Crime, cash, and limited options: Explaining the prison boom*

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Research Summary
An analysis of a state panel of prison populations from 1977 to 2005 shows that the best predictors of prison populations are crime, sentencing policy, prison crowding, and state spending. Prison populations grew at roughly the same rate and during the same periods as spending on education, welfare, health and hospitals, highways, parks, and natural resources. Current and lagged values of state spending on prison construction also accounted for a substantial amount of variation in subsequent prison populations. Public opinion, partisan politics, the electoral cycle, and social threats seem to have had little effect on the number of prisoners.

Policy Implications
The availability of publicly acceptable alternatives to incarceration may not be sufficient to reverse course. Federal funding of alternatives—but not prisons—would provide states with the financial incentive to reduce prison populations.

Keywords: prison population, correctional spending, sentencing policy

* The author is grateful to Steven Raphael and Steven Levitt for providing data, to the Policy Research Institute at the University of Texas for its financial support, and to Philip Cook, Michael Murray, and four anonymous reviewers for their valuable suggestions on modeling and analysis. Any remaining errors are the fault of mischievous gremlins. Direct correspondence to William Spelman, LBJ School of Public Affairs, University of Texas at Austin, P.O. Box Y, Austin, TX 78713-8925 (e-mail: spelman@mail.utexas.edu).

The United States houses a greater proportion of its citizens in prisons than any other country in the world—more than Russia and China, more than South Africa during apartheid, and maybe even more than North Korea (Walmsley, 2007). The direct costs of incarceration are in excess of $20,000 per prisoner. Many economists think the total social costs, which include legitimate income forgone by prisoners and reduced life prospects for their families, are roughly twice that (for example, Donohue, 2007; Kleykamp, Rosenfield, and Scotti, 2008). Evidence suggests that we have obtained some value for our money. Estimates vary widely, but the marginal prison bed seems to prevent somewhere between two and seven crimes, which saves potential victims between $4,000 and $19,000 per year (Levitt, 1996; Spelman, 2005; Western, 2006).

But note the details: If each prison bed reduces costs by no more than $19,000, but costs us $20,000 to $40,000, then do we need this many beds? Clearly not, and it is not (too) difficult to use current estimates of the crime-control effectiveness of prison, the costs of crime to victims and nonvictims, and the costs of prison to show that we overshot the mark sometime in the early 1990s. Enormous cutbacks—reductions of 50% or more in the prison population—are not difficult to justify and would probably save the U.S. public billions of dollars each year.[1] Certainly, there is little economic justification for continuing to build.

1. The elasticity of crime rates with respect to prison rates η is defined as $(\Delta C/C) / (\Delta P/P)$, where C is the number of crimes reported per 1,000 population and P is the number of prisoners per 1,000 population. Then, the number of crimes reduced by putting one additional inmate in prison is equal to $\Delta C = \eta C/P$. We must adjust for nonreporting by dividing C by the reporting rate; in 2005, this rate was about .57 for violent crimes and about .40 for property crimes nationwide (Catalano, 2006). Miller, Cohen, and Wiersema (1996) show that the average cost of a violent crime to the victim is $13,000, and the average cost of a property crime is $1,200 (both in year 2000 dollars). Thus, the average benefit of one additional prisoner is equal to $13000/.57\ \eta_v\ C_v + 1200/.40\ \eta_p\ C_p$. Donohue and Siegelman (1998) show that the average cost of an additional prisoner is about $36,000 per year (again in year 2000 dollars). If we define the optimum level P^* as that prison rate where the benefit-cost ratio is exactly 1.0, then it is not difficult to show that $P^* = .63\ \eta_v\ C_v + .08\ \eta_p\ C_p$. If Levitt's (1996) estimates of $\eta_v = -.38$ and $\eta_p = -.26$ are applied to reported crime and prison rates for 2005 for the 50 states, we find that the average value of $P^* = 1.69$. In 2005, the prison rate in the average state was 4.35. Thus, the average state should reduce its prison population by $1.69/4.35 - 1 \approx 58\%$. Optimal state-by-state reductions range from 4% (in Massachusetts) to 82% (in South Dakota). No state should increase its prison populations. If Western's (2006) considerably lower elasticities are used ($\eta_v = -.03$, $\eta_p = -.07$), then the average state should reduce its prison population by 93%, with a state-by-state range of 87% to 97%. These estimates do not include the benefits to nonvictims of reducing crime, so they should be taken with a grain of salt. Nevertheless, they show how thin the case for prison construction has become.

