Prison Conditions, Capital Punishment, and Deterrence

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Previous research has attempted to identify a deterrent effect of capital punishment. We argue that the quality of life in prison is likely to have a greater impact on criminal behavior than the death penalty. Using state-level panel data covering the period 1950–90, we demonstrate that the death rate among prisoners (the best available proxy for prison conditions) is negatively correlated with crime rates, consistent with deterrence. This finding is shown to be quite robust. In contrast, there is little systematic evidence that the execution rate influences crime rates in this time period.

1. Introduction

For more than two decades the deterrent effect of capital punishment has been the subject of spirited academic debate. Following Ehrlich (1975), a number of studies have found evidence supporting a deterrent effect of the death penalty (Cloninger, 1977; Deadman and Pyle, 1989; Ehrlich, 1977; Ehrlich and Liu, 1999; Layson, 1985; Mocan and Gittings, 2001). A far larger set of studies have failed to find deterrent effects of capital punishment (e.g., Avio, 1979, 1988; Bailey, 1982; Cheatwood, 1993; Forst, Filatov, and Klein, 1978; Grogger, 1990; Leamer, 1983; Passell and Taylor, 1977).[1] Although only one small piece of the broader literature on the issue of

We would like to thank Austan Goolsbee for comments and criticisms. The National Science Foundation provided financial support.

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1. See Cameron (1994) for a survey of econometric studies of capital punishment.

American Law and Economics Review Vol. 5 No. 2,
 DOI: 10.1093/aler/ahg014

deterrence, in the minds of many, the question of capital punishment is inextricably linked to the validity of deterrence.

The great majority of the empirical studies of the deterrent effect of capital punishment have examined U.S. data from the twentieth century.[2] The limited implementation of capital punishment in this period presents a serious impediment to empirical analysis. Between 1946 and 1997 there were a total of 1,885 executions, representing one execution for every 320 reported homicides. Between 1968 and 1976 no executions were performed. Since 1976 execution rates have remained low: approximately one execution per 1,000 homicides. In Texas, the state with the highest execution rate, only one in 300 murders was punished by death between 1976 and 1997. Even if a substantial deterrent effect does exist, the amount of crime rate variation induced by executions may simply be too small to be detected. Assuming a reduction of seven homicides per execution (a number consistent with Ehrlich, 1975), observed levels of capital punishment in Texas since 1976 (a total of 144 executions through 1997) would have reduced the annual number of homicides in Texas by about fifty, or 2% of the overall rate. Given that the standard deviation in the annual number of homicides in Texas over this same time period is over 200, it is clearly a difficult challenge to extract the execution-related signal from the noise in homicide rates.

The empirical focus on deterrent effects of the death penalty, at least as currently practiced in the United States, also appears to be misplaced from a theoretical perspective. In 1997 seventy-four prisoners were executed — the highest total in thirty years. At the end of 1997 there were 3,335 inmates under a sentence of death, meaning that approximately 2% of those on death row were executed. Even among the subset of those eventually put to death, there is a long lag between sentencing and execution. Given the high discount rates of many criminals (Wilson and Herrnstein, 1985) and the fact that many homicides are committed by individuals under the influence of alcohol or drugs, which further foreshorten time horizons, it is hard to believe that punishment with such a long delay would be effective. Furthermore, the execution rate on death row is only twice the death rate from accidents and violence among all American men, and is only slightly greater than the rate of accidental and violent death for black males between the ages of 15 and 34.

2. Avio (1979, 1988), Deadman and Pyle (1989), and Phillips (1980) are notable exceptions.

Among the subsample of individuals engaged in illegal activities, the death rates are likely to be much higher. Levitt and Venkatesh (2000) report a death rate of 7% annually for street-level drug sellers in the gang they analyze. Kennedy, Piehl, and Braga (1996) estimate violent death rates to be 1%–2% annually among all gang members in Boston. With these figures, it is hard to believe that in modern America the fear of execution would be a driving force in a rational criminal's calculus.

Given these arguments, it seems plausible that the quality of life in prisons might exert a far more important effect on criminal behavior than the death penalty. The lower the quality of life in prison, the greater the punishment for a fixed amount of time served.[3] Thus, poor prison conditions are likely to be a deterrent to crime. Unlike capital punishment, prison conditions affect all inmates, regardless of the crime committed. Also, unlike capital punishment, the rarity of which makes it difficult to accurately estimate the likelihood of implementation, knowledge of prison conditions among potential criminals is likely to be accurate, either because of personal experience or that of acquaintances.

It is also possible that poor prison conditions will lead to more crime rather than less. Murton (1976) and Selke (1993), for instance, argue that poor prison conditions have a dehumanizing effect on inmates, arousing greater bitterness and hostility towards society, which manifest themselves as increased rates or severity of deviant behavior upon release from prison. Poor conditions may also serve to elevate the level of violence in prisons, which may in turn inhibit the reassimilation of released prisoners into general society.

In this article we provide the first rigorous empirical estimates of the relationship between prison conditions and crime rates. Lacking a direct measure of prison conditions, we use the death rate (from all causes) among prisoners in a given state and year as a proxy for prison conditions.[4]

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3. The television show *60 Minutes* clearly believes in the deterrent effect of bad prison conditions. Mike Wallace introduced a report on the subject as follows: "If there are very young children in the room with you watching *60 Minutes* tonight, we caution you that this next story is not for them. But if you've got a teenage son there in the room with you, this is a story you should watch together. It is about the cruel and unusual punishment a young man like your son may have to endure if he gets locked up."

4. Although we have explored numerous other potential proxies for prison conditions, we have been unable to locate consistent data series for any other variables. Information on

It is likely that prison death rates correlate with many important aspects of the relative unpleasantness of the prison experience. Inadequate health care, the subject of many recent lawsuits, is likely to be the most important factor in determining death rates among prisoners.[5]

Even after controlling for imprisonment rates in a state, severity of punishment per crime, and a host of other factors, our empirical results using state-level data over the period 1950–90 suggest a strong and robust negative relationship between prison death rates and subsequent crime rates in a state. These estimates appear to be too large to be attributable to the fact that a prisoner who dies will never be released, thus lowering the overall pool of criminals. Therefore, we interpret our results as evidence in favor of deterrence. The findings are robust to the inclusion of a range of demographic covariates, state fixed-effects, state-level trends, region–year interactions, and state–decade interactions. An alternative explanation for our results is that states that become better at identifying and locking up the most serious offenders experience both higher prison death rates (assuming that these serious offenders are more likely to die or to kill other prisoners) and decreases in crime (because scarce prison resources are being used more effectively). Given the data available to us, we cannot rule out this competing hypothesis. Variation in the age distribution of prisoners, on the other hand, does not appear to be a plausible explanation for our findings. Given the life cycle of criminal involvement, prison systems with a high number of old offenders are likely to be using prison resources inefficiently. Controlling for the number of prisoners, which we do, crime is therefore likely to be higher in states with more older prisoners, as are prisoner death rates, biasing our estimates against finding deterrence.

