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Author(s): Steven D. Levitt

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THE EFFECT OF PRISON POPULATION SIZE ON CRIME RATES: EVIDENCE FROM PRISON OVERCROWDING LITIGATION*

STEVEN D. LEVITT

Simultaneity between prisoner populations and crime rates makes it difficult to isolate the causal effect of changes in prison populations on crime. To break that simultaneity, this paper uses prison overcrowding litigation in a state as an instrument for changes in the prison population. The resulting elasticities are two to three times greater than those of previous studies. A one-prisoner reduction is associated with an increase of fifteen Index I crimes per year. While calculations of the costs of crime are inherently uncertain, it appears that the social benefits associated with crime reduction equal or exceed the social costs of incarceration for the marginal prisoner.

The incarceration rate in the United States has more than tripled in the last two decades. At year-end 1994 the United States prison population exceeded one million. Annual government outlays on prisons are roughly $40 billion per year. The rate of imprisonment in the United States is three to four times greater than most European countries [Chappell 1988].

While the evidence on trends in crime rates is mixed, it does not appear that record levels of incarceration in the United States have been accompanied by large declines in crime, as demonstrated in Figure I. Over the same period that the incarceration rate more than tripled, violent crimes reported to the police, as measured by the Federal Bureau of Investigation's *Uniform Crime Reports,* doubled on a per capita basis, while reported property crimes rose 30 percent. Victimization rates from the National Crime Survey, which reflect both crimes that are reported to the police and crimes that go unreported, demonstrate a more favorable pattern. Violent victimizations remained flat over the period, while property crime victimizations fell by 30

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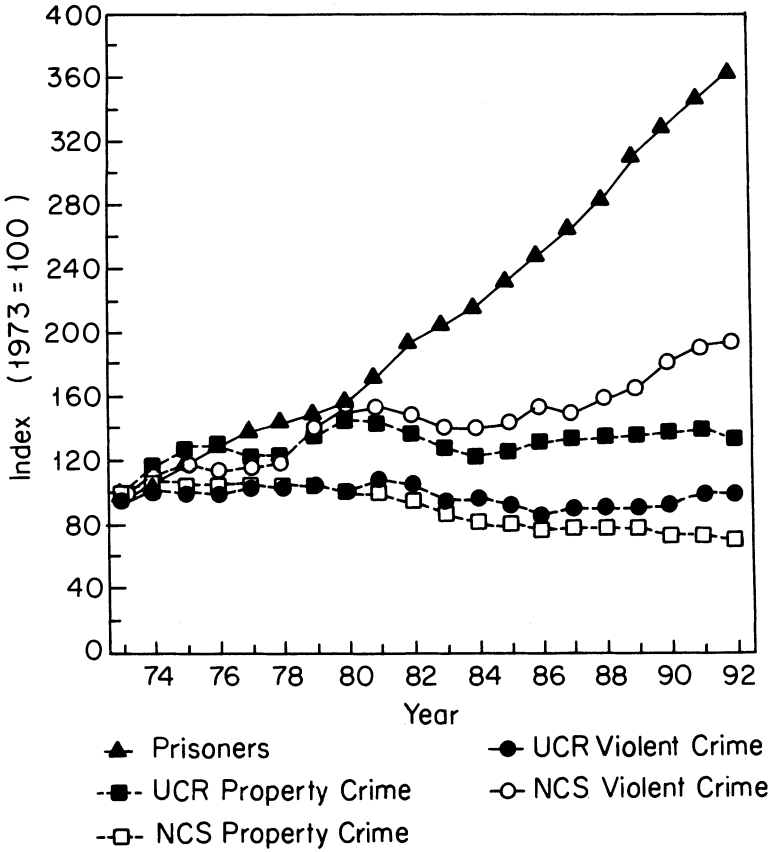

FIGURE I
U. S. Prison Population and Crime Rates 1973–1992

Data are indexed using 1973 as a base year. All underlying variables reflect rates per capita. Prisoners indexes the number of prisoners held per capita in state prisons on December 31. UCR violent and property crimes index the per capita rate of violent and property crimes reported to the police, as compiled in the Federal Bureau of Investigation's *Uniform Crime Reports*. NCS violent and property crimes index victimization rates are drawn from the National Crime Survey.

percent.[1] The fact that crime rates have not fallen dramatically in response to such large increases in imprisonment has led commentators to label the increasing reliance on imprisonment a pol-

1. For a discussion of the various measures of crime in the United States, see O'Brien [1985].

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icy failure, recommending a moratorium on new prison construction, alternative correctional programs, or decriminalization of drug offenses (e.g., Nagel [1977], Rogers [1989], Zimring and Hawkins [1991], and Selke [1993]).

Clearly, however, one cannot conclude that the increased levels of incarceration have been a failure simply based on such time-series patterns. To the extent that the underlying determinants of crime, such as gang involvement, the increase in single-parent families [Bane 1986], and the declining availability of legitimate economic opportunities for teenagers have worsened over time [Grogger 1994; Freeman 1995], the increased use of prisons may simply be masking what would have been a far greater rise in criminal activity.

Increased prison populations can reduce crime through either deterrence (i.e., an increased threat of imprisonment deters people from engaging in criminal acts), or incapacitation (i.e., while incarcerated criminals are unable to commit crimes). While it is more difficult to measure deterrence effects, data on self-reported criminal activity obtained from prisoner surveys provide estimates of incapacitation effects [Peterson and Braiker 1980; Visher 1986; DiIulio and Piehl 1991; Piehl and DiIulio 1995]. The various surveys of prison populations have yielded similar results, the most striking feature of which is the skew of the distribution. In Wisconsin, for instance, the *median* prisoner reports involvement in twelve nondrug-related crimes per year when not imprisoned; the *mean* self-report, in contrast, is 141 [DiIulio and Piehl 1991].[2] Cost-benefit calculations based on prisoner self-reports suggest that the social benefits of incarcerating the median and mean prisoner outweigh the social costs, but that the cost of imprisoning the bottom quartile of inmates outweighs the social benefits [Piehl and DiIulio 1995].

There are, however, a number of risks in using prisoner self-reports as a basis for determining public policy toward the scope of imprisonment. First, there are the standard problems with survey data reliability, especially when the respondents are convicted criminals.[3] A second problem with prisoner self-reports is

2. When drug-related crimes are also included, the median rises to 26, and the mean becomes 1834.

3. A large literature surveyed in Spelman [1994] examines the validity of prisoner self-reports. There is some evidence that, particularly on the extremes, infrequent offenders underreport criminal activity and frequent offenders exaggerate their criminal involvement. Consequently, the median may be a more reliable estimator than the mean.

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that they capture only incapacitation effects of prison sentences and therefore will systematically underestimate the benefits of prisons if deterrence is also present.[4] On the other hand, if crimes are committed in groups, or other criminals step in to replace those incarcerated, the number of crimes prevented through incarceration will be exaggerated. Finally, it is difficult to determine where in the distribution a particular prisoner falls. Attempts to predict recidivism have generally been unsuccessful, although improved econometric techniques have led to greater success [Schmidt and Witte 1989]. Without knowing the capability of parole boards for distinguishing between the risks posed by different prisoners, it is impossible to make an informed choice about the optimal scale of imprisonment since the policy implications of releasing the twenty-fifth percentile prisoner differs dramatically from that of the median or mean prisoner.

An alternative approach to measuring the impact of imprisonment on crime rates is to estimate aggregate elasticities [Bowker 1981; Devine, Sheley, and Smith 1988; Marvell and Moody 1994]. The most relevant of these studies is Marvell and Moody, which uses state-level panel data, obtaining estimates of the elasticity of crime with respect to the prison population of -0.16, a figure that is consistent with other recent estimates in the literature [Spelman 1994; Wilson 1994]. Donohue and Siegelman [1994], using these estimates, conclude that the present scale of imprisonment is approximately optimal from a social cost-benefit perspective.

While the use of aggregate data avoids many of the difficulties inherent in the use of prisoner self-reports, simultaneity bias becomes a critical concern.[5] Increased incarceration is likely to reduce the amount of crime, but there is also little question that increases in crime will translate into larger prison populations. For instance, if there is no change in the punishment per crime, the prison population will trend one-for-one with changes in the crime rate. Consequently, OLS estimates of the effect of prisons on crime are likely to understate the true magnitude of the effect, perhaps dramatically.[6]

4. Ehrlich [1981] and Levitt [1995a] find that deterrence effects are substantially larger than incapacitation effects.

5. Marvell and Moody [1994] do not attempt to control for simultaneity, instead using the results of Granger tests to conclude that there is little evidence of a short-term impact of crime on state prison populations.

