The Weak Effect of Imprisonment on Crime: 1971–1998*

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Objective. This article studies the impact of increasing incarceration rates on crime rates. First we seek to replicate the findings of previous studies utilizing the pooled, fixed-effects models (which are based on the assumption that the effect of imprisonment does not vary across states). Next we test the validity of this assumption. Finally, we present a new methodology to examine the imprisonment-crime relationship. *Methods*. Annual state-level data from 1971–1998 are used to estimate 51 state-specific regression models in which crime rates for seven major categories are functions of incarceration rates and a wide array of socioeconomic and dummy control variables. *Results*. Our findings are consistent with prior studies. More important, the assumptions upon which the fixed-effect models are based were found to be statistically invalid. The results of our new methodology reveal that imprisonment rates are not significantly related to crime in the majority of states for any of the seven crimes studied. *Conclusions*. Because the state-level lagged imprisonment coefficients varied from significant negative effects to significant positive effects (depending on the state and type of crime), we argue that it is inappropriate to speak about "the" effect of imprisonment on any particular crime or at the national level.

Introduction

Do higher incarceration rates reduce crime? For a variety of social commentators and policymakers, the answer has clearly been yes, a belief that has underwritten a boom in prison building, the imposition of mandatory sentences, "three-strikes" legislation, and a reduced possibility of parole.[1]

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[1] Writing in the *Washington Post*, Eugene Methvin exposed "America's best kept secret . . . the huge prison expansion has produced a tremendous payoff" (10/27/91). Former Attorney General William Barr spelled it out in simple terms, "More prison space or more crime" (1992:345). Political scientist John DiIulio wrote in a *New York Times* editorial (1/16/96) that after several years of declining crime, "prisons are a bargain." A year later, fellow political scientist Charles Murray published an article titled "The Ruthless Truth: Prison Works," in which he asserted that there was "no question" prison works (*London*

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Imprisonment, it is argued, reduces crime simply by incapacitating offenders, thereby limiting future transgressions.[2] Additionally, imprisonment can serve as a deterrent, to the extent that individuals choose rationally between legal and illegal activities by weighing expected benefits and costs (Becker, 1968). More certain and/or more severe punishment raises the costs of illegal behavior, thereby convincing at least some individuals to refrain from crime.

Despite faith in imprisonment as an effective crime-control strategy, there are sound theoretical reasons to suspect that increased incarceration might actually exacerbate crime. Recent work based on social disorganization theory suggests that more imprisonment might boost crime rates by weakening the controls on crime imposed by individuals, families, and communities.[3] Greater imprisonment can also diminish future community cohesion by reducing the likelihood that ex-convicts obtain steady, decent jobs.[4] Finally, imprisonment can make community members less willing to work with political institutions to reduce crime by creating a view that the "political system" is an enemy rather than an ally (Rose and Clear, 1998:458). The significance of these possible effects relative to the incapacitation/deterrence impacts of imprisonment is unclear and must be settled empirically.

To date, the empirical evidence is mixed. Depending on the sample, methodology, and control variables used, studies have found negative, positive, and no significant link, causal or otherwise, between incarceration and crime. The most sophisticated research, based on pooled cross-section/time-series analyses of state-level data, which include a wide array of control variables, generally indicates that higher incarceration rates significantly reduce

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Times, 1/12/97). Policymakers have expressed similar sentiments concerning the efficacy of law-enforcement initiatives. Upon release of the 1998 Uniform Crime Reports, which revealed the lowest violent crime rate since 1973, Attorney General Janet Reno credited the additional "police officers on the street and greater partnerships between law enforcement agencies." Congressional Republicans, meanwhile, credited their legislation for "longer prisons sentences" (*Philadelphia Inquirer*, 10/19/99, p. 2).

[2] Zimring and Hawkins suggest that incapacitation is now the "principal justification for imprisonment" (1995:3).

[3] Rose and Clear, for example, warn that increasing imprisonment may well "exacerbate the problems that led to crime in the first place" (1998:441). The authors identify various detrimental effects that imprisonment has on family, economic, and political systems within the communities from which inmates are disproportionately drawn. They explain that these communities display higher rates of family disruption (e.g., more single-parent households), a factor that has been linked to crime. They further note that, prior to incarceration, many offenders were an economic resource within the community. Spelman also has speculated whether the "War on Drugs may have had the unintended side effect of increasing, not reducing, the crime rate" (2000:117).

[4] Crutchfield and Pitchford (1997), Laub and Sampson (1993), and Hagan (1993) found that the incarceration of juveniles reduces the likelihood of obtaining a steady job in the future, while McGahey (1986) provides evidence that persistent unemployment undermines informal social control, which contributes to crime. Therefore, it is plausible to assume that neighborhoods experiencing further economic decline "as a result of incarceration will experience an increase in crime" (Rose and Clear, 1998:461).

crime in various major categories and suggests that imprisonment has a high payoff in terms of crimes prevented (Marvell and Moody, 1994, 1996; Levitt, 1996; Besci, 1999). Nonetheless, the pooled regression technique suffers a serious shortcoming in that it requires the use of questionable model restrictions that the data can easily reject. Thus, conclusions from these analyses are suspect at best.

