Unintended Consequences of Politically Popular Sentencing Policy

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UNINTENDED CONSEQUENCES OF POLITICALLY POPULAR SENTENCING POLICY: THE HOMICIDE PROMOTING EFFECTS OF "THREE STRIKES" IN U.S. CITIES (1980–1999)*

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Research Summary:

Using panel data from 188 large cities during 1980–1999, we examined the possible homicide promoting effects of "three-strikes" laws. Results indicated that cities in states with three-strikes laws experienced short-term increases in homicide rates of 13% to 14% and long-term increases of 16% to 24% compared with cities in states without the laws.

Policy Implications:

Our results emphasize the fact that rarely are the possible unintended negative consequences of policy directives considered and point to the need for policy makers to consider both intended and unintended consequences of policy directives before the directives are codified.

KEYWORDS: Three-strikes laws, Homicide, Sentencing

> Intuitively, most people, and lately most legislators, presume that lengthy imprisonment, determinate sentencing, mandatory minimum sentencing, and severe habitual offender laws offer safety along with retribution. But as criminologists and other social scientists have often shown, intuition alone isn't a sound basis for judging what will or won't work, at what cost, and with what *side effects* [emphasis added] (Skolnick, 1995:3).

Facing intense public pressure to address the problem of high violent crime rates in the late 1980s and early 1990s, policy makers responded by strengthening existing laws targeting repeat offenders. Between 1993 and 1996, a total of 25 states and the federal government enacted what are

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known as "three strikes and you're out" laws, which mandated longer prison terms for offenders with prior convictions who were subsequently convicted of serious crimes like murder, rape, aggravated robbery, aggravated assault, and kidnapping (Austin and Irwin, 2001; Zimring, 2001).

The rationale for the laws is grounded in the "expected utility" principle of deterrence theory, which states that criminals calculate the potential benefits and costs of their actions and are less likely to commit crime when expected costs outweigh the rewards (e.g., Becker, 1968; Ehrlich, 1973). Enhanced terms of incarceration mandated by these laws thus are assumed to raise the expected costs (or lower the expected benefit) for offenders subject to them, which, in theory, would result in decreased levels of crime.

The problem is that three-strikes laws, like other initiatives aimed at controlling crime, may result in significant unintended negative consequences (Merton, 1936) for jurisdictions adopting them. With few exceptions (e.g., DiIulio, 1994), criminologists and other experts on sentencing have identified possible negative side effects of three-strikes laws, including the drain on fiscal resources that would result from the increased costs associated with incarcerating aging inmates (King and Mauer, 2001; Shichor and Sechrest, 1996; Walker, 2001); the costs associated with building and operating new prisons to hold additional inmates sentenced under these laws (Austin, 1996; Greenwood et al., 1996); potential difficulties surrounding correctional control of inmates serving life sentences (Austin and Irwin, 2001; Flanagan et al., 1998); and the impact on courts as more defendants facing a "third strike" charge demand jury trials (Cushman, 1996; Patch, 1998). Previous research on repeat-offender laws has also highlighted the racial disparity in their application that has resulted in the increased incarceration of minorities (Crawford et al., 1998; Economic and Demographic Research, 1992) and the laws' inability to reduce crime rates through deterrence or incapacitation (Kovandzic, 2001; Macallair and Males, 1999; Marvell and Moody, 2001; Stolzenberg and D'Alessio, 1997; but see Shepherd, 2002). Perhaps most alarming, however, is the recent finding by Marvell and Moody (2001) that homicide rates increase following passage of three-strikes laws.

According to Marvell and Moody (2001), criminals facing lengthy prison terms upon conviction for a third strike may take steps to try and reduce the chances of being caught, prosecuted, and convicted by changing their *modus operandi*. That is, during the commission of an ordinarily nonlethal offense, an offender may decide to kill victims, witnesses, or police officers to reduce the chance of apprehension.[1] As Marvell and Moody (2001:91)

1. Recent evidence lends supports to the claim that offenders may respond to three-strikes laws with increased lethality. For example, in a survey of 500 juveniles

suggested, "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" (see also Stigler, 1970). In effect, an offender with "two strikes" faces a term of incarceration for a "third strike" that would parallel the term of incarceration the offender would face upon being convicted of homicide. Circumstances such as these, according to Marvell and Moody (2001:92), result in little marginal deterrence for offenders, creating little dissuasion from committing the homicide if doing so would lessen the chances of resistance by victims, arrest by the police, or conviction at trial.[2]

Accepting the above logic and assuming the circumstances described above are indeed rare, their impact on homicide rates can still be relatively large (see Marvell and Moody, 2001:92–93). As Marvell and Moody (2001) demonstrated, if one assumes that the above circumstances are present in only 1 of every 1,000 violent crimes, because the ratio of homicide to all other violent crimes is approximately .006, additional homicides created by the law would increase the total number of homicides in a three-strikes state by approximately 17%.

To test their hypothesis that three-strikes laws increase homicide, Marvell and Moody (2001) evaluated three-strikes laws in 24 states using state-level panel data for the period 1970–1998. The three-strikes laws were entered as dichotomous dummy variables scored 1 starting the year after a law went into effect, and 0 otherwise. Control variables in their model included age structure, population heterogeneity, economic trends, prison population, year and state dummies, and the dependent variable lagged twice (to correct for autocorrelation). The continuous variables were divided by population and transformed to natural logs to reduce the impact of outliers. The primary models were estimated in levels with regressions weighted by state population to mitigate heteroscedasticity problems. They conducted numerous alternative analyses, including

housed in a residential lock-up facility outside Los Angeles, Schafer (1999) found that 54% of the juveniles indicated they "would kill or would probably kill" witnesses or police officers to avoid a life sentence that would accompany a third strike under California law. This figure increased to 65% among self-identified gang members. In addition, Moody et al. (2000) found that three-strikes laws increased the number of police officers killed in the line of duty by 25%. This translates to one additional police officer murdered every two and a half years in the average three-strikes state.