In contrast to the results on prison death, we find little systematic evidence supporting a deterrent effect of capital punishment. Estimates of the coefficient on executions are extremely sensitive to the choice of controls and invariably estimated with little precision. There simply does not appear

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expenditures for health care, the degree of overcrowding, and attacks on prisoners are available only in sporadic cross sections by prison systems voluntarily reporting such information. Breakdowns of overall prison death rates by cause are available only in recent years.

5. Homicide and suicide are relatively infrequent prison occurrences (a total of between 200 and 300 occurrences in a typical year). Nonetheless these causes of death outnumber executions roughly three to one.

to be enough information in the data on capital punishment to reliably estimate a deterrent effect.

We cannot stress enough that evidence of a deterrent effect of poor prison conditions is neither a necessary nor a sufficient condition for arguing that current prison conditions are either overly benign or unjustifiably inhumane. Efficiency arguments related to deterrence are only one small aspect of an issue that is inextricably associated with basic human rights, constitutionality, and equity considerations. Our research is descriptive, not proscriptive.

The outline of the paper is as follows: Section 2 describes the data set. Section 3 presents the empirical estimates. Section 3 also considers extensions to the basic model and explores alternative explanations for our empirical findings. Section 4 contains a brief set of conclusions, focusing on the important social and moral questions that are raised in this study but not directly addressed in the empirical analysis. A description of our data sources is presented in the Appendix.

2. Data and the Empirical Model

The data used in this study are a panel of annual, state-level observations covering the continental United States for the time period 1950–90 and collected by the authors from the various publicly available sources detailed in the accompanying data Appendix. We begin our analysis in 1950, since this is the first year in which many of the data series become available. Summary statistics for the data are presented in Table 1.

We employ three different crime rates as dependent variables in our analysis: murder, violent crime (excluding rape), and property crime (excluding larceny). In all cases we rely on Uniform Crime Report (UCR) data on crimes reported to the police as our measure of crime.[6] Rape is excluded because data was not collected until 1957. Larceny is omitted because of important changes in its definition over the time period examined. Although there are well-recognized problems with UCR data (Donohue and Seigelman, 1996; O'Brien, 1985), it is the only crime data that is available at the level of geographic disaggregation required. If the

6. As is commonly done in the literature, we simply sum the number of reported crimes across the individual crime categories that comprise violent and property crime. Thus, each reported crime is given an equal weight, regardless of the crime's severity.

Table 1. Summary Statistics

Variable	Mean	SD	Minimum	Maximum
Property crime per 100,000 residents	1,380	735	193	3,586
Violent crime per 100,000 residents	356	255	7	1,186
Murder per 100,000 residents	7.3	3.8	0	20.7
Prison deaths per 1,000 prisoners	3.10	1.64	0	32.53
Executions per 1,000 prisoners	0.11	0.35	0	12.5
Prisoners per crime (−1)	0.095	0.057	0.010	0.408
Prisoners per 100,000 residents (−1)	126.0	63.8	20.3	463.9
Real per capita income	13,724	3,846	4,114	26,146
Insured unemployment rate	3.37	1.62	0.5	12.2
Deaths under 1 yr. per 100,000 live births	1,830	721	622	5,659
Black (%)	0.11	0.08	0	0.45
Urban (%)	0.72	0.14	0.27	0.92
0–24-year-olds (%)	0.42	0.04	0.31	0.58
25–44-year-olds (%)	0.27	0.03	0.20	0.36

Note: The unit of observation is a state-year pair. 1,844 observations. Data cover the years 1950 to 1990, except for 1971, which is missing. Means and standard deviations are weighted by the state's share of the nation's population. Data sources are described in the Appendix.

measurement error in UCR data is simply random noise, our coefficient estimates will be less precise, but still consistent, since crime rates are used as the dependent variable. To the extent that we obtain similar results exploiting different sources of variation in the data (e.g., with or without state trends or region-year interactions), the likelihood that measurement error can account for our results is diminished.

The explanatory variables of primary interest are the execution rate and the death rate among prisoners from all sources other than execution. We define both of these rates per 1,000 state prisoners. The prison death variable will pick up not only direct deterrent effects associated with an increased likelihood of death while one is in prison, but also differences in prison conditions and quality of life for prisoners more generally. Data limitations prevent including more direct measures of prison conditions, such as square footage per prisoner, amount of prisoner-prisoner or guard-prisoner violence, or health care expenditures per prisoner. For most of the time period examined, no further disaggregation of causes of prison death is available. In recent years the cause of death is available. In 1997, for instance, 77% of prison deaths were due to illness, 5% were suicides, 3% were due to accident or killing by another inmate, 2% were executions (although these are not included in our variable definition), and 12% were due to unspecified causes.

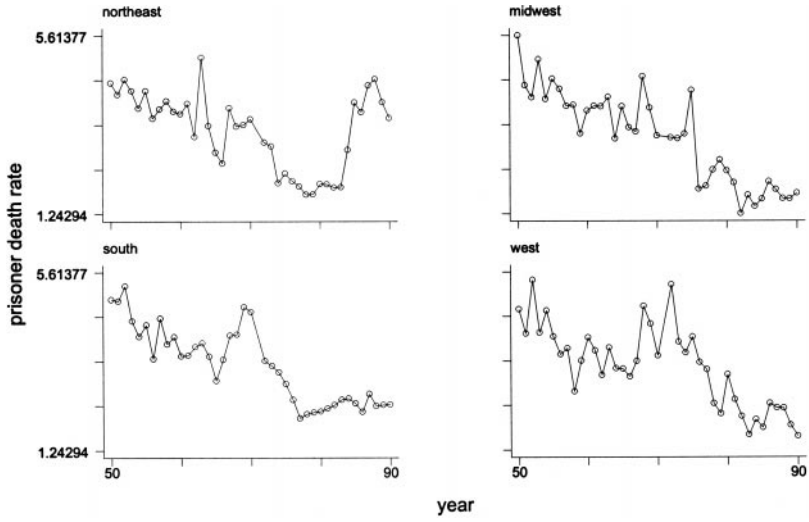

Figure 1. Prison death rate by region.

