Unintended Consequences:
Three-Strikes Laws and the Murders of Police Officers

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Abstract.

Several states have enacted "three-strikes" laws which call for extremely long sentences if a person is convicted of a third serious crime. Criminals faced with such severe penalties might take additional steps to avoid capture. One such step is murdering arresting officers. Even if this rarely occurs it can have a proportionally large impact on police murders. We estimate fixed-effects Poison models of police murders based on panel data for 1973-1998. We find that the laws increase police murders by more than 40 percent. We use the same model to evaluate the impact of the death penalty, imprisonment rates, "shall issue" concealed weapons permit laws, and laws requiring longer sentences for crimes committed with firearms. Only the latter has a significant impact, reducing police murders by roughly 18 percent

I. Introduction.

Twenty-four states enacted "three-strikes and you're out" laws that went into effect during a period of 25 months, between December 1993 and January 1996. These laws call for life imprisonment or extremely long prison terms for criminals convicted of a third serious crime. The crimes typically covered are murder, rape, robbery, kidnapping, aggravated assault, and sexual assault.

Economic theory holds that, all else being equal, a person is less likely to commit a crime when the expected costs increase (e.g., Becker, 1968; Ehrlich, 1973; Heineke, 1978). The additional prison terms called for by three-strikes laws increase the expected costs for persons who are sentenced under the laws. At first glance, then, the expected result is less crime.

Further consideration of the laws and the criminals' possible reactions, however, leads one to question whether the laws have noticeable deterrent impacts. Most of the laws are very narrow in scope and do not apply to most criminals, and very few criminals are convicted under the laws in most three-strikes states (Dickey and Hollenhorst, 1998; Beres and Griffith, 1998; Boorstein, 1998). Some criminals are not aware of the laws (Austin et al., 1999). Repeat criminals can be expected to serve substantial prison terms even in the absence of the laws (Austin, 1996; Beres and Griffith, 1998). Almost all three-strikes states already had habitual criminal statutes under which judges could give criminals with prior convictions very long sentences, but without mandatory sentences (Dickey and Hollenhorst, 1998; Clark, 1997). Because the marginal increase in the prison term takes effect in the future, the additional deterrent will be small if criminals have relatively high discount rates (Polinsky and Shavell, 1999). Accordingly the few studies that have explored the question find that the laws have neither reduced crime nor increased prison population (Austin et al., 1999; Marvell and Moody, 2001; Stolzenberg and D'Alessio, 1997).

The theoretical exploration of criminals' reactions to the laws is more complex than the above discussion indicates. Criminals might try to reduce expected costs by taking evasive actions (which can have substantial costs of their own). Often studied examples are moving to other jurisdictions (Heineke, 1978), switching to crimes that involve less risk of apprehension (although probably less gain per crime) (Andreoni, 1995; Cook, 1979); Malik, 1990), and bribing police (Becker and Stigler, 1974; Bowles and Garoupa, 1997).

In the present study we explore the fact that criminals who (perhaps mistakenly) believe that they face severe three-strikes penalties might murder police in order to escape arrest. Because the three-strikes penalties approach the penalties for homicide, there is little marginal deterrence to dissuade a criminal from murdering police if doing so reduces the probability of arrest. Consequently police homicides might rise following the passage of a three-strikes law.

This possibility has been suggested by several commentators (e.g., Austin et al., 1999; Newton, 1996; Stedman, 1997; Wiatrowski, 1996), and there is some rudimentary empirical support for it. Using a simple one-group, pre-test, post-test study, Stedman (1997) found a 14 percent increase in the number of California police murdered following the law. Also, six of the 20 police killers apprehended since the law already had two strikes, whereas no police killers in the three years prior to the law had two strikes. Stedman (1997) and Austin et. al. (1999) interviewed prisoners and give examples of prisoner's contentions that they might be more likely to kill officers because of the laws. In a study similar to the current research, Marvell and Moody (2001) found that three-strikes laws substantially increase overall homicide rates, presumably because criminals fearing three-strike penalties murder witnesses and others in order to escape detection and capture. If that is the case, criminals might murder arresting officers as well.