6. This simultaneity is pervasive in empirical research on criminal topics. Estimates of the effect of police on crime, for instance, almost invariably carry an unexpected positive sign [Cameron 1988]. Breaking the simultaneity through the

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The objective of this paper is to obtain estimates of the effect of prison populations on crime that are not affected by the presence of simultaneity. To achieve that goal requires an instrumental variable that is correlated with changes in the size of the prison population, but is otherwise unrelated to the crime rates.[7] The variable employed in this paper is the status of state prison overcrowding litigation. Over the past 30 years, prisoners' rights groups have brought numerous civil suits alleging unconstitutional conditions in prisons. In twelve states the entire state prison system either is currently or has formerly been under court order concerning overcrowding.

Not surprisingly, as demonstrated in Section I of this paper, the existence of overcrowding litigation reduces the growth rates of prison populations. For example, in the three years prior to the initial filing of litigation in the twelve states where the entire prison system eventually fell under court control, prison population growth rates outpaced the national average by 2.3 percent annually. In the three years following the filing of litigation, prisoner growth rates lagged behind the nation as a whole by 2.5 percent a year. In the three years after a final court order was handed down, growth rates were 4.8 percent below the national average.

It seems plausible, however, that prison overcrowding litigation will be related to crime rates only through its impact on prison populations, making the exclusion of litigation status from the crime equation valid. Two pieces of evidence support this claim. First, tests of overidentifying restrictions are consistent with the exogeneity of the instruments across all of the specifications considered. Second, changes in litigation status appear to affect crime rates, but not vice versa. Crime rates in earlier years have no predictive value in determining whether overcrowding litigation will be filed in a state. If this were not the case, the exogeneity of the instruments would be suspect.

The results obtained in this paper suggest that the impact of

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use of the timing of mayoral and gubernatorial elections as an instrument for changes in the police force, however, Levitt [1994] finds evidence that adding police lowers crime rates.

7. Nagin [1978] and Sampson [1986] also use instrumental variables to estimate the effect of prison population on crime rates in cross-sectional data. Nagin [1978] employs average state imprisonment rates in the preceding decade as an instrument for the current prison population. Sampson [1986] proposes rated jail capacities as an instrument for jail populations. Nagin does not find that increased incarceration risk reduces crime. Sampson finds a significant negative relationship between jail populations and robbery rates, but not between jail populations and murder rates.

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prison populations on crime is two to three times greater than previous estimates would imply. Prior to instrumenting, I obtain estimates that are actually slightly smaller than those in past research: elasticities of crime with respect to prisoner populations of approximately −0.10. The IV estimates, in contrast, are much larger in magnitude. For violent crime, elasticities of −0.40 are obtained. For property crime, the elasticities are −0.30. My estimates imply that each marginal prisoner released as a result of overcrowding litigation is associated with an increase of fifteen crimes per year, almost exactly the self-reported criminal activity of the median prisoner.

Using the estimates of the costs of crime to victims in Cohen [1988] and Miller, Cohen, and Rossman [1993], the marginal social benefit in crime reduction of adding one prisoner for one year is approximately $50,000. The marginal costs of incarceration are roughly $30,000 per prisoner per year. While cost-benefit calculations of this kind are inherently speculative, it appears that the current level of imprisonment is roughly efficient, although there may be some benefit from lengthening the time served by the current prisoner population.[8]

The outline of the paper is as follows. Section I provides background information on prison overcrowding litigation. Section II describes the data used in the analysis and establishes a negative relationship between overcrowding litigation and the size of the prison population. A positive reduced-form correlation between overcrowding litigation and crime rates is also demonstrated. Section III estimates a relationship between prison populations and crime, using the status of prison overcrowding lawsuits as an instrument. Section IV considers various public policy implications of these estimates. Section V offers a brief set of conclusions.

I. Prison Overcrowding Litigation

Since the first filing of prison overcrowding litigation on the grounds of cruel and unusual punishment in 1965,[9] similar law-

8. The estimates of this paper are less directly applicable to the question of whether expanding the prison population by incarcerating criminals who are currently punished by probation or fines is cost-beneficial. Given the skew in the criminal distribution, however, the benefits of incarceration at this margin are likely to be well below that of longer sentences for the current prisoner population.

9. *Finney v. Arkansas Board of Correction,* 505 F.2d 194 (8th Cir. 1974). A complete summary of litigation can be found in Koren [1993] and other issues of *The National Prison Project Journal.*

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suits have been brought in 47 states and the District of Columbia. The success of plaintiffs in overcrowding litigation has been overwhelming: of the approximately 70 cases brought, at least partial victory has been achieved in all but 6. As of January 1, 1993, litigation was pending in twelve states.

Stipulations of court orders on overcrowding vary substantially. Only on rare occasions do judges mandate the release of prisoners to alleviate overcrowding. More frequently, population caps have been imposed, or "double celling" prohibited, with the prison system administrators and state government left with the freedom to determine the means through which compliance will be attained (e.g., construction of new prison facilities, fewer offenders sentenced to prison terms, early release programs, or reallocation of prisoners across institutions). The court frequently judges compliance to be inadequate, leading to the further step of contempt orders, or court-appointed receivers/monitors.

In twelve states the entire prison system has been under court order concerning overcrowding. In the other states only a portion of the prison facilities has been affected. Whether the entire state prison system is under court control is likely to be critical in determining the impact of such litigation on overall state prison populations. When only some of the prisons are deemed overcrowded, the state has flexibility in shifting prisoners between facilities and need not adjust the total number of prisoners.[10]

The status of prison litigation in a given state at a particular point in time can be classified into one of six categories: (1) no overcrowding litigation filed, (2) litigation filed, but no decision yet handed down, (3) an initial decision reached, but currently under appeal, (4) a final decision reached, (5) further court action such as the appointment of a monitor, and (6) release of the prison system from court supervision. A priori, one would expect categories 2 through 5 to be associated with lower prison growth rates, at least in the short term.[11] Even before a final decision is handed down, prison systems will have incentives to act strategically, improving prison conditions in an attempt to win more favorable court opinions. The sixth category, release of the prison system from court supervision, is likely to be associated with an increased growth rate in the prison population.

Table I identifies the twelve states in which the entire prison

10. There is, however, little shifting of prisoners across state lines.

11. Over a time frame in which the states are able to add prison capacity—four to five years—the effect on prison populations is likely to be less pronounced.

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TABLE I
PRISON OVERCROWDING LITIGATION STATUS 1971–1993
(STATES WITH ENTIRE PRISON SYSTEM UNDER COURT ORDER)

	Prefiling	Filed	Prelim. decision	Final decision	Further action	Released by court
Alabama	71–73	74–75	76–77	78	79–83	84–93
Alaska	71–85	86–89	—	90–93	—	—
Arkansas	—	—	—	71–73	74–81	82–93
Delaware	71–87	—	—	88–91	92–93	—
Florida	71	72–74	75–76	77–79	80–93	—
Mississippi	—	71–73	—	74–93	—	—
New Mexico	71–76	77–79	80–89	90	91–93	—
Oklahoma	71	72–76	—	77–85	—	86–93
Rhode Island	71–73	74–76	—	77–85	86–93	—
South Carolina	71–81	82–84	85–90	91–93	—	—
Tennessee	71–79	80–81	—	82–84	85–93	—
Texas	71–77	78–79	80–84	85–91	92–93	—

Categories of litigation status are defined as follows. Prefiling: no prison overcrowding litigation filed in the state. Filed: litigation filed, but no court decision. Preliminary decision: a court decision has been handed down, but is under appeal. Final decision: a court decision handed down, no further appeals. Further action: subsequent court intervention on the issue of overcrowding, including appointment of special monitors, contempt orders. Released by court: dismissal of case or relinquishing of court's oversight of prisons. The twelve states included in the table reflect all states that have had their entire prison system under court order concerning overcrowding at some point between 1971–1993. Litigation status in a particular year based on information reported in the ACLU National Prison Project Journal (multiple issues) and the court opinions cited therein.

system has come under court control, along with the corresponding dates. There is wide variation in the timing of prison overcrowding litigation status across the different states. Final court decisions were handed down as early as 1971 and as late as 1991. The states falling entirely under court control are disproportionately, but not exclusively, Southern states. Southern states have historically had higher incarceration rates. In 1970, for instance, the prison population per 100,000 residents was 125 in the South, 105 in the West, 86 in the North Central, and 70 in the Northeast [Zimring and Hawkins 1991]. When litigation was first filed in these twelve states, incarceration rates were on average 34 percent greater than the national average.