2. As Marvell and Moody (2001) note, this reasoning assumes the likelihood of arrest for the current offense is equal to the likelihood of arrest for homicide. However, clearance rates for homicide are substantially higher than are clearance rates for other forms of serious violence, and far greater than clearance rates for property crimes. Therefore, the criminal's self-interest would preclude the commission of a homicide during another offense.

entering separate trend variables for each state (to control for state trends that departed from nationwide trends), using first-differences of all variables (to ensure stationarity), not weighting the regressions, not including prison population (due to potential simultaneity problems), and using separate dummy variables for each three-strikes state. In general, they found that three-strikes laws increased homicide rates by 10% to 12% in the short term and by 23% to 29% in the long term, with the impact occurring in almost all 24 states with three-strikes laws. Results for the remaining crimes (i.e., rape, robbery, assault, burglary, larceny, and auto-theft) were generally nonsignificant and were often in the unexpected positive direction. Because there is virtually no evidence that the laws have any crime reduction impacts through deterrence or incapacitation that might compensate for the additional homicides, the authors called for the repeal of three-strikes laws.

The finding that laws designed to decrease crime actually increased homicide, arguably the most serious of all crime, underscores the need for additional research on the potential lethal impact of three-strikes laws. Given the enormity of Marvell and Moody's (2001) findings and their implications for criminal justice policy, we decided to revisit the claim that three-strikes laws actually increase homicide. In the following sections of the paper, the data and methods we used in the study are presented, followed by the results and discussion. We conclude the paper by presenting the policy implications of our results.

DATA AND METHODS

RESEARCH DESIGN

To reexamine the potential homicide-promoting effects of three-strikes laws, we followed procedures outlined by Marvell and Moody (2001) in their analysis. We used a multiple time series (MTS) research design, pooling annual data for the period 1980 to 1999 for 188 cities with a population of 100,000 or more in 1990.[3] As we later explain in more detail, our basic model estimates the average overall change in homicide rates following the passage of a three-strikes law. Homicide rates during periods in which three-strikes laws were in effect are compared with rates during periods without the laws. If the laws were responsible for increasing homicide, the number of homicides should increase in the post-intervention period. To further strengthen the basis for causal inference, we compared

3. Despite having a population greater than 100,000 in 1990, a decision was made to drop the following cities because of severe reporting problems for homicide: Moreno Valley, CA; Rancho Cucamonga, CA; Santa Clarita, CA; Overland Park, KS; Kansas City, KS; Cedar Rapids, IA; and Lowell, MA. In all, we have 3,760 observations, except that homicide data are missing for 53 observations.

the estimated impacts of three-strikes laws on homicides with the impacts on other types of crimes. This additional analysis controls for missing variables that could be confounded with the passage of three-strikes laws. Another causal variable would be confounded with the law only if it influenced homicide and other crimes differently, and if it changed markedly following the passage of three-strike laws.

The MTS design, which continues to gain in popularity for evaluating criminal justice policy (e.g., Levitt, 1996; Lott and Mustard, 1997, Ludwig and Cook, 2000; Marvell and Moody, 1994, 1995; McDowall et al., 2000), has distinct advantages over more commonly used time-series or cross-sectional designs. Campbell and Stanley (1963:55–57) considered the MTS design one of the most powerful designs for social science evaluation when a true experiment is not feasible or practical, as is the present case. The most important advantage of the MTS design is the ability to enter proxy variables (discussed below) for unknown factors (i.e., omitted control variables) that could cause homicide rates to vary over time and across cities. Other advantages of the MTS design include the ability to evaluate many separate policy changes simultaneously, with each city acting as a control group for the others. Finally, the MTS design provides for a very large sample size, which enhances statistical power and efficiency, while allowing one to enter numerous control variables and still retain a large number of degrees of freedom.

Our decision to use cities as the unit of analysis, as opposed to SMSAs or counties, was based on two justifications.[4] First, cities generally exhibit greater per-capita variation in homicide rates than do SMSAs or counties. Second, fewer data problems are encountered with city-level data, compared with SMSAs or counties. The Federal Bureau of Investigation (FBI) only includes crime counts for cities in the Uniform Crime Report (UCR) if the individual law enforcement agency responsible for that jurisdiction submitted 12 complete monthly reports. Consequently, if crime data for a given city in any year are omitted from the UCR report, it is safe to assume that the law enforcement agency did not submit complete crime counts for the entire year. In such cases, one can either estimate the missing data or score it as "missing." On the other hand, county-level crime data, compiled by the National Archive of Criminal Justice Data (NACJD), are generally less useful for conducting criminal justice policy evaluations using longitudinal designs due to the imputation algorithm

4. Marvell and Moody (2001) use state-level panel data, which they argue is the preferred unit of analysis because of missing data problems at lower levels of aggregation. Although it is true that state-level data have less missing data problems than do lower levels of aggregation (e.g., counties, cities, SMSAs), state-level data have the drawback of ignoring heterogeneity in states with regard to homicide rates and variables affecting homicide rates.

used by NACJD for dealing with incomplete agency reporting before 1994.[5] UCR county-level files created by NACJD use the original FBI agency-level tapes to sum the crime counts from individual law enforcement agencies within the county. Prior to 1994, however, any individual law enforcement agency submitting less than 6 monthly reports was coded as "zero" in the county-level aggregation. If, however, the agency submitted 6 to 11 monthly reports, the crime data were weighted to produce 12 monthly equivalents. To illustrate, data from an agency reporting 6 months of crime figures would be multiplied by 2 (12/6). Thus, if an agency reported 5 homicides a month, and submitted 5 reports in one year and 6 the next, then homicides would have appeared to increase in the county from 0 to 60, even if there was no actual change in the number of homicides. Unfortunately, county-level data files do not indicate whether county totals are based on incomplete reporting due to agencies failing to report for all 12 months or complete reporting for all law enforcement agencies in the county. As a result, it is impossible to determine whether changes in a county's homicide rate over time are real or statistical artifacts due to the problem described above. For these reasons, we believe that cities are the preferred unit of analysis for revisiting the three-strikes law and homicide question.

