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THE LETHAL EFFECTS OF
THREE-STRIKES LAWS

*THOMAS B. MARVELL and CARLISLE E. MOODY**

ABSTRACT

Three-strikes laws provide very long prison terms for certain criminals with prior convictions of serious violent crimes. It is likely that the laws increase homicides because a few criminals, fearing the enhanced penalties, murder victims and witnesses to limit resistance and identification. With a state-level multiple-time-series design, we find that the laws are associated with 10–12 percent more homicides in the short run and 23–29 percent in the long run. The impact occurs in almost all 24 states with three-strikes laws. Furthermore, there is little evidence that the laws have any compensating crime reduction impact through deterrence or incapacitation.

I. INTRODUCTION

BETWEEN 1993 and 1995, 24 states enacted ''three strikes and you're out'' laws, which provide longer prison terms for some criminals with repeat felony convictions.[1] The laws vary, but most call for life sentences without the possibility of release for at least 25 years upon a third conviction of a serious violent crime.[2] The crimes include murder, rape, kidnapping, aggravated robbery, aggravated assault, and sexual abuse. A few states include additional crimes, most commonly firearm violations, burglary of occupied dwellings, and simple robbery. The rationale for the laws

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* Justec Research and College of William and Mary, respectively.

[1] National Conference of State Legislatures, Three Strikes Legislation Update (Nat'l Conf. State Legislatures 1997); John Clark, James Austin, & D. Alan Henry, ''Three Strikes and You're Out'': A Review of State Legislation (U.S. Dep't Justice 1997); Walter J. Dickey & Pam Stiebs Hollenhorst, ''Three Strikes'': Five Years Later (Campaign for an Effective Crime Policy 1998); Michael G. Turner *et al.,* ''Three Strikes and You're Out'' Legislation: A National Assessment, 59 Fed. Probation 16 (1995). Apparently the only law passed in 1996 or 1997 is in Alaska (National Conference of State Legislatures, *supra*), which is not included here.

[2] For more detail concerning the variation in laws see the text after note 45 *infra*.

is that the longer prison terms reduce crime by deterring and incapacitating the most active and dangerous criminals.[3]

The basic assumption is that, everything else being equal, a person will be less likely to commit a crime when the expected costs increase.[4] The additional prison terms called for by three-strikes laws increase the expected costs for criminals subject to the laws and, at first glance, the expected result is less crime. It is not clear that, in practice, three-strikes laws have substantial deterrent impacts. Most of these laws have a narrow scope and are applied to relatively few criminals.[5] Criminals might not be aware of the laws, at least initially.[6] Repeat criminals can be expected to serve substantial prison terms even in the absence of the laws.[7] All but one three-strikes state already had habitual criminal statutes under which criminals with prior convictions could be given lengthy sentences, although usually at the discretion of the judge.[8] Because the marginal increase in prison terms takes effect in the future, the additional deterrence is small if criminals have relatively high discount rates.[9] The deterrent effect on homicides is limited in any case because the law probably does not increase sanctions for homicides. However, the law might reduce homicides by deterring robberies and other felonies during which homicides might take place.

Furthermore, some criminals might limit their expected costs by taking evasive action. For example, they could respond by moving to other juris-

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[3] For example, Jonathan Simon, Criminology and the Recidivist, in Three Strikes and You're Out: Vengeance as Public Policy 24–52 (David Shichor & Dale K. Sechrest eds. 1996).

[4] For example, Gary S. Becker, Crime and Punishment: An Economic Approach, 76 J. Pol. Econ. 169 (1968); Isaac Ehrlich, Participation in Illegitimate Activities: A Theoretical and Empirical Investigation, 81 J. Pol. Econ. 521 (1973); J. M. Heineke, Economic Models of Criminal Behavior: An Overview, in Economic Models of Criminal Behavior 1–33 (J. M. Heineke ed. 1978).

[5] Dickey & Hollenhorst, *supra* note 1; Linda S. Beres & Thomas D. Griffith, Do Three Strikes Laws Make Sense? Habitual Offender Statutes and Criminal Incapacitation, 87 Geo. L. Rev. 103 (1998). The amount of sentencing under the three-strikes laws is discussed in the text at note 45 *infra*.

[6] Probably the only evidence available concerning criminals' knowledge of the laws is the amount of publicity given to the laws. One study found that the California law received extensive coverage in Los Angeles. Ray Surette, News from Nowhere, Policy to Follow: Media and the Social Construction of ''Three Strikes and You're Out,'' in Shichor & Sechrest eds., *supra* note 3, at 177–202.

[7] James Austin, The Effect of ''Three Strikes and You're Out'' on Corrections, in Shichor & Sechrest eds., *supra* note 3, at 155–76; Beres & Griffith, *supra* note 5.

[8] Dickey & Hollenhorst, *supra* note 1; Clark, Austin, & Henry, *supra* note 1. The exception is Kansas. As discussed later, four states already had three-strikes laws, and their scope was broadened in 1994 or 1995.

[9] A. Mitchell Polinsky & Steven Shavell, On the Disutility and Discounting of Imprisonment and the Theory of Deterrence, 28 J. Legal Stud. 1 (1999).

dictions[10] or to other types of crime[11] where the expected costs are lower. Criminals can also continue to commit the same crimes and adapt to crime reduction efforts by taking additional measures to reduce the chances of apprehension and conviction.[12] Bribing police is probably the most studied example.[13]

   More applicable to the current research is the fact that criminals can change their modus operandi. We focus on one possibility: when committing an ordinarily nonlethal felony, a criminal might kill victims and others at the crime scene in order to reduce the chances that they will overpower or identify the criminal.[14] Everything else being the same, when the penalties for a crime and for an exacerbated version of that crime are similar, the criminal can be expected to commit the exacerbated version if that reduces the chances of apprehension and conviction.[15] Criminals who already have

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[10] For example, Simon Hakim & George F. Rengert, Crime Spillover (1981); John Eck, Preventing Crime at Places, in Preventing Crime: What Works, What Doesn't, What's Promising 701–62 (Lawrence W. Sherman *et al.* eds. 1997).