More generally, research on the motivations for killings of law enforcement officers suggests that many such murders are to escape arrest. Cardarelli (1968) and Creamer and Robin (1970) found, for example, that police murderers often kill to avoid being apprehended in the course of committing serious crimes. An analysis of 245 murders of New York City police by Margarita (1980a, 1980b) found that most of these criminals murdered police officers to avoid capture. Thus, there appears to be some degree of rational decision making on the part of "cop killers," and it is conceivable that the presence of a three-strikes law can be an additional factor in a criminal's calculations. This reasoning assumes, however, that the risks are similar for the initial crime and the police murder. If the perceived risk for the latter is greater, the expected costs can be greater even if the sanctions are not (Shavell, 1992). In the present situation, the expected costs are usually greater. Law enforcement goes to great lengths to solve police homicides; for example, FBI data for 1986-1995 show that only 1.1% of the 967 suspected police murderers remain at large (Federal Bureau of Investigation, 1997).

The discussion so far indicates that a three-strikes law usually would not prompt the rational criminal to murder arresting officers, mainly because the likelihood of three-strikes penalties is remote and the likelihood of being apprehended for a police murder is large. But the general rule might not apply to all criminals in all situations. Some criminals might not be knowledgeable about the applicability of three-strikes laws. For example, many of the three-strikers in California prisons interviewed by Austin et.al. (1999) knew little about the law's provisions prior to arrest. Some criminals might fear an indirect effect of the laws on plea bargaining. Prosecutors can use the threat of the application of three-strikes laws to persuade criminals to plea guilty to more severe crimes than they ordinarily would plead to (National Conference of State Legislatures, 1997). Although sentences given under three-strikes laws may be only slightly more severe than under prior sentencing laws, criminals might expect longer prison terms because parole possibilities are more limited under three-strikes laws. Finally, due to the circumstances of the particular crime, the criminal might believe that chances of apprehension for killing an arresting officer are slight even though efforts to solve the crime will be substantial.

Thus, although theory might lead one to conclude that the typical criminal is not more likely to murder police following three strikes laws, a few criminals might be motivated to do so. Let us assume that in only one of every 10,000 arrests for violent crimes the criminal believes, perhaps mistakenly, that the three-strikes law will greatly increase his punishment and that his chances of arrest and conviction are significantly reduced by murdering an arresting officer. Let us also assume that attempts to murder police are successful in half these incidents. Since police made 420,000 arrests for major violent crimes (murder, rape, robbery, and aggregated assault) during 1999 (Federal Bureau of Investigation, 2000), the result would be 21 additional police homicides. This would be a 35% increase over the average of 60 police homicides per year.

Another way of looking at the potential impact on police murders is to consider only criminals who are most likely to be covered by the laws. In a survey of criminals newly admitted to prisons in California, Michigan and Texas, Chaiken and Chaiken (1982) classified 15% as "violent predators," criminals who commit very large numbers of both property and violent crimes. Approximately half a million criminals were admitted to prisons in 1996 (Bureau of Justice Statistics, 1999), which implies that more than 75,000 violent predators were arrested (not all arrested are convicted). To cause a 35% increase in police murders, the three-strikes laws need only prompt 0.05% of violent predators to murder police when arrested.

In fact, police murders increased in three-strikes states compared to other states. States that passed three-strikes laws experienced a 17% increase in the number of police murders in the two years following passage of the law compared to the two years before the law. In contrast police killings declined by 8% in the other 26 states.

In sum, the effect of three-strikes laws on the murders of police officers is uncertain. It remains an empirical question and the focus of this paper.

II Data.

The data set is a pooled time series and cross section of 50 states for the period 1973 to 1998. The dependent variable is the number of law enforcement officers feloniously killed in the line of duty, which is available from 1973 (U.S Federal Bureau of Investigation, 1997; http://www.fbi.gov/ucr/killed/98killed.pdf, and earlier sources). The target variable is a dummy variable that takes the unit value in years following the passage of a three-strikes law. For the year in which the law was passed, it is a fraction representing the portion of the year the law was in effect. The twenty-four laws and their effective dates are presented in Table 1.

(Insert Table 1 about here.)