THREE-STRIKES LAWS

The three-strikes laws were measured using a dichotomous dummy variable equal to 1 when the city has a three-strikes law in effect and equal to 0 when there is no such law in effect. In the year a three-strikes law went into effect, the dummy variable is scored as the portion of the year remaining after the law's effective date.[6] For cities in states without three-strikes laws the dummy variable is coded zero for all years. Because the analysis includes year dummies (discussed below), the non-three-strikes cities act as controls. Changes in homicide rates in a city when a three-strikes law goes into effect are compared with changes in homicide rates in all other cities where no such change was made. Inferences about the effect of three-strikes laws on homicide rates are based on the sign and statistical significance of the estimated coefficient of the three-strikes dummy variable. A positive and significant estimate suggests that homicide rates have grown faster (or declined at slower rate) in three-strike cities compared

5. Beginning in 1994, NACJD implemented a new imputation procedure to adjust for incomplete reporting by individual law enforcement agencies.

6. The laws' effective dates were taken from Marvell and Moody (2001). Of the 188 cities with populations greater than 100,000 in 1990, 110 resided in states that passed three-strikes laws between 1993 and 1996. Because Montana, Vermont, and South Carolina did not have cities with populations greater than 100,000 in 1990, their three-strikes laws were not evaluated.

with cities without the laws, whereas a negative and significant coefficient points to the contrary.

HOMICIDE RATES

Homicide data were obtained from the FBI's UCRs (FBI, 1979–2000).[7] The number of homicides, like all continuous variables used at present, were divided by total population figures and logged. Following the recommendation of Marvell and Moody (2001), a 1 is added to all homicide counts before computing rates per 100,000 and logging because homicide observations are 0 for some cities in the study period. Homicide data were missing for 53 observations and were scored as missing data.

SPECIFIC CONTROL VARIABLES

In addition to proxy variables for unknown factors, we included seven specific control variables that prior macrolevel crime research and theory suggests are important correlates of homicide. The decision of which control variables to include in the homicide models was based on a review of previous macrolevel studies linking homicide rates to the structural characteristics of ecological units (see Kovandzic et al., 1998; Land et al., 1990, and the studies reviewed therein). The specific control variables included in the homicide models were percent of persons that are African-American, percent of the population ages 18 to 24, percent of female-headed households, percent of the population living below the poverty line, per-capita income, percent of the population living alone, and prison population (most of these variables account for causal processes emphasized by strain/deprivation, social disorganization, and opportunity/ routine activities theories). Data for percent African-American, percent persons 18 to 24, percent female-headed households, and percent persons living below poverty line data for 1980 and 1990 were obtained from the U.S. Bureau of the Census, *County and City Data Books* (U.S. Bureau of the Census, 1983, 1994). Year 2000 data were obtained from the U.S. Bureau of the Census website (http://www.census.gov) using American Fact Finder. These measures are only available for decennial census years, and we estimated data between decennial census years via linear interpolation. Given the small changes in these variables between decennial census years, a linear trend was assumed and considered justified. Income data for 1980 to 1999 were obtained from the U.S. Bureau of Economic Analysis website (http://www.bea.doc.gov). Income data are county-level estimates, and we used these values as imperfect substitutes for city-level

7. Homicide data for 1978 and 1979 were added in order to avoid losing the first two years of data in the autocorrelation correction.

income. Personal income data were converted from a current dollar estimate to a constant-dollar 1967 basis by dividing personal income by the consumer price index (CPI).

The prison population variable is the number of inmates sentenced to state institutions for more than a year (year-end estimates), available annually at the state level. We used these values as proxies for city-level imprisonment. State-level prison population data were obtained from the Bureau of Justice Statistics website (http://www.ojp.usdoj.gov/bjs/). Similar to Marvell and Moody (2001), the prison population value is an average of the current year and the prior year.[8]

ANALYTIC METHODS

Similar to Marvell and Moody (2001), we used the standard fixed-effects approach to analyze panel data. The fixed-effects model requires adding a dummy variable for each city and year, except the first year and city to avoid perfect collinearity (see Pindyck and Rubinfeld, 1991:224–226). The year and city dummies are an integral part of the fixed-effects approach because they partially control for unobserved or difficult-to-measure factors not entered in the analysis that influence homicide rates. The city dummies control for any unobserved factors that were approximately stable within cities during the study period that caused homicide rates to differ among cities. Examples of these factors might include economic deprivation, criminal gun ownership, and law enforcement strategies. The year dummies controlled for nationwide events that could raise or lower homicide rates in a given year across all cities such as the 1994 Crime Control and Law Enforcement Act, which contained several major crime-reduction programs (e.g., truth-in-sentencing, federal three-strikes law, funds for 100,000 new officers, expansion of death penalty, ban on possession of guns by juveniles, enhanced penalties for drug offenses and using firearms in crimes).

In addition, we followed Marvell and Moody's (2001) recommendation of adding separate linear time trend variables for each city in alternate homicide regressions.[9] The linear time trend variables controlled for trends in a city that departed from the national trends captured by the

8. We realize that some readers might be uncomfortable with including prison population in the homicide regressions because of potential simultaneity bias; that is, homicide rates might affect prison population levels and be affected by them. As Marvell and Moody (2001) note, however, this is unlikely to be the case because murderers make up only a tiny proportion of the overall prison population. More importantly, deleting prison population from the homicide regressions has no impact on the results presented in Table 1.

9. Each city has its own trend variable, which equals 1 in 1980, 2 in 1981, and 20 in 1999.

year dummies. For example, suppose that drug and gang activities have grown faster in cities with three-strikes laws relative to cities without the laws. The coefficient on the three-strikes dummy variable will reveal that homicide rates have grown faster in three-strike cities, even if three-strike laws have no positive effect on homicide rates. Another important reason for including the trend variables is because without them, the coefficient on the three-strikes dummy variable would simply have measured whether homicide rates were 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.