To the extent that the states where the entire prison system is under court control differ systematically from the rest of the country, the use of cross-state variation is potentially misleading. A number of steps are taken to counteract that possibility. First, all of the analysis in this paper focuses on percent changes in variables, eliminating any effects of differences in *levels* of crime rates or imprisonment across states. Second, in some specifica-

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tions, state fixed effects are included to control for the possibility that not only do the *levels* of the variables differ systematically across states, but also the *growth rates*.[12] Finally, the possibility that the coefficients systematically differ between Southern and non-Southern states, even after these precautions, is also examined.

II. Correlations between Prison Overcrowding Litigation, Prison Populations, and Crime Rates in the Raw Data

The data set used in this paper is a panel of annual, state-level observations, running from 1971–1993.[13] Throughout the paper all incarceration and crime rates are defined on a per capita basis. State prison populations are defined as the number of prisoners serving sentences of at least one year under the jurisdiction of the state prison system, and thus include inmates in state prisons, state prisoners held in local jails due to overcrowding, and prisoners housed in other states due to lack of appropriate facilities within the state borders. Prison populations are computed as a snapshot as of December 31.[14]

Data on state crime rates are based on the number of crimes reported to the police over the course of a year, as compiled annually by the Federal Bureau of Investigation in *Uniform Crime Reports*. While victimization data would be preferable to reported crimes from a theoretical standpoint, such data are unavailable at the required level of geographic disaggregation, necessitating the use of reported crime statistics.[15] Reported crime data are available for the seven Index I crime categories: murder and non-

12. Although the identification of the prison population parameter comes only from the twelve states with litigation affecting the entire system, the remaining states are kept in the sample to help estimate the other parameters of the model. When these other states are excluded, neither the coefficient nor standard error on the prison population variable is affected. The standard errors on the other variables, however, more than double.

13. State prison population estimates from before 1971 are not comparable to more recent data. In fifteen state-year pairs, prison data are either unavailable or not comparable. In addition, there are a small number of missing observations on crime rates.

14. Ideally, one would also like to include jail inmates as well as state prisoners (the jail population is roughly half the magnitude of state prison populations). Unfortunately, state-level estimates of jail populations are updated only at five-year intervals. Omission of jail inmates would be particularly problematic if prison overcrowding led to substantial numbers of state prisoners being held in jails and consequently crowded out jail inmates. Only about 1 percent of state prisoners, however, are typically held in local jails due to overcrowding.

15. For conflicting views on the validity of reported crime data, see O'Brien [1985] and Gove, Hughes, and Geerken [1985].

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negligent homicide, forcible rape, aggravated assault, robbery, burglary, larceny, and motor vehicle theft. Precise definitions of each of the crime categories are provided in the appendix. The first four crimes are classified as violent crimes; the latter three crimes are denoted property crimes. The use of reported (as opposed to actual) crime rates is a clear source of measurement error. However, since crime rates are left-hand side variables in the analysis, such measurement error will not lead to bias in the parameter estimates unless the measurement error is correlated with the right-hand side variables.[16] The fact that the paper focuses on growth rates rather than levels and even finds similar results after taking out state fixed effects in growth rates makes it unlikely that systematic measurement error is driving the results.

In considering the effects of prison overcrowding litigation, the analysis will focus exclusively on the twelve states in which the entire prison system has come under court supervision. These states will be unable to comply with court orders on overcrowding simply by redistributing prisoners across institutions. In contrast, states in which only a subset of institutions is affected by a court order have much greater flexibility in responding to court mandates. Empirically, I have been unable to uncover any systematic evidence that court orders affecting individual facilities within a state have any impact on overall state prison populations.

Prison litigation status is captured by a series of indicator variables corresponding to the six litigation categories described in the previous section. Throughout much of the analysis only recent changes in litigation status are considered since the effects on prisoner population *growth rates* are likely to be concentrated in the short run. Table II gives summary statistics for all 50 states for the data described above, as well as for additional variables (police employees, economic factors, and demographic controls) used in the analysis.

Table III presents state-by-state data on prison population growth rates as a function of prison overcrowding litigation status for the twelve states where the entire court system falls under court jurisdiction. In order to control for national trends in prison

16. For instance, a correlation might be expected between the rate of crime reporting and the size of the police force. A larger police force might affect the likelihood that a case is solved, leading victims to report a greater percentage of crimes. Levitt [1995b] finds only weak evidence of such a relationship.

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TABLE II
SUMMARY STATISTICS

Variable	Mean	Standard deviation	Minimum	Maximum
	Per 100,000 residents where applicable			
Prison population	168.1	130.7	20.3	1,541.2
Violent crime	446.5	303.1	38.0	2,921.8
Murder	7.8	6.5	4.1	100.7
Rape	30.9	15.1	0.6	81.1
Assault	255.9	158.8	25.0	1,557.8
Robbery	151.9	158.8	6.5	1,675.8
Property crime	4,408.2	1,277.7	1,255.8	8,839.3
Burglary	1,206.2	422.8	338.8	2,907.9
Larceny	2,799.5	817.1	762.7	5,449.0
Motor vehicle theft	402.5	229.1	83.2	1,590.2
Prison overcrowding litigation status				
Not filed/only partial coverage	0.827	0.378	0.0	1.0
Filed	0.026	0.158	0.0	1.0
Preliminary decision	0.021	0.144	0.0	1.0
Final decision	0.057	0.232	0.0	1.0
Further court action	0.043	0.204	0.0	1.0
Released from court control	0.026	0.158	0.0	1.0
Police employees	259.5	88.4	130.8	907.9
GNP per capita	16,398.9	3,125.9	9,728.8	29,004.0
Unemployment rate	0.066	0.021	0.022	0.180
Demographic variables (percent of population)				
Black	0.108	0.133	0.002	0.922
Metro areas	0.637	0.230	0.145	1.000
Age 0–14	0.237	0.030	0.163	0.342
Age 15–17	0.051	0.008	0.026	0.069
Age 18–24	0.122	0.014	0.085	0.159
Age 25–34	0.161	0.020	0.101	0.236

Observations are annual state-level data, 1971–1993. Where applicable, values in table are per 100,000 residents. The number of observations is 1173. Prison overcrowding litigation categorizations are mutually exclusive. Those states whose entire prison system has never come under court order are categorized under the prison overcrowding litigation category not filed/only partial coverage. Prison population data, crime statistics, and age breakdowns are from Marvell and Moody [1994]. Prison overcrowding litigation status is based on information reported in the ACLU National Prison Project Journal (multiple issues) and the court opinions cited therein. Police employee data are full-time equivalents, and are published annually by the Department of Justice. Economic data are annual, state-level data taken from the *Statistical Abstract of the United States.* Percent Black and percent residing in metropolitan areas are linear interpolations of decennial census data, as reported in the *Statistical Abstract.*

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TABLE III
EFFECT OF PRISON OVERCROWDING LITIGATION ON PRISON POPULATIONS
(ANNUAL DEVIATION FROM NATIONAL AVERAGE)

	Prison overcrowding litigation status					
	Prefiling	Filed	Prelim. decision	Final decision	Further action	Released by court
Alabama	−2.1%	−1.1%	7.0%	−14.5%	3.2%	−0.3%
Alaska	5.1	−2.1	—	1.5	—	—
Arkansas	—	—	—	−0.7	0.1	0.4
Delaware	8.6	—	—	−5.2	0.2	—
Florida	—	−1.3	10.4	−4.4	−2.2	—
Mississippi	—	−3.9	—	1.3	—	—
New Mexico	5.4	−1.0	−1.4	−8.8	−3.5	—
Oklahoma	—	−6.5	—	0.7	—	3.4
Rhode Island	3.4	−1.4	—	0.7	0.9	—
South Carolina	2.3	−3.3	1.3	−2.7	—	—
Tennessee	1.7	−0.2	—	−4.1	−2.9	—
Texas	−1.5	1.0	−4.0	−2.9	9.0	—
Average across all twelve states	4.2	−2.4	0.3	−0.8	−0.5	0.8

Values in the table reflect the average annual deviation in a state's growth rate in per capita prison population from the national average. Data cover the period 1971–1993 (implying observations on growth rates for the years 1972–1993). The average across the states reported in the bottom row of the table reflects the fact that different states satisfy the various litigations categories for different numbers of years, and thus is not a simple average of the table entries. For definitions of litigation status categories and the years that each state falls under that category, see Table I. The twelve states included in the table reflect all states that have had their entire prison system under court order concerning overcrowding at some point between 1971–1993. Prison populations are computed at the end of each year. Litigation status in a particular year is based on information reported in the ACLU National Prison Project Journal (multiple issues) and the court opinions cited therein.

populations, the values reported in the table are deviations from the national average growth rates in prison populations for the years in question. In contrast to the analysis that follows, no distinction is made in Table III between the short-run and long-run effects of litigation status on prisoner growth rates. For the most part the expected pattern of coefficients emerges from Table III, although the effects are not overwhelmingly large. In three-quarters of the cases, growth rates in state prison populations outpaced national growth rates prior to the filing of litigation. Across all cases, the prefiling annual growth rate in prisoners in these states was 4.2 percent above the national mean. In contrast, after the filing of litigation, prisoner growth rates fell below the national average in nine of ten states, with a mean of −2.4 percent annually. The impact of actual court actions emerges less clearly. Growth rates were slightly above the national average

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after preliminary court decisions were handed down, but were slightly below the nation as a whole after final decisions were reached and after further court actions were taken. As expected, prisoner populations grow more quickly after the courts release control, although only three states fall into that category. Of the 46 state-by-state values listed in the table, 30 carry the expected sign. Assuming independence across those observations, that pattern of observations would be expected to occur by chance less than 3 percent of the time.