[11] J. M. Heineke, Substitution among Crimes and the Question of Deterrence: An Indirect Utility Function Approach to the Supply of Legal and Illegal Activity, in Heineke ed., *supra* note 4, at 153–209; Philip J. Cook, Robbery Violence, 78 J. Crim. L. & Criminology 357 (1987).

[12] Arun Malik, Avoidance, Screening and Optimum Enforcement, 21 RAND J. Econ. 341 (1990); James Andreoni, Criminal Deterrence in the Reduced Form: A New Perspective on Ehrlich's Seminal Study, 33 Econ. Inquiry 476 (1995); Philip J. Cook, The Clearance Rate as a Measure of Criminal Justice System Effectiveness, 11 J. Public Econ. 135 (1979). Cook argues that if risks of apprehension increase, the impact on clearance rates is difficult to estimate because a criminal's rational response need not be to reduce criminal activity; he or she may also conduct criminal activity in a way that reduces the chance of arrest. The net effect might even be more crimes and fewer arrests, as criminals shift to safer but less lucrative crime types. Also, it is possible that more severe sanctions increase official crime rates because the sanctions might prompt citizens to report crime more often, since the reporting is more likely to be seen as a meaningful effort. See Steven D. Levitt, The Relationship between Crime Reporting and Police: Implications for the Use of Uniform Crime Reports, 14 J. Quantitative Criminology 61 (1998).
   In addition, criminals can be expected to spend more money to avoid conviction when sanctions are greater. Edward A. Snyder, The Effect of Higher Criminal Penalties on Antitrust Enforcement, 33 J. Law & Econ. 439 (1990); John R. Lott, Should the Wealthy Be Able to ''Buy Justice''? 95 J. Pol. Econ. 1302 (1987); John R. Lott, Do We Punish High Income Criminals Too Heavily? 30 Econ. Inquiry 583 (1992).

[13] For example, Gary S. Becker & George J. Stigler, Law Enforcement, Malfeasance and Compensation of Enforcers, 3 J. Legal Stud. 1 (1974); Roger Bowles & Nuno Garoupa, Casual Police Corruption and the Economics of Crime, 17 Int'l Rev. L. & Econ. 75 (1997).

[14] Louis L. Wilde, Criminal Choice, Nonmonetary Sanctions and Marginal Deterrence: A Normative Analysis, 12 Int'l Rev. L. & Econ. 333 (1992). Speculation that this will result from the three-strikes laws can be found in Michael D. Wiatrowski, ''Three Strikes and You're Out'': Rethinking the Police and Crime Control Mandate, in Shichor & Sechrest eds., *supra* note 3, at 117.

[15] Researchers who have raised this possibility include Wilde, *supra* note 14; Philip J. Cook, Effect of Gun Availability on Violent Crime Patterns, 455 Annals Am. Acad. Pol. &

''two strikes'' can face long prison terms if convicted of a third serious violent crime, a penalty that approaches that for homicide.[16] Under these conditions, there would be little marginal deterrence to dissuade a criminal from committing homicide when doing so lessens the chance of resistance, arrest, and conviction. Consequently, homicide rates might increase following the passage of three-strikes laws.

However, as Steven Shavell[17] notes, this reasoning assumes that the risk of apprehension is the same for the underlying crime and its exacerbated version. If the risk is greater for the latter, its expected costs can be greater even if the sanctions are not. Law enforcement gives more attention to homicides than other crimes, and the homicide clearance rate is especially high.[18] As a general rule, therefore, the criminal's self-interest dictates the avoidance of homicide during the commission of other crimes. Also, as discussed later, in most states with three-strikes laws, very few criminals are sentenced under the law. Knowledgeable criminals in these states, therefore, would not fear direct application of the laws, although the laws might have an indirect effect during plea bargaining when prosecutors threaten to use the law.

Nevertheless, a criminal who fears, perhaps erroneously, application of a three-strikes law might commit murder in a few situations, when, owing to the specific circumstances of a particular crime, there is a high risk of apprehension and conviction if victims and witnesses live and when murdering them will reduce the overall risk in spite of the expected greater law enforcement effort.

Even if such situations are rare, the impact on homicides can be large. Let us assume that 99.9 percent of violent crimes (that would not involve homicide prior to a three-strikes law) in a three-strikes state are by persons who do not believe they have two strikes, who do not believe the law will be applied to them even if they have two strikes, or who do not perceive

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Soc. Sci. 63 (1981); George J. Stigler, The Optimum Enforcement of Laws, 78 J. Pol. Econ. 526 (1970).

[16] In 1992 the mean sentence length for those entering prison after a conviction of murder was 282 months, and the mean time served for murderers released in 1992 was 89 months. Craig Perkins, National Corrections Reporting Program, 1992 (1994).

[17] Steven Shavell, A Note on Marginal Deterrence, 12 Int'l Rev. L. & Econ. 345 (1992).

[18] The Federal Bureau of Investigation estimated that in 1997 the clearance rate for murder was 66 percent, as opposed to 26 percent for robbery, 51 percent for aggravated assault, and 66 percent for rape (crimes typically covered by three-strikes laws). Federal Bureau of Investigation, Crime in the United States, 1998 Uniform Crime Reports (1999). Clearance rates for robbery, assault, and rape are actually much less than these percentages. The rates are based on crimes reported to the police, and some 64 percent of rapes, 45 percent of robberies, and 48 percent of aggregated assaults are not reported. Tina Dorsey & Jayne Robinson, Criminal Victimization in the United States, 1994 (U.S. Dep't Justice 1997).

that eliminating victims or witnesses will reduce chances of apprehension and conviction. The law then might prompt, on average, one homicide per thousand violent crimes (assuming that multiple homicides are balanced by failed attempts).[19] Because the ratio of murder to other violent crimes is approximately .006,[20] these additional homicides would increase total homicides by roughly 17 percent in a three-strikes state.