All of the continuous variables were expressed as natural logs to reduce the impact of outliers and were divided by population so that large cities would not dominate the results.[10] This procedure also allowed coefficients to be interpreted as elasticities—the percent change in the dependent variable expected from a 1% change in the independent variable (Greene, 1993). Heteroscedasticity was detected using the Breusch-Pagan test, mainly because variation in homicide rates was greater over time among the smaller cities (Greene, 1993). To avoid inefficient and biased estimated variances for the parameter estimates, we weighted the homicide regressions by size of city population. Panel unit root tests (Levin and Lin, 1992; Wu, 1996) showed that the homicide time series were stationary in their means. The fact that the homicide series had a constant mean suggested that the analysis be conducted in levels and not first-differences. Autocorrelation was mitigated by including lagged dependent variables (Hendry, 1995), which have the added benefit of controlling for omitted lagged effects. Examination of collinearity diagnostics developed by Belsley, et al. (1980) revealed no damaging collinearity problems for the three-strikes dummy variable. Although there were multicollinearity problems among the proxy variables, this did not impact the results for the three-strikes dummy, and we only measured the significance of proxy variables as groups using the F test. Perfect collinearity among each set of proxy variables was avoided by dropping one year dummy (1980), one city dummy (Birmingham, AL), and one linear time trend variable (Birmingham, AL).

RESULTS AND DISCUSSION

Estimates of the average aggregate impact of three-strikes laws on homicide rates appear in Table 1. Column 1 of Table 1 provides the corresponding coefficient on the three-strikes dummy variable from specifications where year dummies, city dummies, homicide rates lagged one and

10. Results for the three-strikes dummy variables are virtually the same when the continuous variables are not logged.

two years, and the seven specific control variables included as controls; column 2 includes the controls listed in column 1, along with separate linear trend variables for each city. To simplify the table, we only present the results for the three-strikes law dummy variables and specific control variables (results not presented here are available on request).

TABLE 1. THE IMPACT OF THREE-STRIKES LAWS ON HOMICIDE RATES IN LARGE CITIES

	Basic (Without City Trends) (1)		With City Trends (2)	
	Coefficient	*t*-ratio	Coefficient	*t*-ratio
Three-Strikes Laws	**.118**	5.59	**.134**	4.43
Population 18 to 24	**.844**	5.89	**2.28**	8.77
Poverty Rate	**.220**	4.28	–.235	–0.40
Real Personal Income	**.222**	2.08	**1.11**	6.16
Female Headed Hslds.	.140	1.49	**.467**	4.03
Percent Black	**.159**	3.52	**.485**	3.33
Percent Living Alone	**.413**	3.36	–.442	–1.07
Prison Population	**–.146**	–4.01	**–.290**	–5.81
Y (t–1)	**.288**	16.88	**.097**	5.49
Y (t–2)	**.179**	10.28	.030	1.69
F Values for Variable Groups				
7 Control Variables	16.76		28.20	
19 Year Dummies	22.28		37.14	
188 City Dummies	2.97		4.41	
188 City Trends	—		3.81	
N	3,640		3,640	
D.F.	3,424		3,237	
Adj R^2	.88		.90	

NOTE: Coefficients in bold are significant at the .05 level. The dependent variable is the natural log of homicide rates. The three-strikes laws are represented by a step 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. Data cover the period 1980–1999, excluding two years lost in the autocorrelation correction for 188 cities with populations greater than 100,000 in 1990. The number of observations is equal to 3,640 in both columns as a result of occasional missing homicide data. All continuous variables are logged, per capita variables. Both homicide regressions were weighted by city population to mitigate heteroscedasticity problems. The second regression (Column 2) includes separate linear trend variables for each city. The *F* statistics are for the significance of the proxy variables and specific control variables taken as a group. Due to space limitations, the results for individual city dummies, year dummies, and city trends are not presented.

The results in Table 1 provide exceptionally strong support for Marvell and Moody's (2001) claim that homicide rates have grown faster (or declined at a slower rate) in three-strikes cities compared with cities without the laws. In both specifications, the coefficient for the three-strikes dummy variable is positive and is highly significant. As noted above, because the variables were expressed in natural logs, the coefficients are elasticities. However, as Kennedy (1997:223) notes, the coefficient on the dummy variable cannot be interpreted as the percentage impact on homicide rates of a change in the three-strikes dummy from 0 to 1. Instead, the correct expression for estimating the magnitude of a percentage change in the dependent variable and the one used here is $100 \times [\exp(b) - 1]$. Using this formula, the estimates in Table 1 imply that passage of a three-strikes law has increased homicides, on average, by 13% to 14% over the short term, and 16% to 24% over the long term.[11] This translates to approximately 8 additional homicides in each three-strikes city over the short term (or 880 homicides in all 110 three-strike cities) and roughly 1,300 homicides per year in the 110 three-strikes cities over the long term. These results are virtually identical to those obtained by Marvell and Moody (2001), who found a 10% to 12% increase in homicides over the short term (about 1,400 homicides per year in all the three-strike states) and a 23% to 29% increase in homicides over the long term (about 3,300 homicides per year across the three-strikes states).

Are the Results for Homicide Spurious?

As Marvell and Moody (2001) noted, because the theoretical arguments for why the laws might increase homicide do not apply to other crimes, the results for homicide are probably meaningless if regressions for other crimes produce similar coefficients on the three-strikes law dummy variable. This suggests that some other, unmeasured, variables changing around the 1993 to 1995 time frame (e.g., drug market and gang activity) were responsible for the observed homicide increases in three-strike cities, something also affecting violent crime rates and property crime rates in general. On the other hand, if homicide is the only crime to increase following the passage of three-strikes laws, this virtually rules out the possibility that factors confounded with the passage of three-strikes laws, could account for the increases in this crime. Such an explanation would require that confounded factors increased homicides, but not other types of crime, in different years and in different three-strikes cities.