Figures 1 and 2 present regional time-series data on prison death rates and execution rates.[7] Prison death rates generally followed a downward trend between 1950 and 1990 across all regions of the country. The exception to this pattern is the late 1980s in the Northeast, where there is a large spike in prison death, largely attributable to Acquired Immune Deficiency Syndrome (AIDS). Average rates of prisoner death are similar across regions for most of the period. The time series data on executions reveals a very different pattern: high rates early in the sample (particularly in the South), and very low rates in all regions of the country later on.

In addition to the prisoner death and execution rates, a range of criminal justice, economic, and demographic variables are also included in our analysis. The certainty and severity of a state's criminal justice system is proxied by two variables: the number of prisoners per violent crime and ratio of prisoners to state population. These two measures are problematic because of the possibility they are endogenously related to state crime rates. For

7. The regression estimates we present do not exploit the aggregate time-series variation presented in Figures 1 and 2. Because year dummies and state-fixed effects are included in all specifications, our parameters are identified with use of only within-state deviations from national trends.

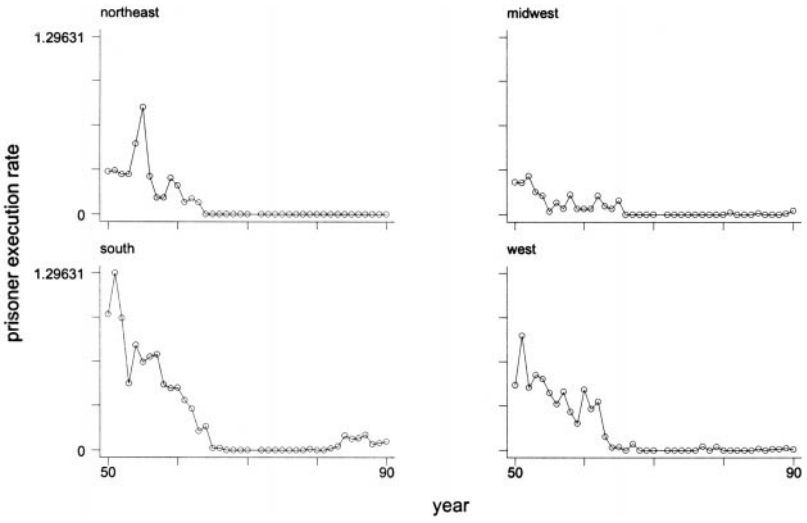

Figure 2. Execution rate by region.

instance, in states where crime rates have recently declined for exogenous reasons, the stock of prisoners will tend to be large relative to the flow of crimes, inducing a spurious negative relationship between crime rates and prisoners per violent crime. A bias in the opposite direction is likely for prisoners per state resident. Places with high crime rates will tend to have a larger fraction of residents incarcerated, *ceteris paribus*. One way in which we attempt to address this problem is to use once-lagged values of the criminal justice variables as regressors. This avoids any contamination of the measures by contemporaneous shocks to crime. Using lags also reduces the likelihood that ratio-bias problems associated with having crime rates in the denominator of a right-hand-side variable. More generally, our research strategy for minimizing the concerns associated with imperfect controls is to present a wide range of specifications exploiting different sources of variation in the data. To the extent that our main findings are robust, our level of confidence in the estimates increases.

The economic measures are real state per capita income and the insured unemployment rate. We use the insured unemployment rate rather than the overall unemployment rate because the latter is not available at the state level in the early part of our sample. Over time periods in which both measures are available, they are highly correlated. Demographic controls include the

percent of a state's population that is black, the percent of the population residing in urban areas, age distribution variables, and infant mortality rates. The two variables measuring percentage black and percentage urban are linearly interpolated between decennial censuses. The remaining variables are available on an annual basis.

The empirical specifications estimated are variations on the following equation:

$$\mathrm{CRIME}_{st} = \beta_1 \mathrm{DEATH}_{st} + \beta_2 \mathrm{EXECUTE} + X_{st}\Gamma + \lambda_s + \delta_t + \varepsilon_{st}, \quad (1)$$

where *s* indexes states and *t* corresponds to time. CRIME is one of the three crime measures described earlier; DEATH and EXECUTE are, respectively, the death rate (excluding executions) and the execution rate per 1,000 state prisoners. *X* is the matrix of criminal justice, economic, and demographic variables detailed above. The indicator variables λ and δ represent state-fixed effects and time dummies.

In addition to the basic specification in equation (1), we also estimate a range of alternatives. Given possible concerns about the appropriateness of some of the control variables, we also estimate specifications, omitting the criminal justice, demographic, and economic controls, relying instead on various forms of indicator variables to "soak up" variation in the data. We present estimates that include region-year interactions, state trends, and state-decade interactions.[8] When region-year interactions are included, the parameters of the model are identified solely off differences across states within a given region and year. With state-decade interactions, only within-state variation around the state's mean value in a given decade is used in estimating the parameters.

3. Empirical Estimates

Empirical estimates of variations on equation (1) for homicide, violent crime, and property crime are presented in Tables 2–4, respectively. The structure of these three tables is identical. Odd columns include only the prison death, execution, and indicator variables. Even-numbered

8. Year dummies, state-fixed effects, or both become redundant once a more complete set of interactions are introduced and therefore are dropped from the model in such cases.

