"Striking out" as crime reduction policy: The impact of "three strikes" laws on crime rates in U.S. Cities

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"STRIKING OUT" AS CRIME REDUCTION POLICY: THE IMPACT OF "THREE STRIKES" LAWS ON CRIME RATES IN U.S. CITIES

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Abstract: During the 1990s, in response to public dissatisfaction over what were perceived as ineffective crime reduction policies, 25 states and Congress passed three strikes laws, designed to deter criminal offenders by mandating significant sentence enhancements for those with prior convictions. Few large-scale evaluations of the impact of these laws on crime rates, however, have been conducted. Our study used a multiple time series design and UCR data from 188 cities with populations of 100,000 or more for the two decades from 1980 to 2000. We found, first, that three strikes laws are positively associated with homicide rates in cities in three strikes states and, second, that cities in three strikes states witnessed no significant reduction in crime rates. [PUBLICATION ABSTRACT]

Full text: Headnote

During the 1990s, in response to public dissatisfaction over what were perceived as ineffective crime reduction policies, 25 states and Congress passed three strikes laws, designed to deter criminal offenders by mandating significant sentence enhancements for those with prior convictions. Few large-scale evaluations of the impact of these laws on crime rates, however, have been conducted. Our study used a multiple time series design and UCR data from 188 cities with populations of 100,000 or more for the two decades from 1980 to 2000. We found, first, that three strikes laws are positively associated with homicide rates in cities in three strikes states and, second, that cities in three strikes states witnessed no significant reduction in crime rates.

Between 1993 and 1996, the federal government and 25 states passed what are popularly known as "three strikes and you're out" laws (Austin &Irwin, 2001). Intended to both deter and incapacitate recidivists, this legislation generally mandates significant sentence enhancements for offenders with prior convictions, including life sentences without parole for at least 25 years on conviction of a third violent felony or for some categories of offenders simply life without parole (Austin &Irwin, 2001; Clark, Austin, &Henry, 1997; Schichor &Sechrest, 1996).1

Proponents of the statutes based their support on published results of career-criminal research (Shannon, McKim, Curry, &Haffner, 1988; West &Farrington, 1977; Wolfgang, Figlio, &Sellin, 1972) and argued that the statutes would deter and incapacitate high-rate recidivist offenders and thus result in lower crime rates. First, under the sentencing schemes, "high-level" offenders (measured both by the type and the number of prior convictions) would be specifically targeted for incarceration (Stolzenberg &D'Alessio, 1997; Walker, 2001; Zimring, 2001). Second, the statutes would significantly reduce judicial sentencing discretion, thereby increasing the certainty of punishment while enhancing the term of imprisonment and thus increasing the severity of the sanction. Finally, states would rely more heavily on prisons for repeat offenders than they had in the past (Dilulio, 1994, 1995, 1997; Jones, 1995; Scheidigger &Rushford, 1999; Wilson &Herrnstein, 1985; Wilson, 1975; Wyman &Schmidt, 1995). Proponents argued that by enhancing recidivists' sentences, ensuring they actually serve enhanced terms, and reducing the chance for early parole release, the statutes would reduce judicial discretion, limit the opportunity for parole boards to release "dangerous" offenders back into the community, and reduce crime levels because offenders would be deterred, incapacitated, or both.

Although these laws have now been in effect for nearly a decade and California's has been evaluated several times (e.g., Greenwood, Rydell, Abrahamse, Caulkins, Chiesa, Model, et al., 1994; Stolzenberg &D'Alessio,

1997; Zimring, Hawkins, &Kamin, 2001), only two larger-scale evaluations have been published (Kovandzic, Sloan, &Vieraitis, 2002; Marvell &Moody, 2001), and they focused mainly on homicide. Thus, while much has been learned about how three strikes laws may work in California or about their impact on one serious crime, no large-scale comprehensive analysis has been published.

This study extends the work of Kovandzic et al. (2002) and Marvell and Moody (2001) by evaluating whether three strikes laws do in fact reduce most forms of serious personal (murder, rape, robbery, and aggravated assault) and property (burglary and motor vehicle theft) crime. Specifically, we examined the potential deterrent and incapacitative effects of the laws on serious crime rates using panel data collected for 188 U.S. cities with populations of 100,000 or more for the period 1980 to 2000. Our evaluation extends previous research in several ways. First, we include numerous control variables in the statistical models to mitigate the problem of omitted variable bias. Second, to examine the potential incapacitative effects of the laws, which would be unlikely to appear until years after the laws had been passed, we use a longer post-intervention period in our models. Finally, we attempt to address, though admittedly with limited success, the issue of simultaneity (i.e., rising crime rates may affect the passage and application of three strikes laws) in our crime rate models. If simultaneity is not adequately addressed, potential crime-reducing effects of the laws might be negated by the positive effects of crime on the passage and application of the laws.

In the sections that follow, we provide an overview of three strikes laws and review published analyses of the impact of the laws on crime. Then we present our methods and data analytic plan. Finally, we present results of our analysis and conclude by discussing our results and their implications for sentencing policy in the United States.

Three Strikes Laws

In 1993, Washington became the first three strikes state when it passed an initiative mandating life terms of imprisonment without possibility for parole for individuals convicted a third time for specified violent offenses. California quickly became the second, passing its welLjpublicized law in 1994. By 1996, 23 other states and the federal government had enacted similar statutes.

Analyses of the content of these laws by Turner, Sundt, Applegate, and Cullen (1995) and Austin and Irwin (2001) reveal several recurring themes. First, almost all the states include serious violent offenses (e.g., murder, rape, robbery, and serious assault) as strikeable. Other states include drug-related crimes (Indiana, Louisiana, California); burglary (California); firearm violations (California); escape (Florida); treason (Washington); and embezzlement and bribery (South Carolina). Second, there is variation in the number of strikes needed for an offender to be out. In eight states, two strikes bring a significant sentence enhancement. Third, states differ in the term of incarceration imposed on offenders who strike out. Eleven impose mandatory life terms of imprisonment without parole, and three allow for parole but only after a specified lengthy term of incarceration (25 years in California, 30 years in New Mexico, and 40 years in Colorado). Additionally, five (Alaska, Arizona, Connecticut, Kansas, and Nevada) call for sentence enhancements, but leave the specifics to the discretion of the court. Finally, six (Alaska, Florida, North Dakota, Pennsylvania, Utah, and Vermont) provide for a range of sentences for repeat offenders that may include life in prison if the final strikeable offense involves serious violence.

Dickey and Hollenhorst's in-depth assessment (1998) reveals-despite claims by policy makers and prosecutors that the laws were an essential crime fighting tool-that most states have not applied three strikes legislation extensively. For example, by mid-year 1998, 17 states had between 0 and 38 offenders sentenced under three strike provisions (Alaska, Arizona, Colorado, Connecticut, Indiana, Maryland, Montana, New Jersey, New Mexico, North Carolina, Pennsylvania, South Carolina, Tennessee, Utah, Vermont, Virginia, Wisconsin). Only three (Florida, Nevada, Washington) had slightly more than 100 offenders serving three strike sentences. The only two states that have applied the legislation with any consistency are California and Georgia. As of mid-year 1998, Georgia had sentenced almost 2,000 offenders under one and two strike provisions, and California more than 40,000 under two and three strike provisions.

Effects of Three Strikes Laws on Crime2

Despite the popularity of the laws and the decade they have been in effect, few published studies have explicitly evaluated their impact on crime. Those that have can be separated into those whose focus was California and those whose focus was national. Additionally, some of the studies focused only onlhe laws' impact on certain crimes (e.g., homicide), while others examined the laws' impact on a larger set of offenses (e.g., serious property crime).3

Evaluating California's Three Strikes Law

The first study to examine the potential incapacitative impact of California's three strikes laws was a projection analysis conducted by Greenwood et al. in 1994. Specifically, the authors used a mathematical model that tracked the flow of criminals through the justice system, calculated the costs of running the system, and predicted the number of crimes criminals commit when on the street. The results of the simulation analysis suggested that a fully implemented law would reduce serious crimes (mostly assaults and burglaries) in the state by 28% per year at an average annual cost of $5.5 billion.4 The authors assumed no deterrent effect of the laws on crime, claiming this assumption was consistent with prior deterrence research.