In order to examine this possibility, we reestimated the model specifications in Table 1 with total reported crime (not including homicide) and violent crime (not including homicide) as the dependent variables. There is little evidence that the results reported in Table 1 for the three-strikes dummy variable reflect in whole or in part the effects of omitted factors

11. In order to obtain the long-term estimate, we followed Marvell and Moody's (2001) procedure of dividing the short-term coefficient (after the correction) by 1 minus the sum of the coefficients on the lagged dependent variables (see also Hamilton, 1994:19–20).

that are not captured by our regression models. The coefficients for the three-strikes law dummy are .03 (t = 2.23) and .04 (t = 1.67), respectively, when total reported crime (less homicide) is the dependent variable in the Table 1 regressions. When violent crime is the dependent variable, the law dummy coefficients are –.09 (t = –1.80) and –.36 (t = –4.46), respectively. To test whether the coefficients for the three-strikes law dummy in the homicide regressions are significantly different from the corresponding coefficients in the total crime and violent crime regressions, we used F tests. The F tests were conducted using the STEST option in the SYSLIN procedure in SAS (SAS Institute, 1993). Similar to Marvell and Moody (2001), results of the F tests suggest that the coefficient for the three-strikes law dummy variable in the homicide regression is significantly different from the coefficient in the total crime regression (F = 13.98, prob. = .0001) and in the violent crime regression (F = 34.43, prob. 0001).[12]

In another set of regressions, we added total crime and violent crime as independent variables. The rationale for doing so was that if violent crime was growing faster in three-strikes cities compared with non-three-strikes cities, it could account for most, if not all, of the impact estimated in Table 1. Adding total crime and violent crime to the regressions in Table 1, however, yielded similar results to the regressions without these variables. The coefficients for the three-strikes law dummy are .14 (t = 6.87) and .14 (t = 4.74), respectively, when total reported crime (not including homicide) is added as an independent variable. With violent crime (not including homicide) added as an independent variable, the coefficients on the three-strikes dummy variable are .15 (t = 6.83) and .17 (t = 5.48). Thus, there is no evidence that comparatively more violent crime growth in three-strikes cities are responsible for the observed increases in homicide rates in three-strikes cities.[13]

Addressing Potential Simultaneity Bias

Another potential threat to the results presented above is simultaneity bias. Simultaneity is possible because policy makers might respond to growing crime problems by passing three-strikes laws. If such a situation existed, the positive effect of homicide rates on the passage of three-strikes laws could be confused with the positive effect of three-strikes laws on homicide rates. Homicide rate models would be underidentified, and parameter estimates for the three-strikes dummy variable would be biased

12. The F test results are for the regressions with city trends. The F tests results are essentially the same without them.

13. We also tried adding a dichotomous dummy variable for the 33 states subject to the Brady law, which became effective on February 28, 1994, and mandated background checks for prospective handgun purchasers. The addition of the Brady law dummy had very little impact on the results reported above.

and inconsistent because the errors for the homicide rate equation would be correlated with the three-strikes dummy variable. Although Marvell and Moody (2001) found no evidence that state-level homicide rates influenced the passage of three-strikes laws using the Granger causality test, we cannot rule out that possibility here. Perhaps more importantly, a reanalysis of Marvell and Moody's data by Shepherd (2002) revealed that the positive association between three-strikes laws and homicide may have been due to reverse causation; that is, states with higher homicide rates were more prone to pass three-strikes laws. The approach used by Shepherd (2002) to address potential simultaneity between three-strikes laws and homicide, however, suffers from numerous problems and the results are almost certainly wrong. First, Shepherd (2002) uses ordinary least-squares regression with a dummy dependent variable, which violates several classic regression assumptions, including error terms, which are heteroscedastic and not normally distributed. Second, the test used by Shepherd (2002) to address simultaneity, the Lagrange Multiplier (LM) test for exogeneity, required a valid instrumental variable, but the variable used, the percent voting Republican in the state in the most recent Presidential election, was not a valid instrument because it can be affected by homicide rates (see also Nagin, 1978). Unfortunately, Shepherd (2002) did not provide any rationale for the selection of her identifying restriction, did not report trying any other identifying restrictions, and did not report the results of any statistical tests assessing the adequacy of her identification restriction.

We agree with Marvell and Moody (2001) that the Granger causality test is probably the best procedure for addressing the potential two-way relationship between three-strikes laws and homicide rates. The Granger test is a standard econometric procedure for addressing causal direction and determines the ability of one variable to predict another (Granger, 1969; Pindyck and Rubinfeld, 1991:216–219). In the present study, we conducted the Granger test using a probit regression with specifications similar to those used in Table 1, except that the three-strikes law dummy variable is the dependent variable and the key independent variables are homicide rates lagged one and two years. If the lagged homicide rate variables are jointly significant and positive as determined by an F test, homicide rates "Granger-caused" the passage of three-strikes laws. The Granger test does have the drawback of underestimating contemporaneous causation because the test only includes lagged homicide rates (the current-year independent variable cannot be entered because the causal direction is unknown). This is problematic, however, only if the causation between homicide rates and the passage of three-strikes laws is instantaneous. If so, the Granger test would fail to indicate causation. It can be safely assumed, however, that homicide rates probably do not have an immediate impact on the passage of three-strikes laws because it takes considerable time for crime statistics to be compiled and for states to enact crime legislation. Also, it is unlikely that state governments would enact legislation based solely on crime rates in the current year, while not considering crime in earlier years. Similar to results reported by Marvell and Moody (2001), the results of the Granger test give no indication that the results in Table 1 suffer from simultaneity problems. That is, there is no evidence that increases in homicide rates prompt state legislatures to enact three-strikes laws. The coefficients for homicide are far from significant, and they do not always have the expected positive sign.

Individual City Results

Like most criminal justice policy evaluations of legal interventions using cross-sectional or time-series data, there is an implicit, albeit unlikely, assumption that the coefficient for the legal intervention under study is identical across all ecological units. Recent research by, among others, Baltagi and Griffin (1997), Black and Nagin (1998), McDowall et al. (1992), and Pesaran and Smith (1995) suggests, however, that the assumption in regression analysis of constant impacts of legal interventions across ecological units is probably unrealistic. At present, by aggregating the three-strikes dummy variables into a single variable, we were assuming that all cities residing in states with three-strikes laws would exhibit similar changes in homicide rates. As Marvell and Moody (2001) noted, however, this assumption is probably unrealistic given variation in (1) the publicity surrounding the passage of the laws, (2) differences in severity of the laws, (3) the amount of discretion prosecutors and judges have in applying the laws, and (4) other contemporaneous changes in criminal law and operations. All of these factors are likely to vary across and within cities in three-strikes states and are probably important in influencing criminals' awareness of the laws, the perceived extent of their use, and the eventual actions of criminals at crime scenes. In such a situation, there will be a distribution of effects for three-strikes laws instead of one common impact. On the other hand, one might expect a few cities to witness increases in homicide following the passage of three-strikes laws as a matter of chance alone, and it is impossible to tell which ones these are. To explore the issue of coefficient heterogeneity, we followed Marvell and Moody's (2001) recommendation of creating separate law variables for each city, which are scored 1 after the law in the particular city and 0 elsewhere.[14] Table 2 presents these results.