Although the above reasoning suggests that the three-strikes laws could increase homicides, other factors might work to offset the effect. For example, the laws might reduce crime in general and thus reduce homicides committed during the commission of other crimes. Deterrence would likely cause an immediate reduction, and incapacitation would have a delayed impact because nearly all three-strikes defendants would have been given prison terms in any event.[21] Therefore, the net effect of the three-strikes law, and the timing of that effect, is an empirical question.

Some initial calculations suggest that the forces increasing homicide predominate, at least in the short run. Twelve three-strikes laws went into effect in 1994 and 12 in 1995.[22] Homicides declined nationwide during and after these years,[23] but less in states with three-strikes laws. In these 24 states, homicides in the 2 postlaw years (that is, either 1995–96 or 1996–97) are 11.5 percent less than in the 2 prelaw years. In the other 26 states homicides declined 19.0 percent from 1992–93 to 1995–96 and 25.7 percent from 1993–94 to 1996–97. Thus, the difference between three-strikes states and other states is roughly 10 percentage points, which translates to some 1,400 extra murders per year in the three-strikes states.

The rest of the paper evaluates the impact of the three-strikes law with a multiple time-series design. Section II describes the regression procedures, and Section III presents the results with respect to the immediate impact on

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[19] As discussed later (in the text at notes 40–42 *infra*), in most three-strikes states very few defendants are sentenced under the laws. This does not eliminate the incentive to commit murder in these states because criminals have imperfect information about the operation of the laws, and some are likely to believe, perhaps incorrectly, that three-strikes sanctions apply to them.

[20] This figure is derived by dividing the number of homicides by the number of rapes, robberies, and aggregated assaults, adjusting the latter for underreporting. Dorsey & Robinson, *supra* note 18; Federal Bureau of Investigation, *supra* note 18.

[21] Daniel Kessler & Steven D. Levitt, Using Sentencing Enhancements to Distinguish between Deterrence and Incapacitation, 42 J. Law & Econ. 343 (1999).

[22] See note 27 *infra*. One of the 12 laws scored as a 1994 law went into effect in December 1993.

[23] The numbers of nationwide homicides are

1991	1992	1993	1994	1995	1996	1997	1998
24,700	23,760	24,530	23,330	21,610	19,650	18,210	16,914

(Federal Bureau of Investigation, *supra* note 18).

homicides, which turn out to be similar to the preliminary calculations above. Next we search for, and eliminate, other possible explanations for the apparent impact, such as differences between three-strikes and other states in general crime trends or in preexisting homicide trends. We also search, largely unsuccessfully, for systematic relationships between variations in the state laws and their estimated impacts. Finally, we search for beneficial effects, possible long-term reductions in homicides through incapacitation and short- or long-term reductions in other crimes. There is little evidence that these effects occur.

II. Research Design and Data

The research design is a fixed-effects model with state data over 50 states and 29 years, 1970–98. This technique allows us to control for unobserved heterogeneity by assigning a dummy variable to each state. Dependent variables are crime rates, primarily homicides. The three-strikes laws are represented by dummy or trend variables.[24] The continuous variables are in per capita logarithms. The primary regression is conducted in levels,[25] and the results are replicated in differences. The homicide regression is weighted by state population.[26]

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[24] This is a common design in crime studies. For example, Steven D. Levitt, The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Litigation, 111 Q. J. Econ. 319 (1996); Thomas B. Marvell & Carlisle E. Moody, The Impact of Enhanced Prison Terms for Felonies Committed with Guns, 33 Criminology 247 (1995); John R. Lott, Jr., & David B. Mustard, Crime, Deterrence, and Right-to-Carry Concealed Handguns, 26 J. Legal Stud. 1 (1997). The Lott and Mustard study is conducted at the county level, which they argue is better than the state level because county crime data vary more (their results at the state level, however, are similar to their county-level results). Also, the impact of the law probably varies from individual to individual, from prosecutor to prosecutor, and court to court. On the other hand, there are fewer data problems at the state level. Data are not available at the individual level, and data at the county level are incomplete. The most widely used county-level crime data, compiled by the National Archive of Criminal Justice Data, changed procedures for handling missing data in 1994 just when many three-strikes laws went into effect. Previously most missing data were entered as zero (estimating only when a department filed reports for 6 months or more). After 1994, all missing data are estimated.

[25] The several tests for stationarity in the context of panel data find that homicide is stationary, thus justifying the analysis in levels. See K. S. Im, M. H. Pesaran, & Y. Shin, Testing for Unit Roots in Heterogeneous Panels (Working paper, Cambridge Univ. 1997); Yangru Wu, Are Real Exchange Rates Nonstationary? Evidence from a Panel-Data Test, 28 J. Money, Credit, & Banking 54 (1996); Andrew Levin & Chien-Fu Lin, Unit Root Tests in Panel Data: Asymptotic and Finite-Sample Properties (working paper, Univ. California, San Diego, 1992); and P. C. B. Phillips & H. R. Moon, Linear Regression Limit Theory for Nonstationary Panel Data, 67 Econometrica 1057 (1999).

[26] This weight was determined by the Breusch-Pagan test. It is necessary to mitigate heteroskedasticity caused by greater per capita variation in small states. The weights are less in regressions with other dependent variables, population to the following powers: .6 for total

In the basic analysis the law variable takes the unit value for years after the year the law went into effect. During that year it is a fraction representing the portion of the year the law was in effect.[27] The results change little if it is scored one in years it is in effect for most of the year and zero otherwise. The laws apply only to crimes taking place on or after the effective date, which is usually 2–3 months after enactment.