Table 2. Impact of Prison Death and Execution on Murder Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Prison deaths/1,000 prisoners	−0.01	−0.05	−0.07	−0.05	−0.02	−0.01	−0.01	−0.02
	(0.06)	(0.03)	(0.04)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
Executions/1,000 prisoners	0.34	−0.70	−0.31	−0.64	0.01	−0.10	−0.07	−0.24
	(0.31)	(0.26)	(0.25)	(0.25)	(0.16)	(0.14)	(0.16)	(0.14)
Prisoners/crime (−1)	—	−3.26	—	−3.37	—	0.71	—	−0.6
		(3.30)		(3.05)		(1.81)		(2.1)
Prisoners/100,000 residents (−1)	—	−0.001	—	0.003	—	−0.014	—	−0.012
		(0.005)		(0.004)		(0.003)		(0.005)
Real per capita income (*1,000)	—	0.14	—	0.23	—	0.52	—	0.42
		(0.13)		(0.15)		(0.15)		(0.16)
Insured unemployment rate	—	−0.20	—	−0.08	—	−0.11	—	−0.15
		(0.10)		(0.08)		(0.06)		(0.08)
Black (%)	—	41.0	—	40.5	—	3.1	—	37.1
		(4.8)		(10.1)		(11.6)		(16.1)
Urban (%)	—	−4.8	—	−21.3	—	−18.2	—	−7.3
		(5.1)		(5.6)		(6.4)		(6.9)
0–24-year-olds (%)	—	37.3	—	29.5	—	0.6	—	2.6
		(16.3)		(13.3)		(6.7)		(10.2)
25–44-year-olds (%)	—	−10.3	—	−40.3	—	−24.1	—	−15.8
		(14.2)		(14.5)		(17.5)		(23.4)
Infant mortality rate (*1,000)	—	−1.2	—	−1.2	—	−0.66	—	0.16
		(0.6)		(0.6)		(0.34)		(0.44)
Adjusted r^2	0.819	0.877	0.859	0.898	0.931	0.940	0.938	0.947
Region-year interactions?	No	No	Yes	Yes	No	No	No	No
State trends?	No	No	No	No	Yes	Yes	No	No
State-decade interactions?	No	No	No	No	No	No	Yes	Yes

Notes: Dependent variable is the murder rate per 100,000 residents. Data are annual, state-level observations for the period 1950–90. Data for 1971 are missing. The regressions are weighted by the state's share in the nation's population. State-fixed effects and year dummies are included in all specifications, except where they are redundant (e.g., year dummies are excluded when region-year interactions are included). Standard errors are clustered by state-decade. 1,844 observations.

Table 3. Impact of Prison Death and Execution on Violent Crime Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Prison deaths/1,000 prisoners	−6.2	−8.3	−10.4	−9.4	−4.8	−3.9	−3.6	−3.4
	(4.5)	(2.8)	(3.1)	(2.2)	(2.0)	(1.6)	(1.5)	(1.5)
Executions/1,000 prisoners	2.0	−38.8	−17.2	−21.6	14.4	6.4	3.7	−4.1
	(13.9)	(12.2)	(11.1)	(9.2)	(4.9)	(3.5)	(3.3)	(3.0)
Prisoners/crime (−1)	—	−864	—	−1,241	—	−412	—	−730
		(220)		(220)		(143)		(178)
Prisoners/100,000 residents (−1)	—	0.92	—	1.46	—	−0.26	—	0.21
		(0.29)		(0.26)		(0.15)		(0.27)
Real per capita income (*1,000)	—	20.4	—	26.9	—	12.0	—	−2.1
		(11.1)		(14.1)		(5.0)		(5.7)
Insured unemployment rate	—	−13.5	—	−5.3	—	−4.8	—	−2.9
		(8.1)		(5.4)		(3.3)		(4.0)
Black (%)	—	2,672	—	2,615	—	1,722	—	1,213
		(889)		(855)		(971)		(1,153)
Urban (%)	—	746	—	208	—	113	—	362
		(360)		(352)		(309)		(327)
0–24-year-olds (%)	—	1,375	—	1,094	—	−284	—	126
		(950)		(943)		(383)		(808)
25–44-year-olds (%)	—	−3,492	—	−3,693	—	803	—	45
		(1,687)		(1,856)		(735)		(1,288)
Infant mortality rate (*1,000)	—	−0.035	—	0.006	—	−0.008	—	0.017
		(0.037)		(0.031)		(0.021)		(0.022)
Adjusted r^2	0.837	0.886	0.857	0.909	0.967	0.970	0.968	0.972
Region-year interactions?	No	No	Yes	Yes	No	No	No	No
State trends?	No	No	No	No	Yes	Yes	No	No
State-decade interactions?	No	No	No	No	No	No	Yes	Yes

Notes: Dependent variable is the violent crime (excluding rape) per 100,000 residents. Rape is excluded because it was not reported until 1957. Data are annual, state-level observations for the period 1950–90. Data for 1971 are missing. The regressions are weighted by the state's share in the nation's population. State-fixed effects and year dummies are included in all specifications, except where they are redundant. Standard errors are clustered by state-decade. 1,844 observations.

Table 4. Impact of Prison Death and Execution on Property Crime Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Prison deaths/1,000 prisoners	−14.0	−11.4	−17.1	−12.5	−12.6	−2.0	−13.7	−9.2
	(8.1)	(7.6)	(7.7)	(6.5)	(8.2)	(5.7)	(6.8)	(5.3)
Executions/1,000 prisoners	25.5	−33.0	−28.4	−35.5	92.3	11.0	19.4	−9.1
	(30.2)	(23.9)	(21.3)	(20.0)	(25.2)	(13.6)	(11.6)	(10.3)
Prisoners/crime (−1)	—	−1,819	—	−1,702	—	−964	—	−1,521
		(580)		(531)		(382)		(536)
Prisoners/100,000 residents (−1)	—	1.53	—	1.90	—	−1.77	—	−1.49
		(0.65)		(0.57)		(0.54)		(0.78)
Real per capita income (*1,000)	—	32.8	—	38.9	—	−61.8	—	−58.0
		(25.1)		(29.5)		(21.9)		(27.9)
Insured unemployment rate	—	39.4	—	44.5	—	30.5	—	23.7
		(18.0)		(16.9)		(10.4)		(12.7)
Black (%)	—	1,119	—	1,534	—	12,853	—	4,592
		(1,754)		(1,819)		(2,650)		(3,639)
Urban (%)	—	−26	—	−1,043	—	−2,961	—	491
		(937)		(971)		(1,197)		(1,078)
0–24-year-olds (%)	—	5,784	—	5,261	—	2,327	—	3,040
		(2,117)		(2,065)		(1,227)		(2,099)
25–44-year-olds (%)	—	−4,662	—	−2,020	—	−8,281	—	−7,116
		(3,370)		(3,486)		(3,347)		(3,736)
Infant mortality rate	—	−0.262	—	−0.089	—	−0.121	—	−0.090
		(0.108)		(0.067)		(0.065)		(0.073)
Adjusted r^2	0.888	0.907	0.920	0.933	0.938	0.959	0.960	0.967
Region-year interactions?	No	No	Yes	Yes	No	No	No	No
State trends?	No	No	No	No	Yes	Yes	No	No
State-decade interactions?	No	No	No	No	No	No	Yes	Yes