The control variables are those commonly used in state-level crime studies, where they are described at length (e.g., Marvell and Moody 1995, 1996). We include several age-structure variables: the percent of the population between 15 and 19, 20-24, 25-30, 30-34, 35-44, 45-54, and 55-64. We also include two other demographic variables, the percent of the population who are African-American (interpolated between census years) and the percent of the population who reside in standard metropolitan statistical areas. There are four economic variables: the unemployment rate, the number employed, real personal income, and the poverty rate. An overall national trend captures otherwise omitted trending variables and year dummies capture factors that affect all states in a given year. We include population as a scale factor to control for the number of possible interactions among police and the general citizenry, any one of which could lead to a shooting.

The research design employed allows us to revisit other policy issues, which have been thoroughly explored with respect to total homicide rates but usually not with respect to police murders. Thus, we also include the following policy variables: prison population per capita, the number of legal executions, a dummy variable indicating the presence of right-to-carry concealed weapons laws (Lott and Mustard, 1997), and a dummy variable indicating the presence of laws that increase prison terms for crimes committed with guns, so-called "firearms sentencing enhancements" (Marvell and Moody, 1995).

Increasing the prison population might reduce crime generally, thereby reducing the number of incidents in which police officers are put at risk. Levitt (1996) and Marvell and Moody (1994) find such an effect at the state level for most crimes, but not for homicide. The prison population is measured as of December 31; so we averaged the current and prior year.

Almost all capital punishment penalty statutes give the fact that the victim is a law enforcement officer as a factor when determining whether to apply the death penalty (Palmer, 1998). However, researchers generally find that the death penalty does not deter criminals from killing police (Bailey, 1982; Bailey and Peterson, 1987; Bailey and Peterson, 1994).

"Shall-issue" concealed weapon laws could increase the number of weapons in the hands of the general public, and potentially increase the number of interactions with the police that could lead to shootings. On the other hand, armed citizens have come to the aid of police officers (Lott, 2000, p. 234). Lott and Mustard (1997) and Lott (2000) find that these laws reduced homicide generally, but Mustard (1999) found that the laws do not affect police killings. We enter a dummy variable for the 24 shall-issue laws that went into effect through 1997 (Moody, forthcoming).

Firearms sentencing enhancements ("Use a Gun, Go to Jail") may reduce the number of criminals using guns in the commission of crimes and indirectly reduce the number of police officers killed by guns. Marvell and Moody (1995) found that the 37 laws passed since 1975 had little if any impact on homicide, but Lott and Mustard (1997) found that they caused a modest reduction. The variable names, definitions, means and variances are given in Table 2.

(Insert Table 2 about here.)

III Econometric Methods.

Because the number of police killed each year is very small (averaging 1.6 per state), and for many states in many years the number is zero, this variable cannot be assumed to be continuous. In fact, police killings represent a special case of a limited dependent variable, namely a count variable which can only take nonnegative integer values. While it is possible to estimate a count model using standard regression techniques, the linear model can result in biased, inefficient, and inconsistent estimates (Long, 1997, p. 217). The appropriate starting point for count data models is the Poisson distribution. If an event can occur in any of a large number of trials, but the probability of the event is small, then the "law of rare events" states that the number of events will follow, approximately, a Poisson distribution (Cameron and Trivedi 1998, p. 5). The histogram of the number of police killings (polkil) in Figure 1 below has the classic Poisson shape.

(Insert Figure 1 about here.)

The distribution has the form,

$$\Pr(y \mid \mu) = \frac{\exp(-\mu)\mu^{y}}{y!} \qquad (1)$$

for y = 1, 2, 3, … The random variable y is the number of times an event occurs over a fixed time interval, and the parameter μ is strictly positive. The mean of the distribution is μ and the distribution has the property that the variance is equal to the mean (equidispersion).

For the Poisson to be applied, the observations must be independent. This means that the probability of an officer being killed in the line of duty should be the same whether or not an officer has already been killed. In fact, police killings may appear in clusters if the police are involved in undercover operations that go wrong, or if many police are involved in a shoot-out in which more than one officer is killed. On the other hand, one incident of a policeman being killed might lead other officers to use more caution, thereby reducing the chance of another killing. Since the data show little sign of clustering, however, the Poisson remains a reasonable starting point.