Using ARIMA time-series analysis with monthly data, Stolzenberg and D'Alessio (1997) examined the impact of California's three strikes law on FBI index offenses in the 10 largest cities in the state from 1985 to 1995. Trends in the pettytheft rate were used as a control group to mitigate possible threats to internal validity. Three different intervention points that signify the effects of the law were considered and the authors opted to use the abrupt permanent change model (i.e., the date the law went into effect, March 1994) because it provided the best fit to the data. They reported that, with the possible exception of Anaheim, the law had little impact on either index crimes or petty theft. They presented three possible explanations: (1) existing sentencing schemes already confined substantial numbers of high-risk offenders in prison, resulting in a diminishing marginal return from increased levels of incarceration; (2) by the time many offenders are confined for their third strike, their criminal careers are already on the downturn; and (3) there is little evidence that juveniles, despite their accounting for a disproportionate amount of crime in California, were affected by the law.5

Males and Macallair (1999) tested the hypothesis that California counties that enforced the law more frequently would see greater reductions in crime and that age group populations (in this case the over-30) most targeted by the law would show greater decreases in crime patterns. To examine this question, Males and Macallair (1999) collected county FBI index offense arrest statistics for the state's 12 largest counties, disaggregated by age, 3 years after the law took effect (1995-1997) and compared those data with 3 years' worth of prior data (1991-1993) in the same counties. They found that county crime data for post-law years failed to support the presumed crime reduction promised by the law, either through selective incapacitation or deterrence. Counties that invoked the law at higher rates did not experience the greatest decrease in crime. In fact, Santa Clara, one of six counties most frequently implementing the law, witnessed an increase in violent crime. Males and Macallair (1999) also failed to find age-related incapacitative effects, regardless of how often the law was invoked. Their study thus suggested that California counties that vigorously and strictly enforced the state's three strikes law did not experience a decline in any crime category compared to counties that applied it less frequently.

Zimring et al. (2001) used various data to examine the potential deterrent and incapacitative effects of the law. They found that "the odds of imprisonment for second and third strike defendants went up only modestly" and that there was "no credible case to be made for dramatic qualitative improvements in the rate of imprisonment from the advent of three strikes in 1994 and 1995" (p. 94). They also argued that lower crime rates found statewide in 1994-1995 were evenly spread among both target (second and third strike offenders) and nontargeted populations (first strike offenders). Overall, they concluded that short-term felony crime reduction in the state as a result of the three strikes law was between 0% and 2%.7

Shepherd (2002) used time-series cross-section data for 58 California counties for the 1983-1996 period to measure the full deterrent effect on crime rates. She suggested that prior studies (Zimring et al., 1999; Greenwood et al., 1994) underestimated the effect because they focused only on repeat offenders. If strike sentences deter only repeat offenders facing their last strike, she hypothesized, then the laws should deter both strikeable and nonstrikeable felonies. On the other hand, if the law deters all potential criminals, then one might expect strike sentences to reduce only strikeable felonies as prospective criminals, fearing initial strikes, avoid committing crimes that qualify as strikes. To examine this possibility, Shepherd regressed county-level crime rates on the number of offenders receiving a two or three strike sentence divided by the total number of those receiving any sentence and used numerous demographic, economic, and deterrence control variables to mitigate omitted variable bias. The findings supported the theory of full deterrence because only strikeable felonies were reduced by the probability of two and three strike sentences. Specifically, Shepherd estimates that strike sentences led to 8 fewer homicides, 12,350 fewer robberies, 5,222 fewer aggravated assaults, 7 fewer rapes, and 144,213 fewer burglaries during the first 2 years. With the exception of Shepherd, then, studies on the impact of three strikes laws in California did not support their efficacy.

National Studies

Two published studies, Marvell and Moody (2001) and Kovandzic et al. (2002), examined the impact of three strikes laws on state crime rates and city homicide rates, respectively. Marvell and Moody (2001) used state panel data for 1970 to 1998 to examine changes in crime rates in three strikes states compared to non-three strikes states. They reported that in states with the laws, homicides increased by 10% to 12% in the short term, and 23% to 29% in the long term. They suggested that offenders facing the possibility of life in prison for a third strike may be more likely to kill witnesses at the crime scene in an effort to avoid detection. Marvell and Moody also found that three strikes laws did not reduce rates of rape, robbery, assault, burglary, larceny, or auto theft. Kovandzic et al. (2002) found similar results for homicide using panel data from 188 cities for the 1980-1999 period. Results indicated that, compared with cities in states without the laws, cities in states with three strikes laws experienced a 13% to 14% increase in homicide rates in the short term and a 16% to 24% increase in the long term.

In summary, published studies of the impact of three strikes laws on crime have generally concluded that the laws either have minimal impact on crime or may "backfire" and cause an increase in homicide. The latter situation may, as Kovandzic et al. (2002) concluded, illustrate the "law of unintended consequences" in action. Not only does the policy choice not reduce the extent or seriousness of the problem targeted, but actually intensifies it.

To what extent has there been a long-term backfire effect of three strikes laws on serious crime? Have cities in states with these laws experienced significant declines or increases in serious crime over time? In the analyses below, we address these and related issues. We first turn to a discussion of the methods and data analytic plan used in the current study.

DATA AND METHODS

This study estimated the overall and state-specific effects of three strikes laws on UCR index crimes using a multiple timeseries design (MTS), with city-level time-series cross-section data for the years 1980 through 2000 for all 188 U.S. cities with a population of 100,000 or more in 1990 and for which relevant UCR data were available. Of the 188 cities with populations of 100,000 or more in 1990, 110 were in states that passed three strikes laws between 1993 and 1996.

MTS is considered one of the strongest quasi-experimental research designs for assessing the impact of criminal justice policy when more thorough experimental control is not possible or practical, as is the case here (Campbell &Stanley, 1963, pp. 55-57).8 Its main advantage is that it allows the researcher to treat the passage of three strikes laws as a "natural experiment," with the 110 cities residing in three strikes states as "treatment cities" and the 78 no-change cities as "controls." Specifically, we compared observed changes in crime rates in the treatment cities (before and after three strike laws) to observed changes in crime rates in the control cities. If three strikes laws reduced crime through deterrence and incapacitation then the treatment cities should experience an immediate drop in crime greater than the control cities at the time the laws were adopted, with an additional reduction spread out over time as offenders began serving the additional portion of their prison terms due to the three strikes sentence enhancement.

Additional advantages of the MTS design include, first, the ability to enter proxy variables for omitted variables that cause crime rates to vary across years and cities (the proxy variables, which number nearly 400 here, are discussed further below); second, a larger sample size (n= 3,320 or more), permitting us to include numerous controls in the crime rate models for factors that might be correlated with other explanatory variables and therefore lead to spurious associations among these variables (Wooldridge, 2000, p. 434); and, third, greater statistical power (due to the large sample size) and with it the ability to detect more modest effects of three strikes laws on crime rates (see Wooldridge, 2000, p. 409).

The city was chosen as the unit of analysis because it is the smallest and most internally homogeneous unit for which UCR crime data for a large national sample of geographical areas were available. Analyses using states or regions are more susceptible to aggregation bias because they are too heterogeneous and necessarily ignore important within-state variation in crime rates and variables affecting those rates. For example, a state could have one jurisdiction with relatively low crime rates where three strikes sentence enhancements are applied quite frequently, and other areas with much higher crime rates and little or no application of three strikes sentence enhancements, consistent with the idea that three strikes sentence enhancements reduce crime.9 However, when the areas are aggregated to the state level, the high-crime areas could dominate the crime measure so much that the state would show a higher-than-average crime rate despite a causal effect of three strikes laws on crime rates operating at lower levels of aggregation.

One drawback of using city data, however, is that disturbance terms for cities within the same cluster (i.e., state) might be serially correlated during a particular year because of some undefined similarity. In such a situation, standard errors are likely to be underestimated, thus inflating t-ratios for the three strikes law variables (Greenwald, 1983; Moulton, 1990). To avoid this problem, we used a Huber-White correction for standard errors (available in SAS 8.0), that accounts for the tendency of withincluster error terms to be correlated.

Econometric Methods for Time-Series Cross-Section Data

Following convention for time-series cross-section data, our basic model is the fixed-effects model, which entails a dichotomous dummy variable for each city and year, except the first year and city, to avoid perfect collinearity (Hsiao, 1986, pp. 41-58; Pindyck &Rubinfeld, 1991, pp. 224-226). The year and city dummies are an integral part of the approach because they partially control for omitted or difficult-to-measure variables not entered in the crime rate equations. Specifically, the city dummies control for unobserved factors that remained approximately stable over the study period and that caused crime rates to differ across cities. Examples include demographic characteristics, economic deprivation, criminal gun ownership, and deeply embedded cultural and social norms. The city dummies also control for measurement errors in UCR crimes due to reporting differences across cities.