The dependent variables are crimes reported to the police, as compiled by the Federal Bureau of Investigation.[28] The remaining independent variables are those typically used in state-level studies of crime, where they are described at length.[29] Age structure variables are census data for the percentage of the population ages 15–19, 20–24, and 25–29, which are the ages with highest arrest rates.[30] Economic variables are the unemployment

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violent crime, .7 for rape, .5 for robbery, .6 or assault, .3 for all property crimes, and .2 for prison population.

[27] The 24 laws and effective dates are Arkansas: Ark. Code Ann. § 5-4-501(d)(1) (Michie 1997), effective July 26, 1995; California: Cal. Penal Code § 667 (West Supp. 1998), effective March 7, 1994; Colorado: Colo. Rev. Stat. § 16-13-101 (West 1998), effective May 31, 1994; Connecticut: Conn. Gen. Stat. Ann. § 53a-40 (1994), effective October 1, 1994; Florida: Fla. Stat. Ann. § 775.084 (Michie Supp. 1998), effective October 1, 1995; Georgia: Ga. Code. Ann. § 17-10-7 (West 1997), effective January 1, 1995; Indiana: Ind. Code § 35-50-2-8.5 (West 1998), effective July 1, 1994; Kansas: Kan. Stat. Ann. § 21-4711 (1995), effective July 1, 1994; Louisiana: La. Rev. Stat. Ann. § 15:529.1 (West Supp. 1998), effective August 27, 1994; Maryland: Md. Ann. Code Ann. art. 27, § 643B (1996), effective June 1, 1994; Montana: Mont. Code Ann. § 46-18-219 (Supp. 1998), effective July 1, 1995; Nevada: Nev. Rev. Stat. Ann. § 207.010 (Michie 1997), effective July 1, 1995; New Jersey: N.J. Stat. Ann. § 2C:43-7.1 (West Supp. 1998), effective June 22, 1995; New Mexico: N.M. Stat. Ann. § 31-18-23 (Michie 1994), effective July 1, 1994; North Carolina: N.C. Gen. Stat § 14-7.7 (Supp. 1998), effective May 1, 1994; North Dakota: N.D. Cent. Code § 12.1-32-09 (1997), effective July 1, 1995; Pennsylvania: Pa. Cons. Stat. Ann. § 42-9714 (1998), effective December 10, 1995; South Carolina: S.C. Code Ann. § 17-25-45 (Law. Co-op. Supp. 1998), effective January 1, 1996; Tennessee: Tenn. Code Ann. § 40-35-120 (1997), effective July 1, 1994; Utah: Utah Code Ann. § 76-3-203.5 (1999), effective May 1, 1995; Vermont: Vt. Stat. Ann. tit. 13, § 11a (1998), effective July 1, 1995; Virginia: Va. Code Ann. § 19.2-297.1 (Michie 1995), effective July 1, 1994; Washington: Wash. Rev. Code Ann. § 9.94A.392 (West 1998), effective December 2, 1993; Wisconsin: Wis. Stat. § 939.62 (West 1996), effective April 28, 1994.

[28] Bureau of Justice Statistics Web site through 1996, http://www.ojp.usdoj.gov/bjs/datast.htm; Federal Bureau of Investigation (FBI), *supra* note 18. Homicide data start in 1968 to allow for lagged dependent variables. Not all police agencies report crime data to the FBI, and the FBI estimates complete numbers by extrapolating from the agencies in the state that do report.

[29] For example, Levitt, *supra* note 24; Marvell & Moody, *supra* note 24; Thomas B. Marvell & Carlisle E. Moody, Specification Problems, Police Levels, and Crime Rates, 34 Criminology 609 (1996). The data are posted on the Web at http://faculty.wm.edu/cemood/threestrikes.data.

[30] The data are from the Bureau of the Census Web site, http://www.census.gov/population/estimates/state.

rate, the number employed, real personal income, and the poverty rate.[31] Demographic characteristics of states are the percentage metropolitan and the percentage African-American, for which intracensus years are extrapolated from census data. Prison population, which is measured as of December 31, is the average of the current and prior years' figures.[32] Other variables are year and state dummies and the dependent variable lagged twice.[33] Also, in an alternative analysis, we add trend variables for each state (that is, interactions of a linear trend and state dummies) to control for state trends that depart from nationwide trends.[34]

III.   Results

A.   Basic Findings

The basic finding in Table 1 is that the three-strikes laws are associated with a 10–12 percent short-term increase in homicides and a 23–29 percent long-term increase.[35] The mean number of homicides in the 24 three-strikes states was 537 during the 2 years prior to the laws, which suggests that on average each law caused roughly 60 additional homicides in the short term. This translates to roughly 1,400 in all 24 states (and by not enacting such laws, the remaining 26 states "saved" roughly 1,200 lives). The long-term impact is roughly 3,300 additional homicides per year in the 24 states.

We conducted a wide-ranging search for other possible explanations, and we found nothing that might bring into question these findings. Changing the regression form produces similar results. For example, the elasticities on the law dummy (with or without state trends) are between .09 and .15 and the t-ratios are substantial when the dependent variable is gun homicide (using vital statistics data through 1997), when variables are differenced, when they are not in logarithms, when they are not per capita, when the

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[31] The latter is unavailable for most years in the mid-1970s and is estimated by extrapolation.

[32] Prison population presents a possible simultaneity problem in crime equations, but this is not likely with homicide since only a small fraction of prisoners are murderers. Also when regressing homicide on prison population, Levitt obtained almost the same estimate with ordinary least squares and two-stage least squares. Levitt, *supra* note 24. The results change little when the prison variable is dropped.

[33] Arrest rates are not included because they introduce simultaneity, because reporting is very incomplete, far more so than for crime, and because the FBI makes no attempt to estimate missing data. See Federal Bureau of Investigation, *supra* note 18.

[34] The trend variable, as well as the year and state dummies, is not entered for the first state (Alabama, not a three-strikes state) to prevent collinearity.