How did we ever get to this point? Why is the prison population so high? No shortage of explanations is available. Some researchers view the prison boom as a straightforward response to the increase in crime during the 1970s and 1980s (although this seems to conflict with increasing prison populations during the crime decrease of the 1990s). Others view prison expansion as a response to demands of an increasingly conservative electorate or as a "wedge" issue that imparts a partisan political advantage to those who champion it. Still others argue that prison expansion is a means of shoring up failing social institutions such as the family and the public schools, an attempt at regaining control in an increasingly chaotic society.

It is fairly easy to document *how* we got to this point. The criminal justice system is in some sense a simple machine, in which the number of prisoners is equal to crime rates, times arrest rates per crime, times incarceration rates per arrest, times sentences served. This process makes it possible to break down annual changes in prison admissions into their component parts, and it has produced some clear findings. Prison growth during the 1980s was primarily caused by increases in incarceration rates among convicted offenders (Langan, 1991); later increases were mostly caused by increases in drug arrest rates and sentences served (Blumstein and Beck, 1999; Sabol, Rosich, Kane, Kirk, and Dubin, 2002). Although such findings tell us how prison populations increased, they beg the more important question: Why did incarceration rates and sentences served increase? Why did we become more punitive?

Answering questions like these requires another approach. Most empirical tests of this kind are of the familiar form:

$$\text{PRISON} = \alpha + \boldsymbol{\beta} \boldsymbol{X} + \varepsilon$$

where PRISON is some transformation of the incarceration rate, $\boldsymbol{X}$ is a vector of predictor variables, and α and $\boldsymbol{\beta}$ are coefficients to be estimated. Table 1 shows the results associated with the three most recent and comprehensive studies. Some findings are consistent across studies: Prison populations seem to increase with the black population and the percentage of Republicans in the legislature and decrease as more is spent on welfare and education. But the effects of crime rates, public opinion, poverty and unemployment, and even sentencing policy are inconclusive, at best. It is difficult to tease a consistent narrative from these findings.[2]

2. All coefficients shown in Table 1 (as elsewhere in this article) are expressed as elasticities—proportionate changes in the dependent variable (here, prison populations per capita) associated with a 1% increase in each dependent variable. Note that some large elasticities are not significant, whereas some small elasticities are. This difference occurs because the scale of the variables varies considerably. For example, pct republicans in legislature has a standard deviation within states equal to 18.222 for the period covered by the Jacobs and Carmichael (2001) study, whereas pct religious fundamentalists (as originally expressed, the logarithm of the percentage of the population that

Table 1. Predictors of prison populations: Previous findings

	Greenberg & West (2001)	Jacobs & Carmichael (2001)	Smith (2004)
Economy			
Unemployment rate	**.193**	.056	.110
Poverty rate	—	—	–.119
Underclass Threat			
Pct black	**.445**	**.080**	**.242**
Pct Hispanic	**–.049**	**.039**	—
Income inequality	.527	—	.055
Pct urban	–.018	—	—
Institutional Failure			
Divorce rate	—	—	.065
Public Opinion			
Citizen conservatism index	**.617**	**.343**	.094
Pct religious fundamentalists	.076	**.090**	—
Partisan Politics			
Republican governor*	.002	—	.011
Pct Republicans in legislature	—	**.055**	**.408**
Electoral Cycle			
Gubernatorial election year*	—	—	**.081**
Presidential election year*	—	—	–.074
Crime			
Violent crime rate	**.316**	**.357**	.285
Property crime rate	.151	—	.030
Drug arrest rate	**.170**	—	—
Prison Crowding & Sentencing Policy			
Prison crowding court order*	–.005	—	—
Pct of population on probation	—	—	–.021
Determinate sentencing law*	**–.020**	**–.027**	–.029
Habitual offender law*	—	—	.010
Marijuana decriminalized*	—	—	–.021
State Resources			
State revenues per capita [–2]	**.197**	—	—
Real state spending on welfare	**–.311**	—	—
Pct GDP spent on education	—	—	**–.316**
Region			
South*	–.030	—	—
Earliest year included	1971	1971	1980
Latest year included	1991	1991	1995
NT, total cases controlled for	147	150	784
STATE effects	No	Yes	No
YEAR effects	Yes	Yes	No
autoregressive effects	Yes	No	Yes
Dependent variable	PRISON	log PRISON	PRISON
R^2	.866	.905	.980

Note. The coefficients for continuous variables are expressed as elasticities. The coefficients for dummy variables are expressed as a proportionate change in prison population associated with the presence of characteristics. Dummy variables are denoted by an asterisk. Statistically significant effects are shown in **bold**.

Part of the problem here is technical. All three of these studies used panel data, but two studies relied on waves 10 years apart, whereas the third used annual data. The studies controlled for state and year effects in different ways. All studies relied on a common base of theory and broke down classes of explanatory variables in similar ways, but each study used a different set of independent variables. All studies used the level of the prison population as the dependent variable (either the number of prisoners or the logarithm), but this technique is liable to produce spurious results if PRISON and its predictors are trending variables (Granger and Newbold, 1974). Although all these researchers made defensible choices, it is not hard to see how these choices may have affected the results.

