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FEAR OF THE FIRST STRIKE: THE FULL DETERRENT EFFECT OF CALIFORNIA'S TWO- AND THREE-STRIKES LEGISLATION

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ABSTRACT

Many states have recently enacted three-strikes laws to increase punishment for frequent offenders. However, only California actively enforces its three-strikes legislation. Existing studies of the impact on crime in California consider only partial deterrence: the deterrence of offenders committing their last strike. The only study addressing full deterrence, the deterrence of all potential offenders, examines the impact across all states in a model that does not consider the simultaneity of crime and the passage of three-strikes laws. I offer a theoretical model that shows that strike laws should deter all offenders and that partial deterrence measurements underestimate the laws' benefits. Theory-based empirical results indicate that strike sentences generally deter the crimes covered by the laws. During the first 2 years of the legislation, approximately eight murders, 3,952 aggravated assaults, 10,672 robberies, and 384,488 burglaries were deterred in California; however, larcenies increased by 17,700 during this period.

I. INTRODUCTION

WITH crime rates falling across the United States, many researchers are currently exploring the possible causes of this downturn. Explanations that have emerged include a booming economy, a change in tastes for certain drugs, and capital punishment laws. Another explanation is tougher sentencing practices.[1] However, many critics of sentencing reforms argue that stricter sentencing is too harsh on criminals and ineffective in achieving deterrence.

During the 1990s, 26 states and the federal government enacted three-strikes legislation, with similar bills introduced in a number of other states. Although many states have passed the laws, only California applies its law with any sort of regularity.[2] Between April 1994 and December 1996, California incarcerated 26,074 "strikes" offenders, more than any other state. It is estimated that well over 90 percent of the strike sentences handed down in jurisdictions with these laws were in California.[3]

The purpose of these laws, which generally fall under the moniker "three strikes and you're out," is to remove repeat offenders from society for long periods of time, if not for life.[4] The laws have both proponents and critics. Proponents of the laws claim that they protect the public by incapacitating and deterring repeat offenders.[5] Critics, however, argue that because of the relatively short length of criminal careers about 10 years on average[6] incapacitating offenders for long periods has little effect. In addition, the highest rates of commission of violent crimes occur when the offenders are in their late teens and early twenties, and the highest commission rates of property crimes occur when offenders are in their late teens.[7] People begin to desist from violent crimes after age 22 and from property crimes after age 17.[8] Thus, the aging of the prison population will weaken the effectiveness of three-strikes legislation because old prisoners would have committed few additional crimes. [9] In addition to the arguments against incapacitation, critics argue that the deterrent effect of three-strikes laws is small at best.[10]

Few studies empirically examine the arguments for and against three-strikes laws. The primary empirical examination explores the legislation's impacts in all states.[11] However, because few states actually enforce their laws, the use of a single three-strikes dummy variable in the primary model underestimates the laws' true impacts. In addition, the state-level data set used in this study causes an aggregation bias because it does not provide information on county-specific attributes and applications of the laws. Furthermore, the study's primary specification does not control for the simultaneity between crime and the introduction of three-strikes laws. Before I undertake my own analysis with superior county-level data, I show that controlling for the simultaneity in Thomas Marvell and Carlisle Moody's model causes the results to change substantially.

The two empirical examinations that focus exclusively on California also have shortcomings. The first performs only a simulation of the effects of the legislation, without using any real data.[12] The second draws conclusions based on the raw data before and after the enactment of the law instead of using regression analysis.[13] In addition, both studies assume that the only deterrence possible under strike laws is partial deterrence, the deterrence of offenders committing their last strike, because the severity of punishment is only increased for these repeat offenders.[14] However, as I show in my model, this assumption is fundamentally wrong; strike laws may also deter individuals contemplating their first offense. Once the deterrent effect on offenders other than offenders facing their last strike is considered, California's laws may prove to be more cost-effective.

In this paper, I will introduce a theoretical model that explains that strike legislation is capable of full deterrence, not just partial deterrence. "Full deterrence" refers to the concept that three-strikes laws can deter all offenders, not just offenders facing their last strike. The model is theoretically similar to many models of investment under uncertainty where the net option value of waiting must be considered in investment decisions.[15] My theoretical model shows that all potential offenders consider the threat of the law in their decisions. I then estimate an econometric model for the state of California to test for the existence of the deterrent effect that the theory suggests. The results suggest that two- and three-strikes laws deter crime not only in the county in which the sentence is imposed, but also in surrounding counties. In addition, the results support the theory of full deterrence. Although all felonies qualify as "last strikes," only a short list of crimes qualify as first or second strikes. The results show that strike laws deter the crimes on this short list more than other crimes; that is, criminals vigorously seek to avoid a first or second strike. My results are robust to many common model specifications.

The paper is structured as follows. Section II examines the details of California's legislation and discusses early assessments of the laws. The theoretical model of delayed punishment is presented in Section III. Section IV develops the econometric model specification, and Section V discusses the data and estimation techniques. Section VI presents the empirical results, and Section VII concludes.

II. The Mechanics of Strike Legislation and Earlier Studies

Because California is the only state that appears to enforce its three-strikes laws with regularity, it provides the best case study for examining the laws' impacts on crime. Although I focus my study on California, the implications of my results can be applied to all states. The results suggest what other states can expect if they either adopt new three-strikes laws or begin to enforce their existing legislation. Before introducing the theoretical and empirical models, I describe the mechanics of California's strike laws and discuss earlier studies on the effectiveness of these laws.

A. California's Two- and Three-Strikes Legislation

The California legislation includes both two- and three-strikes provisions.[16] The law defines the two-strikes zone as any felony if the offender has one prior felony conviction from the list of strikeable offenses (Table 1) and the three-strikes zone as any felony with two prior felony convictions from this list. An offender is "out" by two strikes when he commits first a strikeable offense and then an offense from the strike zone. The three-strikes provision takes effect, and the offender is out upon committing two strikeable offenses and then an offense from the strike zone. The meaning of "out" is defined as follows. For a second-strike offense, there is a mandatory sentence of twice the term for the offense. A three-strikes sentence carries a mandatory life sentence with the minimum term being the greatest of (1) three times the term otherwise required under the law for the felony conviction, (2) 25 years, or (3) the term determined by the court for the new conviction.[17]

TABLE 1 STRIKE ZONE OF CALIFORNIA TWO- AND THREE-STRIKES LAWS

Before the adoption of the current legislation in April 1994, California applied other repeat-offender provisions.[18] However, the current laws are much stricter than the previous ones. Under an earlier law, an offender was out when he committed a violent felony if he or she had two prior violent felony convictions. Under the current law, an offender is out upon committing any felony if he has two (or one for the two-strikes zone) prior serious felony convictions from the list of strikeable offenses. In general, the distinction between violent and serious is the degree of harm caused to victims. In California, violent offenses include murder, robbery of a residence in which a deadly weapon is used, and most rapes. Serious crimes include all violent offenses plus burglary of a residence, arson, assault with intent to commit robbery or rape, grand theft, kidnapping, drug sales to minors, and many others.[19] Hence, several additional crimes are covered by the current laws.

The earlier laws required that the two prior convictions be accompanied by nonconcurrent prison sentences. In contrast, the existing law requires no prior prison time for the application of a second- or third-strike sentence.[20] In addition, the previous laws were much more lenient in allowing the length of prison sentences to be reduced by up to 50 percent through work and good-behavior credits.[21] The current law limits the reduction in sentence length to 20 percent. Furthermore, the previous law did not require a prison sentence at all for the third- or fourth-strike conviction, while the current law mandates a sentence for any second or third felony conviction.[22] Also, the current law counts crimes committed by a minor at least 16 years of age as strikes, whereas the previous laws did not take into account crimes committed by minors.

In Table 2, we see that nearly 90 percent of the 26,074 offenders sentenced under this law between April 1994 and December 1996 were sentenced under the two-strikes provision. The number of offenders receiving two-strikes sentences during this period was 23,267, while only 2,807 received three-strikes sentences. There is considerable variation in the application of these laws among California counties. There seems to be little if any relationship between a county's population, crime rates, and the two- and three-strikes implementation.[23] Rather, the strictness with which the law is enforced seems to be related to county-specific characteristics. The more conservative southern part of the state is very stringent in its application, whereas counties in the urban northern areas are "cautious" in enforcing the law.[24] This issue will be further discussed when considering the exogeneity of strike sentences.

TABLE 2 NUMBER OF TWO- AND THREE-STRIKES CASES ADMITTED TO THE CALIFORNIA DEPARTMENT OF CORRECTIONS BY MONTH

Table 3 provides summary information about strike offenders. Approximately 80 percent of strike offenders are between 20 and 39 years of age. Although most fall within the 2029 age range (46.7 percent of two-strikes offenders and 43.1 percent of three-strikes offenders), the 30-39 age range also accounts for a large percentage of strike offenders (34.1 percent of two-strikes offenders and 35.3 percent of three-strikes offenders).

TABLE 3 CALIFORNIA STRIKE ADMISSIONS AS OF MARCH 1, 1996

The majority of offenders sentenced under these laws in California have been convicted of nonviolent crimes. Between April 1994 and March 1996, only 14.5 percent of two-strikes sentences and 25.5 percent of three-strikes sentences were for crimes against the person. Property crimes accounted for 41.1 percent of two-strikes sentences and 38.8 percent of three-strikes sentences, while drug offenses accounted for 31.6 percent and 22 percent, respectively.

An examination of the sentence lengths indicates that the average sentence length for two-strikes offenses is 4.9 years, while three-strikes sentences average 37.4 years. A more detailed analysis of sentence lengths can be seen in the breakdown by crime in Table 4. The average two-strikes property offense sentence is 3 years, while violent offenders receive a sentence ranging from 7 to 77 years. The sentence length increases dramatically for a third strike. Third-strike property offenses carry an average sentence of 26 to 36 years, while violent offenders' sentences range from 39 to 85 years.