This study reinvestigates the impact of imprisonment on crime based on a new methodology. Annual state-level data from various sources, spanning the years 1971 to 1998, are used to estimate 51 state-specific regression models (all states plus Washington, D.C.) in which crime rates for seven major categories are functions of lagged incarceration rates and numerous socioeconomic and dummy control variables. The article argues that the methodology employed here is superior to those used previously, including pooled cross-section/time-series regressions.

Estimates from the state-level regressions are used to test the null hypothesis:

H_0: *Imprisonment has no effect on crime; against the alternative hypothesis,*
H_1: *Imprisonment affects crime.*

The estimates reveal that the data for a large majority of the states, generally 85 percent or more, fail to reject the null hypothesis. The results also indicate that the effects of imprisonment on crime vary widely across states and across types of crime, both in terms of magnitude and direction. In sum, the evidence provided undermines the view that higher incarceration rates depress crime and calls into question the efficacy of imprisonment as a crime-control strategy.

Previous Research

Bivariate Correlations

Existing empirical studies use alternative methodologies, although all rely on state-level data. The simplest ones entail bivariate correlations of crime rates and overall incarceration rates, or of changes in each.[5] Overall, these analyses have not found significant relationships between increasing incarceration and declining crime rates.

Focusing on Texas and California, Eklund-Olson, Kelly, and Eisenberg (1992) observed that California rapidly expanded its prison system during the 1980s, while the Texas system was prohibited from operating above capacity (as it had done previously). They thus tested whether crime reduc-

[5] The use of overall incarceration rates, rather than incarceration rates specific to individual crimes, rests on the argument that criminals do not specialize in particular crimes and that potential criminals are generally deterred by the greater likelihood of imprisonment (Cohen and Land, 1987).

tions were greater in California than in Texas. The evidence was mixed: while property crime declined in California and increased in Texas, violent crime increased in both states. Irwin and Austin (1997) examined changes in incarceration rates and crime rates between 1980 and 1991, concluding that, "there is no tendency for those states that increased their prison populations the most to have greater decreases in crime, in fact the opposite is true." Currie (1998) studied whether states with the greatest increase in imprisonment experienced the sharpest declines in crime, and vice versa. His simple state-by-state comparison revealed that this is not always the case.

More recently, Lynch (1999) concluded that "we cannot say with any degree of certainty there is a meaningful relationship between changes in incarceration and changes in crime" (1999:357).

Granger-Sims Tests

Evidence from bivariate correlations suffers from the absence of control variables that conceptually can affect crime rates, such as demographic compositions of the state populations and regional economic health. Not accounting for such cross-state differences can either mask an actual link between crime and incarceration or create a spurious relationship. Granger-Sims regressions mitigate the problem to a point. The procedure explores a possible relationship between two variables by regressing each variable on several lags of itself and of the other variable.[6] For the issue at hand, two regressions would be estimated, one for incarceration rates and the other for crimes rates. The own and other variables' lags can be interpreted as a limited set of generic controls in that they reflect the past dynamic history of the variables under study. Ideally, the number of included lags should be large, but is often limited by the sample size.[7]

Several researchers have adopted this approach and their reported results generally indicate that higher imprisonment leads to reductions in various categories of crime. Using data from 1960–1979, McGuire and Sheehan (1983) reported that higher crime rates lead to more imprisonment, which in turn reduces crime ($p < 0.1$). While acknowledging the simultaneity between crime and imprisonment, Devine, Sheley, and Smith (1988) reported that increases in the prison population have strong negative effects on

[6] If a coefficient on a right-hand-side variable is significant, after controlling for the lags of the left-hand-side variable, the right-hand-side-variable is said to "cause" the left-hand-side variable. Nothing precludes two-way causation in such analyses. Note that causality here is nothing more than temporal ordering and need not reflect a true structural relationship. Moreover, the possible influence of expectations on current behavior makes even a temporal ordering difficult to interpret.

[7] Typically, annual state-level data are pooled so that the data for each state in a particular year constitute an observation. The power of the Granger-Sims test rests on the length of the sample period, however, not the number of observations (since lead/lag relationships are tested). Pooling the data across states provides no help in this regard.

homicide, robbery, and burglary. In studying the effect of changing age structure on homicide and vehicle theft (between 1947 and 1984), Cohen and Land (1987) presented evidence of a strong negative effect of imprisonment on both crimes.[8] Marvell and Moody (1994) offered evidence that, between 1971 and 1989, "prison population growth leads to lower crime rates" (1994:109), and concluded that "state prison population changes preceded crime rate changes, but not the other way around" (1994:129).

Pooled Cross-Section/Time-Series Regressions

A pooled, fixed-effects regression constitutes a more complete analytical framework than either bivariate correlations or Granger-Sims regressions. Specifically, it controls for numerous possible influences on crime, including factors specific to individual states and years that are ignored by the other techniques.[9] Such extensive controls allow a more rigorous test of the crime/incarceration hypothesis than the other two techniques.

The pooled, fixed-effects model combines data for all states and all years into a single data set, with the information from a given state in a given year treated as a separate observation. The chosen crime rate (the dependent variable) is then regressed on the incarceration rate and selected economic and demographic control variables. To account for possible state-specific and year-specific effects not captured by the control variables and, hence, to lessen resulting biases, intercept dummy variables for individual states and years are included (the fixed effects). In addition, separate time trends in the crime rates are sometimes allowed for each state.[10]

Formally, the empirical model is written as:

[8] Cohen and Land (1987) included imprisonment rates and unemployment as control variables in their regressions.