[35] The long-term effect is calculated in the usual way: short-run coefficient divided by one minus the sum of the coefficients on the lagged dependent variables. See James D. Hamilton, Time Series Analysis 19–20 (1994).

TABLE 1

REGRESSING HOMICIDE RATES ON THREE-STRIKES LAWS AGGREGATED

	BASIC (1)		WITH TRENDS (2)	
	Coefficient	t	Coefficient	t
Three-strikes laws	.10	5.53	.12	5.74
Population ages 15–19	.04	.31	−.02	.13
Population ages 20–24	.23	1.95	.27	2.21
Population ages 25–29	.16	1.36	.24	1.75
Unemployment rate	−.06	2.17	−.08	2.61
Employed persons	.64	3.40	.47	1.68
Real personal income	−.26	2.18	.00	.02
Poverty rate	−.01	.29	−.01	.37
African-American %	.08	1.68	−.07	.37
Metropolitan %	.02	.17	.65	1.44
Prison population	−.06	2.62	−.18	5.76
$Y(t − 1)$	.42	16.29	.32	11.85
$Y(t − 2)$	.24	9.70	.17	6.40

NOTE.—The regressions use data for 50 states over 29 years. There are 1,357 and 1,308 degrees of freedom, respectively, and the adjusted R^2 are .95 for both. The 24 three-strikes laws are represented by a step dummy variable. Continuous variables are per capita and logged, and regressions are weighted by population. The second regression includes separate trend variables for each state. Coefficients for state dummies, year dummies, and state trends are not shown.

regression is not weighted, when state quadratic trends are added, and when the prison variable is excluded (owing to possible simultaneity). The coefficients are smaller, .08 (t = 3.95) and .09 (3.93) with and without state trends, respectively, and when deleting the three largest states (California, a three-strikes state, New York, and Texas) where homicide declines since 1994 are especially severe.

B. Impacts on Homicide and Other Crimes Compared

Because the theoretical reason for suspecting that the laws cause more homicides does not apply to other crimes,[36] the results reported in Table 1 would be unpersuasive if regressions with other crimes produce similar

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[36] There might be other theoretical reasons for expecting an increase in other crimes. A criminal might avoid crimes that trigger the three-strikes statute and substitute lesser crimes; if the lesser crimes are less lucrative on average, he or she would be tempted to commit more. On the other hand, if a criminal who would otherwise commit a lesser crime that might be covered by the law—say, burglary of an occupied dwelling—might believe that he or she would receive no greater prison term for a more serious (and more lucrative) crime—say, armed robbery—then the temptation would be to substitute the latter.

coefficients on the three-strikes dummy. Since murders often arise out of other violent crimes, especially assaults and robberies,[37] homicide trends are probably associated with trends in these crimes. If other crimes increased more in three-strikes states, it might explain the findings in Table 1. Also, trends in other crimes are rough indicators of the overall success of crime reduction efforts in the different states.

When total reported crime (less homicide) is the dependent variable in the Table 1 regressions, the law coefficients are .00 ($t = .40$) and .01 (.86), respectively. With violent crime (less homicide), they are .01 (1.32) and .02 (1.95).[38] The coefficient on the three-strikes variable in the homicide regression is significantly different from the coefficient in the total crime regression ($F = 17.14$, $p = .0001$) and in the violent crime regression ($F = 13.63$, $p = .0002$).[39] Also, when we added crime as an independent variable in the Table 1 regressions, the law coefficients are .09 (5.42) and .10 (4.95), respectively, with total crime and .08 (4.89) and .10 (4.99) with violent crime. Thus, comparatively more violent crime growth in the three-strikes states can account for at most a small part of the impact estimated in Table 1.

C. Preexisting Trends

Another possible problem is that the three-strikes dummy might capture preexisting trends rather than changes that occurred when the law went into effect. There is evidence that some of the apparent impact of the three-strikes laws in Table 1 might be due to the fact that homicides in three-strikes states grew more (or declined less) than elsewhere immediately before the laws went into effect. For example, homicide rates in three-strikes states increased 3.2 percent between 1992 and 1993 and 4.4 percent between 1990 and 1993, compared to .9 percent and −2.0 percent for the other 26 states.

For several reasons, however, these dissimilar trends have little impact on the results. (1) The difference is much less than the differences between three-strikes and other states in pre- to postlaw changes presented earlier.[40]

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[37] Cook, *supra* note 11; Keith D. Harries, Serious Violence: Patterns of Homicide and Assault in America (1990).

[38] See also Table 3.

[39] The *F*-test uses the STEST option in the SYSLIN procedure in SAS (SAS Institute, SAS/ETS User's Guide, Version 6 (2d ed. 1993)). The *F*-values given are for regressions with state trends; they are larger without them.

[40] See the text following note 23 *supra*.

(2) The comparison with other crimes, discussed above, eliminates the possibility that the results are due to differences in general crime trends between three-strikes and other states. (3) As noted above, the results do not diminish when adding separate linear and quadratic trend variables for each state, which control for state differences in trends, especially differences in later years. (4) Similarly, to control for the possibility that homicide trends in three-strikes states during the crucial years of the 1990s might differ from trends in other states, we added state-level trend variables that are counters for 1990–98 and zero in earlier years. The coefficients on the three-strikes variable are .10 both with and without state trends. (5) We added leads of the law dummy to determine whether the apparent impact actually started in earlier years. Although coefficients on the leads are positive, the coefficient on the current-year law dummy is considerably larger.[41] That is, although homicide rates rose more (or declined less) in three-strikes states than in other states before the laws, the differences became much greater right after laws went into effect.