TABLE 4 CALIFORNIA SENTENCE BY OFFENSE AND TWO- OR THREE-STRIKES LAWS AS OF MARCH 1, 1996

B. Existing Studies of Strike Legislation

Most early studies of the impact of the two- and three-strikes legislation have been primarily concerned with the laws' effects on the courts and prison systems. The preliminary findings show a decrease in plea bargaining and subsequent increase in jury trials.[25] This in turn has lead to an increase in persons awaiting trial in county jails, an early release of sentenced offenders from county jails, less serious and civil cases being pushed out of courts because of backlogs, and increases in the budgets of criminal justice agencies to deal with these problems.[26] However, more recent data show that most counties in California are learning to absorb these increases brought about by the law.[27]

There have been three primary empirical examinations of the impact of the current three-strikes legislation on crime. The most recent investigation studies the effects of three-strikes laws in all states.[28] The primary equation in this study uses the number of crimes in each state as the dependent variable. The independent variables in the primary model are a dummy variable indicating the passage of a three-strikes law, the percentage of the population aged 15 19, 2024, and 25 29, the unemployment rate, the number employed, real personal income, the poverty rate, the percentage of people living in metropolitan areas, the percentage of African-Americans, the prison population, year and state dummies, and the dependent variable lagged twice. The continuous variables are in per capita logarithms, and the regression is weighted by the state population to lessen the heteroskedasticity caused by greater per capita variation in small states.[29] The basic results are that three-strikes laws have no effect on most crimes and that they actually result in an increase in the number of murders.[30] The authors of the study explain that this increase in murders could be the result of offenders killing witnesses to other crimes in order to avoid harsher penalties.[31]

Although the study presents intriguing results and a sound starting point in the analysis of the impact of three-strikes laws, it has three potential problems.[32] First, the three-strikes dummy variable in the primary equation weights the laws of all 24 strike states exactly the same: states either have the laws or they do not. However, similar to many outdated state laws that still exist but are rarely enforced, the three-strikes laws in most states are rarely applied.[33] Indeed, well over 90 percent of all three-strikes sentences imposed across the country have been handed down in California.[34] Even California only handed down 26,074 strike sentences between 1994 and 1996, less than 4 percent of all felony adult sentences in the state during this period.[35]

The dummy variable does not provide any information on how often, if at all, the states apply their three-strikes laws. Such a specification will underestimate the legislation's effects in states that do enforce their laws by grouping them with other states that never enforce their laws. We would not expect a state that never enforces its law to experience a decrease in crime. Therefore, to determine the true impact on crime, we should examine only those states that apply the legislation with regularity.[36]

The study's second potential problem is that it is performed at the state level because, as the authors note, "there are fewer data problems at the state level" than at the county level.[37] However, a study at the state level introduces aggregation bias because it makes no distinction between counties that may enforce the laws differently. There is evidence that the application of three-strikes laws varies widely across counties.[38] In addition, county-specific characteristics may be correlated with criminal justice variables, which produce biased results. In contrast, because a county-level data set allows for the control of the demographic, economic, and jurisdictional differences between counties, it better isolates the effects of three-strikes laws.

The use of state-level data to examine the effects of a law whose enforcement varies between counties will likely underestimate the effectiveness of the law. For example, suppose that in a three-strikes state one county strictly applies the law and experiences a large decrease in crime. Another county never enforces the law and experiences no change or even an increase in crime. When looking at state-level data, the crime decrease in the first county will be diluted by the lack of change in the second county, or it could even be offset or surpassed if crime increased in the second county. Analyzing the three-strikes legislation in this state may lead the researcher to erroneously believe that the legislation has no effect or even that it may increase crime. Because the authors use a dummy variable to represent three-strikes legislation and a state-level data set, it is not surprising that the study finds no significant effects on most crimes.

The third potential problem is that the positive and significant relationship between murder and three-strikes laws may be explained by the simultaneity between the number of murders and the passage of three-strikes laws. As the authors of the study acknowledge, it is expected that states enact stricter sentencing policies, such as three-strikes laws, because their crime rates are higher or rising faster than those of other states. Therefore, an increase in murders may cause the passage of three-strikes legislation instead of these laws causing an increase in murder.

It is necessary and customary for studies that examine laws that states have a choice in enacting, such as capital punishment laws[39] and concealed-weapons laws,[40] to treat the law as endogenous. Moreover, in a replication of the study's results,[41] a Lagrange multiplier test for exogeneity[42] confirms that the passage of a three-strikes law by a state is endogenous in the primary murder equation.[43] When the three-strikes variable is treated as endogenous in the primary murder equation, the positive and significant coefficient on the three-strikes variable disappears.[44] Therefore, three-strikes laws may not cause an increase in murders.

The remaining two empirical examinations of the impact of three-strikes laws have focused their study on California in order to avoid some of the problems discussed above. The earliest study simulates the legislation's effects on the courts and correctional systems but does not use any actual data.[45] In this study, the authors perform a simulation experiment that tracks the flow of criminals through the justice system, calculates the costs of running the system, and predicts the number of crimes that criminals commit. The mathematical model allows the authors to predict the response of the criminal justice system to the three-strikes legislation. The simulation suggests that the law will reduce the number of serious crimes committed by 28 percent by incapacitating repeat offenders. The authors conclude that other alternatives could accomplish the same task at a lower cost. They suggest that the money might be better used to increase police forces and counsel at-risk youths.

In their estimation of crime reduction, the authors assume no deterrent effectclaiming this assumption is consistent with current research.[46] At one point, to check the sensitivity of their results to changes in this assumption, they consider partial deterrence by allowing the deterrence of repeat offenders to increase by 25 percent. The simulation reports that the decrease in the crime rate will be larger by between 4 and 6 percent. This means that the crime rate reduction will be 2930 percent instead of 28 percentnot a substantial reduction. Hence, they conclude the deterrent effect is unimportant if it exists at all.

Peter Greenwood and colleagues assume that only partial deterrence is possible under two- and three-strikes laws. The second and most recent empirical investigation into the effects of the two- and three-strikes legislation also makes this assumption.[47] The authors assume that only offenders facing their last strike are deterred by the law "because they are the only group threatened with increased penalties under the law." The authors compare the proportion of crimes committed by offenders eligible for a last-strike sentence the year before and the 2 years after the enactment of the legislation. Because there is no statistically significant change in this proportion, the researchers conclude there is no deterrence.[48]

Studies that ignore the deterrent effect or consider only the partial deterrent effect of strike legislation may severely underestimate the benefits of these laws. Because repeat offenders commit a very small proportion of overall crimearound 10.6 percent[49]a study that limits the deterrent effect to this group will necessarily understate the legislation's effectiveness. Moreover, because of the previously discussed arguments against incapacitation, the deterrent effect becomes critical to a law that locks up repeat offenders for long periods of time. In this paper, I will introduce a theoretical model that shows that two- and three-strikes legislation can deter all potential offenders, not just those with earlier convictions.

III. A MODEL OF DELAYED PUNISHMENT

The model presented in this section augments the general economic model of crime[50] to capture the deterrent effect of delayed punishment.[51] In my model, offenders base their decisions on factors in the current period only. However, one of these factors is the prospect of higher penalties in the future. This one-period model with foresight seems to escape the problems associated with multiperiod models.[52]

The basic economic model of crime is a model of choice between legitimate and illegitimate activities. In a given period, a person will choose to allocate his time between the two activities based on the expected utility associated with each. The utility expected from committing an offense is

where p_j is the individual's probability of apprehension and conviction per offense, F_j denotes his punishment per offense, W_{ji} represents his monetary and psychic income from committing an illegal act, and U_j is the individual's utility function.

In the standard crime model, F_j captures all losses and penalties from apprehension and punishment. This includes the pecuniary losses of confiscated loot, lost earnings if jail or prison time is served, possible defense costs, and the nonpecuniary losses associated with the punishment.

Now consider the model in a framework in which an offender convicted for the first time (first strike) receives losses from this conviction, F_j, along with an increased risk of more severe punishment in the future (delayed punishment), $_j$. The variable $_j$ refers to the fact that receiving a first strike moves an offender closer to the second- or third-strike mark at which he receives a punishment much greater than the crime's typical punishment. Thus, $_j$ represents the net option value of waiting to commit the first strike. Once an offender receives his first strike, he has spent his option or lost his opportunity to commit a first-strike crime again. The offender has only one more chance (in the two-strikes case) to commit and be convicted of a crime that "pays" (the expected benefits outweigh the expected costs) before receiving the inflated punishment of the two-strikes sentence.[53]

In this augmented model, equation (1) becomes

Taking the first derivative of equation (2) with respect to p_j, F_j, and $_j$ gives

and

which are all negative if we interpret F_j and $_j$ as the monetary equivalent of punishment and assume that the marginal utility of income is positive.

According to the economic model of crime, individuals choose to allocate their time between legitimate and illegitimate opportunities based on the expected utility from each activity. Thus, there is a function relating the number of offenses a person commits to the variables entering the expected utility function, equation (2),

where O_j is the number of offenses committed during a particular period, W_{jl} denotes the individual's legitimate earning opportunities, u_j represents other unobservable influences such as his willingness to commit illegal acts, and the other variables are defined as above. Because an increase in F_j, $_j$, or p_j reduces the utility expected from an offense, it will also reduce the number of offenses committed as either the expected cost of offenses or the probability of "paying" the expected cost increases. Therefore,

and

When a jurisdiction first enacts two- or three-strikes legislation, $_j$ in equation (2) changes from zero to some positive value. An individual considering committing his first crime is faced with two potential costs of committing the crime, $_j$ and F_j, where F_j just represents the punishment he would receive if convicted for the crime in a jurisdiction with no strike legislation. For repeat offenders committing what is potentially their last strike before receiving the larger punishment, $_j$ assumes a value of zero because there is no longer a risk of delayed punishment. For these individuals, however, F_j assumes a higher value because the penalty they will receive if convicted is much higher than what they would have received if it had not been their last strike. Thus, the two- and three-strikes legislation affects all potential criminals by increasing either $_j$ or F_j.