14. Another procedure for examining coefficient heterogeneity is to conduct separate time series regressions for each ecological unit (Pesaran and Smith, 1995). We agree with Marvell and Moody (2001) that such an approach is probably unwarranted

TABLE 2. REGRESSING HOMICIDE RATES ON INDIVIDUAL CITY LAWS

	Basic (Without City Trends) (1)		With City Trends (2)	
	Coefficient	*t*-ratio	Coefficient	*t*-ratio
Alaska				
Anchorage	**.383**	2.11	.220	0.94
Arkansas				
Little Rock	–.027	–0.14	–.376	–1.38
California (S)				
Anaheim	.085	0.57	.028	0.12
Bakersfield	.309	1.72	.201	0.71
Berkeley	–.114	–0.49	.230	0.63
Chula Vista	.251	1.23	.395	1.22
Concord	–.042	–0.19	–.104	–0.30
El Monte	.229	0.96	.679	1.84
Escondido	–.008	–0.04	.028	0.08
Fremont	.326	1.79	.487	1.71
Fresno	–.006	–0.05	–.174	–0.86
Fullerton	.274	1.25	.492	1.43
Garden Grove	.096	0.48	.103	0.33
Glendale	.096	0.52	**.627**	2.10
Hayward	.415	1.87	.589	1.69
Huntington Beach	.059	0.33	.315	1.15
Inglewood	.161	0.70	.107	0.30
Irvine	.041	0.18	**.977**	2.73
Long Beach	.042	0.36	.128	0.70
Los Angeles	–.041	–0.85	.138	1.88
Modesto	.143	0.76	.023	0.08
Oakland	–.006	–0.04	–.164	–0.71
Oceanside	.136	0.65	–.108	–0.32
Ontario	.105	0.50	.479	1.46
Orange	–.025	–0.11	.231	0.66
Oxnard	.042	0.21	.382	1.22
Pasadena	–.207	–1.00	.166	0.52
Pomona	.125	0.60	–.202	–0.62
Riverside	.247	1.55	.026	0.11
Sacramento	.027	0.22	–.069	–0.35
Salinas	.369	1.63	.483	1.37
San Bernardino	.112	0.54	.624	1.04
San Diego	–.108	–1.45	**–.335**	–2.84
San Francisco	.132	1.42	.195	1.37
San Jose	.069	0.79	**.385**	2.79
Santa Ana	.052	0.34	.162	0.71
Santa Rosa	.265	1.19	.277	0.80
Simi Valley	.190	0.79	**1.21**	3.23
Stockton	.070	0.42	–.181	–0.70
Sunnyvale	–.080	–0.36	–.019	–0.06
Thousand Oaks	.195	0.85	.022	0.06
Torrance	.137	0.67	.357	1.13
Vallejo	.245	1.05	.203	0.56

here because the number of years for each city is limited and the lack of year dummies is likely to result in spurious results.

	Basic (Without City Trends) (1)		With City Trends (2)	
	Coefficient	*t*-ratio	Coefficient	*t*-ratio
Colorado				
Aurora	.108	0.64	.121	0.44
Colorado Springs	.033	0.17	–.056	–0.23
Denver	.096	0.87	–.276	–1.55
Lakewood	.210	0.70	.506	1.38
Connecticut (W)				
Bridgeport	.177	0.83	–.300	–0.96
Hartford	.298	1.37	**.983**	3.05
New Haven	.136	0.61	–.131	–0.40
Stamford	–.042	–0.18	–.168	–0.47
Waterbury	.171	0.71	.240	0.67
Florida (W)				
Fort Lauderdale	–.053	–0.25	.458	1.52
Hialeah	–.315	–1.38	–.197	–0.38
Hollywood	.240	1.00	**1.03**	3.05
Jacksonville	.216	1.16	–.091	–0.42
Miami	–.054	–0.39	**.421**	2.21
Orlando	.262	1.18	.616	1.65
St. Petersburg	.190	1.11	.137	0.59
Tallahassee	–.084	–0.34	.094	0.25
Tampa	–.053	–0.34	.146	0.68
Georgia (S)				
Atlanta	.069	0.54	.092	0.51
Columbus	.071	0.27	.041	0.13
Macon	.205	0.90	.066	0.21
Savannah	.143	0.70	.157	0.55
Indiana (W)				
Evansville	.071	0.33	–.098	–0.30
Fort Wayne	**.394**	2.16	.231	0.82
Gary	.287	0.89	.125	0.28
Indianapolis	**.320**	2.12	.393	1.91
South Bend	.421	1.64	.038	0.06
Kansas				
Topeka	.487	1.76	.063	0.14
Wichita	.255	1.82	.116	0.53
Louisiana (W)				
Baton Rouge	.294	1.78	.171	0.70
New Orleans	**.229**	2.04	–.113	–0.68
Shreveport	.043	0.25	–.319	–1.22
Maryland (W)				
Baltimore	**.370**	3.67	.164	1.16
Nevada (W)				
Las Vegas	**.529**	3.25	**.714**	3.94
Reno	.073	0.34	.045	0.15
New Jersey				
Elizabeth	.187	0.74	–.086	–0.24
Jersey City	.113	0.65	**.657**	2.69
Newark	.144	0.89	**.458**	2.05
Paterson	.153	0.68	.552	1.76
New Mexico				
Albuquerque	**.261**	2.13	.069	0.36
North Carolina (W)				
Charlotte	–.044	–0.40	**–.408**	–2.24