A similar potential problem is simultaneity. States might pass three-strikes laws because their crime rates are higher or rising faster than in other states, and any subsequent increase could be simply a continuation of this trend (and any subsequent decline could be interpreted as a regression to the mean, a natural tendency to return to basic trends). It is unlikely that legislatures are influenced by current-year homicide rates, because the processes of legislative enactment and compiling crime statistics take time. A lagged impact is possible, and we tested this by conducting a probit regression with the law dummy on the left and with two lags of homicide plus the 10 remaining regressors listed in Table 1 on the right. The regressions are conducted in both levels and differences and with and without state trends. The lags of homicide are far from significant and are as likely to be negative as positive, so there is no evidence of homicides causing the passage of three-strikes laws.

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[41] This procedure is best conducted in differences because it encounters collinearity problems in levels. Putting in three leads, current year, and three lags of the three-strikes pulse dummy produces the following coefficients:

	Lead 3	Lead 2	Lead 1	Current	Lag 1	Lag 2	Lag 3
Coefficient	.04	.04	.05	.09	.10	.02	.05
t-Ratio	1.20	1.19	1.43	2.65	3.03	.60	1.25

The result is virtually the same when state trends are added. Because coefficients on the lagged dependent variables are negative (summing to $-.55$), the long-term coefficients are smaller, about two-thirds of these figures.

TABLE 2

REGRESSING HOMICIDE RATES ON INDIVIDUAL LAWS

	BASIC (1)		WITH TRENDS (2)	
	Coefficient	t	Coefficient	t
Arkansas	.08	.84	.04	.36
California (S)	.03	1.07	−.03	.77
Colorado	.08	1.13	.14	1.53
Connecticut (W)	.19	2.37	.06	.65
Florida (W)	.04	.80	.16	3.01
Georgia (S)	.00	.05	.10	1.59
Indiana (W)	.16	2.79	.15	1.99
Kansas (W)	.20	2.36	.25	2.30
Louisiana (W)	.12	1.76	.05	.60
Maryland (W)	.21	3.38	.21	2.57
Montana	.27	1.69	.46	2.33
Nevada (W)	.20	1.62	.30	1.96
New Jersey	.12	2.11	.20	2.95
New Mexico	.18	1.64	.30	2.19
North Carolina (W)	.08	1.67	.15	2.25
North Dakota (W)	.42	2.22	.81	3.65
Pennsylvania	.19	4.03	.21	3.93
South Carolina (W)	.15	1.84	.20	2.17
Tennessee	.14	2.24	.15	1.97
Utah (W)	.01	.11	.13	1.00
Vermont (W)	.29	.96	.30	.92
Virginia	.10	1.79	.19	2.79
Washington	.09	1.69	.09	1.26
Wisconsin	.18	2.92	.12	1.54
Means:				
24 coefficients	.15	7.57	.20	5.82
Without North Dakota				
and Vermont	.12	8.32	.15	7.39

NOTE.—These figures are the coefficients on variables representing individual state laws, which are dummies that scored one after the law in the particular state and zero elsewhere. The two regressions are otherwise the same as the Table 1 regressions, and the results for other variables are similar. The F-values for significance of the 24 law dummies taken as a group are 2.62 and 3.44 (both $p = .0001$), respectively. The S refers to laws that are especially severe, and the W refers to laws that are probably weaker than most.

D.   Individual State Results

It is possible that the effects of the three-strikes laws vary across states. In Table 2 separate dummies for each three-strikes state (each dummy is zero for all observations except for the postlaw period in the state) are substituted for the aggregate law dummies. In both regressions 23 of 24 coefficients are nonzero and positive; nine are significant in the first and 13 in

the second.[42] The coefficient means,[43] .15 and .20, are larger than the law coefficients in Table 1 mainly because a few small states have large coefficients.[44] These states have little impact in Table 1 because the regressions are weighted by population.

It is uncertain whether there is any systematic relationship between the coefficients in Table 2 and characteristics of the laws. With only 24 laws, the sample size is too small for meaningful statistical analysis. The differences are likely to be due to the statistical uncertainty in the regression analysis. In fact, only five coefficients in Table 2 are outside 2 standard errors of the mean, not much more than the 2.3 expected by chance (and 12 outside 1 standard error, compared to 15.2 expected by chance).

Nevertheless, we have attempted to categorize the laws according to how severe they appear on the surface and how often they are applied.[45] As stated above, the typical three-strikes law states that upon the third conviction of a serious violent crime, the defendant is given a life sentence without possibility of release for at least several decades. The California law is the strongest because it applies to any felony conviction (but the priors have to be serious violent felonies). The Georgia law is also especially severe because it applies when there is only one prior. Judging from their wording, laws in 12 states appear to be weaker. Four states (Louisiana, Maryland, North Dakota, and South Carolina) already had three-strikes laws on the books and strengthened them in 1994 or 1995 by expanding the types of crimes covered. Eight laws (in Connecticut, Florida, Indiana, Kansas, Ne-

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[42] We also conducted a ''reverse'' analysis of the impact of individual states by creating step dummies beginning in 1995 for the 26 states without three-strikes laws. The majority of the coefficients are negative, as would be expected. The coefficient in New York, where homicides declined 62 percent between 1993 and 1998, is very large. Without that state, the coefficients on the three-strikes law dummy in Table 1 are .07 (t-ratio = 3.90) and .08 (3.80), respectively.

[43] The t-ratios for the means are based on the standard error of the mean, as recommended in M. Hashem Pesaran & Ron Smith, Estimating Long-Run Relations from Dynamic Heterogeneous Panels, 68 J. Econometrics 78 (1995). A second procedure for estimating the standard error when averaging coefficients, which generally leads to larger t-ratios, is to divide the mean standard error of the coefficients by the square root of the number of coefficients. See Badi H. Baltagi & James M. Griffin, Pooled Estimators vs. Their Heterogeneous Counterparts in the Context of Dynamic Demand for Gasoline, 77 J. Econometrics 303 (1997). Both procedures assume independence of coefficients. We do not conduct separate time-series regressions for each unit, as Pesaran & Smith and Baltagi & Griffin do, because the number of years is limited and because the lack of year effects might bias the results.