[9] The details of the fixed-effect regression technique can be found in numerous econometric texts, for example, Maddala (1979).

[10] Theoretically, the stationarity of the data's time-series component is an issue. Nonstationary data render standard statistical theory inapplicable and can give rise to spurious correlations among trending series. In these cases, the data must be filtered to remove the nonstationarity before the models are estimated. The addition of a linear trend to the regression models is an effective filter if the data are stationary around a linear trend. However, if the data are integrated of order equal to or greater than one, then other approaches, such as first differencing or the use of co-integrating relationships, are required. For several practical reasons, we have chosen to de-trend the data by including a linear trend and lagged values of the dependent variable. First, tests for specified degrees of integration and for possible co-integrating relationships among variables require a much longer time series than is available here. Second, since many of the variables used in the analysis are ratios that are bounded from above (by 1) or below (by 0) or both, it is either unlikely or impossible that the variables contain unit roots without deterministic drift. Finally, the testing and estimation of possibly co-integrated systems is very complex, with the results sensitive to the form of the chosen test and, at times, quite difficult to interpret. See Hamilton (1994) for a discussion of issues related to the use of nonstationary data. Visual inspection of the data indicates trending in some of the data series, but does not reveal anything like unit root behavior.

$$C_{it} = \alpha_i + \delta_t + \gamma_i{*}T + \beta_1{*}X1_{it} + \beta_2{*}X2_{it} + \ldots + \beta_k{*}Xk_{it} + \beta_{IN}{*}IN_{it} + \varepsilon_{it}, \quad (1)$$

where i and t index states and time, respectively; C is the chosen crime rate; T is a linear time trend; X1 through Xk are the k socioeconomic control variables; IN is the incarceration rate; ε is a random error term; and, α, δ, γ, and the βs are fixed coefficients to be estimated. Variables are typically measured in log-levels, or in first-differences of log-levels; doing so minimizes the impact of outliers and gives the estimated coefficients an elasticity interpretation.

The most recent research relies on this approach and reports that higher incarceration rates significantly reduce at least some categories of crime. Using data for the period 1971–1989, Marvel and Moody (1994) found that state prison populations (lagged to mitigate possible simultaneity problems) had a significant negative effect on robbery, burglary, larceny, and vehicle theft, but little or no impact on murder, rape, and assault. After controlling for underreporting of crimes, they reported, "a rounded estimate of 17 reduced crimes per additional prisoner" (1994:133). Marvel and Moody (1996) similarly reported "highly significant negative coefficients" for the prison population variable for both state-level and city-level crime rates during the years 1973 to 1993 (1996:626). Besci (1999) follows the general approach in Marvel and Moody (1996), with some relatively minor exceptions, and presents findings consistent with theirs.

Levitt (1996) argues that the potential simultaneity between crime and incarceration rates is more effectively handled using an instrumental variable technique, rather than lagging the imprisonment variable. He constructed an instrument for the prison-population rate using the litigation status of states with prison overcrowding problems. The instrument was employed in fixed-effects regression models of the aggregate categories of violent crime and property crime, and of seven individual crime categories (murder, rape, robbery, assault, burglary, larceny, and auto theft.) His analysis of panel data covering the period 1971 to 1993 produced mixed results. Levitt found that both aggregate crime categories were negatively and significantly affected by imprisonment rates, although only two of the seven individual crime categories were (robbery and burglary).[11] Based on the estimates for the seven crime categories (including the five insignificant prison-rate coefficients), Levitt concludes that "each additional prisoner leads to a reduction . . . of 15 fewer crimes" (1996:345). Spelman (2000) extends Levitt's data set

[11] Levitt (1996) notes that the estimated coefficients on the instrumented prison rate in the different equations are two to three times as large as the analogous ones on uninstrumented prison rates. He takes this as evidence for the importance of controlling for simultaneity by instrumenting. Yet, the instrumented coefficients have very large standard errors and, statistically, are not different from the uninstrumented coefficients. Moreover, the results for the auxiliary instrument regression are not presented, so the adequacy of the litigation dummies cannot be assessed. The practical importance of instrumenting as in Levitt (1996) thus remains debatable.

through 1997 and estimates a model that allows for the separation of the effects of imprisonment on crime from the effects of crime on prisons. He concludes that while the crime rate would have declined during this time period, the "crime drop would have been 27% smaller than it actually was had the prison build-up never taken place" (2000:123).

Methods

The fixed-effects regression model, while useful in some respects, has a serious limitation. In its favor, the model permits researchers to greatly increase the available sample size by pooling data from different units of observation; in this case, samples from different states. Rather than estimating separate regressions for each state using observations equal to the number of years, one can increase the sample size to the number of states times the number of years, and, one hopes, also increase the precision of the estimates. The cost of adopting the strategy, however, is the imposition of arbitrary restrictions on the estimated coefficients. Although the fixed-effects model allows different intercept values for each state and time period (via state and time dummies), it requires that each of the estimated response coefficients (the βs in Equation (1)) are identical for each state.[12]