To extend the analysis to a multivariable context, we use the Poisson regression model where the mean is taken to be a linear function of a number of explanatory variables,

$$\mu_i = E(y_i \mid x_i) = \exp(x_i\beta) \tag{2}$$

where x_i is a vector of observations on the explanatory observations and β is a vector of regression parameters. This model implies a particular form of heteroskedasticity since the assumption of equidispersion implies that the variance equals the mean, or

$$V(y_i \mid x_i) = \exp(x_i\beta) \tag{3}$$

The model is estimated using maximum likelihood. If the true data generating process is Poisson, then maximum likelihood methods will yield a consistent estimate of the variance-covariance matrix and resulting standard errors and t-ratios. The log-likelihood function, given independent trials, is

$$L(\beta) = \sum_{i=1}^{n} \{ y_i x_i \beta - \exp(x_i \beta) - \ln y_i ! \} \tag{4}$$

Differentiating with respect to β yields the maximum likelihood estimator. Iterative methods are used to solve for the parameter estimates. (Cameron and Trivedi, 1998, pp. 20-21.)

Another reason to begin with the Poisson model is that it has an important robustness property. Even when the Poisson distribution does not hold, the maximum likelihood estimates of the regression parameters are consistent and asymptotically normal (Wooldridge 2000, p. 548). If the equidispersion assumption of the Poisson model is violated, the parameter estimates remain consistent, but are inefficient and the standard errors are underestimated. (Gourieroux, Monfort, and Trognon 1984.) In this case we can use the negative binomial model in which the mean μ is replaced by the random variable, θ_i, reflecting an additional source of error.

$$\theta_i = \exp(x_i \beta + \varepsilon_i) = \mu_i \exp(\varepsilon_i) = \mu_i \nu_i \tag{5}$$

Typically, it is assumed that the mean of the error term is unity, $E(\nu_i)=1$. This implies that the distribution has the same mean as the Poisson, namely that

$$E(\theta_i) = E(\mu_i \nu_i) = \mu_i E(\nu_i) = \mu_i \tag{6}$$

The distribution of y_i given x_i and ν_i remains Poisson,

$$\Pr(y_i \mid x_i, \nu_i) = \frac{\exp(-\theta_i)\theta_i^{y_i}}{y_i!} = \frac{\exp(-\mu_i \nu_i)(\mu_1 \nu_i)^{y_i}}{y_i!} \tag{7}$$

However, since we do not know ν_i, we need to partial it out. To do so we average Pr(y | x, ν) by the probability of each value of ν_i.

$$P(y_i \mid x_i) = \int_0^\infty \{\Pr(y_i \mid x_i) \times g(\nu_i)\} d\nu_i \tag{8}$$

where $g(\nu_i)$ is the pdf for ν_i. The negative binomial is derived by solving the above equation using the Poisson distribution for Pr(y | x) and the gamma distribution for $g(\nu_i)$. The negative binomial has the same mean as the Poisson, but the variance is no longer required to be equal to the mean. (Long, pp. 230-233.)

$$Var(y_i \mid x_i) = \exp(x_i \beta)\left(1 + \frac{\exp(x_i \beta)}{\nu_i}\right) \tag{9}$$

For our purposes, we need to extend these regression models to pooled time series and cross section data. Since it is very likely that there is unobserved heterogeneity among the states and that this heterogeneity is correlated with one or more of the explanatory variables, we assume the fixed effects model (Hausman, Hall and Griliches, 1984). The individual fixed effects are taken to be multiplicative since the explanatory variables occur in an exponential function.

$$y_{it} \sim \Pr(\mu_{it} = \alpha_i \exp(x_{it}\beta)),\ i = 1,\ldots,n,\ t = 1,\ldots,T \tag{10}$$

The multiplicative individual effects can be interpreted as intercept shifts (Cameron and Trivedi, 1998, p. 279). The likelihood function and maximum likelihood estimators are generalizations of those presented above.

The fixed effects Poisson and negative binomial models yield consistent estimates of the regression parameters, but the individual fixed effects are concentrated out of the likelihood function and are therefore unavailable. If the regressors include lagged dependent variables (dynamic panels), then the fixed effects model is no longer consistent. This is similar to the problem in linear panel models (Nickell, 1981). In linear models these biases have been shown to be quite large for very short panels. However, the bias is reduced dramatically as the number of time periods increase. Since we have 26 years of data, the bias is unlikely to be substantial.