[44] North Dakota is an extreme outlier. Its homicides numbered 12, 11, 1, 6, 14, 6, and 7 in 1992–98, and its law went into effect in mid-1995. Deleting the state does not change the results in Table 1.

[45] The laws in question are given in note 27 *supra*. See also Dickey & Hollenhorst, *supra* note 1; Clark, Austin, & Henry, *supra* note 1.

vada, North Carolina, North Dakota, and Vermont) state that prosecutors or judges have discretion to apply the law. In Utah and Vermont the enhanced penalties for three-strikers are comparatively mild. Note that the North Dakota and Vermont laws fall into two categories. Overall, however, it is difficult to determine how forceful the laws are in practice; the greatest uncertainty is the extent of discretion exercised by judges and prosecutors, since informal procedures or judicial interpretation can provide discretion not evident in the laws themselves.

Another rough indication of how much the laws are used is the number of defendants sentenced under them. A survey by the Campaign for an Effective Crime Policy[46] found that few defendants had been convicted during the first several years. The most by far was 4,468 through July 1998 in California, or 142 per million population. Nevada had 164, or 112 per million population. Florida, Georgia, and Washington also made substantial use of the laws with 8, 8, and 23 per million population. Most of the remaining states probably had negligible use, although the survey is incomplete. It also notes that the impact might not be limited to the number of three-strikes convictions because prosecutors use the laws as leverage to obtain pleas to more severe sentences than would otherwise have been obtained.[47]

The most severe laws seem to have the least impact in Table 2, possibly because additional murders conducted to evade capture are counterbalanced by reductions due to deterrence and incapacitation. The most severe law, in California, is associated with the least amount of homicide increase (taking into account both regressions in Table 2) and the second most severe, in Georgia, with the second least amount. Also, the coefficients for three other high-use states, Florida, Utah, and Washington, are comparatively low. (Coefficients for California, Georgia, and Florida are the only ones outside 2 standard errors in the first regression, and Florida is one of two outside in the second.) Beyond that, however, there seems to be no relationship between severity and impacts on homicide. The median coefficient for weak laws, .15, is the same as that for all laws.

E.   Deterrence and Incapacitation

The results above are incomplete without exploring possible crime reduction effects through deterrence and incapacitation, the original purpose of the three-strikes laws.[48] If these goals are accomplished, for homicide or

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[46] Dickey & Hollenhorst, *supra* note 1.

[47] *Id.* at 9.

[48] There have apparently been only two studies of the crime reduction impact of three-strikes laws, both in California. They reach different (but not inconsistent) results. The first (Peter W. Greenwood *et al.,* Estimated Benefits and Costs of California's New Mandatory Sentencing Law, in Shichor & Sechrest eds., *supra* note 3, at 53–89) attempts to model inca-

other crimes, the homicide-promoting impacts seen in Tables 1 and 2 might be considered acceptable costs.

Deterrent impacts are likely to occur immediately,[49] although there may be some delay if criminals do not learn about the laws right away. Incapacitation impacts are probably delayed because almost all defendants convicted of serious violent crimes, with two similar convictions on their record, would receive prison terms before the law, probably lengthy terms.[50] The incremental incapacitation effects would occur only after criminals convicted under the laws would otherwise have been released.

As a measure of deterrence we use the three-strikes law variables. This is a useful model of deterrence only in regressions with crimes other than homicide. As a crude measure of incapacitation we use a linear trend starting 1 year after the law and continuing through 1998. This assumes that, in the absence of the law, very few defendants would have escaped prison sentences,[51] so that the incapacitation effect grows over time. (The impact would eventually level off, but our data end in 1998.) We also conduct several analyses with a variety of alternative variables to model any incapacitation impact, and the results do not change. These alternatives include a trend starting in the year of the law, in the second year after the law, or in

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pacitation effects by estimating how much longer prisoners would be in prison because of the law and how many crimes these prisoners would have committed if on the street. The result is a sizeable reduction, over 300,000 crimes per year after the fifth year of the law. The study also predicts that the law would cause prison populations to more than double in that period (in fact, they increased by 38 percent from 1993 to 1998). This study ignores possible deterrent impacts of the law.

The second study (Lisa Stolzenberg & Stewart J. D'Alessio, ''Three Strikes and You're Out'': The Impact of California's New Mandatory Sentencing Law on Serious Crime Rates, 43 Crime & Delinquency 457 (1977)), on the other hand, ignores possible incapacitation effects. It uses step dummies in monthly time-series analyses in 10 California cities to estimate the impact over the first 21 months of the law. The study finds no crime reduction, but it would be unlikely to pick up any incapacitation impact, which would not take effect immediately, as is assumed by the construction of the dummy variable.

[49] Kessler & Levitt, *supra* note 21.

[50] It is possible, however, that some incapacitation impact is immediate. Because three-strikes laws are an expression of public and legislative sentiment, they might cause parole boards and prison authorities to release fewer prisoners having several convictions for violent crimes. Also, prosecutors might use threats of the three-strikes penalties to persuade defendants in weak cases to accept prison terms in plea agreements that they would otherwise not accept. In California the law applies to any felony conviction, so some three-strikes defendants might have escaped prison sentences before the law.

[51] Kessler & Levitt, *supra* note 21. The median time served was 25 months (and the mean was 36 months) for state prisoners convicted of violent crimes and released in 1992. Craig Perkins, National Corrections Reporting Program, 1992, at 40 (U.S. Dep't Justice 1994). When attempting to model the impact of the California three-strikes law (effective March 7, 1994), Greenwood and his colleagues estimated that it would have almost no crime reduction impact owing to incapacitation in 1994 and progressively larger impacts, with a convex trend, through 1999 when the impact levels off (Greenwood *et al., supra* note 48).

the third year after the law; others are the postlaw trend logged and separate step dummies for the year of the law and each of the 4 following years.