Notes: Dependent variable is the property crime rate (excluding larceny) per 100,000 residents. Larceny is excluded because of changes in its definition over the sample period. Data are annual, state-level observations for the period 1950–90. Data for 1971 are missing. The regressions are weighted by the state's share of the nations's population. State-fixed effects and year dummies are included in all specifications, except where they are redundant. Standard errors are clustered by state-decade. 1,844 observations.

columns include a full set of criminal justice, economic, and demographic covariates, in addition to a varying set of indicator variables. Columns (1) and (2) of each table contain the basic specification with year dummies and state fixed-effects. Additional controls are included in the other columns, as indicated at the bottom of each table. In all cases, the method of estimation is weighted least squares, with the weight equal to the share of the U.S. population residing in the state. The parameter estimates are not, however, sensitive to this choice of weighting. White-heteroskedasticity-consistent standard errors (clustered by state-decade to take into account within-state serial correlation) are reported in parentheses.

Table 2 presents estimates with homicide as the dependent variable. In all cases a very large fraction of the overall variation in homicide rates is explained by the regressions. The coefficient on prison death is negative in all eight specifications, is precisely estimated, and varies over a relatively small range. In contrast, the execution rate coefficient is extremely sensitive to the choice of specification and has estimated standard errors five to ten times larger than those for prison death. Evaluated at the mean of the data, the implied decline in homicides associated with one additional prison death ranges from -0.1 to -0.8 across specifications.[9] For executions, the range is $+3.1$ to -5.6. The largest of these estimates for capital punishment is not substantially different from the estimate in Ehrlich (1975). The variability of the estimated effects of capital punishment across specifications is consistent with the sensitivity of the findings in the previous literature to the choice of functional form (Cameron, 1994; Forst, Filatov, and Klein, 1978).

Evaluated at the means of the data, the implied elasticity of the murder rate to the prison death rate ranges from -0.005 to -0.048. The corresponding elasticity for execution is essentially zero (the largest estimated magnitude of this elasticity is -0.00015). There are simply so few executions that, even if there is a large effect per execution, a doubling of the execution rate would not have a noticeable impact on murder rates.

The performance of the other variables in Table 2 are mixed. Adding the covariates to the specification (i.e., comparing even and odd columns)

9. The coefficients in the table are in terms of rates. To translate the coefficient into an effect per death, one must multiply the coefficient by (state residents/state prisoners)/100. It is necessary to divide by 100 because the dependent variable is defined per 100,000 residents, whereas the death rates are per 1,000 prisoners. Evaluated at the means of our data reported in Table 1, the adjustment factor is equal to 8.73.

explains only a small amount of variance, particularly after the more complete set of interactions are added to the specifications in columns (5)–(8). Theory predicts negative coefficients on prisoners per crime and prisoners per 100,000 residents. This prediction is generally borne out in the data, but not uniformly. As would be expected, given the bias stories presented earlier, the addition of better controls leads the prisoners-per-crime variable back towards zero, whereas the prisoners-per-capita variable becomes increasingly negative. The economic variables enter with a counterintuitive sign. High income is consistently associated with high murder rates (see Ruhm, 2000, for a similar finding), as are low unemployment rates. Murder rates are higher when the percentage black is high and lower, suprisingly, in highly urbanized states, once other factors are controlled for. The age and infant mortality variables are not statistically significant once the full set of controls are introduced.

The results for violent crime are presented in Table 3. The coefficient on prison death is now negative and statistically significant at the .05 level in almost all cases. Both the magnitude of the coefficient and the standard error fall as better controls are introduced. Evaluated at the mean of the data, each additional prison death is associated with a decline of 30–98 violent crimes. The implied elasticity of violent crime with respect to prison death ranges from -0.05 to -0.17 across specifications. The magnitudes of the crime reduction are too large to simply reflect the elimination of future crimes by the deceased. Estimates of the number of reported crimes reduced per additional prisoner-year of incapacitation are generally no greater than ten (Levitt, 1996; Marvell and Moody, 1994). Thus, it appears that poor prison conditions, as proxied by prison death rates, have a deterrent effect on criminal activity.

In contrast to prison death, there is no apparent effect of executions on violent crime. The execution coefficient is extremely sensitive to the choice of specification and takes on a positive sign as frequently as a negative sign. The estimated change in violent crimes per execution, evaluated at the mean, ranges from approximately -300 to 100. The lack of a systematic relationship between violent crime and execution is not, however, particularly surprising, since neither robbery nor aggravated result are capital crimes.

Violent crime rates appear to be negatively related to the incarceration likelihood per crime, a result that is consistent with deterrence. States with more prisoners overall tend to have more violent crime in specifications

without state trends or state-decade interactions. Once these controls are added (columns [6] and [8]), however, this result disappears, strongly suggesting that the presence of omitted factors is driving this result. Economic factors appear to be fairly weakly associated with violent crime. This mirrors previous empirical work that finds strong business cycle effects on property crime, but not on violent crime (e.g., Freeman, 1995). The fraction of black and urban residents exhibits a positive association with violent crime across all specifications. The fraction of the population under age 25 is positively related to crime rates only in specifications with a limited set of controls.

The pattern of estimates for property crimes, presented in Table 4, is almost identical to that for violent crimes. Once again the coefficient on prison death is negative, although statistically significant in only a few specifications. The estimated reduction in property crimes per prison death (20 to 150) is roughly comparable to that for violent crime. Since property crime is roughly five times as frequent as violent crime, however, the implied elasticities are smaller for property crime. The coefficient on executions is again extremely sensitive to the choice of specification and is equally likely to be positive or negative. Both the number of prisoners per crime and the number of prisoners per resident are negatively related to property crime once state trends or state-decade interactions are included. High unemployment rates are associated with higher property crime across all specifications. A one percentage point increase in the unemployment rate is associated with roughly a 2%–4% increase in property crime rates. The percentage black and fraction under age 25 are also correlated with higher property crime rates in most specifications. Higher infant mortality rates are associated with lower property crime rates, although we do not have a good explanation why this should be the case.