There are two ways to treat the lagged dependent variable. The first is to simply add y_{t-1} to the explanatory variables (exponential feedback), so that the conditional mean becomes

$$\mu_i = \exp(x_i\beta + \rho y_{t-1}) \tag{12}$$

This approach has the potential problem that the model is explosive for $\rho>0$.

The second approach adds the lag of the log of the dependent variable as an explanatory variable. The conditional mean becomes,

$$\mu_i = \exp(x_i\beta + \rho \ln y^*_{i,t-1}) = \exp(x_i\beta)(y^*_{i,t-1})^{\rho} \tag{13}$$

where $y_i^* = y_i + c$ where c is some positive constant to avoid taking the log of zero. (We set c=1.) This is a more natural model because the dependent variable is already logged (Cameron and Trevidi, 1998, p. 238). We present results for both static and dynamic models using both exponential feedback and log feedback.

In Table 3 below, we present the means and variances for police killings by state. The national variance is much greater than the corresponding mean, almost certainly due to unobserved heterogeneity. However, the state-by-state means and variances show very close correspondences. Since we are estimating a fixed effects model, the individual state means and variances are of more interest. Because the means and variances are so close in each state, we choose the Poisson fixed effects model as our basic model, but we also estimate the fixed effects negative binomial model for comparison. We estimate all models using Stata.

(Insert Table 3 about here.)

In any crime study, there is always a possibility of simultaneous equation bias due to endogeneity of the dependent variable. If police killings cause states to enact three-strikes laws, there could be potentially serious specification error. However, police killings are very rare events and as such are unlikely, by themselves, to cause laws to be passed. In any case, it takes a considerable amount of time for a law to be proposed, debated, enacted, and implemented. Therefore, while police killings may cause the passage of three-strikes laws with a lag, it is unlikely that there is contemporaneous reverse causality from police killings to the passage of three-strikes laws.

Finally, we note that the fixed effect Tobit model is not appropriate. It suffers from the "incidental parameters problem" in that as the number of cross section observations goes to infinity, so does the number of parameters to be estimated (the fixed effects). In the Tobit model, the maximum likelihood estimators of the fixed effects and the other regressors are not independent of each other. Consequently, the parameter estimates of the fixed effect Tobit estimator are biased and inconsistent as the panel size goes to infinity (Honore, 1992). The random effects Tobit model does not suffer from this problem, but is likely to be biased by the presence of unobserved heterogeneity. Because individual state effects are concentrated out of the likelihood function for the fixed effects Poisson and negative binomial models, they do not suffer from the incidental parameters problem and are consistent (Cameron and Trivedi, 1998: 281-2). The problem also arises if we correct for scale effects by applying the Tobit model to police murders divided by population. This approach sacrifices all the advantages of the Poisson methodology. As noted above, we correct for scale effects by including population as an independent variable.

IV Results.

The primary regression results for the policy variables are presented in Table 4. There are three basic models: a static model; a dynamic model using two lags of the dependent variable logged, and a dynamic model using two lags of the dependent variable in natural units ("exponential feedback"). Each model is estimated with and without year fixed effects. All versions of the Poisson fixed effects model show positive and significant effects of three-strikes laws on police murders. The fixed effects negative binomial model yields very similar results, with slightly lower levels of significance.

The firearms sentencing enhancements variable, indicating the presence of a law generating longer sentences for crimes committed with guns, is significant in all of the Poisson models, although the results in the negative binomial versions are not significant. None of the other policy variables are significant in any of the regressions. The death penalty, even though the murder of a police officer is an aggravating factor, does not appear to be an effective deterrent. This finding corroborates earlier research by Bailey (1982) and Bailey and Peterson (1987, 1994). The presence of a liberal right-to-carry concealed weapons law appears to have no significant effect on police killings. This indicates that the early opposition to the passage of shall-issue laws from police chiefs and district attorneys, who feared that more guns in the hands of ordinary citizens would put police officers at risk, is apparently unfounded (Lott, 2000, p. 14). State prison population does not appear to affect state-level police killings, a result that corresponds to findings with respect to homicides in general. The coefficients on the lagged dependent variables are generally significant, although quite small. The exponential feedback model is not explosive.

(Insert Table 4 about here.)

We replicate these results using the same models but with the continuous variables expressed as logarithms (Table 5). The results are virtually identical.

(Insert Table 5 about here.)