Conceptually, such restrictions are problematic and can lead to erroneous conclusions. As discussed at length in our Introduction, the relationship between incarceration and crime is theoretically complex. Given the conflicting effects that incarceration can have on criminal activity, it is unreasonable to expect incarceration to have a single, homogeneous effect on all types of crime in all states. There can be considerable variation in regional populations and their experiences with crime and crime-control efforts, in existing social relations within communities both at the individual and institutional levels, as well as in political climates. Depending on these differences, the incapacitation/deterrence effects of incarceration might predominate, leading to less crime; social disorganization effects might predominate, leading to greater crime; or the competing effects might largely balance, leading to no noticeable effect on crime. And while imprisonment might appear to have a single, significant effect in pooled regressions, such a result can simply reflect the influence of especially strong relationships in a few states and hide large disparities in the impact of incarceration across the majority of states. Any statistical attempt to measure the impact of incar-

[12] The point is well known; indeed, Marvel and Moody (1994:125) explicitly acknowledge it: "The analysis could have been done by treating each state as a separate time series, but that would mean that we only have 19 observations. If we assume, as is done in cross-section regressions, that the coefficients for the separate states are not significantly different from each other, then we can create a constrained time series regression by pooling across states." Marvel and Moody's discomfort with separate regressions for each state based on 19 years of data is puzzling, given their reliance on the same number of years for the Granger causality tests in other parts of their analysis.

ceration on crime must account for the impact's likely variation across states.

Consequently, it is critical to test the validity of the restrictions themselves before estimating a pooled model. Violation of the restrictions invalidates the pooled model and requires the separate estimation of regression models for each state, a feasible strategy given the current length of state sample periods (Maddala, 1979:322–26). Studies to date have given virtually no attention to the validity of the coefficient restrictions, a serious shortcoming that calls into question both the basic estimates of incarceration's effects and the policy conclusions that flow from the estimates.[13]

The present empirical analysis takes as a starting point the pooled cross-section/time-series model. Unlike previous analyses, however, this article examines whether the coefficient restrictions imposed by the pooled model are statistically valid. This is accomplished using a standard analysis of variance, as described below. Because the restrictions are found to be invalid, an alternative methodology is presented in which 51 individual state-level regressions are estimated (all states plus Washington, D.C.), thereby allowing incarceration to have potentially different impacts on crime in each of the states. The estimates from the 51 state-level models are then combined, using classical sampling theory, to test the significance of the crime/incarceration link.

Testing the Validity of the Pooling Restrictions

The validity of the coefficient restrictions imposed by the pooled regression model is evaluated using a so-called Chow test.[14] First, one estimates separate equations for each state and adds the residual sum of squares across all state regressions. This total is termed the unrestricted sum of squares, or URSS, since the estimated βs are free to vary by state. Next, one estimates the pooled model and obtains the residual sum of squares from the pooled regression. This is termed the restricted sum of squares, or RSS, since the estimated βs are constrained to be equal across states. These two magnitudes are then used to form an F statistic, computed as:

$$[(RSS\text{-}URSS)/URSS] \;/\; [(df_{RSS} - df_{URSS})/df_{URSS}], \qquad (2)$$

[13] Levitt (1996) reported results from two alternative estimates for the aggregate categories in which the coefficient on the uninstrumented imprisonment rate was allowed to vary according to whether: (1) a state was in the "south" or not; and (2) whether a state's entire prison system was under a court order or not. The coefficients on prison rates were significant and the null hypothesis of no difference in the coefficients for each bifurcation could not be rejected.

[14] A formal discussion of tests on pooling restrictions is found in Maddala (1977:322–26).

where df_i represents the degrees of freedom for the unrestricted and restricted sum of squared residuals.[15] Essentially, the F statistic indicates the percentage increase in the residual sum of squares due to the restrictions, adjusted for degrees of freedom. If the calculated F exceeds the critical values (meaning the increase in the residual sum of squares due to the restrictions is statistically large), then the pooling restrictions are rejected. In such cases, individual, state-specific models must be estimated.

An alternative version of the test, one that reduces the chances that the pooling restrictions are found to be invalid, allows only the imprisonment coefficient to vary across states, while restricting the remaining coefficients to be equal across states. Doing so avoids the possibility that the pooling restrictions are rejected due to cross-state variation in coefficients other than the imprisonment coefficient. It has the added benefit of increasing the degrees of freedom in the estimation. In an effort to give the pooling restrictions the benefit of the doubt, and thus to strengthen the case against them should they be rejected, the alternative version of the test is used.

Results

Pooled Results

The first stage of this analysis employs data from Marvell and Moody (1994), which spans the period 1971–1992. We examined the impact of incarceration rates on the FBI's seven Index Crimes using a fixed-effects model. Each pooled model regresses a particular crime rate on a constant, the lagged prison rate, and a set of control variables including the percent of a state's population that is African American, percent of a state's population residing in a metro area, the percent of a state's population aged 17 to 24 years, the percent of a state's population officially classified as poor, the state's unemployment rate, the state's inflation-adjusted per capita personal income, and two lagged values of the crime rate.[16] All variables are measured in natural log-levels. Each of the seven pooled regressions also includes state dummy variables and separate state time trends.[17] Each year's values

[15] URSS equals: (number of states × number of years) − (number of estimated parameters × number of states); RSS equals: (number of states × number of years) − number of estimated parameters. Different states have different sample lengths because some states lack key variables, such as the unemployment rate, for certain years. All state samples lose two annual observations due to the inclusion of two lags of the dependent variable in the regressions.

[16] Different studies have included different sets of control variables, dummies, and trends. The formulation used in this article is based on that in Marvell and Moody (1996). It is quite comprehensive and incorporates the key control variables used in the literature.