4. Extensions to the Basic Model and Alternative Explanations for the Empirical Results

Our basic results are consistent with a deterrent effect of prison death rates (presumably proxying for prison conditions more generally) on crime. In this section we examine the sensitivity of this result to different sets of assumptions and also explore possible alternative explanations for the empirical regularities we observe.

In Table 5 we present specifications allowing for prison death and execution rates to have both contemporaneous and lagged effects on crime rates. Theoretically, lagged prison death rates might be expected to influence current crime rates if (1) there are lags in information transmission about prison conditions to those not incarcerated, (2) bad prison conditions lower the likelihood that current inmates will recidivate after their release, or (3) bad prison conditions interfere with reassimilation into society. For each crime category two columns of results are presented, one with state trends and the other with state-decade interactions. The full set of year dummies and criminal justice, economic, and demographic characteristics are included in all specifications, but are not shown in the table. The odd (or even) columns of Table 5 therefore mirror column (6) (or [8]) of Tables 2–4, except that three lags of the death and execution rates are added.

The first two columns of Table 5 examine murder. There is little evidence of systematic effects. The sign on the individual coefficients is highly variable for both prison death and execution. The joint impact of the coefficients is presented in the bottom portion of the table. The prison death coefficients are jointly positive, but not significant at the .05 level in column (2). For executions, even though the contemporaneous effect on murder is negative, the lagged values are generally positive, and the overall impact is not statistically significant.

The results for violent and property crimes are in columns (3)–(4) and (5)–(6), respectively. The contemporaneous and lagged effects of prison death are negative in virtually every instance. Past prison death rates appear to exert an independent influence on current crime rates, with the magnitude of the impact eroding over time. The crime reduction associated with a prison death two to three years ago is roughly half as large as for a prison death in the current period.[10] The individual prison death variables are not statistically significant, but they are borderline jointly statistically significant in most cases, as shown in the bottom portion of the table. The cumulative effect of the prison death variables is greater than the corresponding coefficient on contemporaneous prison death in Tables 3 and 4 in all cases. In contrast, there is little evidence that execution rates affect violent or property crime in Table 5.

10. Inclusion of further lags yielded coefficients that were substantively small and statistically insignificant.

Table 5. Lagged Impacts of Prison Death and Execution

	Murder		Violent Crime		Property Crime	
	(1)	(2)	(3)	(4)	(5)	(6)
Prison deaths/1,000 prisoners	−0.016 (0.017)	−0.016 (0.018)	−2.90 (1.28)	−3.27 (1.44)	−1.03 (4.90)	−8.29 (4.98)
Prison deaths/1,000 prisoners (−1)	0.010 (0.015)	0.022 (0.017)	−2.58 (1.30)	−2.45 (1.84)	−3.74 (3.76)	−10.02 (4.09)
Prison deaths/1,000 prisoners (−2)	−0.002 (0.017)	0.015 (0.022)	−2.35 (2.17)	−2.26 (2.72)	−1.93 (4.39)	−6.09 (4.07)
Prison deaths/1,000 prisoners (−3)	0.027 (0.021)	0.046 (0.023)	−1.23 (1.05)	−3.27 (1.44)	3.88 (3.06)	−2.97 (3.44)
Executions/1,000 prisoners	−0.132 (0.130)	−0.257 (0.142)	5.32 (3.23)	−3.37 (2.88)	8.48 (11.77)	−6.39 (10.32)
Executions/1,000 prisoners (−1)	0.115 (0.093)	0.040 (0.091)	8.31 (2.87)	0.35 (3.09)	14.38 (10.95)	1.75 (9.24)
Executions/1,000 prisoners (−2)	0.033 (0.115)	−0.052 (0.108)	5.56 (2.61)	−0.89 (3.01)	16.66 (11.79)	8.67 (10.78)
Executions/1,000 prisoners (−3)	0.035 (0.093)	0.013 (0.078)	8.78 (2.73)	3.45 (3.91)	10.60 (11.91)	1.64 (13.14)
Year dummies; demographic, economic, and socioeconomic covariates	Yes	Yes	Yes	Yes	Yes	Yes
State-fixed effects?	Yes	No	Yes	No	Yes	No
State-trends?	Yes	No	Yes	No	Yes	No
State-decade interactions?	No	Yes	No	Yes	No	Yes
Sum of prison death coefficients	0.019 (0.034)	0.067 (0.049)	−9.06 (4.56)	−11.25 (7.71)	−2.82 (12.61)	−27.37 (13.70)
Sum of execution coefficients	0.051 (0.294)	−0.256 (0.260)	27.97 (9.21)	−0.46 (15.90)	50.12 (39.25)	5.67 (40.09)
Adjusted r^2	0.940	0.947	0.970	0.972	0.959	0.967

Notes: Dependent variables are property crime rate (excluding larceny), violent crime rate (excluding rape) and murder rate per 100,000 residents. Larceny is excluded because of changes in its definitions over the sample period. Rape is excluded because it was not reported until 1957. Data are annual, state-level observations for the period 1950–90. Data for 1971 are missing. The regressions are weighted by the state's share in the nation's population. Standard errors are clustered by state-decade. 1,778 observations.

In Table 6 the sample is split into time periods using 1971, a year for which we do not have data, as the break point. In addition to splitting our data roughly in half, 1971 is a logical choice because the early 1970s mark the transition from a period of stable prison populations to the steadily increasing number of prisoners that continues through the present time. In all cases a full set of controls, including state trends, are included in the regressions. With the exception of murder rates, the prison death coefficients are stable across the two time periods. Interestingly, the prison death rate coefficients from the split sample with state trends are more similar to the estimates from the overall sample when state-decade interactions are included than when state trends are present. This suggests that the assumption of a constant linear trend across the entire period is not consistent with the patterns in the data. The execution patterns are dramatically different in the two parts of the sample. In the early period the execution coefficients are relatively small, negative, and precisely estimated. In the latter years the coefficients are ten times larger in magnitude, as are the standard errors. The standard errors rise because so few executions are performed in the latter period. There is also a great deal of instability in the other covariates included in the specifications, with the signs often reversing across the two time periods. The fraction of the population aged 25 to 44 enters with the expected sign and statistical significance in all of the specifications except one.