The estimated coefficients on the three strikes dummy give the impact of these laws on police killings. The long run effects, in the dynamic models, are found by dividing the coefficient on the three-strikes dummy variable by one minus the sum of the coefficients on the lagged dependent variables. It is interesting to note that the long run effects are smaller than the impact due to the negative coefficients on the lagged dependent variables. Using the estimate from our preferred model (Table 4, column 4, Poisson model) we estimate that the passage of a three-strikes law can be expected to increase police killings by approximately 44 percent (exp(.446/(1+.052+.161))-1). Using the coefficient on the firearms sentencing enhancement dummy variable from the same model, we estimate that such "use a gun, go to jail" laws reduce police killings by approximately 18 percent per year.

V Summary and Conclusions.

Theory concerning the likely impact of three-strikes laws on murders of police is complex. We expect that the laws do not prompt the large majority of criminals to murder arresting officers because the laws do not apply to most criminals and most crimes, and because murdering an officer increases the effort on the part of the police to apprehend the killer. However, the impact of the law depends crucially on actions taken by an extremely small number of criminals. General theory does not have universal application to all actors. That is, because there are very few police murders in relation to the number of arrests, the three-strikes laws can have a large impact on the number of police murders even if they prompt only a minuscule proportion of criminals to take evasive action by killing arresting officers. There are no *a priori* theoretical or empirical reasons for determining whether that minuscule proportion exists. Our research indicates that it does exist. Using state-level data from 1973 to 1998, we estimate Poisson and negative binomial fixed-effects models to determine the impact of the 24 three-strikes laws on murders of police. We find an estimated impact of 44% more murders in years following the laws. In the average state there were 1.2 police murders per year in the 1990s; so the typical three-strikes law leads to an additional police murder roughly every other year. This means that approximately 0.0006% of arrests for major violent crimes in three-strike states involve police murders that would not have occurred without the laws.

The pooled regression model also provided the opportunity to evaluate several other criminal justice policies that might affect police murders. Laws requiring sentencing enhancements for crimes committed with firearms appear to reduce police killings by roughly 18 percent. On the other hand, the size of prison population, the number of executions, and the presence of right-to-carry concealed weapons ("shall issue") laws have no discernable impact.

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Appendix.

The three-strikes laws and their effective dates were determined through research in the state codes. The 24 laws and effective dates are: 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.

See also, National Conference of State Legislatures (1997); Clark, Austin, and Henry (1997); Dickey and Hollenhorst (1998); and Turner (1995). The only state that passed three-strikes legislation in 1996 or 1997 is Alaska (National Conference of State Legislatures, 1997).

Table 1
Three-Strikes Laws: dates of implementation

State	Date
Arkansas	7/26/1995
California	3/7/1994
Colorado	5/31/1994
Connecticut	10/1/1994
Florida	10/1/1995
Georgia	1/1/1995
Indiana	7/1/1994
Kansas	7/1/1994
Louisiana	8/27/1994
Maryland	6/1/1994
Montana	7/1/1995
Nevada	7/1/1995
New Jersey	6/22/1995
New Mexico	7/1/1994
North Carolina	5/1/1994
North Dakota	7/1/1995
Pennsylvania	12/10/1995
South Carolina	1/1/1996
Tennessee	7/1/1994
Utah	5/1/1995
Vermont	7/1/1995
Virginia	7/1/1994
Washington	12/2/1993
Wisconsin	4/28/1994

Table 2
Variable means, variances, and definitions.

Name	*Definition*	*Mean*	*Variance*
Polkil	Number of police killed	1.58	4.90
3 strikes	Three strikes law	0.75	0.66
Shall	Shall issue law	0.13	0.12
Exec	Executions	0.30	1.62
FSE	Firearms Sentencing Enhancements	0.67	0.21
Pop	Population	4.78	26.30
P1519	Percent population between 15-19	8.31	1.63
P2024	Percent population between 20-24	8.29	1.40
P2529	Percent population between 25-29	8.11	0.98
P3034	Percent population between 30-34	7.84	1.09
P3544	Percent population between 35-44	13.43	5.82
P4554	Percent population between 45-54	10.46	1.56
P5564	Percent population between 55-64	8.77	0.93
Unrate	Unemployment rate	6.30	4.43
Employ	Total employment per capita	53.15	36.97
Income	Real total personal income per capita	4.31	0.64
Poverty	Poverty rate	13.16	17.41
Black	Percent African-American	9.36	83.86
Urban	Percent metropolitan	63.46	498.17
Prison	Prison population per capita	19.23	152.31
Trend	Linear trend	16.50	56.29

Note: Data from 1973-1998 for 50 states; 1300 observations. The data used in this study are available on the internet.