It may be unreasonable to expect prison overcrowding litigation to have long-run effects on growth rates in prison populations. More likely, overcrowding litigation has only a short-run effect on prison population *growth* rates, after which normal growth rates resume, although starting from a lower base than otherwise would have been the case.[17] Table IV isolates the short-run effects of changes in overcrowding litigation status on prisoner growth rates. The data are broken down according to status changes *during* a particular year, and changes two to three years directly after a status change.[18] Column 1 displays the number of observations falling into a given category, and column 2 shows annual prisoner growth rates relative to the national average. The pattern of coefficients in column 2 is similar to that of Table III, but the short-run effects are substantially larger in magnitude than the long-run effects, supporting the view that the bulk of the effects of overcrowding litigation accrue in the short run.[19] As before, prison populations grow more quickly than the national average before filing. The number of prisoners continues to grow unabated in the first year of filing (2.7 percent above na-

17. Therefore, unless "catching up" in prison populations occurs in later periods, the long-run level of the prison population will be lower as a consequence of overcrowding litigation.

18. The litigation status categories are defined so as to be mutually exclusive. For example, assume that a case is filed in 1980, a preliminary decision handed down in 1984, and a final decision made in 1985. In 1980 the litigation status would be "filed, year of status change." In 1981 and 1982 the litigation status would be "filed, two to three years following a status change." In 1983 the litigation status would be "none of the above," since the last status change was more than three years previous. In 1984 litigation status would be "preliminary decision, year of status change." Because the final decision is handed down the next year, 1985 would be "final decision, year of status change." 1986 and 1987 would be classified as "final decision, two to three years following a status change." Later years would be classified as "none of the above," unless further court action was taken.

19. In fact, beyond three years, for the five litigation categories where one would expect prison population rates to be reduced, growth rates are actually 1.0 percent above the national average, suggesting that some catching up is occurring in the longer run.

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TABLE IV
SHORT-RUN RESPONSE OF PRISON POPULATIONS AND CRIME TO CHANGES IN PRISON OVERCROWDING LITIGATION STATUS
(PERCENT ANNUAL DEVIATION FROM NATIONAL AVERAGE GROWTH RATE)

Litigation status	Number of observations	Prison population	Violent crime	Property crime
Prefiling	22	2.3	−0.4	−0.4
Year of status change				
Filing	9	2.7	−4.4	−2.5
Preliminary decision	5	−0.5	1.8	4.4
Final decision	11	−5.1	3.6	0.5
Further court action	9	−3.2	4.2	0.9
Released from court control	3	5.2	−0.5	−1.6
Two or three years following status change				
Filing	17	−5.1	0.1	0.5
Preliminary decision	8	−0.2	2.7	4.1
Final decision	18	−4.6	3.3	2.5
Further court action	17	1.3	−1.6	−0.9
Released from court control	6	−0.6	6.4	1.3

Data cover the period 1971–1993 (implying observations on growth rates for the years 1972–1993). Based on data from the twelve states with their entire prison system under court order. Litigation status categories are mutually exclusive; i.e., a second status change within three years overrides the previous status change. Prisoner populations are computed at the end of the year, and crime rates correspond to the following year. Figures in the table are computed using only data within three years of a change in litigation status. Litigation status in a particular year is based on information reported in the ACLU National Prison Project Journal (multiple issues) and the court opinions cited therein. Not all states fall into all litigation status categories.

tional average), but then drops sharply in the two years following initial filing (5.1 percent below the national average). Preliminary court decisions appear to have little effect, either initially or with a lag. Final court decisions, however, have a substantial effect, both initially (−5.1 percent) and in the following two years (−4.6 percent). In the three years after a final court order affecting the entire prison system, therefore, prisoner population growth is almost 15 percent below the rest of the nation. Since incarceration rates grew at approximately 6 percent per year on average during the sample period, this implies that prison populations are almost flat after final decisions. Further court actions have an initial limiting effect on prison populations, but that change is largely undone in the two years that follow. Release from court supervision leads to an immediate jump in prisoners, but no lagged effect.

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If changes in overcrowding litigation status are truly exogenous shifters of the prison population, then a comparison of the patterns of prisoner population growth and changes in the crime rate under the different litigation categories should provide a rough measure of the effects of prison populations on crime. Columns 3 and 4 of Table IV report changes in violent and property crime rates relative to the nation as a whole.[20] If decreases in the prison population have a large impact on crime rates, then one would expect the sign of the values in columns 3 and 4 to be the opposite of those in column 2. That pattern of opposite signs holds true in all eleven categories for both violent and property crimes, a fact that is somewhat remarkable given the small number of observations used in constructing Table IV.[21] This result, while merely suggestive, foreshadows the large elasticities that will be obtained when litigation status is used as an instrument.

Table V reproduces the analysis of Table IV, adding a wide range of covariates including year dummies, economic factors, percent changes in police staffing, and changes in demographic factors such as racial composition and the age distribution, and year dummies.[22] The omitted litigation status categories are more than three years prior to filing or more than three years since a status change, so all coefficients are relative to those categories. All regressions in Table V are estimated using ordinary least squares, with White-heteroskedasticity consistent standard errors in parentheses. Even-numbered columns also include state-fixed effects, which allow for growth rates of the key variables to

20. Both the prison population data and litigation status reflect a snapshot as of December 31 of the year in question. Therefore, the once lagged values are used in explaining changes in crime rates. For instance, if a final court decision is handed down in July 1990, the percent change in the crime rate is computed as $\ln(\text{crime}_{1991})-\ln(\text{crime}_{1990})$. To the extent that some of the increase in crime may come in the second half of 1990, this measure may understate the true change in the crime rate.

21. In contrast, this negative relationship between prisoner growth rates and changes in crime rates does not emerge in the sample more generally. Of the approximately 1000 state-year observations, in 51.1 percent (54.4 percent) of the cases are the signs on changes in prisoners and violent (property) crime rates opposite. The contrast between those numbers and the results in Table IV suggest both that endogeneity is a problem in the data and that prison litigation status is breaking the endogeneity.

22. Data on age distributions within a state are from Marvell and Moody [1994]. Data on police employees are the total number of public police FTEs (including both municipal police and state troopers) in a state, published annually by the Department of Justice. All other variables are available in the Statistical Abstract of the United States. Income and unemployment data vary annually. In contrast, both the percent of a state's population who is Black and who resides in a metropolitan area are linearly interpolated from figures reported in the decennial censuses.