[17] Estimates were generated using both ordinary least squares and weighted least squares, where the weights equal the square root of the states' populations (see, e.g., Marvell and Moody, 1994). Weighting is a standard procedure used to mitigate problems of heteroskedasticity. The two sets of estimates produced no meaningful differences and we focus on the simple unweighted estimates.

TABLE 1

Regression Results: 1971 to 1992[a]

Crime	Pooled Regressions: Estimated Coefficients on Lagged Imprisonment in Pooled Regressions[b]	Pooled Regressions: F Statistic to Test the Validity of the Pooling Restrictions[c]	Individual State Regressions: Percent of States Where Null Hypothesis is Rejected: Negative β[d]	Individual State Regressions: Percent of States Where Null Hypothesis is Rejected: Positive β[d]
Murder	–0.121*	1.475*	9.8	3.9
	(–2.275)			
Rape	–0.031	1.516*	7.8	5.9
	(–0.627)			
Assault	–0.020	1.201	5.9	5.9
	(–0.614)			
Robbery	–0.099**	2.114**	13.7	5.9
	(–3.826)			
Burglary	–0.151**	2.106**	5.9	5.9
	(–3.674)			
Larceny	–0.095**	2.431**	5.9	2.0
	(–2.365)			
Car theft	–0.114**	2.272**	13.7	3.9
	(–3.217)			

[a]Data are available at <http://morton.wm.edu/~cemood/crime.dat>. Observations are annual and span the period 1971 to 1992.

[b]The dependent variable in each case is the state-level deviation of the natural log of the per capita crime rate from the national average. Explanatory variables are percent of a state's population that is African American, percent of a state's population officially classified as poor, percent of a state's population residing in a metro area, percent of a state's population aged 17 to 24 years, the state's unemployment rate, the state's inflation-adjusted per capita income, two lagged values of the crime rate, the lagged per capita prison rate, and state-specific dummy variables and time trends. The prison, demographic, and economic variables are entered as natural logs. The standardized β is the estimated coefficient on the lagged prison rate divided by the crime variable's standard deviation. T-statistics are in parenthesis below the coefficient. * and ** indicate null hypothesis of no effect is rejected at the 5 percent and 1 percent level, respectively.

[c]The F statistic is used to test the validity of the pooling restrictions as discussed in the text. The calculated statistic has 50 degrees of freedom in the numerator and 895 degrees of freedom in the denominator. * indicates significance at the 5 percent level (critical value = 1.36); ** indicates significance at the 1 percent level (critical value = 1.54). Critical values from Lapin (1987).

[d]For each crime category, the fraction of the 51 state-level coefficients on the lagged prison rate that is significant at the 5 percent level.

for the dependent variables (the crime rates) are measured as deviations from the national average value for each year. Doing so accounts for nationwide changes in crime rates potentially missed by the independent variables and obviates the need for year dummy variables. The 51 individual state-level models (for each crime) take the same form as the corresponding pooled regressions, absent the state dummies and the time trends for the other states.

Table 1 summarizes the results of the estimations based on the original Marvell and Moody data for the years 1971 to 1992. The results indicate that lagged prison rates have a negative and significant impact ($p < 0.05$ level) on five of the seven crime rates (rape and assault are the exceptions). These findings are consistent with the literature using the fixed-effects model. Marvel and Moody (1994), for example, also reported that lagged prison rates had significant impacts on robbery, burglary and larceny, and auto theft. These results indicate a stronger prison effect than those suggested by Levitt (1996). Although he reported larger prison coefficients, only robbery and burglary were statistically significant. Our findings are also consistent with Becsi (1999), who found that prison rates significantly affect homicide. Overall, the pooled results appear to make a credible case for the efficacy of incarceration as an anti-crime tool.[18]

Testing the Pooling Restrictions

As previously discussed, the pooled analysis relies on the assumption that the βs are not significantly different across states. Column 3 of Table 1 contains the calculated F statistic, based on the residual sum of squares from the overall regressions and the 51 individual state-level regressions in which only the coefficient on lagged imprisonment is allowed to vary by state. The table entries indicate that the pooling restrictions are statistically invalid (i.e., rejected at the 1 percent level) for six of the seven crimes (assault was the exception—but it was not significantly affected by imprisonment rates). Because the pooling restrictions are invalid, conclusions based on the pooled regressions are of questionable value. Therefore, the effects of prison rates on crime rates must be estimated using individual state-level regressions.

A Test of Proportions

Columns 4 and 5 of Table 1 summarize the regression estimates by listing, for each crime, the proportion of states for which the coefficients are either significantly negative or positive (and hence for which the null hypothesis of no effect is rejected). As can be seen, these proportions are small, ranging from 6 to 14 percent for negative coefficients and from 2 to 6 percent for positive coefficients. Even these proportions overstate the real effect

[18] Attempts at estimating the equations using instruments for the prison rate instead of a lagged value (to account for possible simultaneity) gave disappointing results. Similar to Levitt (1996), our models produced very large standard errors and generally insignificant coefficients on the prison-rate variable. We use the pooled results based on the lagged prison rate, as these more strongly support the view that prisons matter and, thus, offer a more difficult challenge to our alternative perspective.

of imprisonment because the proportions are estimated values and are subject to sampling variability. For instance, given random sampling variation, one can expect that the data for up to 5 percent of the states will indicate a significant impact of crime when none exists.

