
Column 1- Probit model, Column 2- Logit model
Marginal effects reported at the mean of the data

	(1)	(2)
VARIABLES		
TwoStrikes*Cal*1994	-0.0137	-0.0197
	(0.0204)	(0.0211)
OneStrike*Cal*1994	0.0192	0.0170
	(0.0196)	(0.0205)
TwoStrikes*1994	-0.0203	-0.0138
	(0.0206)	(0.0209)
OneStrike*1994	-0.0244	-0.0219
	(0.0193)	(0.0201)
OneStrike*Cal	-0.0483***	-0.0445***
	(0.0133)	(0.0141)
TwoStrikes*Cal	0.0105	0.0148
	(0.0139)	(0.0149)
One Strike	-0.0190	-0.0225
	(0.0140)	(0.0148)
Two Strikes	0.00559	-0.00438
	(0.0191)	(0.0206)
Cal*1994	-0.0546***	-0.0519***
	(0.00661)	(0.00577)
State-Fixed Effects	Y	Y
State-Year Effects	Y	Y
Age Spline	Y	Y
Individual Characteristics	Y	Y
PCH Variables	Y	Y
Observations	32,607	32,607

Table 28. Effect of the Three Strikes Law on the Probability that criminals commit a violent crime
Sample limited to those with a previous felony arrest
Column 1- Probit model, Column 2- Logit model
Marginal effects reported at the mean of the data

	(1)	(2)
VARIABLES		
TwoStrikes*Cal*1994	-0.162***	-0.210***
	(0.0316)	(0.0306)
OneStrike*Cal*1994	-0.0734**	-0.0997***
	(0.0332)	(0.0338)
TwoStrikes*1994	-0.180***	-0.180***
	(0.0318)	(0.0320)
OneStrike*1994	-0.173***	-0.173***
	(0.0324)	(0.0328)
OneStrike*Cal	0.0349	0.0567*
	(0.0306)	(0.0304)
TwoStrikes*Cal	0.121***	0.161***
	(0.0293)	(0.0275)
One Strike	0.181***	0.180***
	(0.0301)	(0.0300)
Two Strikes	0.160***	0.158***
	(0.0332)	(0.0332)
Cal*1994	-0.300***	-0.296***
	(0.00850)	(0.00829)
State-Fixed Effects	Y	Y
State-Year Effects	Y	Y
Age Spline	Y	Y
Individual Characteristics	Y	Y
PCH Variables	Y	Y
Observations	14,509	14,509

Table 29. Overall Estimate of the Deterrent Effect of the Three Strikes Law: Alternative Recidivism Definitions
Sample limited to those with at least one previous felony arrest
Column 1- Recidivism defined as any arrest
Column 2- Recidivism defined as a felony arrest and a conviction for any crime

VARIABLES	(1)	(2)
TwoStrikes*Cal*1994	-0.0717***	-0.0638***
	(0.0170)	(0.0159)
OneStrike*Cal*1994	-0.0352*	-0.0518***
	(0.0176)	(0.0127)
TwoStrikes*1994	-0.0182	-0.00312
	(0.0174)	(0.0165)
OneStrike*1994	-0.0164	-0.00428
	(0.0172)	(0.0127)
OneStrike*Cal	-0.0218*	0.0293***
	(0.0122)	(0.00868)
TwoStrikes*Cal	0.0385***	0.0348***
	(0.0123)	(0.0117)
One Strike	-0.0175	-0.0327***
	(0.0123)	(0.00936)
Two Strikes	0.0132	-0.0206
	(0.0152)	(0.0163)
Cal*1994	0.0179***	-0.0242***
	(0.00497)	(0.00461)
FRCY	0.0145	0.0643***
	(0.0109)	(0.0128)
Total Incarceration Time	-0.000468***	-0.000470***
	(0.000169)	(0.000170)
Murder	0.00350	0.0254*
	(0.0136)	(0.0138)
Rape	0.00108	-0.00651
	(0.00498)	(0.00481)
Assault	0.0117**	0.0137***
	(0.00442)	(0.00406)
Burglary	0.0256***	0.0285***
	(0.00336)	(0.00362)
Robbery	0.0239***	0.0292***
	(0.00399)	(0.00355)
Theft	0.0134***	0.0144***
	(0.00152)	(0.00132)
Drug Crimes	0.0156***	0.0102***
	(0.00237)	(0.00145)
Other Crimes	0.0182***	0.0108***
	(0.00176)	(0.00173)
Black	0.130***	0.0831***
	(0.00951)	(0.00703)
Hispanic	0.0380***	0.00782
	(0.00943)	(0.0122)
Male	0.0718***	0.0580***
	(0.0116)	(0.00896)
SplineAge18_24	-0.0240***	-0.0171***
	(0.00354)	(0.00366)
SplineAge25_29	-0.00926***	-0.00724***
	(0.00218)	(0.00252)
SplineAge30_34	-0.0121***	-0.00826***
	(0.00263)	(0.00220)

SplineAge35_39	-0.0143***	-0.01000***
	(0.00269)	(0.00217)
SplineAge40_44	-0.0164***	-0.00998***
	(0.00245)	(0.00242)
SplineAge45plus	-0.00800***	-0.00296***
	(0.000946)	(0.000965)
State-Year Effects	Y	Y
State-Fixed Effects	Y	Y
Constant	1.069***	0.637***
	(0.0831)	(0.0860)
Observations	32,607	32,607
R-squared	0.170	0.122

Appendix A. Calculating the Average Cost of a Serious or Violent Felony

Serious or Violent Felony[115]- 31.2% of total re-offenses
- Homicide- .8%
- Rape- .6%
- Robbery- 6.2%
- Assault- 13.7%
- Burglary- 9.9%

Percent of Serious or Violent Felonies that are:[116]
- Homicide- 2.6%
- Rape- 1.9%
- Robbery- 19.9%
- Assault- 43.9%
- Burglary- 31.7%

Breaking Down Cost to Society Per Serious or Violent Felony:[117]
- Homicide- $3,652,087 * .026 = $94,954
- Rape- $107,739 * .019 = $2,047
- Robbery- $11,333 * .199 = $2,250
- Assault- $11,975 * .439 = $5,257
- Burglary- $1,191 * .317 = $378

Average Cost to Society per Serious or Violent Felony = **$104,886**

[115] This data comes from Table 10 of the "Recidivism of Prisoners Released in 1994" codebook.
[116] To get these numbers I divide each of the above percentages by 31.2%.
[117] These cost estimates come from Table 13 in Lochner and Moretti (2004). I adjust their figures to 2001 dollars (from 1993 dollars) to match the incarceration costs data, using an inflation adjusting calculator at <http://www.westegg.com/inflation/infl.cgi>.