The year dummies control for national events that could raise or lower crime rates in a given year across the entire country. For example, the 1994 Crime Control and Law Enforcement Actwhich contained several major crime-reduction programs including truth-in-sentencing, the federal version of a three strikes law, funds for 100,000 new police officers, expansion of the death penalty, a ban on possession of guns by juveniles, and enhanced penalties for drug offenses and using firearms in crimes-could have affected crime rates throughout the country. Another example is the emergence and proliferation of crack cocaine in the mid-1980s, which many scholars have suggested was indirectly responsible for dramatic increases in violent crime, especially homicide and robbery, in most American cities during the late 1980s and early 1990s (Blumstein, 1995). Because the analysis includes fixed-effects for both years and cities, the coefficient estimates for the three strikes law variables and specific control variables (discussed below) are based solely on within-city changes over time. Finally, we followed Ayres and Donahue's (2003) and Marvell and Moody's (1996, 2001) recommendation of including linear-specific time-trend variables for each city. Each of the time-trend variables is coded zero for all observations except in a particular city, where it is a simple counter. The trend variables control for trends in a city that depart from national trends captured by the year dummies. They are important because without them the coefficient on the three strikes law variables would simply measure whether crime rates are higher or lower for the years after the law (relative to national trends captured by the year dummies), even if the increase occurred before or well after the law went into effect. The city-specific trend variables, however, do not control for trends that are erratic (e.g., drug market and gang activity) or that depart from nationwide trends.

Three Strikes Laws

The laws and their effective dates were obtained from Marvell and Moody (2001), and verified by checking relevant secondary sources (Dickey &Hollenhorst, 1998; Clark, Austin &Henry, 1997; Turner et al., 1995). Because three strikes laws are designed to both deter and incapacitate highly active criminals, and because both of these effects are unlikely to manifest themselves at similar time points, we could not measure and evaluate the effects of the laws using a single variable. Instead, we created two separate variables to account for both causal processes.

To capture any deterrent effects, we used a post-passage dummy variable scored "1" starting the full first year after a law went into effect and "0" otherwise. In the year a law went into effect, the variable is the portion of the year remaining after the effective date. The post-passage dummy variable allowed us to test for a once-and-for-all deterrent effect as prospective strike offenders learned about the stiffer penalties provided by the laws, most likely through "announcement effects" surrounding passage of the laws. If three strikes law supporters are correct that passage of these laws reduces crime by deterrence, one would expect to see a sudden and persistent drop in crime captured by the post-passage dummy in the city panel regression. Because the dependent variables in the panel regressions are the natural logs of the crime rates, the coefficient on the post-passage dummy can be interpreted as the percent change in crime associated with adoption of the law-that is, the law will raise or lower crime, by (for example) 5%. Because it is possible the laws had a greater deterrent effect in later years as prospective strike offenders learned about the laws through application to other offenders, we also estimated crime models with the post-passage dummy variable lagged one year. Although the results are not shown, lagging the post-passage dummy variable one year has virtually no impact on the results. That variable might, however, reflect mild incapacitation effects of three strikes laws, because some offenders would not have received prison sentences prior to the passage of the laws. For example, California's two and three strike laws mandate that offenders convicted of any second (for the two strike law) or third felony be sentenced under the law's provisions. Because the majority of offenders sentenced under the laws have been convicted of nonviolent crimes such as burglary, drug possession, and weapons possession (Zimring et al., 2001), it is conceivable that some of the less serious offenders would have escaped receiving prison terms in the absence of the laws (Marvell &Moody, 2002). The laws may also have an immediate incapacitative impact by leading potential strike defendants to plead to greater crimes than they would have prior to passage of the law (Marvell &Moody, 2001).

While it is therefore conceivable for incapacitative effects to begin immediately, one would not expect them to reach full longterm impact until a substantial portion of strike defendants begin serving the extended portions of their prison terms due specifically to the three strikes sentence enhancement. Because most convicted felony offenders with serious prior criminal records would probably have received lengthy prison terms prior to the three strikes laws, these effects would not occur until many years after the laws are passed (Clark et al., 1997; King &Mauer, 2001; Kovandzic, 2001; Marvell &Moody, 2001). That most strike defendants would have received prison terms even in the absence of the laws may explain why Marvell and Moody (2001) and others have found no immediate impact on state prison populations. Providing additional support for the claim that most strike defendants would have received prison terms before the laws, Kovandzic (2001) found that roughly 80% of those sentenced under Florida's 1988 habitual offender law would have received mandatory prison terms even if they had been sentenced under the state's sentencing guidelines. Another 17% fell in a discretionary range and could have received prison terms. Perhaps more noteworthy is Kovandzic's (2001) finding that of the habitual offenders who would have been subject to mandatory prison terms in the absence of the habitual offender law, 75.2% would have received prison terms of 3 years or more, 61% terms of 5 years or more, and 18% terms of 10 years or more.

Because it is impossible to know exactly when strike defendants would have otherwise been released from prison had they not been sentenced under three strikes provisions, we followed Marvell and Moody's (2001) approach of using a post-passage linear trend variable indicating the number of years since enactment of three strikes legislation. For example, consider a city in California, which passed its law in 1994. In this case, in 1995 the time trend variable is equal to one, in 1996 it is equal to two, in 1997 it is equal to three and so on, until the year 2000 where the time trend variable is equal to six. The post-passage linear trend variable assumes that each year an increasing number of strike defendants are serving that portion of their prison term due specifically to the three strikes provision, such that a time trend emerges after adoption reflecting a dampening effect on crime that grows progressively stronger over time (at least until the increase in the number of defendants serving extended prison terms under the three strikes laws came to an end). If the estimated coefficient on the post-passage trend variable were virtually zero, one would conclude that three strikes laws have no incapacitative impact on crime rates.

Crime Rates

The dependent variables are the rates of homicide, robbery, assault, rape, burglary, larceny, and motor vehicle theft, per population of 100,000. The crime data were taken from the FBI's Uniform Crime Reports (1981-2001), which reports crime counts for a city only if the individual law enforcement agency responsible for that jurisdiction submits 12 complete monthly reports. Despite having a population greater than 100,000 in 1990, we dropped seven cities due to missing data problems: Moreno Valley, CA, Rancho Cucamonga, CA, Santa Clarita, CA, Overland Park, KS, Kansas City, KS, Cedar Rapids, IA, and Lowell, MA.

Specific Control Variables

In addition to the year dummies, city dummies, and city-trend variables, we included eight specific control variables that prior macro-level research has suggested are important correlates of crime (see Kovandzic et al., 1998; Land, McCall, &Cohen, 1990; Sampson, 1986; Vieraitis, 2000). Most account for causal processes emphasized by strain/deprivation, social disorganization, and opportunity/routine activity theories. Failure to control for these factors could suppress (i.e., mask any negative impact of three strikes laws on crime) or lead to spurious results if they are correlated with the passage of three strike laws and with crime rates.

The specific control variables in the crime rate models included percent of the population that was African American, percent that was Hispanic, percent aged 18-24 and 25-44, percent of households headed by females, percent of persons living below the poverty line, percent of the population living alone, per capita income, and incarceration rate. These data for 1980 and 1990 were obtained from U.S. Bureau of the Census (U.S. Bureau of the Census, 1983, 1994). Year 2000 data were obtained from the U.S. Census Bureau website using American Fact Finder (http://factfinder.census.gov). Because these measures were available only for decennial census years, we used linear interpolation estimates between decennial census years. Given the small changes in these variables, a linear trend was assumed and considered justified. Income data for 1980-2000 were obtained from the U.S. Department of Commerce's Bureau of Economic Analysis website (http://www.bea.doc.gov). Income data were county-level estimates that we used as imperfect substitutes for city-level income. Personal income data were converted from a current dollar estimate to a constant-dollar 1967 basis by dividing per capita income by the consumer price index (CPI). Prison population was the number of inmates sentenced to state institutions for more than a year divided by state population, available annually at the state level; these values were used as proxies for city-level imprisonment. State prison population data were obtained from the Bureau of Justice Statistics website (http://www.ojp.usdoj.gov/bjs). Because the prison population data were year-end estimates we took the average of the current year and prior years to estimate mid-year prison population.