Whether the observed proportions of significant coefficients reflect something more than random sampling variability can be formally tested. Each state's time series constitutes a separate sample that produces an estimated imprisonment coefficient. Under the null hypothesis that imprisonment has no impact, the observed proportion of states with a significant coefficient, P, is distributed normally with mean p and standard deviation s $= ((p(1\text{-}p)/n)^{1/2}$, where n is the sample size. The normal deviate, with a correction for continuity, is $z = (|P\text{-}p| - 1/2n)/s$. Here, $p = 0.05$, $n = 51$, and P is obtained from the regression results for each major crime.[19] Based on the given values of p and n, the observed proportion of states (P) must equal or exceed 12.12 percent to be significantly greater than 5 percent at the 95 percent level of confidence.[20] This is equivalent to finding negative (or positive) and significant imprisonment coefficients in seven or more states.

The estimates indicate that the observed proportions of states with significant negative imprisonment coefficients exceed 12.12 percent for only two crimes—robbery and auto theft. Even in these two cases, imprisonment's effect was limited to seven states for auto theft and 10 states for robbery. Furthermore, only one state, Florida, is common for each crime. Not only does imprisonment have a small measured impact on crime, but also the effects in each of the states are dependent on the particular crime chosen. Contrary to the pooled regression results, the more appropriate state-level results provide at best very weak support for the use of incarceration as a crime-control tool and only in isolated instances. None of the proportions of positive and significant coefficients were significantly different from 5 percent.[21]

Results Using Updated Data

Several years have passed since the original Marvel and Moody work. In addition, some of their data, such as personal income, unemployment rates, and Consumer Price Index, have been revised. To examine the implications

[19] The following test is described in Snedecor and Cochran (1967:209–13).

[20] Under the null hypothesis that $p = 0.05$, and given $n = 51$, the standard error of each crime's proportion equals 0.030518 (using the formula for s in the text). Multiplying the standard error times the 95 percent critical value of 2.01 (for $n = 51$) and adding the correction for continuity of 0.0098 yields the required difference from 0.05. This equals 0.0712. Adding the required difference to 0.05 gives the needed 12.12 percent proportion.

[21] A referee suggested that we check the robustness of the results by omitting the two lags of the crime rate, thereby estimating models that allow only an impact effect. The presence of the lags indicates distinct impact and long-run effects. We redid the entire analysis after reestimating every equation eliminating the two lagged crime rates. Doing so leaves our conclusions unchanged.

TABLE 2

Regression Results: 1971 to 1998[a]

	Pooled Regressions		Individual State Regressions	
Crime	Estimated Coefficients on Lagged Imprisonment in Pooled Regressions[b]	F Statistic to Test the Validity of the Pooling Restrictions[c]	Percent of States Where Null Hypothesis is Rejected: Negative β[d]	Percent of States Where Null Hypothesis is Rejected: Positive β[d]
Murder	–0.052 (–1.102)	1.574**	7.8	5.9
Rape	0.016 (0.621)	0.151	11.8	3.9
Assault	–0.077 (–1.798)	1.676**	7.8	11.8
Robbery	–0.095 (–1.536)	2.870**	17.7	7.8
Burglary	–0.110** (–4.974)	1.615**	11.8	7.8
Larceny	–0.056** (–3.511)	1.959**	11.8	9.8
Auto theft	–0.135** (–3.531)	1.447*	19.6	5.9

[a]The original data are available at <http://morton.wm.edu/~cemood/crime.dat>, and span the period 1971 to 1992. Updated series and observations for 1993 to 1998 were obtained from Current Population Surveys, Census Bureau publications, and Department of Labor publications.

[b]The dependent variable in each case is the state-level deviation of the natural log of the per capita crime rate from the national average. Explanatory variables are percent of a state's population that is African American, percent of a state's population officially classified as poor, percent of a state's population residing in a metro area, percent of a state's population aged 17 to 24 years, the state's unemployment rate, the state's inflation-adjusted per capita income, two lagged values of the crime rate, the lagged per capita prison rate, state-specific-time trends, and state dummy variables. The prison, demographic, and economic variables are entered as natural logs. The standardized β is the estimated coefficient on the lagged prison rate divided by the crime variable's standard deviation. T-statistics are in parenthesis below the coefficient. * and ** indicate null hypothesis of no effect is rejected at the 5 percent and 1 percent level, respectively.

[c]The F statistic is used to test the validity of the pooling restrictions as discussed in the text. The calculated statistic has 50 degrees of freedom in the numerator and 1,034 degrees of freedom in the denominator. * indicates significance at the 5 percent level (critical value = 1.36); ** indicates significance at the 1 percent level (critical value = 1.54). Critical values from Lapin (1987).

[d]For each crime category, the fraction of the 51 state-level coefficients on the lagged prison rate that is significant at the 5 percent level.

of using more current data, we updated and augmented the data to 1998 using information from the March Current Population Surveys and publications from the Census Bureau, Department of Labor, and Department of Justice. We then redid the analysis with the data on the same variables from 1971–1998. The results are summarized in Table 2.