The specifications considered thus far assume a linear relationship between crime rates and prison death rates. Table 7 presents estimates with crime rates (but not prison death or execution rates) logged.[11] This functional form implies that a given change in the prison death rate will lead to a greater change in the number of crimes when the crime rate is high. The eight columns in Table 7 correspond to the columns in Tables 2–4. Coefficients are presented for only the prison death and execution variables, with separate rows for the three different crime categories. The results are similar to those of previous tables. The prison death coefficients are consistently negative and sometimes statistically significant for violent and property crime, and are mixed for murder. Evaluated at the sample means, the implied elasticities of violent (property) crime with respect to prison death rates are as high as −0.06 (−0.05). These values are similar in magnitude to those

11. Prison death and execution rates are frequently equal to zero and thus cannot be logged.

Table 6. Impact of Prison Death and Execution by Time Period

	1950–70			1972–90		
	Property Crime (1)	**Violent Crime (2)**	**Murder (3)**	**Property Crime (4)**	**Violent Crime (5)**	**Murder (6)**
Prison deaths/1,000 prisoners	−7.82	−2.57	0.008	−6.65	−1.9	−0.015
	(3.57)	(1.17)	(0.024)	(4.33)	(1.3)	(0.026)
Executions/1,000 prisoners	−4.61	−1.60	−0.105	−66.6	11.68	−1.15
	(8.75)	(2.10)	(0.121)	(169.5)	(33.70)	(0.78)
Prisoners/crime (−1)	−1019	−323	0.7	−5,322	−2,186	−8.7
	(448)	(126)	(2.2)	(1,060)	(504)	(9.3)
Prisoners/100,000 residents (−1)	1.18	−0.20	−0.015	−0.29	0.46	−0.013
	(1.07)	(0.34)	(0.008)	(0.70)	(0.25)	(0.006)
Real per capita income (*1,000)	36.3	16.8	0.59	−94.1	−10.8	0.83
	(24.3)	(10.6)	(0.19)	(25.3)	(6.1)	(0.23)
Insured unemployment rate	−3.88	−4.99	−0.012	9.77	−11.62	−0.22
	(8.89)	(3.88)	(0.065)	(13.72)	(4.44)	(0.10)
Black (%)	42,532	11,077	−41.0	−6,122	−5,417	−25.6
	(10,989)	(4,191)	(52.6)	(5,936)	(3,887)	(56.4)
Urban (%)	−7	247	−12.1	−2,898	399	−2.3
	(815)	(361)	(8.2)	(2,141)	(886)	(25.5)
0–24-year-olds (%)	−960	−1,214	−0.5	−1,365	−3,648	−26.6
	(1,495)	(704)	(10.4)	(2,109)	(1,509)	(28.2)
25–44-year-olds (%)	6,215	4,385	62.5	7,613	4,237	−44.1
	(5,364)	(2,059)	(38.5)	(4,065)	(1,837)	(37.8)
Infant mortality rate	−0.070	−0.017	−0.0006	0.091	0.074	0.0014
	(0.053)	(0.017)	(0.0004)	(0.110)	(0.032)	(0.0007)
Adjusted r^2	0.958	0.922	0.939	0.958	0.976	0.943
State-trends?	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variables are property crime rate (excluding larceny), violent crime rate (excluding rape), and murder rate per 100,000 residents. Larceny is excluded because of changes in its definition over the sample period. Rape is excluded because it was not reported until 1957. Data are annual, state-level observations. The regressions are weighted by the state's share in the nation's population. State-fixed effects and year dummies are included in all specifications. Standard errors are clustered by state-decade. 36 observations in columns 1–3, 904 observations in columns 4–6.

Table 7. Impact of Prison Death and Execution on Crime Rates in Logs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Violent crime rate								
Prison deaths/1,000 prisoners	−0.009	−0.010	−0.019	−0.016	−0.014	−0.009	−0.007	−0.007
	(0.010)	(0.007)	(0.008)	(0.006)	(0.027)	(0.004)	(0.005)	(0.004)
Executions/1,000 prisoners	0.139	−0.019	−0.017	−0.001	0.066	−0.006	0.038	−0.010
	(0.048)	(0.032)	(0.031)	(0.032)	(0.027)	(0.013)	(0.029)	(0.015)
Property crime rate								
Prison deaths/1,000 prisoners	−0.010	−0.005	−0.010	−0.005	−0.011	−0.004	−0.008	−0.006
	(0.006)	(0.004)	(0.005)	(0.003)	(0.005)	(0.003)	(0.004)	(0.004)
Executions/1,000 prisoners	0.083	0.008	−0.014	−0.005	0.093	0.002	0.035	−0.003
	(0.024)	(0.015)	(0.015)	(0.013)	(0.021)	(0.010)	(0.014)	(0.010)
Murder rate								
Prison deaths/1,000 prisoners	0.007	0.002	−0.002	0	−0.001	0.001	0.001	−0.001
	(0.009)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Executions/1,000 prisoners	0.139	−0.059	−0.009	−0.045	0.027	−0.013	0.013	−0.020
	(0.043)	(0.027)	(0.025)	(0.026)	(0.023)	(0.018)	(0.025)	(0.019)
Demographic covariates?	No	Yes	No	Yes	No	Yes	No	Yes
Region-year interactions?	No	No	Yes	Yes	No	No	No	No
State trends?	No	No	No	No	Yes	Yes	No	No
State-decade interactions?	No	No	No	No	No	No	Yes	Yes

Notes: Dependent variables are crime rates per 100,000 residents. Data are annual, state-level observations for the period 1950–90. State-fixed effects and year dummies are included in all specifications, except where they are redundant (e.g., year dummies are excluded when region-year interactions are included). The regressions are weighted by the state's share in the nation's population. Standard errors are clustered by state-decade.

obtained in the earlier tables. The capital punishment coefficient is once again extremely sensitive to the choice of specification and takes on a positive sign roughly half of the time.

4.1. Alternative explantions for the negative relationship between prison death rates and crime rates. We have argued that the negative association between prison death rates and violent and property crime is an indication that poor prison conditions (as proxied by death rates) serve as a deterrent to crime. It is important, however, to give careful consideration to other possible explanations for the empirical regularities that we observe.