	Basic (Without City Trends) (1)		With City Trends (2)	
	Coefficient	*t*-ratio	Coefficient	*t*-ratio
Durham	.367	1.77	.376	1.17
Greensboro	.284	1.63	.344	1.26
Raleigh	.195	1.21	.297	1.15
Winston-Salem	.067	0.35	.017	0.06
Pennsylvania				
Allentown	**.513**	1.96	.435	1.28
Erie	.110	0.43	.400	1.20
Philadelphia	**.335**	2.63	.241	1.85
Pittsburgh	.239	1.67	.315	1.72
Tennessee				
Chattanooga	.150	0.77	.005	0.02
Knoxville	.212	1.13	.241	0.84
Memphis	.126	1.27	–.025	–0.16
Nashville	**.217**	1.97	.327	1.94
Utah (W)				
Salt Lake City	.272	1.23	.030	0.10
Virginia				
Alexandria	.077	0.34	.323	0.91
Chesapeake	.0002	0.00	.045	0.15
Hampton	.103	0.49	.084	0.26
Newport News	.193	1.03	.225	0.78
Norfolk	.293	1.72	.034	0.14
Portsmouth	.250	1.04	–.131	–0.36
Richmond	.327	1.90	.098	0.37
Virginia Beach	.170	1.23	.013	0.06
Washington				
Seattle	.070	0.67	.158	0.98
Spokane	.092	0.54	.287	1.08
Tacoma	.002	0.01	–.291	–1.07
Wisconsin				
Madison	.071	0.41	**.699**	2.57
Milwaukee	**.289**	2.90	–.298	–1.90
Other Variables:				
Population 18 to 24	**.755**	4.38	**3.03**	9.41
Poverty Rate	**.258**	3.83	–.366	–0.54
Real Personal Income	.169	1.31	**1.17**	6.24
Female-Headed Hslds.	.068	0.57	**.500**	3.39
Percent Black	**.108**	2.12	**.652**	3.69
Percent Living Alone	**.316**	2.10	.320	0.63
Prison Population	**–.133**	3.83	**–.285**	–5.43
Y (t–1)	**.264**	15.16	**.059**	3.31
Y (t–2)	**.166**	9.35	.003	0.14
Means				
110 Coefficients	**.149**	10.40	**.183**	6.24

	Basic (Without City Trends) (1)		With City Trends (2)	
	Coefficient	*t*-ratio	Coefficient	*t*-ratio
Percent Positive	84%		76%	
N	3,640		3,640	
d.f.	3,315		3,128	
Adj R^2	.88		.90	

NOTE: Coefficients in bold are significant at the .05 level. This table is the result of regressions that are the same as those in Table 1, except that three-strikes laws are represented as separate variables for each city (as opposed to the single aggregate three-strikes law variable presented in Table 1), which are scored "1" after the law in the particular city and "0" elsewhere. The "S" and "W" notations for states refer to laws categorized by Marvell and Moody (2001) as "severe" or "weaker."

When the three-strikes laws are represented by separate variables, the results vary widely, but the overall impression is the same—three-strikes laws increase homicide. Ninety two of the 110 laws entered into the homicide regression without city trends have positive coefficients and 11 are significant to the .05 level. None of the law variables are significant and negative. With respect to the homicide regressions with separate linear trend variables for each city, 84 of the 110 law variables have positive coefficients and 11 are significant to the .05 level. Similar to Marvell and Moody (2001), we found that the coefficient means for the individual law variables, .15 and .18, respectively, were slightly higher than were the coefficients for the aggregate three-strikes law dummy variable. The most likely explanation for this finding is that a few small cities have larger coefficients. Because the regressions were weighted by size of city population, these cities had little impact on the results in Table 1.[15]

As noted above, it is possible that variation in the severity of the laws and their enforcement might be responsible for the widely varying results for homicide found in Table 2. Similar to Marvell and Moody (2001:101–102), we categorized California and Georgia as having especially "severe" laws (based on their wording) and Connecticut, Florida, Indiana, Kansas, Louisiana, Maryland, North Carolina, and Utah as having laws that are probably less severe than most. Regarding enforcement of the laws, we used the results of a survey conducted by the Campaign for an Effective Crime Policy, which noted greater application of the laws in California, Nevada, Florida, Georgia, and Washington (Dickey and Hollenhorst, 1998).[16] Like Marvell and Moody (2001), we found that the most severe laws appeared to have the smallest impact on homicide rates (Table

15. As Marvell and Moody (2001) note, the survey is not complete and does not take into account the possibility that prosecutors may be using the laws as leverage during the plea bargaining process in order to get defendants (probably in weak cases) to accept more severe sentences that they would otherwise not accept.

16. The *t*-ratio is based on the standard error of the mean, as recommended by

2), suggesting that the increased murders committed to avoid capture in these cities may have been offset by some deterrence effects. For example, Table 2 shows that although the coefficients on the three-strikes dummy variables are disproportionately positive, only 4 of the 82 individual law dummy variables for cities residing in California are significant and positive, whereas none are significant and positive for the 8 individual city law dummy variables in Georgia. On the other hand, 9 of the 22 significant and positive individual city law dummies are in states with laws that are probably weaker than most. Likewise, the coefficients for the individual city law dummy variables do not give the impression that homicide rates increase more in areas where the laws are applied most often. A possible exception is Nevada, where 2 of the 4 individual city law dummy variables (both for Las Vegas) are significant and positive. Beyond that, however, there is little evidence that increased application of the laws produced greater increases in homicide. There are numerous reasons why the laws might not have similar or even stronger impacts for three-strike cities residing in states where the laws are strictly enforced, and we cannot determine which ones explain the results in Table 2. First, the lack of any apparent positive impact of three-strikes laws in these cities could be due to counterbalancing effects of opposite sign, such that increases in homicides are offset by negative impacts of the laws due to deterrence and incapacitation. Another possible explanation for the lack of stricter third-strike sentencing increasing homicide is criminal migration. If criminals leave states where the laws are strictly enforced to commit crimes in states where the laws are rarely applied or do not exist at all, the increases in homicide might roughly cancel out the decreases in homicide due to criminals relocating. Finally, because it is highly unlikely that prospective three-strikes offenders would have accurate information about how often strike sentences are imposed in their particular location (i.e., judicial circuit), one could argue that variation in sentencing practices across cities is largely irrelevant for purposes of assessing the deterrent effects of three-strikes laws on crime. Nonetheless, there is little doubt that the impact of the laws in cities in states where the laws are applied most frequently is positive.