Data Transformations and Regression Assumptions

All continuous variables were expressed as natural logs to reduce the impact of outliers and divided by population figures to avoid having large cities dominate the results. This procedure allowed coefficients for the continuous variables to be interpreted as elasticities-the percent change in the crime rate expected from a 1% change in the independent variable (see Greene, 1993). With respect to the dichotomous and post-passage linear trend variables, exponentiating the variables and subtracting the result from 100 produced the useful interpretation of the percent change in the crime rate associated with the passage of a three strikes law and the percent change in the crime rate for each additional year the law is in effect, respectively (see Wooldridge, 2000). Heteroscedasticity was detected using the Breusch-Pagan test, mainly because variation in crime rates was greater over time in the smaller cities than in the larger ones. To avoid inefficient and biased estimated variances for the parameter estimates, we weighted the crime models by functions of city population as determined by the test (Breusch &Pagan, 1979). Results of panel-unit-root-tests (Levin &Lin, 1992; Wu, 1996) indicated that the crime rate series were stationary, i.e., the unit root hypothesis was rejected in all instances. That the crime rate variables had a constant mean suggested that the analysis be conducted in levels and not differenced rates. In any event, we reestimated the crime rate models using differenced rates and the parameter estimates for the three strikes law variables were similar to those in Table 1. Autocorrelation was mitigated by including lagged dependent variables (Hendry, 1995); lagged dependent variables also have the added benefit of controlling for omitted lagged effects (Moody, 2001). The results for the three strike variables were essentially the same without them. Examination of collinearity diagnostics developed by Belsley, Kuh, and Welsh (1980) revealed no serious collinearity problems for the three strike variables. While there were collinearity problems among the proxy variables, this did not impact the results for the three strikes variables, and we measured only the significance of proxy variables in groups using the F test.

RESULTS

Crime Trends Before and After Implementing Three Strikes Laws

Before proceeding to results of the more sophisticated econometric analysis, we began our empirical investigation of the effect of three strikes laws on crime by graphing the pattern of index crime rates (per 100,000 population) over time in three groups of cities: those in states that adopted a three strikes law in 1994, those in states that adopted the laws in 1995, and those in states that never adopted them.11 As discussed, if passage of a three strikes law reduces crime primarily through deterrence, presumably through announcement effects, one would expect cities in states with the laws to experience a more sudden and persistent drop in crime than that in cities in states without the laws. On the other hand, if three strike laws reduce crime mainly through incapacitation, one might expect cities in states with the law to experience a more gradual and continuing decrease in crime than that in cities in states without the laws as offenders in three strikes cities begin serving the extended portion of their prison terms due to sentence enhancement.

Analysis reveals a number of interesting findings (see Figure 1). First, crime rates in all three city groupings moved roughly in tandem over the past 20 years: crime rates declined in the early 1980s, began rising in the mid-1980s, and then declined markedly through the 1990s. This pattern indicates broad forces that tended to push crime rates up and down nationwide. second, despite all three city groupings having experienced a sizeable drop in crime throughout the 1990s, crime rates in three strikes cities declined slightly faster. Because the drop in crime grows gradually over time rather than abruptly, it appears that incapacitation is the main force behind three strikes laws. As a result, if one were forced to make causal attributions based on Figure 1, one might conclude that three strikes laws tend to reduce crime rates through incapacitation.

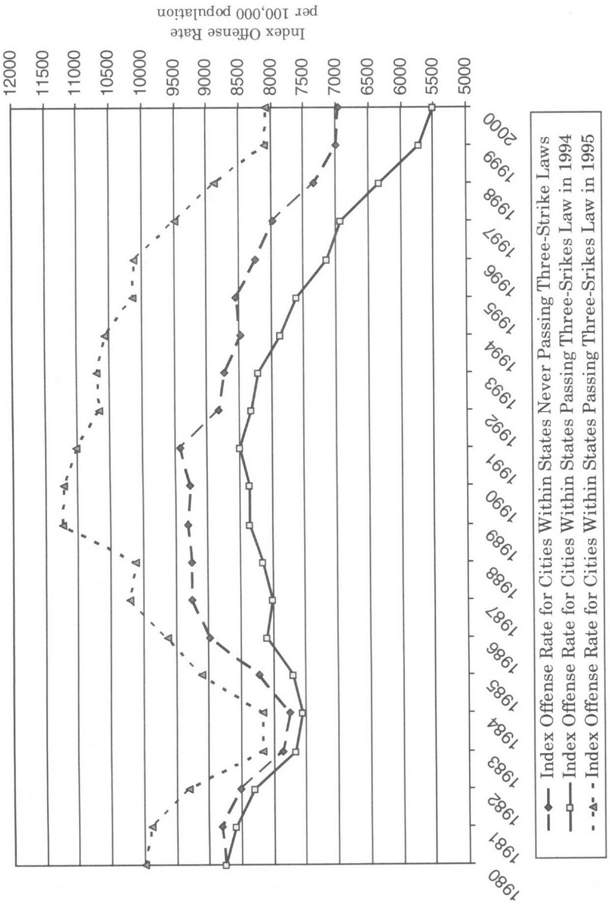

Figure 1: UCR Index Offense Rates for Cities With 100,000+ Population (1980–2001)

Of course, one cannot place much confidence in such a conclusion because the evidence in Figure 1 assumes that the only unique factor working to influence crime in three strikes cities is the three strikes law. As discussed, prior macro-level crime theory and research have identified numerous correlates of crime. If any of these factors were correlated with both the laws and with lower crime, then the apparent causal relationship between the laws and crime observed in Figure 1 would be spurious. For example, states that enacted three strikes laws may have also relied more heavily on incarceration as part of a larger effort to "get tough on crime," such that their prison populations grew faster than in other states. If this was the case, then the apparent incapacitative effects noted in Figure 1 might really be due to an overall increase in prison populations, for which the graph does not control. Because crime rates in all three city groupings began declining well before the passage of most three strikes laws in 1994 and 1995 this seems like a logical possibility. We therefore now turn to regression analysis to examine the deterrent and incapacitative impact of three strike laws on crime while controlling for numerous potential confounding factors.

Estimating the Impact of Three Strikes Laws

Estimates of the aggregate impact of three strikes laws on city crime rates using the described regression procedures are presented in Table 1. The major features include using aggregate post-passage dummy and post-passage trend variables, logarithmically transformed rates for all continuous variables, city dummies, year dummies, and city-trend dummies. The use of the aggregate law variables implicitly assumes the laws have a uniform impact on crime, which turns out not to be the case given the large number of negative and positive coefficients found for the disaggregated law variables (see state-specific analysis below). The results in Table 1 do not support what was shown in Figure 1, that three strikes laws were associated with slightly lower crime rates, most likely due to incapacitation. Although six of the seven postpassage trend variables are, as expected, negative and therefore consistent with the hypothesis that three strike laws reduce crime through incapacitation, the coefficients are small and not close to statistically significant, even at the generous .10 level. Given the large number of degrees of freedom (D.F. = 3,320 or more in each model), even modest incapacitative effects should have produced significant negative coefficients for the post-passage trend variable.

Table 1. Deterrent and Incapacitative Effects of Three Strikes Laws on City UCR Index Crime Rates

Three Strikes Law Variables:	Dependent Variables (UCR index crime rates per 100,000 resident population, in natural logs)													
	Homicide		Rape		Robbery		Aggravated Assault		Burglary		Larceny		Auto-Theft	
	Coef.	t	Coef.	t	Coef.	t	Coef.	t	Coef.	t	Coef.	t	Coef.	t
Post-passage Dummy	.12	2.32	.03	1.16	.01	.55	.03	1.29	.00	.18	-.00	-.10	-.02	-.69
Post-passage Trend	-.01	-.67	.01	.82	-.02	-1.52	-.01	-.68	-.01	-.82	-.00	-1.12	-.01	-.67
Control Variables:														
Pct. ages 18 to 24	1.54	4.03	-.03	-.10	.58	2.62	-.27	-1.19	.18	1.12	.32	2.40	.33	1.00
Pct. ages 25 to 44	-.87	-.60	.86	1.61	-.04	-.07	-.29	-.61	-.13	-.45	-.41	-1.17	-.09	-.16
Poverty Rate	-.06	-.18	.28	1.67	.08	.47	-.20	-1.13	-.13	-1.20	.01	.14	.08	.43
Per Capita Income	.80	2.34	.42	2.88	.20	1.21	-.03	-.21	-.05	-.37	-.01	-.07	.30	2.09
Pct. Black	.30	1.17	.15	.57	.25	1.92	.08	.86	.22	3.02	.21	4.18	.32	3.56
Pct. Hispanic	.06	.70	-.13	-2.30	.05	.90	-.01	-.23	.12	3.10	.17	3.66	.13	1.91
Pct. Female Hslds.	.33	2.99	.03	.35	-.01	-.14	.04	.54	-.03	-.44	.03	1.03	-.04	-.43
Pct. Living Alone	-.94	-1.58	.35	.59	-.60	-2.45	-.03	-.11	-.28	-1.43	.09	.42	-.67	-1.60
Prison Population	-.30	-3.80	-.06	-1.11	-.21	-3.63	.04	.81	-.21	-3.77	-.12	-3.10	-.15	-2.15
Crime, 1 year lag	.07	1.92	.38	6.23	.55	20.94	.55	14.25	.55	8.40	.44	4.86	.60	9.26
Sample Size	3,845		3,755		3,845		3,845		3,804		3,804		3,801	
Degrees of Freedom	3,439		3,357		3,439		3,439		3,397		3,397		3,394	
Adjusted R-square	.90		.90		.97		.97		.94		.92		.94	

Notes: The dependent variable is the natural log of the crime rate listed at the top of each column. The data set is comprised of annual city-level observations. While not shown, city, year, and city trend effects are included in all specifications. All regressions are weighted by a function of city population as determined by the Breusch Pagan test. Standard errors are corrected for clustering by state. Coefficients that are significant at the .10 level are italicized. Coefficients that are significant at the .05 level are italicized and underlined.