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TABLE V

THE SHORT-RUN IMPACT OF CHANGES IN PRISON OVERCROWDING LITIGATION STATUS

Variable	(1)	(2)	(3)	(4)	(5)	(6)
	Δln Prison population		Δln Violent crime		Δln Property crime	
3 Years prefiling	.016	−.002	.001	.002	−.007	−.008
	(.016)	(.018)	(.016)	(.020)	(.010)	(.011)
Year of status change:						
Filing	.021	.004	−.031	−.032	.002	−.001
	(.019)	(.021)	(.022)	(.025)	(.025)	(.025)
Preliminary decision	.016	.011	.018	.013	−.012	(−.012
	(.069)	(.071)	(.021)	(.022)	(.024)	(.024)
Final decision	−.047	−.065	.035	.035	−.004	−.007
	(.022)	(.022)	(.030)	(.029)	(.012)	(.013)
Further court action	−.014	−0.25	.039	.036	.001	−.001
	(.031)	(.030)	(.015)	(.016)	(.020)	(.021)
Released from court control	.047	.041	−.000	−.005	−.016	−.032
	(.019)	(.022)	(.019)	(.020)	(.006)	(.009)
Two to three years after status change:						
Filing	−.047	−.064	−.006	−.008	.002	−.000
	(.018)	(.019)	(.011)	(.013)	(.011)	(.011)
Preliminary decision	−.005	−.013	.005	−.001	.030	.030
	(.018)	(.019)	(.025)	(.026)	(.014)	(.016)
Final decision	−.045	−.066	.022	.024	.033	.032
	(.018)	(.020)	(.013)	(.014)	(.011)	(.012)
Further court action	.014	−.000	−.023	−.025	−.019	−.024
	(.015)	(.018)	(.017)	(.018)	(.006)	(.009)
Released from court control	−.002	−.010	.058	.053	−.011	−.026
	(.013)	(.016)	(.017)	(.019)	(.016)	(.015)
Δln Income per capita	−.452	−.440	.510	.515	−.006	−.027
	(.180)	(.179)	(.175)	(.176)	(.076)	(.074)
Δ Unemployment rate	.003	.057	.458	.514	.934	.872
	(.325)	(.324)	(.334)	(.335)	(.184)	(.179)
Δln Police	.083	.093	.075	.075	.020	.008
	(.041)	(.040)	(.060)	(.061)	(.036)	(.036)
Δ % Black	−.009	−.104	−.007	.034	−.044	.010
	(.024)	(.053)	(.027)	(.068)	(.017)	(.034)
Δ % Metro	−.013	−.001	.016	.024	.009	.011
	(.009)	(.020)	(.014)	(.032)	(.003)	(.012)
State controls?	No	Yes	No	Yes	No	Yes
R^2	.182	.223	.232	.244	.597	.618
P-Value: all status changes	<.001	<.001	<.001	.003	<.001	.001
P-Value: status change this year ≠ 2–3 years after status changes	.023	.038	.013	.018	.157	.105

The dependent variable is Δln state prison population, Δln violent crime rate, or Δln property crime rate depending on the column. Prisoner populations and litigation status variables correspond to December 31 of the year in question. Once-lagged status changes are used as explanatory variables in columns (3)–(6). Columns (1) and (2) use annual state-level data from 1971–1993 (implying observations on changes from 1972–1993). Columns (3)–(6) use annual state-level data from 1972–1993 (implying observations on changes from 1973–1993). The number of observations is equal to 1120 in columns (1) and (2) and 1063 in columns (3)–(6). White heteroskedasticity-consistent standard errors are in parentheses. Age and year controls are included in all columns. Prison overcrowding litigation variables are indicators corresponding to whether prison litigation status has changed in the current year, or two to three years earlier. Prison overcrowding litigation status refers only to states whose entire prison system is under court control. Litigation indicators are mutually exclusive. The omitted litigation categories are four or more years prior to filing and four or more years since the last litigation status change. For the definitions of status categories, see text.

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systematically differ across states. There are some scenarios in which inclusion of state-fixed effects is appropriate, even though the dependent variable is already in terms of percent changes. For instance, if all states are converging to similar incarceration rates in the long run, but Southern states had higher rates at the beginning of the sample, then one might expect slower growth rates in prisoners in Southern states in the sample.

Similar patterns continue to emerge after adding covariates. In columns (1) and (2) only one coefficient changes sign from Table IV. The largest impacts on prison populations continue to be associated with final court decisions and (with a one-year lag) initial filing. Slightly less than half of the status change indicators are individually statistically significant from zero. More importantly, however, the status change indicators are jointly significant at the .001 level. The null hypothesis of equal effects of status changes in the current year and the two following years is rejected at the .05 level in both columns (1) and (2), suggesting that classifying the observations in this manner is a useful characterization of the data.[23]

Columns (3)–(6) present estimates of reduced-form correlations between litigation categories and crime rates. Crime rates continue to move in the opposite direction of prisoner populations with only a few exceptions. For instance, in the three years following the court's handing down a final decision, prison populations are estimated to grow a total of 13.7–19.7 percent more slowly than if there had been no litigation, while violent crime rates are 7.9–8.3 percent higher, and property crime rates are 5.7–6.2 percent higher.

If prison overcrowding litigation provides an exogenous source of variation in prison populations, changes in crime rates should not be driving changes in litigation status. Put another way, information about whether litigation will be filed in the future should not be related to current crime rates (see, for instance, Model [1993]). The coefficient on the litigation status indicator "will file within three years," presented in the top row of Table V, allows a test of that prediction. Consistent with the claim that the timing of litigation filing is orthogonal to changes in the crime rate, the coefficients in columns (3)–(6) are substan-

23. Tests for significance of further lags of litigation changes could not reject the null hypothesis of no effect on prison population growth rates. That result provides more evidence that changes in overcrowding litigation status only affect prisoner growth rates in the short run.

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tively small, statistically insignificant, and flip signs between violent and property crimes.[24]

III. Estimates of the Elasticity of Crime with Respect to Prisoner Populations

Having demonstrated in the previous section a relationship between prison overcrowding litigation and prison population growth rates, as well as a reduced-form relationship between such litigation and changes in crime rates, this section applies instrumental variables techniques to estimate elasticities of crime with respect to prisoner populations.

It is assumed that percent changes in crime rates and prison populations are determined according to

$$\Delta\ln(\text{CRIME}_{st}) = \beta\Delta\ln(\text{PRISON}_{st-1}) + X'_{st}\theta + \gamma_t + \varepsilon_{st},$$

where the subscript s corresponds to states, t indexes years, and l denotes the various prison litigation categories. CRIME_{st} and PRISON_{st-1} are the relevant per capita crime and incarceration rates. Because data on prison populations are snapshots as of December 31, the once-lagged value is used in explaining crime rates, which correspond to calendar years. X_{st} is a vector of covariates, and γ_t is a vector of year dummies. In some cases, state-fixed effects are also included. Because both crime rates and prisoner populations are in terms of log changes, β is an elasticity. It is likely that the number of prisoners is positively correlated with the residuals of the crime equation, potentially inducing a positive bias in estimates of β. If the exclusion of prison litigation status from the crime equation is valid, however, two-stage least squares estimation using the litigation status as instruments will lead to consistent estimates.[25]

24. While not directly testable, it is also possible that the timing of later changes in litigation status, e.g., the handing down of a final decision, are endogenously related to changes in crime rates. To the extent that endogeneity exists, serial correlation in changes in crime rates will exacerbate the problem. A large jump in crime may trigger a judge to hand down a final decision. Crime rates will continue to be high in the ensuing years due to a serial correlation, inducing a spurious relationship between litigation status changes and crime rates. There is, however, little serial correlation in changes in crime rates. Once the national trend is removed, the serial correlation in state-level changes in violent (property) crime is .033 (.039).

25. One argument against the validity of overcrowding litigation as an instrument is that such litigation may be correlated with overall prison conditions. To the extent that more pleasant prison conditions reduce the deterrent effect of

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Table VI presents estimates of the crime equation separately for violent crimes and property crimes using the same data set and covariates described in the preceding section. In columns 1 and 4, OLS coefficients, which do not control for the endogeneity of prison populations, are presented. In the other columns indicator variables corresponding to litigation status changes are used as instruments for Δ ln(PRISON). For each of the five litigation categories, separate indicators are employed for the year of a status change, and the two years following a status change, yielding a total of ten instruments.[26] Columns (3) and (6) also include state-fixed effects. Feasible generalized least squares is employed in the IV regressions to allow for heteroskedasticity across states.

When prison populations are treated as exogenous in columns (1) and (4), the estimated elasticity of crime with respect to prisoners is -0.099 for violent crime and -0.071 for property crime. In both cases, the estimates are quite precise. These elasticities are roughly consistent with, but somewhat smaller than, previous estimates in the literature, which have typically been in the neighborhood of -0.10 to -0.20 [Marvell and Moody 1994; Spelman 1994].

Instrumenting for the prison population has a pronounced effect on the estimated elasticities. The estimated elasticities for violent crime in columns (2) and (3) are -0.424 and -0.379, respectively, four times greater than without instrumenting. While the estimates are much less precise due to instrumenting, they are nonetheless statistically different from zero. The increases for property crimes (columns (5) and (6)) are also substantial. Instrumenting leads to estimates of -0.321 and -0.261, again almost four times higher than the uninstrumented case. The standard errors once again increase, but the estimates are precise enough to attain statistical significance at the .05 level. Prisoners per capita increased by 272 percent in the United States between 1971 and 1993. Assuming that the instrumented elasticities ob-

incarceration, overcrowding cases may increase crime rates through channels other than the number of prisoners. If this scenario is true, it is likely that the IV estimates will overstate the impact of changes in size of prison populations.