We can see from this model that the imposition of two- and three-strikes sentences increases the expected marginal costs of illegitimate activity for all individuals, not just those committing their last strike.[54] Thus, strike legislation can deter all potential offenders, not just repeat offenders.

This model predicts that the offenses covered by two- and three-strikes legislation will be deterred. We can also predict which crimes will be the most strongly deterred. The last strike can be imposed for any felony, but the first strike (and second strike under the three-strikes legislation) can be imposed only on offenders that commit a strikeable offense (see Table 1). Potential offenders facing their last strike should be deterred from committing any felony. Potential offenders facing any other strike should be deterred only from committing a strikeable offense. Because there are more individuals facing their first strike (and second strike under the three-strikes legislation) than those facing their last strike, we would predict that the strikeable offenses would be more strongly deterred. Murder, aggravated assault, robbery, rape, and burglary are the strikeable offenses that I consider.[55] Therefore, we would expect to see stronger deterrence of these crimes than the nonstrikeable crimes that I consider.

The deterrence for the crimes of murder and rape may be more complex. In 1990, the national average maximum sentence imposed for the crimes of murder and nonnegligent manslaughter was 233 months, or 19.4 years. In 1996, the sentence had grown to 253 months, or 21.1 years.[56] For older criminals, these prison sentences are effectively equivalent to a life sentence. Even younger criminals may discount their futures so greatly that they perceive 20 years as not significantly less than a life sentence.[57] Because strike legislation may not significantly increase criminals' perceptions of the potential prison sentence for murder, the deterrence of this crime may not be as strong as the deterrence of other strikeable offenses.

We would also expect the results for rape to be weaker than the results for the other strikeable offenses. Although strike legislation should decrease the number of committed rapes, it may also be expected to increase the percentage of those rapes that are reported to authorities: studies have shown that rape victims are more willing to report rapes and to subject themselves to the potential stigma or embarrassment of trial if the perpetrator faces stiffer penalties.[58] My data measure only reported rapes. The combination of fewer committed rapes but more reported rapes may make the net effect of strike sentencing on the number of reported rapes very small.

Furthermore, there may be substitution among crimes. Strike laws increase the penalty on all felonies only for criminals facing their last strike. However, for offenders who are not facing their last strikethe majority of offenders strike laws increase the penalty only for strikeable crimes.

This may result in early strike offenders substituting out of the harshly penalized strikeable crimes and into the nonstrikeable crimes with lesser penalties. If both a strikeable offense and a nonstrikeable offense have positive expected net benefits before the passage of the strike legislation, the rational criminal should commit both crimes. However, because time is scarce, at least some offenders will have time to commit only one of the crimes. For those who have initially chosen the strikeable crime, a strike law that increases the costs of strikeable crime may make the nonstrikeable crime more attractive in comparison. Therefore, an offender who previously committed a strikeable offense may instead choose to commit a nonstrikeable offense. Therefore, the strikeable crimes in my datamurder, aggravated assault, robbery, rape, and burglary may decline while the nonstrikeable crimeslarceny and auto theftcould conceivably increase.

Whether the nonstrikeable crimes increase will depend on the relative sizes of two opposing effects. The strike laws' substitution effect will induce those not facing their last strike to substitute from strikeable crimes into nonstrikeable crimes. In contrast, the strike laws' deterrence effect will discourage those facing their last strike from committing all felonies, including nonstrikeable crimes.

We are at an opportune point for studying the effects of strike laws on crime. Examining the impact on crime in the years immediately following a law change allows for the separation of the legislation's deterrent effect from its incapacitation effect.[59] The data analyzed in this paper end in 1996, so less than 3 years had passed since the enactment of California's two- and three-strikes legislation. Even offenders sentenced before the enactment of the current legislation would, for the most part, not yet be released by the end of 1996.[60] Thus, any additional incapacitative effect on crime rates resulting from the new strike laws cannot yet be seen in these data, and therefore most of the variation in crime rates can be attributed to deterrence.[61]

IV. ECONOMETRIC MODEL SPECIFICATION

In this section, I will discuss three different issues related to the specification of the model to be estimated. First, I will develop an estimable function for the theoretical model presented in Section III. Then I will aggregate this function and discuss the appropriate functional form to estimate. Finally, I will present the econometric model and discuss issues of simultaneity.

A. The Estimable Function

Equation (6) in Section III represents the behavioral function that relates an individual's participation in crime to the determinants of participation. From equation (9), we see that the probability of apprehension and conviction, $_j$, should be inversely related to criminal participation. Equations (7) and (8) indicate that the degree of punishment and risk of delayed punishment, F_j and $_j$, should also be negatively related to participation in illegal activity. An increase in legitimate wages, W_{ji}, or a decrease in illegitimate earnings, W_{jl}, should have a similar crime-reducing effect.

Many of the variables in equation (6) are not readily available. For example, variables that can accurately measure legal and illegal earning opportunities are impossible to obtain. Instead, I will use several economic and demographic variables as proxies. For example, measures of income and transfer payments, population density, age, gender, and race may influence earning opportunities and can therefore serve as reasonable approximations.

Similarly, the variables F_j and $_j$ are difficult to measure. It is impossible to obtain exact measures of F_j because county-level data on sentence lengths are not disaggregated enough to obtain measures of sentence lengths for different crimes. The exact value of $_j$, the perceived risk of delayed punishment, is complicated to measure without knowing certain aspects of the individual's pattern of crime, such as rate of desisting from crime and the expected future payoffs from crime. Instead, I will use a variable to proxy for the perceived likelihood of obtaining a two- or three-strikes sentence in the future. This variable is the percentage of sentenced prisoners that receive a strike sentence.

The estimable individual function is thus

where O_j is the number of offenses committed during a particular period, $_j$ is the perceived probability of apprehension and conviction, $_j$ denotes the perceived risk of receiving a strike sentence in the future, Z_j contains individual-specific economic and demographic variables, and u_j represents other unobservable influences such as willingness to commit illegal acts.

B. Aggregation and Functional Form

Because it is impossible to estimate this function for all individuals, it must be aggregated to a level that can be estimated. I will use the county as the level of observation. With aggregation, the measurement of the variables in equation (10) changes somewhat. The value of O_j becomes the number of crimes committed in the county, instead of the number of crimes committed by the individual. The qualitative elements in Z_j become percentages, and the level elements are transformed into per capita measures. The variables $_j$ and $_j$ are not altered.

The issue of aggregation is imperative in the consideration of the correct functional form for equation (10).[62] Many studies of crime choose somewhat arbitrary functional forms to estimate their modelssingle log, double log, and linear are commonly used.[63] However, because equation (10) seeks to describe the behavior of an individual, when we aggregate the equation to the county level we must add up the equations of the J individuals in the county. If we base our estimation on an equation derived for a single individual, we are implicitly claiming that that equation is invariant to aggregation. Only the linear functional form is invariant to aggregation, because the sum of J single-log or double-log equations is not another single-log or double-log equation. The linear form of the supply-of-offenses equation for individual j in period n is

where $u_{j,n}$ is the error term with mean zero and variance ^{2}. Aggregating equation (11) to the county level involves summing the equation over all J individuals in county c and then dividing by J, where $C_{c,\,n}$ is the crime rate for a crime in county c in period n (number of crimes divided by county population). As I discussed earlier, $_{c,\,n}$ and $_{c,\,n}$ remain unchanged, while $Z_{c,\,n}$ is transformed into percentages and per capita measures. The new error term is

which is heteroskedastic because its variance $^{2}/J_c$ is proportional to county population. To correct this, the estimation technique must be revised because ordinary least squares estimation produces inefficient coefficient estimates. Therefore, I employ weighted least squares estimation where the weights are the square root of county population.[64]

C. Econometric Model

1. *The Supply of Offenses*

The supply-of-offenses equation (12) provides the foundation for the empirical estimation. The variable $_{c,\,n}$, the probability of apprehension and conviction, can be separated into two distinct probabilities: the probability of arrest and the conditional probability of being convicted if arrested. However, because data on convictions are not collected and reported, the probability of conviction given arrest cannot be estimated. Instead, I will use the probability of imprisonment given arrest as a proxy.[65] Hence, the supply-of-offenses equation to be estimated is

where Pa is the probability of arrest (defined as number of people arrested for each crime divided by the number of crimes) and $Pi|a$ is the conditional probability of imprisonment given arrest (defined as the number of offenders admitted to prison for each crime divided by the number of arrests for that crime). The perceived probability of receiving a strike sentence, , is defined as the number of offenders receiving a strike sentence divided by the number of offenders admitted to prison. These three probabilities are included in the supply-of-offenses equation following the theoretical predictions of the economic model of crime that crime rates are inversely related to probabilities of apprehension, conviction, and imprisonment.