The most likely reason for the disparate results between Figure 1 and Table 1 is prison population, which is associated with statistically significant lower rates in five of the seven crime categories. To examine this possibility further, we re-ran the crime regressions from Table 1 without the prison population variable. The results confirmed our initial suspicions that prison population growth was largely responsible for the small incapacitation effects observed in Figure 1. Although not shown, the coefficients for the post-passage trend variables were negative and statistically significant for robbery and larceny (the bulk of the index crime rate) and marginally significant for homicide, burglary, and auto theft. These findings support the supposition that states adopting three strikes laws were the same ones relying more heavily on incarceration as a crime-control strategy during the "imprisonment binge" of the 1980s and 1990s. The finding that prison population growth reduces crime is consistent with a sizable body of research showing that incarcerating criminals reduces crime, especially homicide (Devine, Sheley, &Smith, 1988; Kovandzic et al., 2002; Levitt, 1996; Marvell &Moody, 1994, 1997, 1998, 2001).

There is also no evidence that three strikes laws reduce crime through deterrence. The coefficients for the post-passage dummy are about evenly divided by sign and are far from significant, except for homicide, whose coefficient is positive and significant. These particular results suggest that homicide rates in cities increase, on average, by 10.4% after a three strikes law is adopted. This finding is consistent with results reported by Kovandzic et al. (2002) and Marvell and Moody (2001). The most likely explanation is that a few criminals, facing lengthy prison terms on conviction for a third strike, may attempt to avoid such penalties by killing victims, witnesses, or police officers to reduce their chances of apprehension and conviction.

Robustness Checks

Additional analyses (not reported in Table 1) indicated that the nonsignificant effects of three strikes laws on crime rates appear to be fairly robust under varying model specifications. That is, the results for both variables were fairly consistent under alternative analyses with other possible model specifications and regression procedures. These included entering the three strikes law variables in the crime regressions separately rather than simultaneously, using differenced rates, dropping the city-trend variables, not logging the continuous variables, not weighting the crime regressions, dropping the lagged dependent variables, and using conventional standard errors. The major exception occurs for homicide, where the coefficient on the post-passage dummy variable in the homicide regression is no longer significant when using differenced rates and is highly significant when using conventional standard errors.

Other Notable Findings

Although not the focus of this study, results for some of the control variables should be noted (Table 1). First, increases in the percentage of a city's population that is African American or Hispanic appear positively associated with property crime rates but has little impact on rates of violence. second, prison population growth is negatively associated with crime rates, though the coefficients are somewhat smaller than those found in other state and national studies (Marvell &Moody, 1994, 1997; Levitt, 1996). As expected, increases in the number of persons in a city between the ages 18 to 24 are positively related to rates of homicide, robbery, and larceny. Our results contradict recent works by Levitt (1999) and Marvell and Moody (2001) which concluded that age structure changes have little impact on crime rate trends. Per capita income appears positively associated with rates of homicide, rape, and auto theft. This finding is inconsistent with theoretical expectations, but mirrors findings reported by other studies (Marvell &Moody, 1995; Lott &Mustard, 1997). Finally, the number of families headed by females is positively associated with homicide rates. To our knowledge, this is the first time this variable has been related to cross-temporal variation in homicide rates.

Is Adopting a Three Strikes Law Endogenous?

One possible explanation for the lack of impact of three strikes laws on crime rates is simultaneity, which can happen if unusual increases in crime lead policy makers to enact three strikes laws. In other words, adopting and applying three strikes laws may be endogenous to the crime rate. If simultaneity does occur, the coefficients on the three strikes law variables would be biased, most likely positively, and mask any crime-reduction impact of the laws.

The most common procedure used to address potential endogeneity problems in evaluations of legal interventions is twostage least squares (2SLS) regression. As Marvell and Moody (1996) and others (Kennedy, 1998) have noted, the problem with 2SLS is that it requires at least one identifying restriction-at least one variable that is strongly correlated with the endogenous explanatory variable (i.e., adoption of three strikes laws), is uncorrelated with the error term in the crime rate equation and does not conceptually belong in the crime equation, or is a proxy for a variable that should be in the crime rate equation. These requirements are extremely difficult to satisfy, mainly because the instruments cannot be considered convincingly exogenous or are only weakly correlated with the endogenous explanatory variable.

Perhaps the easiest way to test whether three strikes laws have been adopted in response to unusual increases in crime is to simply exclude from the model specifications the years immediately before the laws were adopted. If an upward trend in crime is responsible for the law, then dropping these years from the model specifications should produce significant negative coefficients for the law variables. To examine this possibility, we excluded observations of the 3 years prior to the adoption of the laws but included the year of the adoption (it is unlikely that current-year crime could impact crime legislation contemporaneously). The results of this estimation procedure for all seven UCR crimes are presented in Table 2, but to conserve space only the coefficients for the three strike law variables are presented.

The coefficients on the three strikes law variables reported in Table 2 are roughly identical to those reported in Table 1, which indicates that the lack of significant results for the three strikes law variables in Table 1 is not the result of abnormal crime spikes in the years immediately before a three strikes law was adopted. We also tried dropping 2 years prior to the passage of a law and obtained similar estimates. Simultaneity also seems to be ruled out by Figure 1, because there is no evidence that crime rates were growing faster (or declining more slowly) in three strike cities. In fact, Figure 1 suggests that crime rates were actually declining slightly faster in three strikes cities than in others immediately before the adoption of most three strikes laws in 1994 and 1995. Thus, there is no statistical evidence that policy makers passed three strikes laws because crime rates in their states were rising more quickly, or declining more slowly, than in other states. This finding is not surprising given that the public and policy makers respond mostly to news accounts of highly publicized crimes (e.g., Polly Klaas), which in turn are uncorrelated with actual or official crime rates (Rappeler, Blumberg, &Potter, 1996; McCorkle &Miethe, 2002; Surette, 1998).

Table 2. Three Strikes Law Variables With Observations From 3 Years Prior to the Adoption of Three Strikes Law Excluded

	Three Strikes Law Variables			
	Post-Passage Dummy		Post-Law Linear Trend	
Dependent Variable	Coef.	*t*	Coef.	*t*
Homicide	*.21*	3.01	-.00	-.11
Rape	.06	1.69	.01	.85
Robbery	*.09*	2.24	-.01	-.78
Assault	.05	1.49	-.00	-.40
Burglary	.06	1.68	.00	.12
Larceny	*.05*	1.89	-.07	-1.37
Auto Theft	.01	.21	-.00	-.41

Notes: This table presents the results of crime regressions with observations for the three years prior to the adoption of a three strikes law excluded. Only the results for the three strikes law variables are presented. The control variables are similar to those used in Table 1. Standard errors are corrected for clustering. Coefficients that are significant at the .10 level are italicized. Coefficients that are significant at the .05 level are both italicized and underlined.

Estimating State-Specific Effects of Three Strikes Laws on Crime Rates

There is little evidence in the results presented in Table 1 to support the claim that three strikes laws reduce crime rates through either deterrence or incapacitation. However, the regressions shown in Table 1 estimated an aggregated effect for the laws across all cities in three strike states. If, for example, the impact of the laws on crime rates varies significantly across states, then the model presented is misspecified. Moreover, as noted, the dangers of estimating a single aggregated effect are particularly acute in this case because of vast differences in, first, the contents of three strikes legislation across the states (e.g., what constitutes a "strike" as well as prosecutorial and judicial discretion in applying the laws, see Clark et al., 1997); second, publicity surrounding passage of the laws; and, third, the application of the laws in practice.