In the first row of Table 3 we reestimated the homicide regressions in Tables 1 and 2 with the addition of postlaw trends. (Regressions in Table 3 contain individual state trend variables; the results are essentially the same without the trends.) If there is an incapacitation impact on homicides, it would be confounded with the step dummy in Tables 1 and 2, and in Table 3 the postlaw trend coefficients would be negative while the step dummy would probably become larger. There is little evidence that such a pattern occurs. The step dummy coefficient is larger, but only slightly so, with both aggregate and individual state three-strikes law variables. The coefficient on the postlaw trend variable is negative, but not significant, in both instances. These small effects disappear, moreover, when North Dakota and Vermont are dropped from analysis with individual law variables (Table 3, regression (3)).

We next searched for possible crime reduction impacts on the other six Uniform Crime Reports crimes. Here again a step dummy estimates the net impact of deterrence and any crime-enhancing impact of the laws.[52] The latter are expected to be smaller than for homicide. The step dummy coefficients are positive (Table 3), although much smaller than in the homicide regressions. At the least, there is no evidence of a deterrent impact. Also, we find no evidence of a significant incapacitation effect with respect to these crimes. Coefficients on the postlaw trends are usually negative, but they are nonsignificant and small in size.

If the three-strikes laws have a significant incapacitation effect, we would expect that they would increase prison population. We test for this effect by regressing prison population on the three-strikes law variables in the same manner as in the crime regressions (Table 3). As expected, the three-strikes laws have no discernable immediate impact, as measured by the step dummy (Table 3). If the three-strikes laws produce longer prison terms, the impact might appear in the postlaw trend, and one might expect the coefficient to be a mirror image of the coefficients in the crime regressions. (However, the state might balance the longer terms for dangerous criminals by reducing the terms for others. The net effect on crime in this case would be difficult to predict.) The coefficients on the postlaw trend are indeed positive, though small and not significant (Table 3), which is consistent with the slight evidence of incapacitation impacts on crimes in Table 3. We cannot rule out the possibility, however, that there might be an impact that is delayed beyond 1998, the last year of data here, especially if most of the

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[52] Possible reasons why the laws might increase crimes other than homicide are given in note 36 *supra*.

TABLE 3

REGRESSING UNIFORM CRIME REPORTS CRIMES ON THREE-STRIKES LAW VARIABLES AND PRISON POPULATION

	Aggregate Variables (1)				State-Level Variables							
					All 24 States (2)				Without North Dakota and Vermont (3)			
	Step Dummy		Postlaw Trend		Step Dummy		Postlaw Trend		Step Dummy		Postlaw Trend	
	Coefficient	t	Coefficient	t	Mean Coefficient	t	Mean Coefficient	t	Mean Coefficient	t	Mean Coefficient	t
Homicide	.14	5.50	−.017	1.55	.22	3.49	−.044	1.12	.12	5.34	.008	1.10
Rape	.04	.98	−.003	.37	.05	4.06	−.016	1.42	.04	3.74	−.014	1.86
Robbery	.02	.80	−.007	.68	.01	.46	.048	1.05	.02	1.49	−.004	.94
Assault	.05	2.56	−.013	1.49	.07	2.09	−.058	1.29	.04	1.77	−.011	1.15
Burglary	.02	1.25	.000	.07	.02	2.48	−.018	.99	.02	2.09	−.002	.76
Larceny	.00	.04	.000	.10	.01	.86	−.020	1.10	.01	.85	−.003	.84
Auto	.03	1.60	−.012	1.31	.03	1.76	.011	.50	.03	1.80	−.009	1.40
Prison population	−.00	.31	.007	1.35	−.01	.55	.008	1.44	−.00	.13	.003	.96

NOTE.—Summary of the results from 24 regressions, three for each dependent variable listed on the left. The first two sets of regressions are the same as the second regression in Table 1, except that postlaw trends are added, the dependent variables and weights differ, and the prison population regression does not have that variable on the right. The second set of regressions is with individual state dummy variables and individual postlaw trend variables for each three-strikes state. The third set of regressions is the same as the second set except that North Dakota and Vermont are excluded (the first regressions change little without them).

worst three-strikes criminals would have prison terms of at least 5 years even without the laws.

IV.   Conclusion

Because three-strikes laws call for harsh prison terms for criminals with prior convictions, criminals who fear the laws because they have two strikes would be expected to take extra steps to avoid punishment. One such step might be to murder those who can aid in their capture and conviction. The benefit of removing opposition and eliminating potential witnesses, however, must be weighed against the heightened level of effort by the authorities to solve a homicide as opposed to a lesser crime. Because homicides are relatively rare, even if the balance is tipped toward eliminating victims and witnesses in only a very small portion of violent crimes, homicides can still increase noticeably.

Our basic finding is that three-strikes laws produce a 10–12 percent short-term increase in homicides, which implies that roughly .06 percent of violent crimes result in homicides that would not have occurred without the laws (assuming none are multiple homicides). The long-term impact is a 23–29 percent increase, which implies additional homicides in roughly .15 percent of violent crimes. In the absence of three-strikes laws, the homicide rate would initially be 7 percent lower, and the long-run impact would be a 17 percent reduction in homicides. This translates into a long-run impact of 3,300 additional homicides each year. Using $3.2 million dollars as the value of life,[53] we find that three-strikes laws have a long-run social cost of $11 billion per year.

The analysis becomes more complex when possible deterrent and incapacitation effects are taken into account, but in the end the basic results are not affected. We found almost no evidence that the laws have crime reduction impacts that might compensate for the additional homicides, although it is possible that there is an incapacitation impact delayed many years. The absence of a noticeable incapacitation impact is supported by the fact that the three-strikes laws have resulted in little or no prison population growth. Given their unintended consequences in terms of human lives, we see no justification for three-strikes laws.

[53] Levitt, *supra* note 24.