FIGURE 1

Estimated Effects of Imprisonment on Crime
Auto Theft

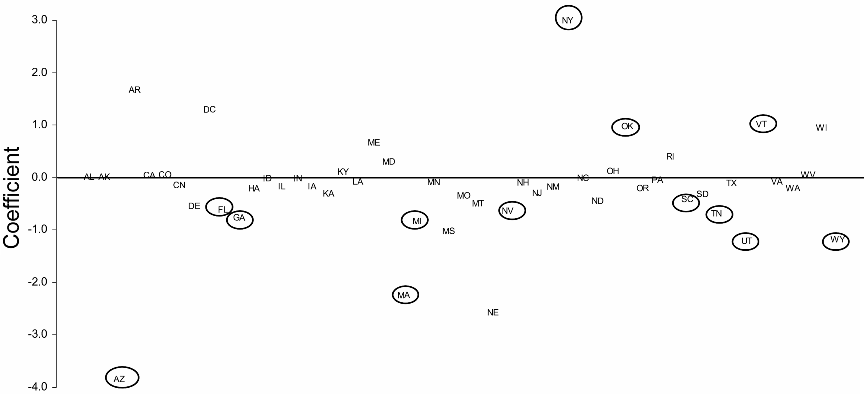
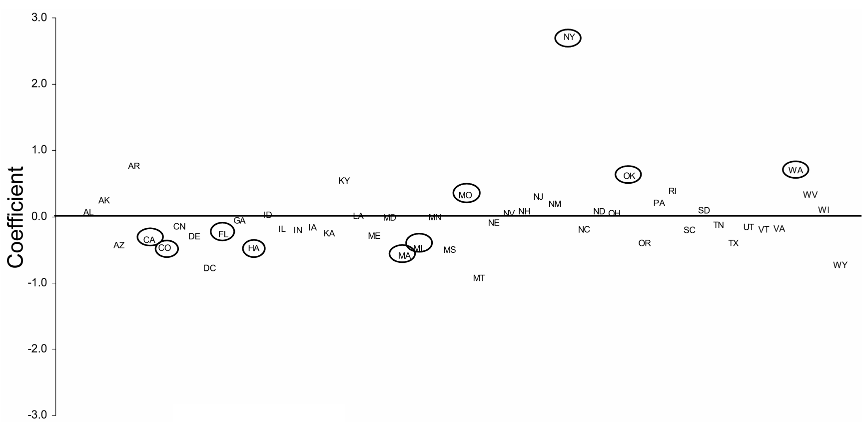


FIGURE 2

Estimated Effects of Imprisonment on Crime
Burglary



The pooled models for each crime rate (Column 2) reveal that lagged prison rates have a negative and significant impact ($p < 0.05$) on only three of the seven crimes (burglary, larceny, and car theft). The finding that homicide is not significantly affected by imprisonment rates is consistent with Besci (1999) and Marvel and Moody (1994). More important, however, the results in Column 3 show that the pooling restrictions are rejected

FIGURE 3

Estimated Effects of Imprisonment on Crime
Larceny

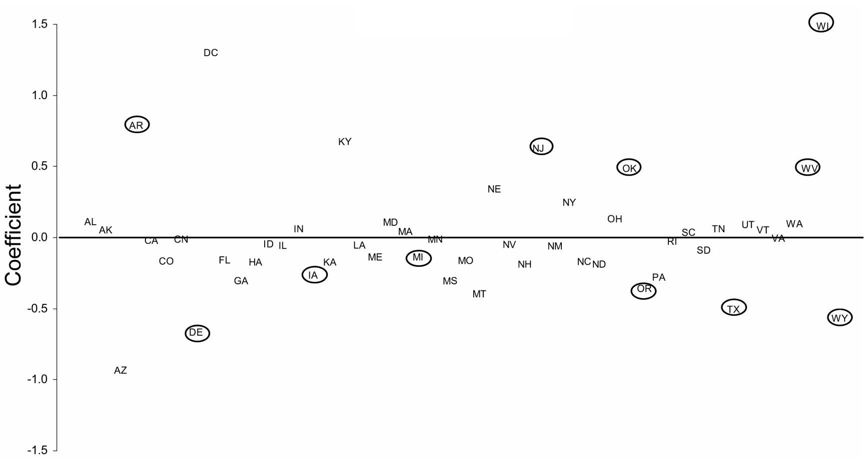
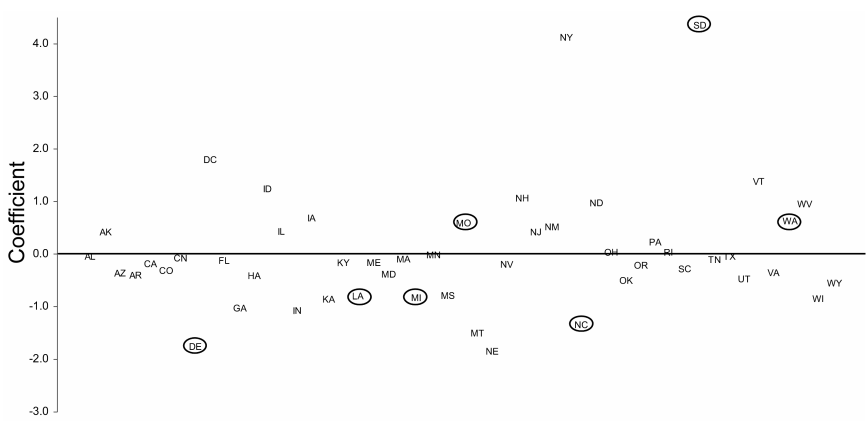


FIGURE 4

Estimated Effects of Imprisonment on Crime
Murder



more easily than with the 1971–1992 Marvel and Moody data ($p < 0.01$). Columns 4 and 5 of Table 2 present the proportions of negative and positive coefficients that are significantly different from 5 percent. Once again, the proportion of rejections is high, producing Z values that reject the hypothesis of a negative effect in five of the seven crime categories.