The Z variable includes several economic and demographic variables that serve as proxies for the legitimate and illegitimate earning opportunities, discussed in Section III, that affect an individual's decision to commit a crime. The economic variables are real per capita personal income, real per capita unemployment insurance payments, and real per capita income maintenance payments. The income variable measures both the labor market prospects of potential criminals and the amount of wealth available to steal. The unemployment payments variable is a proxy for overall labor market conditions and the availability of legitimate jobs for potential criminals. The transfer payments variable represents other nonmarket income earned by poor or unemployed people. Other studies have found that crime responds to both measures of income and unemployment but that the effect of income on crime is stronger.[66]

The demographic variables included in Z are population density, the percentage of the county population that is between 10 and 19 years of age, the percentage of the county population that is between 20 and 29 years of age, the percentage of the county population that is male, the percentage of the county population that is African-American, and the percentage of the county population that is some minority group other than African-American. Population density is included to capture any relationship between drug activities in inner cities and crime rates. The age, gender, and race variables represent the possible differential treatment of certain segments of the population by the justice system, changes in the opportunity cost of time through the life cycle, and gender-/race-based differences in earning opportunities. These county-level economic and demographic variables are included following other studies based on the economic model of crime.[67]

The variable TD in equation (14)is a set of time dummies that captures trends in crime or attitudes toward crime that do not vary across counties but change through time. In addition, county dummies are included to control for unobservable variables that differ among counties, such as differences in crime, attitudes toward crime, or differences in the justice system. Finally, u is the regression error term.[68]

The three probabilities, the probability of arrest, the conditional probability of imprisonment if arrested, and the conditional probability of receiving a strike sentence if imprisoned, may be endogenous to the crime rate in equation (14). The police and court system may respond to increases in crime by increasing their own efforts to combat crime. I will use the economic model of crime to identify the equations associated with these variables and then estimate the model as a system of simultaneous equations. Tests for endogeneity will confirm which variables should be treated as endogenous in the empirical estimation.

2. *The Production Functions of the Police and Court System*

The probability of arrest and the probability of imprisonment given arrest represent the activities of the police and the court system as they protect the public from criminals. In the economic model of crime, the relationship between the activities of the criminal justice system and the supply of crime is summarized via a production function.[69] The production function representing the activities of the police identifies the probability of arrest, while the production function representing the activities of the judicial system identifies the probability of imprisonment given arrest.

The activities of the criminal justice agencies are determined by the public's allocation of resources, as they demand more or less protection from criminals. The crime rate will determine society's demand for protection. As crime increases, a community will demand more protection and will allocate more resources to that protection. This public expenditure on law enforcement and the court system will determine the productivities of these groups (probabilities of arrest and imprisonment given arrest). Therefore, equations that characterize the relationship between enforcement activities and crime must include expenditure variables.[70] The production function equations are and

where PE, the expenditure on the police, and JE, the expenditure on the judicial and legal systems, serve as instruments in the equations. The crime rate, *C*, captures the effects of specific crimes on the arrest and imprisonment rates for those crimes. The expression OC is defined as the crime rate of property crimes when a violent crime is estimated and as the crime rate of violent crimes when a property crime is estimated.[71] As violent crime rates increase, increased public demand for enforcement may induce the police and court system to devote more effort to fighting both violent and property crimes. Thus, the probability of arrest and imprisonment for property crimes may increase. Alternatively, an increase in violent crime may encourage the police and court system to concentrate their efforts on violent crimes, thus decreasing the probabilities of arrest and imprisonment for property crimes.[72] The expression TD represents a set of time dummies that capture trends and influences that impact all counties but vary over time, and and are regression error terms.

3. *The Probability of a Strike Sentence*

A third probability, the conditional probability of receiving a strike sentence if imprisoned, may also be endogenous to this system of equations. Although evidence suggests that the counties' population, crime rates, and the two- and three-strikes implementation are unrelated,[73] it is easy to imagine how the strike probability could be affected by the crime rate. Not only could the stricter imposition of strikes deter crime, but increasing crime may also convince the criminal justice system to impose more strike sentences. The equation for the probability of receiving a strike sentence is

where *C* and OC again capture the effect of specific crime rates or other-category crime rates on the strike sentence probability. A partisan influence variable, PI, is defined as the percentage of each county's population voting Republican in the most recent presidential election. This variable captures the apparent differences in strike law implementation between the conservative southern counties and the more liberal northern counties.[74] Once again, TD is a set of time dummies that capture trends and influences that impact all counties but vary over time, and *v* is the regression error term. Equations(14)(17) are my primary system of equations.

V. Data and Estimation

I use a panel data set that covers all 58 California counties for the period 198396. By using county-level data with a time dimension, county-specific characteristics can be controlled for so that the effects of the two- and three-strikes legislation can be better isolated. Fixed-effects estimation can control for the unobservable heterogeneity that arises from the county-specific attributes that seem to determine the strictness with which this legislation is applied. Thus, I will condition the two-stage estimation on the presence of county fixed effects.

The data set includes crime and arrest data for the violent crimes of murder, aggravated assault, robbery, and rape and the property crimes of larceny, burglary, and auto theft.[75] These data are from the Federal Bureau of Investigation's Uniform Crime Reports.[76] The county arrest rates (number of arrests divided by number of crimes) are used to estimate the probability of arrest. The county-level crime numbers divided by the county population are used for the crime rate in equations (14)(17). In addition, the other crime rate variable represents the rate of all violent or property crimes.

The probability of imprisonment given arrest is estimated with data from the Bureau of Justice Statistics (BJS) National Corrections Reporting Program (NCRP).[77] Since 1983, the BJS has compiled the NCRP data series that collects individual inmate records for prison admissions and releases and parole discharges. It is the only national-level database that is collected annually at the county level with information on prison population movement and parole population. During the 1990s, between 35 and 41 states participated in the NCRP. So while this sample is not exhaustive across the nation, it does provide complete information on each state that participates. Luckily, California has participated in this program since its onset in 1983. To estimate the probability of imprisonment given arrest, I divide the number of people sentenced to prison for each crime by the number of people arrested for that crime in each county.[78]

The risk of receiving a strike sentence is estimated as the number of offenders receiving a two- (or three-) strike sentence divided by the number of offenders imprisoned.[79] The data on the number of strike sentences are obtained from the California Department of Corrections, Offender Information Services Branch.[80] The data on police and judicial/legal expenditures are collected from the BJS.[81]

The Z variable includes several economic and demographic variables. The economic variables, real per capita personal income, real per capita unemployment insurance payments, and real per capita income maintenance payments, are obtained from the Regional Economic Information System of the Bureau of Economic Analysis.[82] The demographic variables are population density, the percentage of the county population that is between 10 and 19 years of age, the percentage of the county population that is between 20 and 29 years of age, the percentage of the county population that is male, the percentage of the county population that is African-American, and the percentage of the county population that is some minority group other than African-American. This data are obtained from the U.S. Bureau of the Census.[83]

To estimate the simultaneous system of equations (14)(17), I use the method of two-stage least squares, weighted by the square root of county population to correct for the heteroskedasticity of the u term.[84] Tests indicate autocorrelation of the disturbance terms.[85] Therefore, I estimate a model with first-order autoregressive disturbance terms, where is estimated by from the residual regression of $u_{c,n} = u_{c,n\text{-}1}$.

The results of Hausman[86] and Lagrange multiplier tests for endogeneity[87] indicate that the probabilities of arrest and imprisonment are endogenous to the system of equations, but that the probability of a strike sentence should be treated as exogenous.[88] Therefore, my empirical estimation will be confined to equations (14)(16). Although the passage of the law at the state level may be endogenous in the supply of crime equations, once the law is adopted, the actual implementation of the laws seems to be related only to county-specific characteristics. Regardless, the results in Section VI show that my primary model's results are also robust to specifying the probability as endogenous. In addition, tests of overidentifying restrictions indicate that the model is correctly specified and employs valid instruments for the majority of the crimes.[89]

VI. Empirical Results

I first report the results from the primary system of equations (14)(16). Then I discuss the robustness of the results to other model specifications and the possibility that strike sentences cause criminal migration instead of deterrence.

A. Results of the Primary System of Equations

The results of the two-stage least squares weighted estimation with fixed effects and first-order autoregressive disturbance terms are reported in Tables 5, 6, 7, and 8. The simultaneous equation system (14)(16) is estimated for each crime separately, but only the results of the supply of offenses equation (14) are reported in the tables.[90] Table 5 reports the estimated coefficients for the violent crimesmurder, aggravated assault, robbery, and rapewhen one of the explanatory variables is the probability of a two-strikes sentence. Table 6 contains estimates of the same equation for the property crimesburglary, larceny, and auto theft. Tables 7 and 8 report estimated coefficients for violent and property crimes, respectively, when the strike sentence variable is the probability of a three-strikes sentence.[91]

TABLE 5 Two-Stage Least Squares Regression Results for Violent Crime Rates: Probability of Two-Strikes Sentence

TABLE 6 Two-Stage Least Squares Regression Results for Property Crime Rates: Probability of Two-Strikes Sentence

TABLE 7 Two-Stage Least Squares Regression Results for Violent Crime Rates: Probability of Three-Strikes Sentence

TABLE 8 Two-Stage Least Squares Regression Results for Property Crime Rates: Probability of Three-Strikes Sentence

Two-strikes sentences have a significant deterrent effect on murder, aggravated assault, robbery, and burglary, as indicated by the negative coefficient on the variable for the probability of two-strikes sentences. For rape, larceny, and auto theft, the coefficients on the two-strikes sentencing variable are positive but insignificant. The impact of three-strikes sentences on murder, robbery, and burglary is negative and significant. In contrast, the impact of three-strikes sentences on larceny is positive and significant. The coefficient on the three-strikes variable is negative but insignificant for aggravated assault and auto theft, and the coefficient remains positive and insignificant for the crime of rape.

The results confirm the theoretical predictions that strikeable offenses will be more strongly deterred than other felonies. Murder, aggravated assault, robbery, rape, and most burglaries are strikeable offenses, whereas auto theft and most larcenies are not.[92] Therefore, we would expect these crimes to be the most strongly deterred. The negative coefficients on both the two- and three-strikes variables for murder, aggravated assault, robbery, and burglary imply that these crimes are deterred. The positive coefficients on the strike variables for larceny and on the two-strikes variable for auto theft suggest that these crimes are not deterred.