One possibility is that the direction of causality runs in the opposite direction (i.e., that low crime rates cause high prison death rates).[12] For instance, if current crime rates are low, the inflow of young prisoners may be lessened, leading to an aging prison population with greater age-related mortality. Although detailed information on the age of prison populations is not available on a state-by-state basis, we offer two arguments against this hypothesis, arguments based on aggregate statistics. First, the aged make up a very small fraction of prison populations. In 1991 only 0.7% of state prison inmates were 65 years of age or older. In comparison, 12.6% of Americans were at least 65 years of age in 1991. In the U.S. as a whole, mortality rates for 70-year-old men are about 20 times higher than those of 30-year-old men. Assuming mortality rates by age in prison are proportional to those on the outside, a doubling of the fraction of inmates over the age of 65 would induce a 6% increase in the prison death rate, or only one-tenth of a standard deviation. Second, if the direction of causality runs from crime rates to prison death rates, it is difficult to explain the systematic relationship between lags of prison death and current crime rates in Table 5. Rather, one would expect *leads* of prison death rates to be correlated with current crime rates; that is, low crime in the current period should predict high prison death rates in the future. When leads of prison death rates are added to the specifications in Table 5, however, the coefficients are substantively small, frequently positive, and never jointly statistically significant.

12. One might just as easily argue, however, that low crime rates would lead to more pleasant prison conditions and reduced prison death rates. For example, prison overcrowding is likely to be less acute in states where crime rates are falling.

Another explanation for our results might be that prison death rates are correlated with other factors that affect crime but have been omitted from the specifications, such as a state's effectiveness in identifying the most serious offenders, or the degree of police brutality. State prison systems that are most effective in identifying the worst offenders will have low crime rates and high prison death rates, if the more serious offenders die (or kill) at greater rates when imprisoned. The most likely channel through which more serious offenders would have elevated death rates would be through increased rates of violent death. Death at the hands of another prisoner, however, is a relatively rare occurrence. Only about 5% of prison deaths in recent years were murders. It is also possible, however, that death rates of serious offenders are higher for other reasons, such as worse health as a result of past risky behavior. The data available do not allow us to analyze prisoner death rates by type of criminal.

Another possible explanation for our results is that a "climate of oppression" in a state may reduce crime, with high prison death rates representing only one element of the oppression. In general, the fact that our prison death estimates are not particularly sensitive to the choice of controls (including two measures of criminal justice system punitiveness), argues against this claim. Furthermore, the inclusion of state trends and state-decade interactions "soaks up" long-term drifts in such variables. For these stories to hold true, the omitted variables must be correlated with short-term fluctuations in a state's prison death rate.

A final alternative to our interpretation of the results is that fluctuations in criminal health status are driving both prison death rates and crime rates, under the assumption that unhealthy criminals commit fewer crimes. It is hard to imagine, however, that short-term fluctuations in criminal health (as opposed to changes in prison conditions, such as the degree of overcrowding or the quality of health care) is the primary source of variation in prison death rates.

5. Conclusion

Using annual state-level data for the period 1950–90, we uncover a robust negative relationship between prison death rates and violent and property crime rates. In contrast, there is little evidence in support of a deterrent effect of capital punishment as presently administered. In terms of crimes reduced

per prison death, the estimated effects are quite large: 30–100 violent crimes and a similar number of property crimes.

Although our estimates are large when measured in crimes per prison death, in another sense, our results are small. The elasticities obtained, generally smaller than .05, imply that a doubling of the prison death rate would reduce the crime rate by only a few percentage points. Over the period 1950–90 reported crime rates rose sharply. The decline in prison death rates between 1950 and 1990 explains only between 2% and 3% of the observed increase in crime rates over that period.

Although there may be temptation to draw public policy implications from our results, we strongly caution against doing so. As demonstrated in the preceding paragraph, the aggregate impact of changing prison conditions on crime rates appears to be small. Given the limited efficiency gains implied by these estimates, the moral and ethical considerations surrounding these issues would appear to dominate any economic arguments. In a society predicated on civil liberties, the social costs of degrading living conditions in prisons beyond their current state are likely to overwhelm any marginal reductions in crime.

Without further analysis of the question, we also caution against extrapolating our results to argue that the elimination of prison amenities such as cable television and athletic facilities will prove a deterrent to crime. Although that position is consistent with our findings, it is by no means a direct implication of our results. Substantial changes in prison death rates are categorically different from minor alterations of the quality of life associated with the removal of weight rooms. Before drawing any conclusions about the latter, we would be prudent to compile evidence that is more directly relevant to the issue.

Appendix

Crime rates. All crime rates are Uniform Crime Report (UCR) data on reported crime rates from *Crime in the United States*, published annually by the Federal Bureau of Investigation. Our measure of violent crime is the sum of reported murders, aggravated assaults, and robberies. Data on forcible rape is not collected until 1957 and therefore is excluded from our sample. Our measure of property crimes is the sum of burglaries and motor vehicle thefts. Larcenies are excluded from our calculations because of a definitional change in the 1960s.

Prisoner counts and prison death rates. Annual data on the number of prisoners and prison death rates by state for the period 1950–70 is published annually by the Federal Bureau of Prisons under numerous different titles, but most frequently as *Prisoners in State and Federal Institutions*. This data was collected by the Law Enforcement Assistance Administration from 1971–79. For 1971, however, no published data on prisoner death rates are available. Since 1980 the data has been published annually by the U.S. Bureau of Justice Statistics in *Correctional Populations in the United States*. In some cases, our data is drawn from a secondary source, *Sourcebook of Criminal Justice Statistics*, rather from the original document.

Executions. Data on the number of state executions are compiled by the same sources listed above for prisoner counts and prisoner death rates, but published in separate reports originally entitled *Executions* and later called *Capital Punishment*. Since 1980 the capital punishment data has been included in *Correctional Populations in the United States*.

State population. Available annually in *The Statistical Abstract of the United States*.

Real per capita income. State per capita income is available annually in *The Statistical Abstract of the United States*. Values are converted to real dollars using the Consumer Price Index.

Insured unemployment rate. (This is the average number of workers claiming state unemployment compensation benefits as a percent of all workers covered.) The rate published in U.S. Department of Labor (1996).

Infant mortality. Death rates under one year of age per 100,000 live births is annual state-level data from *Vital Statistics of the United States*.

State population, per capita income, percentage black, percentage urban, and state age distributions. All of these data are available in the *Statistical Abstract of the United States*. The percentage black and percentage urban are based on decennial census data and are linearly interpolated between censuses.

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