26. The results presented below have also been replicated not differentiating between the year of a status change and the following two years with very similar point estimates, but larger standard errors. I opt for the larger set of instruments both because an F-test of equal effects in the year of a status change and the following two years is rejected in the first stage (see Table V), and because the likely consequence of choosing a set of instruments that is too large is to induce a bias in the direction of OLS, which does not appear to arise moving from five to ten instruments.

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TABLE VI
THE IMPACT OF PRISON POPULATIONS ON AGGREGATE CRIME CATEGORIES

	Δln Violent crime			Δln Property crime		
Variable	OLS (1)	IV (2)	IV (3)	OLS (4)	IV (5)	IV (6)
Δln Prison population(t−1)	−.099	−.424	−.379	−.071	−.321	−.261
	(.033)	(.201)	(.180)	(.019)	(.138)	(.117)
Δln Income per capita	.485	.384	.410	.014	.076	.055
	(.117)	(.127)	(.127)	(.066)	(.072)	(.070)
Δ Unemployment rate	.564	.411	.451	1.032	1.138	1.063
	(.333)	(.301)	(.302)	(.186)	(.188)	(.181)
Δln Police	.026	.054	.063	−.004	.012	.002
	(.059)	(.048)	(.048)	(.033)	(.030)	(.029)
Δ % Black	−.015	−.018	.007	−.043	−.038	.000
	(.029)	(.025)	(.058)	(.016)	(.016)	(.035)
Δ % Metro	.013	.006	.027	.006	−.000	.005
	(.011)	(.012)	(.021)	(.006)	(.006)	(.011)
Δ % Age 0–14	−.287	−.075	−.127	.220	.121	.399
	(.412)	(.393)	(.447)	(.230)	(.234)	(.257)
Δ % Age 15–17	−.041	.169	.180	.351	.320	.390
	(.213)	(.205)	(.226)	(.119)	(.121)	(.127)
Δ % Age 18–24	.320	.282	.286	.277	.079	.126
	(.253)	(.235)	(.253)	(.141)	(.139)	(.144)
Δ Age 25–34	.648	.748	.828	.384	.354	.436
	(.335)	(.329)	(.350)	(.187)	(.195)	(.202)
Year controls?	Yes	Yes	Yes	Yes	Yes	Yes
State controls?	No	No	Yes	No	No	Yes
Instrument?	No	Yes	Yes	No	Yes	Yes
R^2	.247	—	—	.606	—	—
P-value overidentifying restrictions	—	.369	.424	—	.416	.164

The dependent variable is Δln Violent crime rate or Δln Property crime. The data set is comprised of annual state level data from 1972–1993 (implying observations on changes for the years 1973–1993). Number of observations is equal to 1063 in all columns. Prison population data correspond to December 31 of the year. Consequently, the once-lagged value is used as an explanatory variable. In all cases, estimation allows for heteroskedasticity across states. In instrumental variables specifications, ten indicator variables corresponding to changes in prison overcrowding litigation status in the current year/two preceding years are used as instruments for the percent change in the prison population. In all columns using IV, the test of overidentifying restrictions is computed using an $N \times R^2$ test, where N is the number of observations and R^2 is the R^2 from a regression of the residuals from the second-stage regression on all of the exogenous variables and the instruments. This test statistic is distributed χ^2 with degrees of freedom equal to the number of overidentifying restrictions (in this case nine). Overcrowding litigation status refers only to states whose entire prison system is under court control. For the definitions of status categories, see text.

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tained here are generalizable to the nation as a whole, violent crime would be approximately 70 percent higher today if the increase in prisoners had not occurred, and property crime would be almost 50 percent more frequent.

The other parameters of the model, while also of interest, generally yield mixed and imprecise coefficients. Increases in per capita income are positively correlated with violent crime, but are not strongly related to property crime. The reverse holds true with changes in the unemployment rate. Each one-point change in state unemployment rates leads to an increase of slightly less than 0.5 percent in violent crime and a 1 percent increase in the property crime rate.

Changes in the number of police are weakly positively correlated with changes in the crime rate, a finding that is common in studies such as these (see Cameron [1988] for a survey). The most likely explanation for that result is endogeneity of police hiring: when crime worsens, the public policy response is to hire more police.[27] The percent of the population that is Black and the percent residing in metropolitan areas are generally statistically insignificant and switch signs across specifications. In all cases, an increase in the fraction of the population between the ages 15 and 34 is related to higher crime rates. Somewhat surprisingly, the greatest impact comes from the 25–34 age range. The age coefficients are imprecisely estimated, however, making it difficult to draw strong conclusions.

Given the dramatic change in the estimated effects of incarceration in the instrumented regressions, and the fact that these estimates are two to three times greater than conventional wisdom on the subject would predict, special scrutiny of the results is warranted. In the following paragraphs three separate issues

27. Following Levitt [1994], an attempt was made to instrument for changes in the police force using the timing of state elections. Levitt [1994] finds that police hiring in the nation's largest cities is disproportionately concentrated in mayoral and gubernatorial election years, and exploits that fact to estimate the effect of police on crime. Electoral cycles in police hiring, however, are much less pronounced outside of large cities, possibly because crime is a less critical political issue. Consequently, the first-stage correlations between overall state police and gubernatorial elections, while positive, is weak. Instrumenting for police with gubernatorial elections had little impact on the estimated effects of prison population on crime. The elasticity of violent crime with respect to the prison population is -0.37 ($SE = 0.17$), and for property crimes the elasticity is -0.22 ($SE = 0.10$). The coefficient on $\%\Delta$ in police becomes negative in the violent crime equation (an elasticity of -0.44 ($SE = 0.36$), but remained positive, 0.09 ($SE = 0.18$) in the property crime equation. Eliminating the police variable from the equation entirely has little effect on the prisoner coefficients.

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are considered: the validity of the instruments, the robustness of the results, and the generalizability of the findings.

Because the number of instruments exceeds the number of endogenous variables, it is possible to test the overidentifying restrictions on the excluded instruments. The test statistic is calculated as $N \times R^2$, where N is the number of observations and R^2 is the R^2 from a regression of the residuals of the crime equation on all of the exogenous variables, including the instruments. The test statistic is distributed χ^2 with degrees of freedom equal to the number of overidentifying restrictions, in this case nine. The P-values for this test are presented in the bottom row of Table VI. In all cases, the test statistic is well within conventional bounds, supporting the claim of exogeneity of the instruments.[28]

The results presented here appear to be robust to a variety of alternative specifications. The point estimates rise slightly when the demographic, economic, and age variables are removed (elasticities of violent crime with respect to prison population size of -0.50 to -0.42, and -0.40 to -0.26 for property crime). I have also experimented with estimation in log-levels rather than log-differences and obtained similar results. Perhaps the strongest evidence that the results are authentic comes from disaggregating the crime data into individual crime categories. Table VII presents crime-by-crime estimates of specifications corresponding to columns (3) and (6) of Table VI.[29] The bottom row of Table VII also presents the uninstrumented coefficient on the prison variable for comparison purposes. The estimates across the seven crime categories are consistent with the earlier results. The estimated elasticities with respect to prison populations range from -0.147 to -0.703. Because of large standard errors, only two of the seven estimates are statistically significant at the .05 level, with two others significant at or around the .10 level. In all seven cases, instrumenting leads to more negative estimates. Tests of overidentifying restrictions are safely within accepted bounds for each crime category. Assault, robbery, and burglary are the two crimes most responsive to increases in imprisonment.

28. Creating ten separate instruments out of the litigation status variable may predispose the overidentification test toward acceptance of the exogeneity of the instruments. When the ten instruments are reduced to five by eliminating the distinction between the year of the status change and the following two years, the P values on the test of overidentifying restrictions range between .104 and .427 across the columns of Table VII, still within acceptable bounds in all instances.

29. The results presented in Table VII do not allow for correlation in errors across the different crime categories. The results are similar when the seven crime categories are jointly estimated, allowing for cross-crime correlations in the errors.