My results support the theory of full deterrence. If strike laws deterred only offenders facing their last strike, then we would expect the results to show that both strikeable and nonstrikeable felonies are deterred; any felony, whether strikeable or not, can serve as a last strike. However, my results indicate that deterrence is strongest among the strikeable offenses, the crimes that can count as an initial strike; nonstrikeable offenses are not deterred.[93] Fearing initial strikes, potential criminals commit fewer crimes that qualify as initial strikes.

The negative coefficients on the strike sentence variables are much smaller in magnitude for murder than for the other strikeable offenses; the coefficients are positive and insignificant for the crime of rape. These findings confirm the theories' predictions that stricter sentencing may not lead to a substantial decrease in the number of reported murders and rapes. If criminals discount their future as heavily as many believe, stricter sentencing may not substantially increase criminals' perceptions of the prison sentence for murder. The relatively small coefficients on the strike sentence variables for murder support this theory. In addition, stricter sentencing may deter rapes, but also increase the number of rapes that are reported as victims become more willing to report the crime. The positive and insignificant coefficients on the strike sentence variables for rape suggest that the net effect of this combination may be a small increase in the number of reported rapes. Although impossible to measure, the number of committed rapes that are deterred may be much larger than the coefficients suggest.

Furthermore, as predicted, some criminals appear to be substituting away from strikeable offenses to nonstrikeable offenses. The positive and significant coefficient on the three-strikes variable for larceny indicates that strike legislation results in an increase in the number of larcenies. Although insignificant, larceny and auto theft also have positive coefficients on the two-strikes variable. Although criminals committing their final strike should be deterred from all felonies, it appears that criminals committing early strikes prefer to commit crimes such as larceny and auto theft that have lower penalties. The net effect appears to be an increase in the nonstrikeable offenses.

The coefficient on the probability of arrest is negative and significant for all crimes, which indicates deterrence. The probability of imprisonment also has many negative coefficients.[94] The results for the economic and demographic variables vary, depending on the crime. It appears that the relative attractiveness of legitimate and illegitimate earning opportunities for different crimes depends on the potential criminal's income and demographic status.

Thus, the results of the econometric tests seem to support the deterrence theory for two- and three-strikes legislation. Because the interval of time between the imposition of this legislation (1994) and my most recent year of data (1996) is small, my results are picking up little, if any, incapacitation effect. Although the strike law alters the length of prison sentences, not enough time elapsed between 1994 and 1996 for the number of criminals in prison because of these laws to be much greater than the number resulting from previous laws; even under the old laws, most of the prisoners would have still been in prison in 1996.

We can use the coefficients in Tables 5, 6, 7, and 8 to estimate the number of crimes that two- and three-strikes laws deter.[95] The coefficients indicate that during the period 1994 96, each two-strikes sentence resulted in approximately four fewer aggravated assaults, eight fewer robberies, and 144 fewer burglaries.[96] To compute a conservative estimate of the total number of crimes deterred by two-strikes laws, I will presume that each strike sentence has a deterrent effect only on the particular crime for which the sentence is imposed. In other words, a two-strikes sentence imposed for robbery deters only robberies and does not deter murders. This assumption probably underestimates the number of crimes deterred by two- and three-strikes laws.

In Table 4, we see that between April 1994 and March 1996 there were 988 two-strikes sentences imposed for aggravated assault, 929 sentences for robbery, and 2,147 sentences for burglary. Hence, during the first 2 years after the enactment of the strike legislation, a total of 3,952 aggravated assaults, 7,432 robberies, and 309,168 burglaries were deterred by two-strikes laws.[97]

The coefficients in Tables 7 and 8 indicate that each three-strikes sentence imposed between 1994 and 1996 resulted in one less murder, 18 fewer robberies, and 280 fewer burglaries.[98] However, each three-strikes sentence also led to 118 more larcenies as offenders substituted away from strikeable offenses to nonstrikeable offenses.[99] Between April 1994 and March 1996, there were a total of eight three-strikes sentences imposed for murder and nonnegligent manslaughter, 180 three-strikes sentences for robbery, 269 three-strikes sentences imposed for burglary, and 150 three-strikes sentences for larceny. Therefore, during the first 2 years after the enactment of the strike legislation, the imposition of three-strikes sentences deterred approximately eight murders, 3,240 robberies, and 75,320 burglaries.[100] Three-strikes sentences also resulted in about 17,700 more larcenies between 1994 and 1996.[101]

A recent National Institute of Justice study estimates the costs to victims for different crimes based both on tangible losses such as lost productivity, medical expenses, and property damage and on intangible losses such as pain, suffering, and lost quality of life.[102] The authors find that the each murder costs victims an average of $3,126,032; each aggravated assault, an average of $25,519; each robbery, approximately $8,506; each burglary, $1,489; and each larceny, approximately $393 (in 1996 dollars). Using the numbers from my Tables 5, 6, 7, and 8, two-strikes laws have saved victims over $624 million and three-strikes laws have saved victims almost $165 million by deterring potential offenders.[103] However, the increase in larcenies has also cost victims almost $7 million.[104]

The amount of money saved by two- and three-strikes laws is much larger when we also consider the nonvictim costs of crime. One study has computed estimates of the costs of society's response to violent behavior by estimating the costs of criminal justice processing, legal defense, sanctions, and losses in productivity if the offender is incarcerated.[105] Averaged over all victimizations and attempts, each murder costs nonvictims approximately $133,798 and each aggravated assault and robbery costs approximately $7,218 (in 1996 dollars). The study does not compute nonvictim costs for property crimes. Although this study provides the most complete estimate of the costs of society's response to crime that is currently available, it excludes some significant costs. For example, the study ignores the costs of the precautionary measures and fear due to violent crime: monetary expenditures for prevention, crime prevention behavior, and fear of crime. In addition, the estimates omit the time spent by victims and witnesses with police and the criminal justice system. Also excluded are the costs of other noncriminal justice programs such as social and neighborhood groups designed to reduce the exposure to victimization or the propensity of people to commit offenses. Furthermore, this study's estimate of nonvictim costs includes in its average not only committed crimes, but also attempted crimes. It is reasonable to suppose that a committed crime will impose greater social costs than an attempted crime; an estimate of the average cost of both victimizations and attempts will be much lower than an estimate of the average cost of actual victimizations alone. Because my paper estimates the number of actual crimes deterred, not crimes and attempted crimes, the cost savings reported here will undervalue the true costs avoided by deterring offenders.

Although the available data on the nonvictim costs of violent crime underestimate the true costs, we can use these data to obtain conservative estimates of the total cost savings of two- and three-strikes laws. If each of the 3,952 aggravated assaults and 7,432 robberies deterred by two-strikes laws would have cost society an additional $7,218 in nonvictim costs, then this legislation has saved society an additional $82.17 million by deterring would-be offenders.[106] Because the existing studies do not compute the nonvictim costs of property crimes, this number ignores the cost savings to nonvictims of the 309,168 burglaries that were deterred by two-strikes laws. Even so, this brings our final estimate of the money saved by two-strikes laws by deterring offenders to approximately $706 million.[107] Similarly, the eight murders and 3,240 robberies deterred by three-strikes laws saved society over $24 million in nonvictim costs.[108] This number ignores the nonvictim costs avoided by the deterrence of 75,320 burglaries and the nonvictim costs arising from the 17,700 new larcenies. Nevertheless, during the first 2 years of this legislation, over $182 million in victim and nonvictim costs have been saved by the imposition of three-strikes laws.[109] Overall, two- and three-strikes laws have saved victims and society almost $889 million.[110]

B. Robustness of Results to Alternative Model Specifications

I also test that my results are robust to other common model specifications. I reestimate the

primary system of equations (14) (16) in single-log and double-log functional forms, in first differences, eliminating the smallest and largest county, in an unweighted model, with two lags of the dependent variable as regressors, with a linear time trend, with individual county-level time trends, with variables in levels instead of probabilities, and with an endogenous strike sentence probability. Table 9 reports the coefficients and *t*-statistics for the perceived probability of receiving a second-strike sentence in the different model specifications.[111] The results of these alternative models indicate that my primary results are robust.

TABLE 9 ROBUSTNESS OF PROBABILITY OF TWO-STRIKES SENTENCE TO COMMON MODEL SPECIFICATIONS

Although Section IV shows that a linear model is the theoretically correct functional form for my system of equations, I estimate using the single-log (the log of the crime rate is the dependent variable) and the double-log (all continuous variables are in logs) specifications. In addition, I estimate the model in first differences by differencing all continuous variables in equations

(14) (16). To ensure that my results are not driven by a very small or very large county, I also estimate the model eliminating the smallest and largest counties over the time period.

Although the error terms in my model are heteroskedastic without weights, I next estimate an unweighted version of the model. Another common model specification includes lags of the dependent variable as independent variables. Therefore, I estimate the model with two lags of the crime rate as regressors in equation (14). In addition, to capture changes in crime or attitudes toward crime that do not vary across counties but change through the years, I estimate equations

(14) (16) using a linear time trend instead of year dummy variables. I also estimate the model with individual county time trends to control for any trend in crime or attitudes toward crime occurring in individual counties.

The denominator in the probability of receiving a second-strike sentence, the number of total prison sentences, is some proportion of the numerator in the dependent variable, the number of crimes. To make certain that correlated errors in these two measures are not leading to a spurious negative coefficient, I estimate the model using the number of strike sentences instead of the ratio of strike sentences to the total number of prison sentences. The dependent variable is the number of crimes, and all other variables are also specified in levels.

Tests for endogeneity confirm that the probabilities of arrest and imprisonment are endogenous to the system of equations and that the probability of a strike sentence is exogenous. To test the sensitivity of my results to this assumption, I reestimate the model with an endogenous strike sentence probability. The new specification assumes not only that stricter imposition of strikes may deter crime, but also that increasing crime may convince the criminal justice system to impose more strike sentences.