FIGURE 5

Estimated Effects of Imprisonment on Crime
Rape

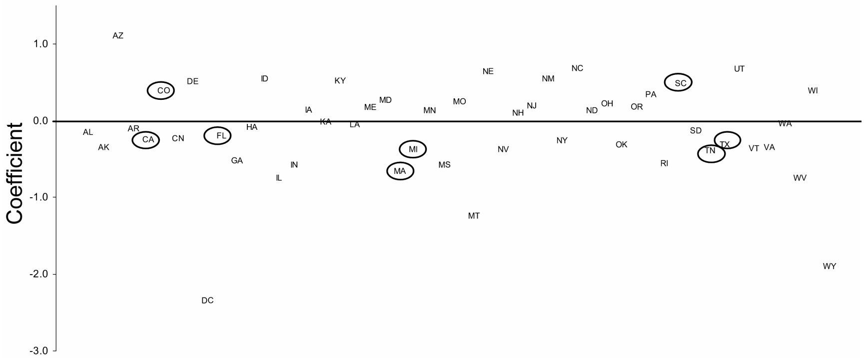
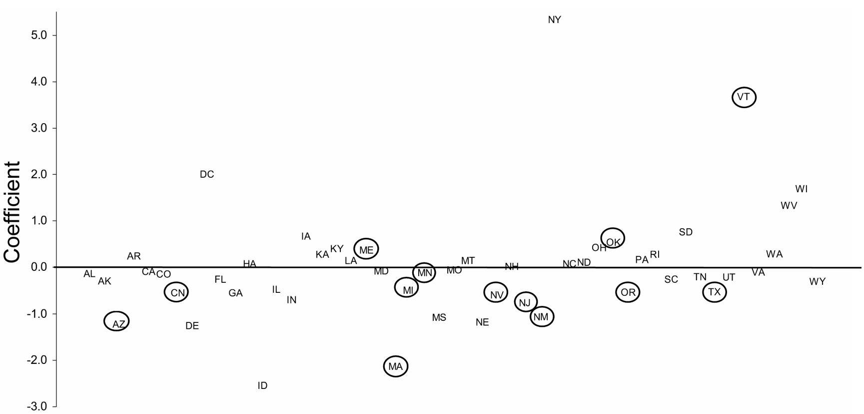


FIGURE 6

Estimated Effects of Imprisonment on Crime
Robbery



To emphasize the great variability in the estimated effects of imprisonment on crime, we present the actual coefficients for all crimes except assault (where the pooling restriction was valid) from the state-level regressions (Figures 1–6). Within each crime category, the sizes of the coefficients vary widely and the coefficients straddle both sides of zero. And as seen in Table 2, relatively few of the estimated coefficients are significantly different from zero at the 5 percent level (states with significant coefficients are identified by a circle around the two-letter abbreviation).[22]

Conclusions

The relationship between incarceration and crime is theoretically and methodologically complex. The search for a possible link has thus led researchers to use increasingly sophisticated methods, from simple bivariate correlations to more complex pooled, cross-section/time-series regression models. These models typically indicate that higher incarceration rates significantly decrease several categories of crime.

Although complex techniques can conceivably improve our understanding of the issue, the validity of the conclusions rests on key assumptions that can be overlooked. This article has reexamined evidence from the pooled regression model, focusing attention on the model's underlying premise that the effects of incarceration on crime are the same across all states. Formal tests clearly show the coefficient restrictions imposed by the model to be invalid, rendering the associated estimates and conclusions questionable at best. When separate regressions are estimated for each state, as is appropriate, the data reveal that imprisonment has no statistically significant effect in the majority of states for any of the seven crimes studied.

For the period 1971 to 1998, increasing imprisonment reduced only robbery and auto theft, with the coefficients differing substantially from state to state. The significant coefficients for these two crimes are the result of the large effect in relatively few states. Thus, it is imprudent and misleading to speak about "the" effect of incarceration on any particular crime, or across states, or at the national level. Policymakers should reconsider further prison expansion as the primary crime-control strategy. There was variation in the estimated effects of imprisonment on crime, ranging from significant negative effects to significant positive effects. These findings are consistent with Spelman's speculation that "some states could very well benefit from further prison expansion, and others may have expanded too much as it is" (2000:125).

The potential offsetting effects of deterrence and social disorganization constitute a plausible reason for incarceration's minimal observed effect on crime reduction. It may well be that increasing imprisonment will reduce crime, but at some threshold, Rose and Clear (1998) are correct and imprisonment then exerts a positive effect on crime. The critical question is why does increasing imprisonment reduce crime in some states, while increasing crime in other states. We are currently investigating the determinants of the interstate variations in the impact of imprisonment using a probit model.

[22] We note here that some coefficients that are statistically insignificant are larger than others that are significant. This occurs because significance depends on the size of the estimated coefficient relative to its standard error, and not simply its raw magnitude.

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