Figure 1

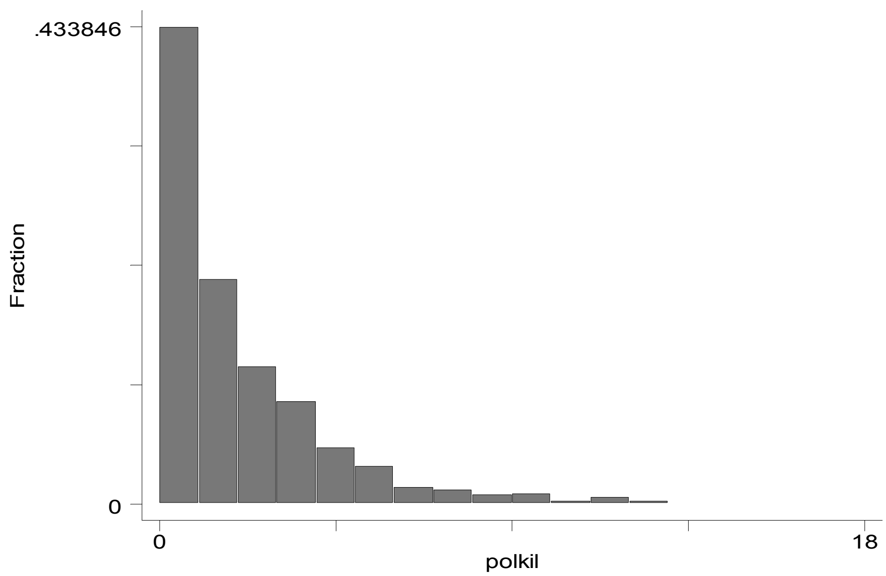

Table 3
Police Killings: Means and Variances, by State.

State	*Mean*	*Variance*	*State*	*Mean*	*Variance*
Alabama	2.27	2.36	Montana	0.35	0.40
Alaska	0.62	0.48	Nebraska	0.38	0.49
Arizona	1.69	2.54	Nevada	0.58	0.65
Arkansas	1.53	2.18	New Hampshire	0.19	0.40
California	7.65	10.39	New Jersey	1.58	2.81
Colorado	1.35	2.71	New Mexico	0.88	0.99
Connecticut	0.23	0.18	New York	5.04	8.12
Delaware	0	0	North Carolina	2.54	2.98
Florida	4.77	5.86	North Dakota	0.15	0.22
Georgia	3.46	4.42	Ohio	2.46	3.38
Hawaii	0.19	0.24	Oklahoma	1.35	3.12
Idaho	0.31	0.31	Oregon	0.46	0.42
Illinois	2.96	4.83	Pennsylvania	2.31	3.18
Indiana	1.54	2.66	Rhode Island	0.04	0.04
Iowa	0.31	0.38	South Carolina	1.85	2.78
Kansas	0.81	0.40	South Dakota	0.19	0.24
Kentucky	1.50	1.70	Tennessee	2.08	3.35
Louisiana	2.19	3.12	Texas	7.27	12.60
Maine	0.12	0.11	Utah	0.35	0.32
Maryland	1.62	1.77	Vermont	0.04	0.04
Massachusetts	1.03	1.48	Virginia	1.81	1.76
Michigan	2.58	5.38	Washington	0.88	0.67
Minnesota	0.96	0.76	West Virginia	0.85	1.66
Mississippi	2.65	2.24	Wisconsin	1.35	2.08
Missouri	1.81	2.32	Wyoming	0.12	0.11