Any endogeneity would cause the results from the estimation of equations (14) (16) to underestimate the strike legislation's deterrent effect. The positive causation running from crime rates to strike sentencing would be in the opposite direction of the negative causation running from strike sentencing to crime rates; this would cause overly conservative coefficient estimates. I reestimate the model with an endogenous strike sentence probability by using all four equations

(14) (17) as discussed in Section IV.

In Table 9, we can see that the results are robust to alternative model specifications, confirming the primary model's results. Murder, aggravated assault, robbery, rape, and burglary appear to be deterred by strike legislation. The coefficient on the strike sentence probability is negative for murder in all but one specification but is sometimes insignificant. For aggravated assault, the coefficient is always negative and almost always significant. Burglary and robbery have negative and significant coefficients on the strike sentence probability in all but one specification. Rape also has several negative and significant coefficients. The negative coefficients are smaller in magnitude for murder and rape. This finding supports the theories that criminals may discount their futures too heavily to consider a strike sentence for murder to be much greater than a standard murder sentence and that the net effect of a decrease in committed rapes but increase in reported rapes is quite small.

For auto theft and larceny, the coefficients and significance vary greatly depending on the specification.[112] However, the strike sentence variable is usually positive for these crimes, suggesting that at least some criminals may substitute from the strikeable offenses of murder, aggravated assault, rape, robbery, and burglary into the nonstrikeable offenses of larceny and auto theft. Because strike laws deter last-strike offenders from these crimes while causing early-strike offenders to substitute into them, the net effect is probably small, as the coefficients indicate.

In conclusion, the results of the primary model are generally robust to several common model specifications. Nevertheless, tests for heteroskedasticity, endogeneity, and significance of variables[113] indicate that the primary model is correctly specified.

C. Strike Sentences and Criminal Migration

When using the county as the level of observation, the possibility of criminal migration arises. If one county engages in stricter sentencing practices than the surrounding counties, criminals may leave the strict county to commit crimes in the lenient neighboring counties. If a researcher improperly ignores the possibility of criminal migration, then he or she may erroneously infer that stricter sentencing in county A that results in a crime decrease in county A is evidence of deterrence. However, this result is also consistent with criminal migration. Stricter sentencing practices may not cause any decrease in crime; the practices may simply cause a relocation of crime. To prove that stricter strike sentencing actually deters criminals, I examine the impact of sentencing rates on crime rates in neighboring counties.[114]

To test for criminal migration, I estimate the primary system of equations with three extra variables: the neighboring counties' probability of arrest, probability of imprisonment if arrested, and probability of receiving a strike sentence if imprisoned. These equations are as follows:

and

where subscript *x* denotes the neighboring counties' probabilities.

The results of the estimation are in Tables 10 and 11.[115] A positive coefficient on the neighboring county's strike sentence probability would indicate that stricter sentencing in a county results in higher crime rates in neighboring counties as criminals migrate to commit illegal acts. However, the coefficients on the neighboring county's probability of receiving a second-strike sentence are negative and significant for the crimes of aggravated assault, robbery, burglary, and auto theft. The results suggest that stricter sentencing in a county actually decreases crime in the neighboring counties.

TABLE 10 Two-Stage Least Squares Regression Results for Violent Crime Rates: Criminal Migration

TABLE 11 Two-Stage Least Squares Regression Results for Property Crime Rates: Criminal Migration

Why would criminals in one county care about sentencing practices in another county? In large cities, news reports or publicity about stricter sentencing practices may not specify exactly which county is imposing the stricter sentencing. In addition, criminals may not be sure where the actual county lines are located. Furthermore, criminals may not be aware of exactly how the criminal justice system chooses the jurisdiction in which to prosecute the criminal: is the appropriate jurisdiction the one in which the crime took place, where the criminal lives, or where the criminal was apprehended? Regardless of the reason, we can conclude from the empirical results that strike sentences not only deter criminals within a county, but also deter criminals in surrounding counties. This suggests that my calculations of the cost savings of strike laws underestimate the true benefits.

VII. Conclusions

The model of delayed punishment presented in Section III is based on the economic model of crime and augmented to capture the threat of an increased punishment in the future. The model shows that three-strikes legislation will deter all potential offenders, not just repeat offenders. Because repeat offenders commit only 10 percent of crimes, studies that ignore the deterrent effect or measure only the partial deterrence severely underestimate the benefits of these laws. When the full deterrence is measured, the decline in crime attributed to three-strikes legislation should be quite large.

To study the full deterrent effect, I analyze the effect of two- and three-strikes legislation in the state of California, the only state that actively enforces its strike legislation. I focus my study on this state because we would not expect strike legislation in other states, which rarely apply the laws, to affect crime. I use a panel data set covering all California counties over the period

1983 96 to capture the county-specific attributes that could affect law enforcement practices and better isolate the effects of the current legislation.

The empirical results from the theory-based system of equations (14) (16) support my

model's predictions. The empirical tests suggest a deterrent effect for the strikeable offenses murder, aggravated assault, robbery, and burglary. However, there may be some substitution from

these offenses into the nonstrikeable crimes larceny and auto theft. These results support the theory of full deterrence: strike legislation deters all offenders, not only offenders facing their last strike. Although all felonies qualify as last strikes, only a short list of crimes qualify as initial strikes. The results show that strike laws deter the crimes on this short list more than other crimes; criminals diligently try to avoid an initial strike. In addition, the deterrence is not limited to crime in that county; strike sentences also deter crime in surrounding counties. The estimation results are robust to many alternative model specifications.

During the first 2 years after the legislation's enactment, approximately eight murders, 3,952 aggravated assaults, 10,672 robberies, and 384,488 burglaries were deterred in California by the two- and three-strikes legislation. However, the laws also resulted in 17,700 more larcenies as criminals substituted out of strikeable offenses and into nonstrikeable offenses. The deterrence of these crimes saved society approximately $889 million. The true cost savings are actually much larger if one includes the costs of precautionary measures and fear, the time that victims and witnesses spend with the criminal justice system, the costs of other noncriminal justice programs, the nonvictim costs of property crimes, and the deterrence of crime in neighboring counties. Nevertheless, $889 million is a significant amount of savings in only the first 2 years of the law's implementation.

Moreover, the total benefit of the strike legislation when compared with a system with no repeat-offender laws is much larger than that reported here. This paper computes estimates of the additional crimes deterred and costs saved by the two- and three-strikes legislation compared with the situation under California's preexisting repeat-offender laws. The preexisting laws probably already deterred many crimes. Changing from a system with no repeat-offender laws to a full two- or three-strikes system would be expected to increase deterrence not only by the amount reflected in my results, but also by the additional amount that California's preexisting system had already achieved.

To determine the effectiveness of California's two- and three-strikes legislation, the cost-saving benefits should be compared with the costs of implementing the law.[116] However, calculation of the costs is beyond this paper's scope. The cost with which the benefits would be compared would be the increase in costs of the strike legislation over the previous laws, not the total cost of the strikes program.

My results suggest that earlier studies of two- and three-strikes legislation that ignore or severely discount the deterrent effect of these laws were in error. To consider fully the impacts of the laws, and especially in the context of a cost-benefit analysis, the full deterrent effect cannot be ignored. Any analysis that does not consider full deterrence is incomplete.

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[22] Under the prior laws, convicted felons could be granted probation or placed in an alternative punishment or treatment program. *Id*.

[23] Robert C. Cushman, Effect on a Local Criminal Justice System, in Vengeance, *supra* note 10, at 90, 106; Clark, Austin, & Henry, *supra* note 2, at 4 5.

[24] Malcolm Feeley & Sam Kamin, The Effect of "Three Strikes and You're Out" on the Courts: Looking Back to See the Future, in Vengeance, *supra* note 10, at 135, 148.

[25] Clark, Austin, & Henry, *supra* note 2, at 3; Esparza, *supra* note 17, at 3 4; David Schultz, No Joy in Mudville Tonight: The Impact of "Three Strike" Laws on State and Federal Corrections Policy, Resources, and Crime Control, 9 Cornell J. L. & Pub. Pol'y, 557, 575 (2000).

[26] Esparza, *supra* note 17, at 5 6.
[27] Clark, Austin, & Henry, *supra* note 2, at 3. Other studies have examined the impacts of California's previous repeat-offender laws on sentencing. Daniel P. Kessler & Anne Morrison Piehl, The Role of Discretion in the Criminal Justice System, 14 J. L. Econ. & Org. 256 (1998); Kessler & Levitt, *supra* note 18.
[28] Marvell & Moody, *supra* note 11.

[29] *Id*. at 94 96.

[30] *Id*. at 96 97.
[31] *Id*. at 106.
[32] Besides the three main arguments presented here, Section IV*B*, *infra*, provides additional arguments against the log and level specifications used in the Marvell and Moody study.
[33] Clark, Austin, & Henry, *supra* note 2, at 1.
[34] Zimring, Kamin, & Hawkins, *supra* note 3, at 1.
[35] California Criminal Justice Profiles (California Dep't Just., Crim. Just. Stat. Ctr. 1999).
[36] In a supplementary regression, the authors assign a three-strikes dummy variable for each state. This specification separates the effect of each state's laws, but it does not provide information on the regularity of the laws' enforcement.
[37] Marvell & Moody, *supra* note 11, at 94 n.24.