A more interesting possibility stems not from differences among these studies but from what they all had in common: All studies attempted to connect prison populations to the economic, social, and political conditions prevailing at the time. For example, they measured the effects of Republican control of the state legislature in 1991 on the prison population in 1991. But the primary effect of Republican control (and other variables) may not have been immediate. The legislature may have authorized construction of a new prison, which could take years to complete.[3] Even an immediate influx of prisoners will have long-lasting effects if prison populations are slow to shift over time.[4]

belongs to a sect that believes in a literal translation of the Bible) has a within-state standard deviation of .130. This complicates the interpretation of the coefficients. Jacobs and Carmichael report coefficients of .28 (for republicans) and .09 (for fundamentalists), but this is misleading because a one-unit change in republicans is 18.222/.130 = 140 times as likely to happen in the average state as a one-unit change in fundamentalists. In addition, republicans is expressed in terms of percentages (0 to 100), whereas fundamentalists is expressed in terms of the logarithm of the proportion (0 to 1). The dependent variable also differs across studies. The point is that a large elasticity is not necessarily evidence of a large effect, and a small elasticity is not necessarily evidence of a small effect. In the Results section, an alternative method of analysis is used that measures the importance of each group of variables more directly.

3. In 1998, California estimated a time to completion of 42 months after a site was chosen and acquired (Little Hoover Commission, 1998). In the early 1990s, the Pennsylvania average was 5 years (Gauger and Pulitzer, 1991); in Texas, the estimate was “up to seven years,” which included site identification and acquisition (Texas Criminal Justice Policy Council, 1992:14).

4. Suppose $P_t = \phi P_{t-1} + \varepsilon_t$, where ϕ measures the extent to which prison populations from the previous period carry over to the next. Then ϕ is the proportion of a one-time-only jolt in population that will carry on to the next period; ϕ^2 will carry on two periods in the future, ϕ^3 three periods, and so on. If $\phi = .95$ (see footnote 10, below), then it will take 14 or 15 periods before the effect of the jolt is reduced by even 50%. As William Faulkner once wrote, “The past is never dead. It’s not even past” (*Requiem for a Nun,* Act 1, scene 3).

This fact suggests that, in part at least, previous studies looked in the wrong place for their correlates. Today's unemployment, partisan political control, and per capita crime rates cannot be expected to be accurate predictors of capital spending and incarceration decisions made 5 years ago. It is also possible that the short-term, dynamic effects of changes in unemployment, politics, and crime are different from the long-term, equilibrium effects. To improve the accuracy of our explanations, we need to consider timing and to separate short-term results from long-term outcomes.

The analysis detailed below considers the same social, economic, and political variables as those examined in previous econometric studies. It includes more independent variables and uses current best practices to define the dependent variable and the model. More important, however, it considers timing in two ways. First, it includes capital spending as an intervening variable. As shown below, capital spending decisions are predictable and largely respond as expected to changes in current conditions and (arguably) to expectations of future prison needs; prison populations, in turn, seem to depend on previous capital decisions. Second, the analysis includes both dynamic and equilibrium elements, and it shows that these two effects differ, for some variables at least. The result is a remarkably simple explanation for what caused the prison boom of the last 30 years: persistently increasing crime rates, sentencing policies that put more offenders behind bars and kept them there longer, and sufficient state revenues to pay for it all.

Data

Most previous econometric explanations of the prison boom rely on either the national time series (e.g., Beckett, 1997; Jacobs and Helms, 1996) or a cross section of states (Beckett and Western, 2001; Michalowski and Pearson, 1990). The usual concerns about these designs apply: At $T = 30$ or so, the national time series is too short to allow for the testing of many variables, and the critical decisions are made at the state level, not at the national level. The state cross section is slightly larger but allows only the modeling of differences across states rather than changes over time, and coefficients are biased unless all critical variables are included (which they cannot be). An alternative design, which relies on a panel of states over time, dramatically increases the sample size and allows for dynamic modeling over time; fixed state effects can be used to control for factors that do not change over time but cause state prison populations to differ, which reduces the omitted variable bias problem. This example is (more or less) the design used in the three most recent studies described in Table 1.[5] As described in the next section, one of the critical dependent variables is only available beginning in 1977, so this period does not cover the entire increase in nationwide incarceration rates, which began in the early 1970s. But it does cover 80% to 90% of it.[6]

Dependent Variables

Consistent with previous studies, let us measure the prison population as the jurisdictional population per 1,000 state residents. Thus, PRISON includes not only prisoners in state facilities (the custody population) but also convicted offenders doing state time in local jails, private correctional facilities, federal prisons, and facilities in other states.