Pesaran and Smith, (1995). One of the drawbacks of this procedure, as with other procedures for estimating the standard error when averaging coefficients, is that it assumes independence between units. That is, it assumes that the error terms are not correlated between cities. Because we cannot rule out that possibility here, we believe that the t-ratios reported in Table 2 should only be considered approximations.

CONCLUSION AND IMPLICATIONS

This study revisited the claim by Marvell and Moody (2001) that homicide rates might increase following the passage of three-strikes laws. Using a fixed effects model with city-level panel data for 1980 to 1999 and numerous control variables, we found that in cities with three-strikes laws, homicide rates increased on average 13% to 14% in the short term and 16% to 24% in the long term, compared with cities without the laws.

The present findings lend further support to existing theoretical and empirical research demonstrating the disutility and potentially lethal danger of three-strikes laws (e.g., Macallair and Males, 1999; Marvell and Moody, 2001). Despite their public support and political popularity, policy makers should seriously consider repealing three-strikes laws. They are simply not the panacea for the nation's violent crime problem, and according to a growing body of scientific research, they may actually exacerbate the most serious crime—homicide.

Three-strikes laws are based on the principle of incapacitation—violent recidivist criminals would be unable to commit further crimes while incarcerated. But it was also hypothesized that an added benefit to these laws would be their ability to deter others from continuing or contemplating a life of crime. Inherent in this hypothesis is the assumption that offenders are rational and thus weigh the costs and benefits of criminal behavior prior to engaging in it. That is, offenders must reason that the potential benefits of crime outweigh its costs. If true, a "rational" offender with a lengthy criminal history that includes two felony convictions would calculate the best way to minimize the risk of apprehension and subsequent conviction as a three-strikes offender would be to eliminate victims or police officers—those who could increase the likelihood of the offender being caught and prosecuted. This would be a rational (i.e., calculated) response in that it minimizes an important cost—apprehension.

The public's fear of crime and its frustration with repeat offenders was largely induced by the amplification of violent crime in the national media, in particular, the saturation of attention afforded to a few select, albeit horrendous, cases (e.g., Polly Klaas). Public outcry over these cases galvanized policy makers to "get tough" and "do something" to "fix" the crime problem. In response, legislators passed three-strikes laws.

Although policy makers anticipated the laws would "fix the problem" by deterring active criminals and incapacitating repeat offenders (thus preventing them from preying on citizens), the climate of fear and hysteria in which the statutes were passed actually increased the likelihood of failure or negative unintended consequences. In a climate of fear or "moral panic," people are more willing to believe that something must be done and done quickly without looking carefully at the full set of possible consequences of the proposed solutions (see: Kleck, 1999).

Based on the theoretical and empirical research on three-strikes legislation and with the recognition that policy making is inherently a political process, we offer the following suggestions for policymakers and criminal justice researchers. First, legislators should be aware of "claimsmakers" and media distortion of social problems (Cohen, 1972; Fishman, 1976; Kappler et al., 1996; McCorkle and Miethe, 2002; Surette, 1998) and resist passing "quick fix" solutions in a climate of public fear. Second, policy makers should take more care to weigh, not just the potential benefits of a proposed crime control solution, but the potential costs as well, and researchers should become more active in providing assistance in this process. Third, researchers should resist the tendency to separate policy criticism, positive or negative, from criminological science (Clear, 2002) by continuing to conduct empirical studies of major crime control policies to assess their impact and, more importantly, analyze and discuss the implications of their findings. Considering the impact of crime control policies on not only offenders, but also the public in general, researchers have an obligation to inform policy makers of the potential benefits as well as of the costs of these policies. Future research can assist in this process.

Two studies have now found that three-strikes laws increase homicide rates. Future research, however, is clearly warranted and might take several approaches. One avenue for future research would involve conducting surveys of active offenders in three-strikes states to determine whether offenders are aware of three-strikes enhancements and, if so, to what lengths they would go to avoid detection and prosecution. One such study was done in California, but it was limited to interviews with active juvenile offenders (Schafer, 1999). Further research of this nature would, through interviews with active offenders, reveal either the possible crime-promoting or the deterrent effects of these laws. Additional research could also examine, using official data combined with surveys of judges and prosecutors, the extent to which these laws are actually implemented. This would allow researchers to examine variation in the implementation of the laws, as well as the impact on crime rates of variability in their application. Finally, future research could expand the scope of the present study by including serious crimes of violence (e.g., robbery, aggravated assault) and serious property crimes (e.g., burglary) as the dependent variable. Such a study would address the extent to which three-strikes laws impact other serious crimes.

As Dye (1978:3) observed, "[p]ublic policy is whatever government chooses to do or not to do," and when government "chooses to do something," the outcome of that choice can affect citizens both directly and indirectly through intended and unintended consequences (Gilsinan, 1990). Of course, not all unintended consequences are negative. The problem becomes the extent the impacts of a policy may actually "backfire" and make the target of the problem worse.

Criminal justice policies have consequences for both individuals and society as a whole, some of which are unintended and some of which are not necessarily positive. As the present study demonstrated, these unintended consequences can be lethal. Although this is only the second published study that examined the possible homicide promoting effects of three-strikes laws, it contributes to a growing body of literature on how these laws have failed to reduce crime (Kovandzic, 1999; Macallair and Males, 1999; Marvell and Moody, 2001; and Stolzenberg and D'Alessio, 1997) and other unintended negative side effects (Crawford et al., 1998; Cushman, 1996; Greenwood et al., 1994). In light of previous theoretical and empirical research on the "costs" of three-strikes laws, we join others (Marvell and Moody, 2001;Walker, 2001) advocating their repeal. It is possible that policy makers and the public are willing to "pay" some of these "costs"; however, it is questionable as to whether they are willing to "pay" in human lives.

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