[38] Cushman, *supra* note 23, at 106; Clark, Austin, & Henry, *supra* note 2, at 4 5.
[39] Hashem Dezhbakhsh, Paul Rubin, & Joanna Mehlhop Shepherd, Does Capital Punishment Have a Deterrent Effect? New Evidence from Post Moratorium Panel Data 13 (Working paper, Emory Univ., Dep't Econ. 2001).
[40] John R. Lott, Jr. & David B. Mustard, Crime, Deterrence and Right-to-Carry Concealed Handguns, 26 J. Legal

Stud. 1, 39 48 (1997).
[41] I am grateful to Carlisle Moody for making the study's data available on his Web site.
[42] Robert F. Engle, Wald, Likelihood Ratio, and Lagrange Multiplier Tests in Econometrics, in Handbook of

Econometrics 776, 812 17 (Zvi Griliches & Michael D. Intriligator eds. 1983).
[43] I performed two variations of the Lagrange multiplier test with the same result: a weighted least squares regression and an unweighted probit regression of the dummy variable equation were used to obtain estimates of the residuals. This equation has the dummy variable representing the passage of the law as the dependent variable and two lags of homicide and 10 other exogenous regressors are independent variables. I used the percentage of the state population voting Republican in the most recent presidential election as the instrumental variable in this regression. This variable represents political pressure to "get tough" on crime and is often used as an instrument to predict the passage of a law at the state level. Lott & Mustard, *supra* note 39, at 14; Dezhbakhsh, Rubin, & Shepherd, *supra* note 38, at 16, 22. I perform the least-squares regression because probit regressions cannot control for the fixed effects among states that the authors assume are critical in all other regressions in the study. William H. Greene, Econometric Analysis 655 (1993).
[44] I perform a two-stage least squares regression with two variations of the first-stage prediction of the three-strikes variable: a weighted least squares regression and an unweighted probit regression of the equation. See note 42 *supra*. I again use the percentage of the state population voting Republican in the most recent presidential election as the instrumental variable in this regression.
[45] Greenwood *et al*., *supra* note 10, at 55.
[46] See note 10 *supra*.

[47] Zimring, Kamin, & Hawkins, *supra* note 3, at 75 76. Acknowledging that some deterrence is possible, even if only among repeat offenders, is a concession for two of the authors, who once believed that the fundamental strategy

on which repeat-offender laws are based to incapacitate repeat offenders for long periods of time automatically assumes that the criminal justice system can neither deter nor rehabilitate these offenders. Frank E.

Zimring, & Gordon Hawkins, Incapacitation 22 25 (1995).
[48] In 1993, 13.9 percent of felony arrests involved offenders that would have been eligible for a last-strike sentence. In 1994 and 1995, this proportion fell slightly to 12.8 percent.

[49] Zimring, Kamin, & Hawkins, *supra* note 3, at 1 2.
[50] See generally Isaac Ehrlich, Participation in Illegitimate Activities: A Theoretical and Empirical Investigation, 81 J. Pol. Econ. 521 (1973); Gary S. Becker, Crime and Punishment: An Economic Approach, 76 J. Pol. Econ. 169 (1968).
[51] This innovation resembles the approach used in the literature on options and investment under uncertainty. See generally Dixit & Pindyck, *supra* note 15; Chirinko, *supra* note 15, at 1,805.
[52] Other papers have produced related results using two-period models rather than this one-period model with foresight. However, the other models have not been entirely successful because often they produce counterintuitive results or require unrealistic assumptions. One paper obtained the result in a two-period model that some individuals will commit more crimes when punishment policy becomes more severe. Ariel Rubinstein, On an Anomaly of the Deterrent Effect of Punishment, 6 Econ. Letters 89 (1980). Another assumes that repeat offenders decide exactly how many crimes to commit ex ante and obtain the result that it is better to punish the first crime more severely than subsequent crimes. Moshe Burnovski & Zvi Safra, Deterrence Effects of Sequential Punishment Policies: Should Repeat Offenders Be More Severely Punished? 14 Int'l Rev. L. & Econ. 341 (1994). A third finds that sequential punishment increases first-period deterrence but decreases second-period deterrence. A. Mitchell Polinsky & Steven Shavell, On Offense History and the Theory of Deterrence, 18 Int'l Rev. L. & Econ. 305 (1998).

[53] We can either assume that $_j$ increases with the number of strikes or remains constant without affecting the implications of the model.
[54] Intuitively, this seems obvious. A baseball player who can make only three strikes chooses which pitches to swing at much more cautiously than a player who can make unlimited strikes.
[55] Auto theft is not a strikeable offense. In my data, the crime of larceny includes both grand and petty larceny. Only grand theft with a firearm is a strikeable offense. Because this offense represents a very small proportion of larcenies, we would not expect to see strong deterrence for the entire category. In addition, only burglaries of an occupied residence are considered to be strikeable offenses. Because these serious burglaries account for about 60 percent of burglaries (Greenwood *et al.*, *supra* note 10, at 57), the deterrence of the entire category of burglaries may be weaker than we would otherwise expect.
[56] Paula M. Ditton & Doris Wilson, Truth in Sentencing in State Prisons 7 (NCJ 170032, 1999).
[57] See Blumstein, *supra* note 7, at 415.
[58] Amy Jo Everhart, Predicting the Effect of Italy's Long-Awaited Rape Law Reform on "The Land of Machismo," 31 Vand. J. Transnat'l L. 671, 696 (1998).
[59] Kessler & Levitt, *supra* note 18, at 345.
[60] Ditton & Wilson, *supra* note 55, at 7.
[61] A small incapacitation effect may exist if convicted offenders who would otherwise not receive a prison sentence are sent to prison under the two-strike provision. An incapacitation effect could also exist if strike laws prompt more defendants to plead guilty to crimes with prison sentences in order to avoid harsher strike sentences. However, existing studies find guilty pleas actually decrease with the harsher sentences of three-strike laws (Schultz, *supra* note 25, at 575; Esparza, *supra* note 17, at 329), mandatory minimums (Steven Wisotsky, Exposing the War on Cocaine: The Futility and Destructiveness of Prohibition, 1983 Wis. L. Rev. 1305, 1389 (1983)), and "truth-in-sentencing" laws (Shepherd, *supra* note 1). A decrease in guilty pleas would actually decrease the incapacitation effect.
[62] Dezhbakhsh, Rubin, & Shepherd, *supra* note 38, at 9, 10.
[63] Ehrlich, *supra* note 49, at 545, the first to estimate the economic model of crime with data, chose to use the double-log form.
[64] The Breusch-Pagan test statistics ($n \times R^2$) with a Koenker correction indicate that the error terms in the unweighted regressions are indeed heteroskedastic. Roger Koenker, A Note on Studentizing a Test for Heteroskedasticity, 17 J. Econometrics, 107, 111 (1981). Tests indicates that the heteroskedasticity has been corrected after weighting by the

square root of the county population for all crimes except auto theft. See Tables 5 8 *infra*.
[65] Omitting this variable may underestimate the true effect of arrest rates on crime. Studies have found that the omitted-variable bias resulting from the exclusion of the probability of conviction may understate the true impacts of

the arrest rate on crime by 11 43 percent. David B. Mustard, Reexamining Criminal Behavior: The Importance of Omitted Variable Bias 16 (Working paper, Univ. Georgia, Dep't Econ. 2001).
[66] Eric D. Gould, David B. Mustard, & Bruce A. Weinberg, Crime Rates and Local Labor Market Opportunities in

the United States: 1979 1997, 84 Rev. Econ. & Stat. 22 23 (in press).
[67] Lott & Mustard, *supra* note 39; John R. Lott, Jr., & William M. Landes, Multiple Victim Public Shootings, Bombings, and Right-to-Carry Concealed Handgun Laws: Contrasting Private and Public Law Enforcement (John M. Olin L. & Econ. Working Paper No. 73, Univ. Chicago L. Sch. 2000); Mustard, *supra* note 64; Earl L. Grinols, David B. Mustard, & Cynthia H. Dilley, Casinos, Crime and Community Costs (Working paper, Univ. Illinois & Univ. Georgia 2000); Dezhbakhsh, Rubin, & Shepherd, *supra* note 38; Shepherd, *supra* note 1.
[68] To include a proxy of F_j, the severity of punishment, I also consider another variable, $MS_{c,n}$, the average sentence length for each crime. When this variable is included in equation (14), the results actually become slightly stronger in support of the deterrence hypothesis. However, $MS_{c,n}$ is not a perfect measure of sentence length for each crime. The data set includes data on the total sentence length imposed, not the sentence imposed for each separate crime for which the offender has been convicted. Because many offenders are convicted for several crimes, it is impossible to determine the sentence length for each crime. Because $MS_{c,n}$ is an imperfect measure, I will consider equation (14) without this variable in the body of this paper.
[69] See generally Becker, *supra* note 49; Ehrlich, *supra* note 49.
[70] According to the standard market model, the supply of crime depends on the efforts of police and prosecutors, the efforts of police and prosecutors depend on the level of police and judicial resources, and the level of police and judicial resources depend on the supply of crime. Isaac Ehrlich, Crime, Punishment, and the Market for Offenses, 10

J. Econ. Persp. 43, 49 51 (1996). The third equation, the police and judicial resources equation, represents society's demand for protection. However, many studies do not specify an endogenous resources equation (Isaac Ehrlich, The Deterrent Effect of Capital Punishment: A Question of Life and Death, 65 Am. Econ. Rev. 397 (1975); Isaac Ehrlich, Capital Punishment and Deterrence: Some Further Thoughts and Additional Evidence, 85 J. Pol. Econ. 741 (1977); Lott & Mustard, *supra* note 39; Dezhbakhsh, Rubin, & Shepherd, *supra* note 38; Shepherd, *supra* note 1) because tests indicate that police variables are exogenous. William N. Trumbull, Estimations of the Economic Model of Crime Using Aggregate and Individual Data, 56 S. Econ. J. 423, 428 (1989). My own tests for exogeneity confirm that the police and judicial resources are exogenous. The exogeneity may be the result of a lack of data and a misunderstanding of the actual allocation of criminal justice resources and the incentives of the bureaucrats who decide how to allocate the resources. Bruce L. Benson, Iljoong Kim, & David W. Rasmussen, Estimating Deterrence Effects: A Public Choice Prospective on the Economics of Crime Literature, 61 S. Econ. J. 161, 162 (1994). Because the police and judicial resources are exogenous to the system of equations but still affect the efforts of police and prosecutors, they enter into the production function equations.
[71] Evidence shows that violent crime rates and property crime rates are not related. In the last 20 years, violent crime rates have exhibited both substantial increases and decreases, while property crime rates have been steadily declining.