Table 4
Fixed Effects Models : Policy Variables

Variable (1)	*Static, with year dummies* (2)	*Static, no year dummies* (3)	*Dynamic, log Y_{t-1}, with year dummies* (4)	*Dynamic, logY_{t-1}, no year dummies* (5)	*Dynamic, Yt-1,with year dummies* (6)	*Dynamic, Yt-1, no year dummies* (7)
	Poisson Model					
3 Strikes	**.399**	**.390**	**.446**	**.435**	**.540**	**.515**
	(2.12)	(2.37)	(2.34)	(2.57)	(2.81)	(3.01)
Shall	.049	.034	.094	.079	.085	.073
	(0.40)	(0.28)	(0.74)	(0.64)	(0.66)	(0.58)
Exec	-.003	-.002	-.004	000	-002	.001
	(0.19)	(0.11)	(0.23)	(0.00)	(0.12)	(0.07)
FSE	**-.197**	**-.196**	**-.253**	**-.237**	**-.296**	**-.283**
	(2.01)	(2.03)	(2.30)	(2.20)	(2.65)	(2.60)
Prison	.005	.002	.011	.008	.012	.008
	(0.56)	(0.30)	(1.34)	(0.96)	(1.37)	(1.01)
Y_{t-1}			-.052	-.050	**-.036**	**-.035**
			(1.02)	(0.97)	(2.55)	(2.53)
Y_{t-2}			**-.161**	**-.168**	**-.038**	**-.039**
			(3.09)	(3.26)	(2.77)	(2.96)
	Negative Binomial Model					
3 Strikes	.331	.294*	.375*	**.359**	**.430**	**.397**
	(1.60)	(1.65)	(1.81)	(1.98)	(2.08)	(2.18)
Shall	.140	.130	.131	.116	.125	.119
	(1.05)	(1.00)	(0.94)	(0.86)	(0.90)	(0.86)
Exec	-.013	-.012	-.017	-.014	-.016	-.014
	(0.68)	(0.64)	(0.82)	(0.69)	(0.82)	(0.71)
FSE	-.135	-.141	-.174	-.168	-.190	-.190
	(1.26)	(1.34)	(1.46)	(1.44)	(1.59)	(1.61)
Prison	-.000	-.000	.001	.000	.001	.000
	(0.05)	(0.25)	(0.63)	(0.35)	(0.64)	(0.38)
Y_{t-1}			-.023	-.023	-.021	-.021
			(0.42)	(0.41)	(1.45)	(1.44)
Y_{t-2}			**-.143**	**-.151**	-.023	-.026*
			(2.55)	(2.73)	(1.59)	(1.76)

Note: the coefficients are percent changes. T-ratios are in parentheses (* indicates significance at the .10 level, **bold** indicates significance at the .05 level, two-tailed). The control variables are listed in Table 2. Complete results and data used in the analysis are available on the internet.

Table 5
Fixed Effects Models: logarithmic specification: 3-Strikes and Firearms Sentencing Enhancement Laws

3strikes	*T-ratio*	*FSE*	*T-ratio*	*Model*	*Dynamic / Static*	*Feedback*	*Year Dummies*
.343	2.16	-.160*	1.65	Poisson	static	no	no
.351*	1.92	-.149	1.51	Poisson	static	no	yes
.413	2.52	-.206*	1.90	Poisson	dynamic	log	no
.436	2.35	-.191*	1.73	Poisson	dynamic	log	yes
.466	2.83	**-.228**	2.11	Poisson	dynamic	linear	no
.496	2.67	-.211*	1.90	Poisson	dynamic	linear	yes
.347	2.00	-.131	1.25	Neg bin	dynamic	no	no
.362*	1.79	-.122	1.14	Neg bin	dynamic	no	yes
.409	2.31	-.167	1.44	Neg bin	static	log	no
.425	2.11	-.160	1.36	Neg bin	static	log	yes
.451	2.56	-.185	1.60	Neg bin	dynamic	linear	no
.478	2.38	-.173	1.47	Neg bin	dynamic	linear	yes

Notes to Table 5. The control variables included in all the regressions are the same as in Table 4, except that all continuous regressors are expressed as logarithms. The Feedback column refers to the treatment of the lagged dependent variables. "No" means a static model with no lagged dependent variables, "log" means that police killings are logged before being lagged, and "linear" refers to the exponential feedback model in which the lagged dependent variable is kept in its natural units. In all cases we use two lags of the dependent variable. (* indicates significant at the 10 percent level , **bold** indicates significance at 5 percent level, two-tailed.) Complete results are available on the internet.