One way to avoid aggregation bias is to change the model specification to estimate a state-specific effect for each state adopting a three strikes law. In other words, one would include in the panel data regressions for each crime category a separate postpassage dummy and post-passage linear trend variable for each group of cities in a three strikes state. These estimates for all seven index crime categories are presented in Table 3, which shows that the coefficients on the post-passage dummy and post-passage trend variable separately estimate the deterrent and incapacitative effects of three strikes laws for each of the 25 states that passed the laws between 1993 and 1996.

Results presented in Table 3 reject the more constrained specifications of the aggregate regressions, which implicitly assumed that the impact of three strikes laws was constant across jurisdictions. Indeed, for each crime type, we were able to reject the hypothesis that the 21 post-passage dummies and 21 post-passage trend variables were essentially equal. This suggests that the panel data regressions presented in Table 1, which assumed uniform impacts for all three strikes cities, are too restrictive. With the exception of homicide and auto theft, the coefficients on the post-passage dummy variables from the disaggregated analysis suggest that the number of states experiencing a statistically significant decrease in crime after adopting three strikes law is roughly identical to the number experiencing a statistically significant increase (see Table 3). For example, for robbery, six states saw a decrease and four an increase. For homicide, the disparity was nine to three. For auto theft, the numbers were nine and five. Of the 147 estimated impacts of the law on crime rates (21 states by seven crime categories), 42 represented statistically significant decreases in crime on passage of the laws and 44 represented statistically significant increases. Overall, Table 3 shows 73 decreases and 74 increases in crime.

Results from the disaggregated analysis for the post-passage trend variables also suggest substantial heterogeneity in the laws' impact on city crime rates over time. For every crime type, the number of states experiencing statistically significant decreases in crime rates over time was roughly equivalent to the number of states experiencing significant increases. Specifically, out of 147 estimated impacts on crime over time, 54 exhibited statistically significant decreases and 43 exhibited statistically significant increases. Overall, the results for the state-specific post-passage trend variables indicate 76 decreases and 71 increases (Table 3).

The net 5-year deterrent and incapacitative impact of three strikes laws on crime rates for each state are reported in Table 4. To calculate the net 5-year impact, it is necessary to add the coefficients on the post-passage dummy and post-passage trend variables for individual years and then sum the yearly impacts.13

Estimates of the 5-year impacts of three strikes laws on crime reveal that only one state (Arkansas) shows a net 5-year decrease in all seven crime categories without showing a statistically significant increase in another crime category. Three states (California, Louisiana, and New Jersey) show a statistically significant decrease in four or more crime categories, but a statistically significant increase in at least one crime category as well.

Table 3. Deterrent and Incapacitation Effects of Three Strikes Laws on UCR Index Crime Rates, Jurisdiction Specific Estimates

		Homicide		Rape		Robbery		Aggr. Assault		Burglary		Larceny		Auto Theft	
		Coef.	t	Coef.	t	Coef.	t	Coef.	t	Coef.	t	Coef.	t	Coef.	t
Alaska	*Postpassage Dummy*	.44	6.28	-.12	-2.88	-.20	-6.33	-.02	-.60	-.05	-1.39	-.04	-1.53	-.09	-1.57
	Postpassage Trend	-.19	-13.9	.06	4.02	-.03	-3.41	-.02	-2.40	-.01	-.73	-.00	-.76	-.02	-1.63
Arkansas	*Postpassage Dummy*	-.52	-7.87	-.07	-1.90	-.16	-4.66	-.60	-13.4	-.24	-7.38	-.10	-4.84	-.18	-4.23
	Postpassage Trend	.05	3.84	-.01	-.64	-.04	-4.71	.04	4.00	.03	3.9	-.02	-4.64	.00	-.04
California	*Postpassage Dummy*	.09	1.28	.07	2.01	-.01	-.44	.01	.38	-.02	-1.27	-.01	-.53	-.10	-3.13
	Postpassage Trend	-.04	2.13	.03	2.45	-.04	-3.92	-.02	-2.77	-.02	-2.25	-.02	-3.10	-.04	-3.49
Colorado	*Postpassage Dummy*	.04	.53	.12	3.09	-.02	-.55	-.15	-4.44	.02	1.01	.03	1.24	-.05	-1.52
	Postpassage Trend	-.01	-.60	.03	2.28	.01	1.20	.01	1.34	.01	.94	-.00	-1.03	.03	3.25
Connecticut	*Postpassage Dummy*	.05	.70	-.07	-2.29	.02	.40	-.01	-.48	-.03	-1.52	.08	3.72	-.12	-3.61
	Postpassage Trend	-.06	-3.73	-.02	-1.99	-.01	-.95	.00	.41	-.05	-4.29	-.02	-3.97	.00	.17
Florida	*Postpassage Dummy*	.15	2.69	.11	3.83	-.00	-.03	.06	3.45	.02	1.11	-.00	-.11	-.01	-.53
	Postpassage Trend	.02	1.95	-.03	-2.79	-.02	-1.92	-.00	-.45	-.02	-2.69	-.02	-5.79	-.02	-2.24
Georgia	*Postpassage Dummy*	-.01	-.23	-.06	-2.06	-.01	-.26	.04	2.08	-.02	-1.7	.01	.86	-.02	-.87
	Postpassage Trend	.06	4.14	.02	2.15	.02	2.47	.04	6.17	.02	2.87	-.02	-4.69	.03	3.66
Indiana	*Postpassage Dummy*	.38	8.34	.01	.24	.19	5.59	-.05	-2.47	.18	6.68	.10	4.01	.13	3.03
	Postpassage Trend	-.04	-3.45	-.03	-2.67	-.05	-5.76	-.03	-4.16	-.05	-5.58	-.04	-8.67	-.04	-4.79
Kansas	*Postpassage Dummy*	.14	3.17	.08	2.64	-.15	-5.34	.12	4.29	-.13	-6.04	.04	2.27	-.10	-2.15
	Postpassage Trend	.03	1.78	-.02	-1.84	-.03	-3.58	-.00	-.12	-.02	-2.81	-.02	-4.68	-.01	-.88
Louisiana	*Postpassage Dummy*	.05	1.06	.37	6.75	-.05	-1.80	.05	1.49	-.04	-2.3	.03	1.52	-.09	-2.78
	Postpassage Trend	-.03	-2.00	-.04	-4.05	-.03	-4.40	-.06	-7.71	-.02	-2.94	-.02	-5.43	-.00	-.15
Maryland	*Postpassage Dummy*	-.08	-1.71	.12	2.67	.04	1.40	.16	5.09	-.01	-.29	.07	2.05	.01	.13
	Postpassage Trend	.10	5.23	-.05	-5.39	-.02	-1.86	.02	2.40	-.01	-1.1	-.03	-5.77	-.07	-9.42
Nevada	*Postpassage Dummy*	.41	8.59	-.09	-2.83	-.02	-.89	-.05	-1.73	-.02	-.5	-.11	-5.21	-.05	-1.32
	Postpassage Trend	.05	4.17	.09	8.44	.07	9.32	.03	4.08	.08	11.52	.05	9.62	.11	14.42
New Jersey	*Postpassage Dummy*	.19	1.95	.11	2.88	-.07	-1.66	.08	2.09	-.01	-.38	-.01	-.21	-.14	-3.00
	Postpassage Trend	.05	3.37	-.09	-8.14	-.04	-3.92	-.03	-3.18	-.07	-6.66	-.04	-6.79	.02	1.76
New Mexico	*Postpassage Dummy*	.14	2.58	.30	7.72	.17	6.40	-.28	-8.10	.02	.9	.09	4.68	.41	7.16
	Postpassage Trend	-.02	-1.13	-.02	-2.25	.00	.02	.02	3.66	-.01	-.65	-.00	-.17	-.07	-6.02

Table 3. Deterrent and Incapacitation Effects of Three Strikes Laws on UCR Index Crime Rates, Jurisdiction Specific Estimates