Bureau of Justice Statistics, National Crime Victimization Survey (1980 2000).
[72] Violent crimes and property crimes are often substitutes among criminals. Lott & Mustard, *supra* note 39, at 24; Shepherd, *supra* note 1.

[73] Cushman, *supra* note 23, at 106; Clark, Austin, & Henry, *supra* note 2, at 4 5.
[74] Feeley & Kamin, *supra* note 24, at 148.
[75] I have used the crime and arrest data and several other variables in a previous paper. Dezhbakhsh, Rubin, & Shepherd, *supra* note 38; Shepherd, *supra* note 1. I am grateful to John Lott and David Mustard for providing us with the data that they used in their paper. Lott & Mustard, *supra* note 39.
[76] Although the FBI Uniform Crime Report Data are the best county-level crime data currently available, there may be some problems associated with the estimation of missing data. U.S. Department of Justice, Federal Bureau of

Investigation, Uniform Crime Reports for the United States (1983 96). The manner in which the missing data are estimated changed in 1994, possibly leading to data that are not comparable with earlier years. However, the estimation problems hardly affect California's county-level data because California has consistently kept reliable data. In my 3 years of data after the change in estimation procedure (1994 96), over 97 percent of California counties reported 100 percent of the crimes committed and thus required no estimation of missing data. Over 99.4 percent of California counties reported at least 90 percent of crimes and thus required very little estimation. Similarly, over 78 percent of California counties reported 100 percent of arrests made, which required no estimation; over 98 percent of counties reported at least 90 percent of arrests, which required very little estimation. Moreover, dropping the counties that did not have 100 percent reporting from my estimation did not affect my results.

[77] U.S. Department of Justice, Bureau of Justice Statistics, National Corrections Reporting Program (1983 96).
[78] The MS variable is also found in this data set. See note 67 *supra*.
[79] Note that this variable is the same for all crimes in a given county for a given year. The data on the number of strike sentences imposed are not separated by crime categories. Although this is not a perfect measure of the probability of receiving a strike sentence for committing a particular crime, it is a good indication of how strict a county is in imposing strike sentences.
[80] California Department of Corrections, Offender Information Services Branch, Inmate Admissions Statistical Report (1994 99).
[81] U.S. Department of Justice, Bureau of Justice Statistics, Expenditure and Employment Data for the Criminal Justice System (1983 96).
[82] U.S. Department of Commerce, Bureau of Economic Analysis, Regional Economic Information System (1983 96).

[83] U.S. Department of Commerce, U.S. Bureau of the Census, Current Population Reports (1983 96).
[84] I elect to use the single-equation method of two-stage least squares because systems methods like three-stage least squares have significant problems. Greene, *supra* note 42, at 616. A specification error in any equation of the model will be propagated throughout the system when estimated by a systems method, leading to inconsistency when there is an incorrect restriction. The single-equation methods, on the other hand, confine the error to the particular equation in which it appears. Since I am interested primarily in the supply of offenses equation (14), systems methods seem too risky. Moreover, the finite-sample variation of the estimated covariance matrix is carried through the entire system by three-stage least squares, so the finite sample variance may actually be larger than that of two-stage least squares. In light of the weaknesses of the systems methods, two-stage least squares is the better choice of estimation.
[85] The test statistics (t-statistics of the lagged residuals) from the Gauss-Newton regression to test for autocorrelation indicate that the specifications with first-order autoregressive disturbance terms produce efficient estimations.

Russell Davidson & James G. MacKinnon, Estimation and Inference in Econometrics 357 58 (1993). See Tables 5 8 *infra*.

[86] *Id*. at 237 42.
[87] Jack Johnston & John E. DiNardo, Econometric Methods 256 (1997).
[88] The tests indicate that the strike sentence probability is exogenous for the majority of the crimes, as predicted by current evidence. Cushman, *supra* note 23, at 106; Clark, Austin, & Henry, *supra* note 2, at 4 5. For the other crimes, the probability is only borderline endogenous (endogenous at the 5 percent level but exogenous at the 10 percent level).
[89] The test statistics ($n \times R^2$) from the Gauss-Newton regression to test for overidentification indicate that the hypotheses of correct model specification and valid instruments cannot be rejected for all of the crimes except auto theft. Davidson & MacKinnon, *supra* note 84, at 235 36. See Tables 5 8 *infra*.
[90] The results of the other equations are available from the author on request.
[91] The variables I use to represent the probability of receiving a two- and three-strikes sentence are not exact

probability measures. I estimate the true probability by the ratio of two- or three-strikes sentences imposed to all imposed sentences. Nevertheless, this measure is most likely closer to what potential criminals view as the "correct" measure; there is evidence that offenders form perceptions based on what they observe happening to other offenders. Raaj K. Sah, Social Osmosis and Patterns of Crime, 99 J. Pol. Econ. 1272, 1273 (1991).

[92] See note 54 *supra*.

[93] It is impossible to distinguish between the nonstrikeable and strikeable burglaries and the nonstrikeable and strikeable larcenies. Nevertheless, the deterrence of the entire category of burglaries is most likely because the majority of burglaries are strikeable offenses. Similarly, the lack of deterrence of the larceny category is probably because the majority of larcenies are nonstrikeable offenses.

[94] However, the presence of positive coefficients for certain crimes may indicate that this variable is not a good measure of the probability of imprisonment given arrest.

[95] This computation is based on California's mean population, 31,530,511 (U.S. Census Bureau) and the California courts' average number of new commitments to state institutions, 47,672 (California Department of Justice) between 1994 and 1996.

[96] The 95 percent confidence interval for the number of aggravated assaults that each two-strikes sentence deters is

[0 8]; for robberies the confidence interval is [6 10]; and for burglaries it is [121 168]. Less than one murder is deterred.

[97] The 95 percent confidence interval for the number of aggravated assaults deterred between 1994 and 1996 is

[0 7,904]; for robberies the confidence interval is [5,574 9,290]; and for burglaries it is [259,787 360,696].

[98] The 95 percent confidence interval for the number of murders that each three-strikes sentence deters is [0 1];

for robberies the confidence interval is [9 28]; and for burglaries it is [212 348].

[99] The 95 percent confidence interval for the increase in larcenies resulting from each three-strikes sentence is

[35 201].

[100] The 95 percent confidence interval for the number of murders deterred between 1994 and 1996 is [0 8]; for

robberies the confidence interval is [1,620 5,040]; and for burglaries it is [57,028 93,612].

[101] The 95 percent confidence interval for the increase in the number of larcenies between 1994 and 1996 is

[5,250 30,150].

[102] Ted R. Miller, Mark A. Cohen, & Brian Wiersema, Victim Costs and Consequences: A New Look 9 (NCJ 155282, 1996).

[103] The 95 percent confidence interval for the savings in victims' costs by two-strikes laws is [$434,235,282 $817,799,260]. The 95 percent confidence interval for the savings in victims' costs by three-strikes laws is

[$98,694,412 $207,266,764].

[104] The 95 percent confidence interval for the increase in victims' costs caused by the increase in larcenies is

[$2,063,250 $11,848,950].

[105] Mark A. Cohen, Ted R. Miller, & Shelli B. Rossman, The Costs and Consequences of Violent Behavior in the United States, in Understanding and Preventing Violence 67, 144 (Albert Reiss, Jr., & Jeffrey Roth eds. 1994).

[106] The 95 percent confidence interval for the savings in nonvictims' costs by two-strikes laws is [$40,233,132 $124,106,292].

[107] The 95 percent confidence interval for the total savings in victims' and nonvictims' costs by two-strikes laws is

[$474,468,419 $941,905,552].

[108] The 95 percent confidence interval for the savings in nonvictims' costs by three-strikes laws is [$11,693,160 $37,449,104].

[109] The 95 percent confidence interval for the total savings in victims' and nonvictims' costs by three-strikes laws is

[$108,324,322 $232,866,918].

[110] The 95 percent confidence interval for the total savings in victims' and nonvictims' costs by two- and three-strikes

laws is [$582,792,741 $1,174,772,470].

[111] For conciseness, I report only the results for the second-strike data. The results are also robust for the three-strikes data.

[112] The results are weaker for the nonstrikeable offenses than for the strikeable offenses because there are two opposing effects that influence the nonstrikeable offenses. The strike laws' substitution effect will induce those not facing their last strike to substitute from strikeable crimes into nonstrikeable crimes. In contrast, the strike laws' deterrence effect will discourage those facing their last strike from committing all felonies, including nonstrikeable crimes. The story may be even more complicated for larceny. Early strike offenders should be deterred from committing some larcenies (grand larceny is a strikeable offense) but may substitute into other larcenies (petty larceny is not a strikeable offense). Nevertheless, it still appears that the net effect is a slight increase in the number of larcenies and auto thefts.

[113] I tested the significance of the variables added to the model in this section. The lags of the dependent variable and the linear time trend were insignificant in the majority of the crimes specified. Although some of the individual county-level time trends were significant, they were insignificant when tested as a group.

[114] I define neighboring counties as all counties that are directly adjacent to the county.

[115] For conciseness, I report only the estimation of the criminal migration model for the second-strike data; results are essentially identical for the third-strike data.

[116] Greenwood *et al.*, *supra* note 10, attempts to estimate the costs of the program by performing a simulation that is not based on any actual data. Indeed, they performed their study before the three-strikes program even began. Early projections of the impact of this legislation severely overestimated how many strike sentences would be imposed. See Clark, Austin, & Henry, *supra* note 2, at 4. This necessarily overestimates the costs of the legislation.