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A final consideration in interpreting the instrumented coefficients is whether such estimates are generalizable to the full set of states, or to other potential changes in imprisonment policy. When instrumenting, the parameters are identified solely based on variation in prison populations in states where the entire prison system falls under court control. These states tend to be disproportionately Southern and have higher initial incarceration rates. One might consequently expect that the marginal prisoner is less criminal in such states, implying larger elasticities for other states. On the other hand, it is possible that states that rely on higher levels of incarceration are self-selected. They imprison more criminals precisely because incarceration has a greater crime-reducing impact in these states. As a test of whether either of those scenarios appears to hold true, the coefficient on prisoners was allowed to vary (in the uninstrumented regressions) according to whether or not a state is Southern. The estimated elasticities were somewhat smaller in Southern states. For violent crime the elasticities were -0.016 ($SE = 0.063$) and -0.118 ($SE = 0.035$) in Southern and non-Southern states, respectively. For property crime the elasticities were -0.034 ($SE = 0.035$) and -0.081 ($SE = 0.020$). In neither case, however, could the null hypothesis of no difference in the coefficients across Southern and non-Southern states be rejected at the .05 level. If the marginal impact of increased incarceration on crime is actually lower in Southern states, the estimates of this paper will tend to understate the true benefits of increased imprisonment in the nation as a whole.[30]

Perhaps the more pressing question of applicability for the estimates in this paper concerns whether prison population changes that are court-ordered are similar to other sources of variation in prison populations. An important observation with respect to this issue is that all of the reduction in prison populations due to overcrowding litigation appears to be due to early release of prisoners. The litigation indicator variables have essentially no explanatory power with respect to the number of prisoners committed to the prison system on the front end. Thus, the estimates presented here are likely to be more applicable to

30. As an additional check, the sample was also divided according to whether or not a state's entire prison system eventually fell under court control. For both violent and property crime, the point estimates on the effect of prison populations on crime were nearly identical across the two sets of states: -0.068 ($SE = 0.047$) versus -0.099 ($SE = 0.033$) for violent crime; -0.081 ($SE = 0.029$) versus -0.079 ($SE = 0.019$) for property crime. Again, in neither case can the null hypothesis that the two sets of states have equal coefficients be rejected.

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TABLE VII
THE IMPACT OF PRISON POPULATIONS ON SPECIFIC CRIME CATEGORIES
INSTRUMENTING WITH CHANGES IN PRISON OVERCROWDING LITIGATION STATUS

Variable	Murder	Rape	Assault	Robbery	Burglary	Larceny	Auto theft
Δln Prison population	−.147	−.246	−.410	−.703	−.401	−.277	−.259
	(.373)	(.250)	(.249)	(.309)	(.172)	(.147)	(.235)
Δln Income per capita	.851	.281	.434	.359	.000	.052	.561
	(.300)	(.168)	(.157)	(.191)	(.095)	(.076)	(.127)
Δ Unemployment rate	−.762	.370	.332	.788	1.327	1.051	.851
	(.629)	(.384)	(.371)	(.450)	(.243)	(.200)	(.339)
Δln Police	−.081	−.012	.104	.011	.001	.019	−.040
	(.097)	(.060)	(.058)	(.071)	(.038)	(.031)	(.052)
Δ % Black	−.015	−.007	−.022	−.042	−.036	−.034	−.075
	(.048)	(.033)	(.029)	(.036)	(.020)	(.017)	(.027)
Δ % Metro	−.012	.011	−.004	−.017	.008	−.003	−.011
	(.027)	(.016)	(.015)	(.017)	(.008)	(.007)	(.011)
Δ % Age 0–14	−.074	−1.079	.101	−.441	.107	−.038	.906
	(.830)	(.514)	(.483)	(.570)	(.308)	(.245)	(.400)

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Δ % Age 15–17	.849	.198	−.074	.441	.411	.252	.376
	(.445)	(.270)	(.251)	(.298)	(.159)	(.127)	(.205)
Δ % Age 18–24	−.364	−.011	.372	.291	−.101	.052	.846
	(.512)	(.310)	(.290)	(.343)	(.180)	(.147)	(.240)
Δ % Age 25–34	1.165	.146	.576	1.151	.465	.111	1.467
	(.721)	(.428)	(.408)	(.483)	(.255)	(.204)	(.339)
P-Value overidentifying restrictions	.816	.745	.396	.125	.201	.505	.386
Coefficient on Δln prison population w/o instrumenting	−.138	−.015	−.075	−.115	−.124	−.035	−.081
	(.117)	(.057)	(.057)	(.071)	(.025)	(.022)	(.039)

Dependent variable is Δln of the applicable crime category. The data set comprised of annual state level data from 1972–1993 (implying observations on changes for the years 1973–1993). The number of observations is equal to 1063 in all columns. Prison population data correspond to December 31 of the years. Consequently, the once-lagged value is used as an explanatory variable. In all cases, estimation allows for heteroskedasticity across states. Year dummies are included in all specifications. Ten indicator variables corresponding to changes in prison overcrowding litigation status in the current year/two preceding years as used as instruments for the percent change in the prison population. The test of overidentifying restrictions is computed using an $N \times R^2$ test, where N is the number of observations and R^2 is the R^2 from a regression of the residuals from the second-stage regression on all of the exogenous variables and the instruments. This test statistic is distributed χ^2 with nine degrees of freedom (the number of overidentifying restrictions). The last row of the table reports estimates of the elasticities of various crimes with respect to prison populations when estimated using OLS, not instrumenting for the prison population. Overcrowding litigation status refers only to states whose entire prison system is under court control. For the definitions of status categories, see text.

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changes in policies affecting time served, such as parole policies. The court only rarely mandates the release of prisoners, instead taking actions such as enjoining double bunking or closing portions of prisons, which must then be resolved by the state prison systems. Consequently, court-ordered fluctuations in prisoners may in fact be quite similar in their impact to other sources of variation in prison populations.

IV. Policy Implications of the Estimates

In order to use the results of the previous section in formulating public policy, estimates of the social costs of crime are required. The estimates of Cohen [1988] and Miller, Cohen, and Rossman [1993] are used for that purpose. Those papers attempt to capture both monetary costs of crime (medical bills, property loss, lost productivity) and quality of life reductions caused by pain and suffering. To gauge the quality of life reductions, jury awards in civil suits, excluding punitive damages, are estimated for a wide range of injuries. Those awards are then mapped to the distribution of injuries associated with the various crime categories. The cost estimates do not include the additional preventative measures taken by victims, lifestyle changes associated with the marginal crime, costs to employers, or legal costs, and therefore may understate the true costs of crime. On the other hand, the cost estimates correspond to the *average* crime, which may be more serious than the marginal crime, and therefore may exaggerate the costs of crime.

Another consideration in interpreting the estimates of the previous section in a policy context is the extent to which the results obtained using reported crimes carry over to unreported crimes. This question is important since victimization surveys suggest that only 38 percent of all index crimes are reported to the police. Even serious crimes such as robbery are reported little more than half of the time. In what follows, it is assumed that the elasticity of unreported crime to the number of prisoners is identical to that for reported crime. To the extent that criminals do not know in advance whether a crime will be reported, this seems to be a reasonable assumption.[31]

Table VIII presents the estimated impact of adding one pris-

31. It is possible that reported and unreported crimes differ systematically. For instance, crimes perpetrated by strangers are more likely to be reported. The fact that the criminal is in prison might be a signal that the criminal has a propensity to commit crimes that get reported.

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TABLE VIII
ESTIMATED IMPACT ON CRIME FROM ADDING ONE ADDITIONAL PRISONER
(EVALUATED AT 1993 SAMPLE MEAN)

	Change in reported crimes	Change in total crime (assumes same elasticity for unreported crimes)	Cost per crime		Social benefit of reduced crime
			Monetary	Quality of life	
Murder	−0.004	−0.004	$17,000	$2.7 Million	$10,800
Rape	−0.031	−0.053	9,800	40,800	2,700
Assault	−0.55	−1.2	1,800	10,200	14,000
Robbery	−0.55	−1.1	2,900	14,900	17,800
Burglary	−1.3	−2.6	1,200	400	4,300
Larceny	−2.6	−9.2	200	0	1,800
Auto theft	−0.5	−0.7	4,000	0	2,500
Total	−5.54	−14.86	—	—	53,900

Based on estimates of the elasticity of crime with respect to prison population by individual crime category from Table VII (instrumenting with indicator variables for change in prison overcrowding litigation status in current year, and in the preceding two years). Values in table are computed using sample means in 1993. Estimates of reporting rates for each type of crime are based on *Criminal Victimization in the United States, 1991* [Washington, DC: U. S. Department of Justice, 1992, p. 102]. The estimates of the social costs of crime are from Cohen [1988] and Miller, Cohen, and Rossman [1993], adjusted to 1992 dollars. The final column applies the cost of crime to the reduction in combined reported and unreported crime in column 2.

oner at the margin for a jurisdiction that is at the 1993 national average for all variables.[32] The values in Table VIII are based on the crime-by-crime point estimates presented in Table VII. Column 1 is the number of reported crimes reduced annually for each additional prisoner. Column 2 reflects the combined reduction in reported and unreported crime using reporting rates from the National Crime Survey [U. S. Department of Justice 1992].