		Homicide		Rape		Robbery		Aggr. Assault		Burglary		Larceny		Auto Theft	
		Coef.	*t*	Coef.	*t*	Coef.	*t*	Coef.	*t*	Coef.	*t*	Coef.	*t*	Coef.	*t*
North Carolina	*Postpassage Dummy*	-.11	-1.51	-.09	-1.94	-.13	-2.95	.09	2.38	-.03	-1.31	.00	.000	.23	6.02
	Postpassage Trend	.02	.98	-.00	-.62	.00	.19	.01	1.30	-.00	-.17	.00	.26	.01	1.19
Pennsylvania	*Postpassage Dummy*	.22	4.64	.04	1.34	.14	6.52	.03	1.82	.07	3.61	.02	1.45	-.05	-1.83
	Postpassage Trend	.01	1.18	.06	5.92	-.02	-1.74	.08	8.20	-.01	-1.34	.01	1.58	.04	5.51
Tennessee	*Postpassage Dummy*	-.02	-0.31	.05	1.27	.01	.24	-.06	-1.73	-.05	-2.45	.02	.57	.04	1.10
	Postpassage Trend	.05	2.80	.00	.30	.01	1.10	.01	.76	.02	3.07	.01	2.63	-.00	-.31
Utah	*Postpassage Dummy*	.13	2.05	-.07	-1.95	.14	4.13	.22	5.92	.06	2.33	.01	.45	.46	7.23
	Postpassage Trend	-.00	-.01	.01	.16	.06	2.83	.07	1.84	.02	.79	.01	.51	-.08	-2.52
Virginia	*Postpassage Dummy*	.07	1.17	-.11	-2.59	-.00	-.08	.04	1.59	.02	1.31	-.02	-1.01	-.10	-2.80
	Postpassage Trend	-.03	-1.99	-.01	-1.22	-.00	-.34	.01	.91	.01	1.32	-.01	-1.69	.02	2.31
Washington	*Postpassage Dummy*	.06	1.61	-.03	-1.41	.01	.49	-.12	-6.47	.04	1.93	-.00	-.04	.06	2.87
	Postpassage Trend	.00	.33	-.01	-.88	.01	2.32	.01	1.87	.04	4.71	.00	.48	.04	4.03
Wisconsin	*Postpassage Dummy*	-.18	-4.20	-.19	-4.33	-.05	-1.99	.39	11.7	.00	.03	-.03	-1.92	-.17	-6.02
	Postpassage Trend	.09	4.50	.01	.75	.04	4.93	.02	1.59	.05	6.51	.04	5.01	.02	2.76
Summary for Postpassage Dummy															
Negative & Significant		3		9		6		7		5		3		9	
Negative & Not Significant		3		1		7		2		7		6		5	
Positive & Significant		9		8		4		9		4		5		5	
Positive & Not Significant		6		3		4		3		5		7		2	
Summary for Postpassage Trend															
Negative & Significant		6		8		10		6		7		11		6	
Negative & Not Significant		2		4		2		2		5		3		4	
Positive & Significant		9		6		5		6		6		3		8	

Notes: The dependent variable is the ln(crime rate) named at the top of each column. All regressions are weighted by a function of city population as determined by the Breusch Pagan test. Standard errors are corrected for clustering. Due to space limitations only the results for the post-passage dummy and post-passage trends are shown. The remaining controls are those listed in Table 1 including year dummies, city dummies, and city trend dummies. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are italicized. Coefficients that are significant at the .01 level are italicized and underlined.

While it would be tempting to conclude that three strikes laws are responsible for the majority of the crime drop in these states, especially in California, where three strike provisions are applied quite frequently, one must account for the fact that the results for some laws are probably nothing more than random artifacts or are proxies for other contemporaneous changes taking place around the adoption of a three strikes law, not explicitly controlled for in the model specifications. Moreover, if one is willing to conclude from Table 4 that the laws reduce crime in these states then one has to at least entertain the prospect that the laws also lead to large crime increases as well. Take, for example, Nevada and Pennsylvania, which experienced large statistically significant increases in crime following the adoption of a three strikes law. Unless one is willing to conclude the laws have had the unintended consequence of increasing crime in these states, then one cannot simply select the states that seem to do well under the law and conclude that the laws work to reduce crime. That is, one cannot cherry-pick those states that appear to benefit from the passage of a three strikes law and ignore states where the laws appear to have a deleterious impact on crime.

Turning now to the individual crime categories themselves, there does not appear to be a strong correlation between the passage of a three strikes law and a decrease in any individual crime category. In most cases the number of states that exhibited a statistically significant decrease in any individual crime category was roughly identical to the number of states that exhibited a statistically significant increase. The greatest disparity between significant increases and decreases occurs for homicide, with eight states showing a statistical increase in homicide and only one reporting a statistical decrease. Overall, 72 of the 147 tests indicate that three strikes laws reduced crime, with 29 of these estimates being statistically significant (at the .05 level). At the same time, 31 of the 147 estimated net 5-year effects indicated a statistically significant increase in crime, resulting in a ratio of about one crime decrease for every one increase.

Table 4. State-Specific Annualized 5-Year Impact of Three Strikes Laws On UCR Index Crime Rates.

	Homicide	Rape	Robbery	Aggr. Assault	Burglary	Larceny	Auto Theft
Alaska	-13.0%	5.5%	-30.1%	-8.9%	-6.5%	-4.8%	-13.7%
Arkansas	-35.8%	-9.3%	-29.3%	-48.8%	-15.5%	-16.7%	-17.7%
California	-1.5%	14.7%	-12.3%	-5.2%	-9.1%	-6.2%	-21.6%
Colorado	1.7%	19.8%	1.5%	-11.2%	3.9%	1.4%	4.6%
Connecticut	-12.0%	-13.3%	-.6%	-.2%	-17%	2.2%	-10.9%
Florida	20.8%	3.3%	-4.9%	4.5%	-4.0%	-6.8%	-6.9%
Georgia	16.6%	-.2%	5.9%	15.5%	3.9%	-3.7%	6.9%
Indiana	24.8%	-8.5%	4.7%	-13.4%	3.5%	-2.0%	.3%
Kansas	22.4%	3.1%	-23.4%	12.1%	-18.7%	-.8%	-11.8%
Louisiana	-4.7%	26.1%	-14.8%	-13.1%	-9.5%	-3.4%	-9.7%
Maryland	23.0%	-3.9%	-.4%	20.9%	-3.6%	-1.3%	-19.6%
Nevada	57.3%	17.1%	19.7%	4.1%	22.2%	3.2%	26.6%
New Jersey	34.8%	-17.6%	-19.0%	-.3%	-22.2%	-13.2%	-8.4%
New Mexico	8.5%	22.8%	17.1%	-20.9%	.1%	8.9%	21.5%
North Carolina	-6.0%	-11.1%	-12.1%	12.2%	-3.6%	.3%	26.2%
Pennsylvania	26.1%	20.6%	9.7%	27.2%	3.2%	4.3%	7.8%
Tennessee	12.1%	5.8%	3.8%	-4.1%	1.3%	5.1%	2.7%
Utah	12.6%	-4.7%	33.0%	41.6%	11.1%	3.3%	23.0%
Virginia	-3.7%	-14.2%	-1.2%	6.8%	6.0%	-4.1%	-3.1%
Washington	6.7%	-5.1%	5.2%	-8.2%	14.4%	.5%	17.6%
Wisconsin	9.8%	-15.9%	7.2%	45.4%	14.2%	8.9%	-10.5%
Summary of 5-Year Effects							
Negative & Significant	1	8	7	6	6	6	9
Negative & not significant	6	3	4	5	4	5	2
Positive & significant	8	6	7	9	4	4	6
Positive & not significant	6	4	3	1	7	6	4

Notes: The dependent variable is the natural log of the crime rate listed at the top of each column. The data set is comprised of annual city-level observations. While not shown, city, year, and city trend effects are included in all specifications. All regressions are weighted by a function of city population as determined by the breusch pagan test. Standard errors are corrected for clustering by state. Coefficients that are significant at the .10 level are underlined. Coefficients that are significant at the .05 level are italicized. coefficients that are significant at the .01 level are italicized and underlined.

DISCUSSION AND CONCLUSION

Consistent with other studies, ours finds no credible statistical evidence that passage of three strikes laws reduces crime by deterring potential criminals or incapacitating repeat offenders. The results of the aggregate law variable analysis provided no evidence of an immediate or gradual decrease in crime rates, and homicide rates were actually positively associated with the passage of three strike laws. The findings for the state-specific analysis were mixed, with some states showing increases in some crimes, and others showing decreases. Overall, 29 of the 147 tests were negative and significant, indicating that three strikes laws reduced crime, while 31 demonstrated a statistically significant increase in crime.