According to column 1, each additional prisoner leads to a reduction of between five and six reported crimes. Including unreported crimes raises the total to fifteen. Each additional prisoner eliminates 0.004 murders annually, one-twentieth of a rape, and between two and three other violent crimes. The bulk of the crime reduction, however, is in the less socially costly property crimes.

32. Because the relationship between prisoners and crime is estimated as an elasticity in this paper, the effect of an additional prisoner depends on the number of prisoners relative to the amount of crime. Prison population growth has outpaced increases in the crime rate over the last 25 years. Consequently, the estimated social benefit of adding one prisoner is substantially lower (almost 40 percent) when evaluated at 1993 means vis-à-vis the mean of the entire sample. When considering the implications for current public policy, use of 1993 means seems most appropriate. My use of 1993 means also explains why the number of crimes attributable to the marginal prisoner is similar to that reported in Table V of Marvell and Moody [1994], despite the fact that my estimated elasticities are much larger.

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The estimate of fifteen crimes eliminated per prisoner per year is remarkably close to the median number of crimes obtained from surveys of prisoner self-reports, which have ranged from twelve to fifteen [Peterson and Braiker 1980; DiIulio and Piehl 1991; Piehl and DiIulio 1995]. The similarity between the two sets of results does not necessarily imply that the marginal prisoner is also the median prisoner since prisoner surveys only take into account incapacitation effects of prison, and also do not capture replacement effects when one criminal is arrested. In contrast, the estimates of this paper incorporate incapacitation, replacement, and deterrence effects.

Columns 3 and 4 are the Cohen [1988] and Miller, Cohen, and Rossman [1993] estimates of the monetary and quality of life losses due to crime.[33] For violent crimes the bulk of the costs are associated with quality of life reductions. For property crimes the costs are almost exclusively monetary. Column 5 combines the information in the second, third, and fourth columns to provide an estimate of the social benefit of crime reduction. The largest social benefits are associated with reduced murders, assaults, and robberies, together accounting for over $40,000 per prisoner per year. No other type of crime reduction yields a social benefit greater than $5000. The reduction in larcenies, though far and away the largest in raw numbers, carries the smallest social benefit across the crime categories. Combining all of the crime categories, incarcerating one additional prisoner yields a social benefit of $53,900 annually.[34] The true benefits of crime reduction are likely to exceed this total since it reflects only the seven crime categories examined in this paper and thus omits any benefits from reductions in other illegal activities such as drug offenses, arson, fraud, and driving under the influence.

A number of estimates are available of the average taxpayer cost of incarceration.[35] Freeman [1995] computes the average an-

33. The Miller, Cohen, and Rossman [1993] estimates, which update Cohen [1988], are only available for violent crimes.

34. To get a rough estimate of the precision of the aggregate social benefit total of $53,900, a standard error was computed, assuming that the costs per crime were exact and the point estimates across the different crime categories were uncorrelated. Both of those assumptions will lead the computed standard error to understate the true value. Given those assumptions, the estimated standard error on the social benefit per marginal prisoner is approximately $30,000. The overall standard error is greatly inflated by the imprecision of the estimates of the murder coefficients. If murder is excluded from the calculations, the social benefit falls to $43,100 with a standard error of $12,000.

35. All of the estimates discussed below have been translated into 1992 dollars for comparability to the social cost of crime estimates.

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nual cost per prisoner and jail inmate as approximately $23,500. DiIulio and Piehl [1995] use $25,000 as a "widely asserted" benchmark. Waldfogel [1993] calculates the annual cost of locking up a prisoner to be $28,500. Donohue and Siegelman [1994], correcting apparent shortcomings in the approach of Cavanaugh and Kleiman [1990], estimate the cost to be roughly $35,000. Confirming these estimates, the state of Missouri, faced with excess prison capacity, rented prison space to other states at an annual rate of $27,000 [Corrections Compendium 1989].

The above estimates of prison costs fail to capture the true social costs of incarcerating the marginal prisoner in a number of ways. The estimates do not take into account tax distortions that make the social cost of raising a dollar greater than a dollar,[36] lost social surplus from wasted human capital due to incarceration, post-release decline in wages [Lott 1992; Waldfogel 1994], or the pain and suffering of prisoners and their families. All of those omissions lead the estimates to understate the true social costs. On the other hand, these estimates reflect the *average* costs of running prisons rather than the *marginal* costs of an additional prisoner, which is almost certainly lower.

Given the practical limitations and conceptual problems associated with the estimates of both social costs and benefits, any comparison of the two is uncertain. Nonetheless, it is interesting to note that the two sets of estimates carry roughly the same magnitude, suggesting that the criminal justice system is roughly efficient in its determination of the scope of incarceration.[37] If anything, the results of this paper suggest that increasing the amount of time served by the current pool of prisoners would be socially beneficial.

An important caveat concerning the application of these results to public policy is that the social benefit of radically expanding the prison population through the incarceration of increasingly minor criminals is likely to be well below the estimates presented here. Given the apparent skew in the distribution of criminal activity, the current group of prisoners is likely to be much more criminally active than the marginal pool of individuals that come into contact with the criminal justice system but are not imprisoned. If the amount of incarceration is to be increased, keeping the current pool of prisoners behind bars for

36. Feldstein [1995] suggests that the deadweight loss associated with raising marginal federal funds may be much higher than previously thought.

37. Easterbrook [1983] and Waldfogel [1993] also argue that the criminal justice system achieves efficient outcomes.

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a longer period of time is likely to be a more advisable public policy approach.

V. Conclusions

Using prison overcrowding litigation as an instrument for changes in the prison population, this paper attempts to estimate the marginal productivity of increased incarceration in reducing crime. The estimates obtained are two to three times larger than the conventional wisdom. The results are robust across all of the crime categories examined. Incarcerating one additional prisoner reduces the number of crimes by approximately fifteen per year, a number in close accordance with the level of criminal activity reported by the median prisoner in surveys. While any cost benefit analysis is dependent on many questionable assumptions, the estimates presented in this paper suggest that the marginal costs of incarceration are at or below the accompanying social benefits of crime reduction.

The finding that increased prison populations appear to substantially reduce crime does nothing to reduce the importance of identifying and correcting those factors that lie at the source of criminal behavior. If truly feasible, prevention or rehabilitation would likely be preferable to long-term incarceration from both a cost-benefit and humanitarian perspective. Toward that end, Donohue and Siegelman [1994] survey a number of early-childhood and family-intervention programs that have achieved encouraging results in small samples but have not yet been attempted on a larger scale. While labor market interventions have generally not been successful in this realm, the Job Corps appears to be a possible exception [Donohue and Siegelman]. Finally, recent experimentation with alternatives to traditional prisons, including community-based sentences and "boot camps," represents an important avenue of investigation. In the absence of strong alternatives to imprisonment at the present time, however, increased reliance on incarceration appears to have been, and continue to be, an effective approach to reducing crime.

Appendix: Definitions of the Uniform Crime Reports Crime Categories

Murder and Nonnegligent Manslaughter

The willful killing of one human being by another. Deaths caused by negligence, attempts to kill, assaults to kill, sui-

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cides, accidental deaths, and justifiable homicides are excluded. Justifiable homicides are limited to the killing of a felon. Traffic fatalities are excluded.

Forcible Rape

The carnal knowledge of a female forcibly and against her will. Included are rapes by force and attempts or assaults to rape. Statutory offenses (no force used—victim under age of consent) are excluded.

Robbery

The taking or attempting to take anything of value from the care, custody, or control of a person or persons by force, or threat of force, or violence, and/or by putting the victim in fear.

Aggravated Assault

An unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily injury. This type of assault usually is accompanied by the use of a weapon or by means likely to produce death or great bodily harm. Simple assaults are excluded.

Burglary

The unlawful entry of a structure to commit a felony or a theft. Attempted forcible entry is included.

Larceny

The unlawful taking of property from the possession of another. Examples are thefts of bicycles or automobile accessories, shoplifting, pocket-picking, or the stealing of any property or article which is not taken by force and violence or by fraud. Attempted larcenies are included. Embezzlement, "con" games, forgery, and worthless checks are excluded.

Motor Vehicle Theft

The theft or attempted theft of a motor vehicle.

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