We offer several possible explanations for why passage of three strikes laws does not appear to be negatively correlated with crime rates. First, ethnographic research on criminals (Hochstetler &Copes, 2003; Jacobs, 1999; Shover, 1996; Wright &Decker, 1994, 1997) suggests that rarely are they concerned about getting caught (i.e., they are confident in their ability to commit crime or they can successfully manage any fear), or they simply aren't aware of the laws or the way in which the laws operate (Marvell &Moody, 1995; Kovandzic, 2001). In addition, many offenders are under the influence of alcohol and/or drugs (U.S. Department of Justice, 2003) and this may serve to lessen their concerns with getting caught (Shover &Honaker, 1999). second, as Stolzenberg and D'Alessio (1997) suggest, the laws frequently target offenders beyond the peak age of offending, and thus, the impact on crime is minimal because they are already committing fewer crimes. In addition, the effectiveness of three strikes laws for reducing crime rates depends on the ability of the system to identify potential high-rate offenders before they commit a large number of crimes. This would entail incarcerating youthful offenders because the peak ages for offending are between the ages of 15 and 24. However, it is incredibly difficult to predict which offenders are most likely to become career criminals. Moreover, the vast majority of youthful offenders stop their criminal behavior on their own, without imprisonment (Clear, 1996). Third, the ability of incapacitation to reduce crime is also limited by the possibility that offenders are simply replaced by other offenders. To the extent that the social conditions in which crime occurs remain the same, there will likely be a ready supply of motivated potential offenders to replace those removed through incarceration. Moreover, a large percentage of crime, particularly drug crimes and robbery, is committed by offenders acting in groups (Reiss, 1988). Incarcerating one of a group of co-offenders may not end the group's criminal behavior because it persists with one less member or simply replaces that member with another (Clear, 1996).

Fourth, the failure of three strikes laws to reduce crime may be explained by the fact that most offenders were receiving enhanced penalties prior to passage of the laws. Three strikes laws would thus not have a significant effect on crime rates simply because they did not raise the severity of punishment appreciably (Stolzenberg &D'Alessio, 1997). Fifth, some would argue that the laws do not reduce crime because they are not enforced, are not severe enough, or both. The results of the state-level analysis (see Table 4) show mixed results in crime rate trends between states that apply the law frequently or have severe laws versus states that apply the law less frequently or have less severe laws. For example, California's law, which is severe and frequently enforced, exhibits an incapacitation effect on six out of seven crimes. However, in Georgia, also identified as a state with a severe and frequently enforced three strikes law, there was an increase in crime in five out of seven categories. As we will discuss, this possibility is best tested with methodologies other than those used in this study.

Given our findings and the sophistication of the methodology, as well as results of studies by Marvell and Moody (2001) and Kovandzic et al. (2002), policy makers should reconsider the costs and benefits associated with three strikes laws. Although the laws have failed to produce what is arguably one of the most important benefits, a reduction in crime, researchers have identified numerous costs associated with three strikes and other habitual offender laws. These include the racial disparity in their application (Crawford, Kleck, &Chiricos, 1998; Males &Macallair, 1999); the financial costs of increased trials (as offenders opt to take their chances with juries; Cushman, 1996), of building and staffing prisons (Austin, 1996; Greenwood et al., 1994) and of providing medical care to aging prisoners (King &Mauer, 2001); and, perhaps most costly, the potential increase in homicide rates as offenders attempt to avoid striking out by eliminating potential witnesses (Kovandzic et al., 2002; Marvell &Moody, 2001).

Despite the growing body of empirical work examining the effects on crime and other social phenomena of three strikes laws, researchers should continue to explore this topic, especially in light of the continual advances in research methodology. In addition, researchers should use qualitative methods to explore the law in action in various jurisdictions because there is comparatively little information from jurisdictions outside of California. Considering our finding that the laws reduced crime in some states, a more comprehensive analysis (e.g., publicity of the law, offenders' perspectives, prosecutorial and judicial discretion) of what is going on in those particular states can provide information that should help to establish what is or is not working and why. Interviews with offenders would further our understanding of the possible deterrent effects of three strikes laws by assessing offenders' levels of awareness of, behavioral responses to, and their experiences with the laws. Research on prosecutors could generate valuable insight into how frequently the law is used and how it is used, e.g., as a plea bargaining tool. Although three strikes laws were designed in part to limit judicial discretion, there is still a range of possible sentences within the guidelines. Thus, interviews with judges regarding how they exercise discretion should also contribute to our understanding of the law in action.

Footnote

1 There is significant variability in the offenses that "trigger" the strike as well as in the specific sentences administered under the laws. See Austin and Irwin (2001) for an excellent analysis of this variation.

2 There has been a great deal of commentary on the impact of three strikes laws on prison populations (Austin 1994), their racial disparity (Crawford, Chiricos, &Kleck, 1998), the constitutionality of the laws (Kadish, 1999), and their fairness (Dickey &Hollenhorst, 1998; Vitiello, 1997). Because the current study examined the potential impact of the laws on crime rates, the literature review is limited to published studies addressing that question.

3 Studies included for review clearly do not represent a comprehensive review of published research on California's three strikes law. They were selected either because they used sophisticated quantitative evaluative designs or, in the case of Zimring, Hawkins, and Kamin (2001), because of the depth of the analyses.
4 Greenwood et al. (1994) made a series of assumptions, some of which could be characterized as questionable, which had significant implications for the results. For example, they assumed the fraction of citizens becoming active criminals over the 25-year period would remain roughly constant, they did not allow offenders to switch back and forth between high and low offense rates, and that the law would be implemented and enforced as written.
5 A similar argument was made by Schmertmann, Amankwaa, and Long (1998) in their analyses of the impact of three strikes laws on prison population figures. Schmertmann et al. concluded that failing to consider age effects on criminal activities results in an incomplete analysis of the costs and benefits of the policy in which the costs of the policy are underestimated while its benefits are overestimated.
6 The counties included Alameda, Contra Costa, Fresno, Los Angeles, Orange, Riverside, San Bernardino, San Francisco, Sacramento, Santa Clara, San Diego, and Ventura.
7 The Zimring et al. (2001) study did not focus exclusively on the crime-reducing effects of California's three strikes statute. Rather, it was a much broader-based analysis of the politics, jurisprudence, and impact of the statute.
8 The MTS design has been utilized in many recent evaluations of criminal justice interventions including juvenile curfew laws (McDowall et al., 2000), firearm sentence enhancement laws (Marvell &Moody, 1995), concealed-carry handgun laws (e.g., Ayres &Donahue, 2003; Kovandzic &Marvell, 2003; Lott &Mustard, 1997), Brady law (Ludwig &Cook, 2000), and earlier studies examining the effects of three strikes laws (Kovandzic et al., 2002; Marvell &Moody, 2001; Shepherd, 2002).
9 Zimring et al. (2001) made this very point. In California, for example, there apparently is wide variation in how the state's three strikes law is applied to offenders with second and third strikes. Obviously, despite what the law says, how the sentencing policy is implemented has tremendous implications for any possible crime reducing effects generated by the law.
10 Of course, one way that prospective three strikes defendants could avoid the additional penalties from such a law would be to move their criminal activity to a more hospitable jurisdiction (presumably one without a three strikes law).
11 Because Washington adopted its three strikes law in late 1993 (December 1993), we decided to include Seattle, Spokane, and Tacoma in the 1994 grouping of cities. Similarly, we decided to include Anchorage, Alaska in the 1995 grouping of cities since the law was adopted in early 1996 (March, 1996).
12 To calculate this percentage we used the approximation 100 * [exp (δ) - 1].
13 The predicted impact of a law for individual years is:
Year 1: I*beta(post-passage dummy for cities in state X) + I*beta(postpassage trend for cities in state X)
Year 2: 2*beta(post-passage dummy for cities in state X) + 2*beta(postpassage trend for cities in state X)
Year 3: 3*beta(post-passage dummy for cities in state X) + 3*beta(postpassage trend for cities in state X)
Year 4: 4*beta(post-passage dummy for cities in state X) + 4*beta(postpassage trend for cities in state X)
Year 5: 5*beta(post-passage dummy for cities in state X) + 5*beta(postpassage trend for cities in state X)
Where: beta (post-passage dummy) and beta (post-passage trend) represent the estimated coefficients on the post-passage dummy and post-passage trend variables. Summing the individual year impacts, we were able to calculate a net five-year impact as: beta (post-passage dummy for cities in state X) + 3*beta (post-passage trend for cities in state X). We also tested whether this linear combination of regression coefficients was significantly different from zero and report results of this testing in Table 3.

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