Let us also measure P.CAPITAL, which is state spending on land and building acquisition and on new construction for the correctional system. These figures were taken from the Annual Survey of Governments conducted by the U.S. Bureau of the Census.[7] Not all of this spending is on prisons; for the period 1987–2005, roughly 10% of spending was for noninstitutional purposes, mostly office space for administrators as well as

5. Both Greenberg and West (2001) as well as Jacobs and Carmichael (2001) relied on a panel of U.S. states with only three time-series observations separated by 10 years (1970, 1980, and 1990). Jacobs and Carmichael relied on both fixed- and random-effects models, and they show that (for these data) the results are very similar. Smith (2004) examined annual state prison populations for 1980–1995 and reported results of a model relying (more or less) on annual differences, which often accomplishes the same result as a fixed-effects model. A fixed-effects model apparently produced very similar findings (Smith, 2004:932, footnote 5).

6. State prison rates bottomed out in 1972, when the rate of prisoners per 1,000 population was .934. By 2005, the rate had climbed to 4.882 per 1,000, which is an increase of 3.948 prisoners per 1,000 and 1.265 million prisoners. The increase between 1977 and 2005 was 3.603 prisoners per 1,000 population and 1.179 million prisoners. Thus, the 1977 to 2005 period accounts for 92% of the total rate increase, and 93% of the total prisoner increase. Results presented later suggest that capital decisions precede prison population increases by 3 to 5 years; nevertheless, the 1982–2005 period accounts for 80% of the rate increase and 84% of the prisoner increase.

7. Data were obtained from the Inter-University Consortium for Political and Social Research for the period 1977–1990 and from the U.S. Census Bureau website for 1991–2004.

probation and parole officers. Nevertheless, no breakdown is available for the 1977–1986 period, and 10% is sufficiently small that we can safely ignore it.[8] As usual, we adjust for inflation by using the gross domestic product (GDP) price deflator.[9]

Previous research has shown that the prison population is nonstationary; that is, it changes slowly over time, and the best predictor of prison in any given year is the value of prison for the previous year.[10] The problem here is that any other variable that drifts up or down in the same way may seems to be correlated with prisons, even though these two variables are not related. This result is especially likely for variables such as GDP or state spending that, like PRISON, trend over time.

This approach has implications for both the conduct of the research and our interpretation of the findings. From a technical viewpoint, consistent results can be obtained in two ways. One is cointegration: We may find that a long-run equilibrium exists between prison and one or more independent variables, such that the deviation between the expected equilibrium value of prisoners and the true value is stationary. For example, we might find that states tend to spend a fixed proportion of GDP on prisons. That is,

$$P_t = \beta \; GDP_t + \varepsilon_t \tag{1}$$

where β represents the proportion of GDP. Thus, prison rates and GDP each wander up and down in small increments over time; neither is stationary. If the residuals are also nonstationary, then the relationship between prisons and GDP may very well be spurious and caused only by chance. If, however, the residuals are stationary, then prison rates track GDP over time such that the deviation between the two never can be too large. In fact, it can be shown to be self-correcting; large deviations tend to be followed by smaller ones, as prison rates shift back into place (Engle and

8. An attempt was made to use the pattern of institutional and noninstitutional spending for the 1987–2005 period to predict the proportion of institutional spending for the 1977–1986 period. The best model relied on state fixed effects and a folded log of time. Although statistically significant, the predictions were not particularly accurate (R^2 = .423, $F(50,899)$ = 13.157). More to the point, the resulting variable was not as effective a predictor of subsequent prison capacity and population as was total correctional capital spending.

9. The more familiar consumer and producer price indices are highly correlated with the GDP price deflator, but these indices focus on consumer items and raw industrial materials rather than on construction costs. In practice, experimentation showed that the choice of deflator has no important effect on the results.

10. More technically, $\text{PRISON}_t = \Sigma_k \beta_k \text{PRISON}_{t-k}$, and $\Sigma_k \beta_k \approx 1.0$. In the long run, PRISON cannot be exactly unit root. Because we generally define it as a proportion of resident population, it is theoretically bounded by 0 and 1. For the period 1960–2005, however, it is very close (Spelman, 2008). Similar results are obtained for the state panel from 1977 to 2005; in particular, the average root is .946. Whether exactly unit root or not, the risk of spurious relationships is very high unless PRISON is differenced.

Granger, 1987). In this case, we could be sure that Equation 1 measures a true long-run equilibrium relationship and not a spurious one. If we cannot find evidence of cointegration, then we are on shaky ground unless we define the dependent variable, not as the number of prisoners in a given year, but as the change (or percentage change) in prisoners over the previous year. Relying on levels risks spurious results.

Independent Variables

Previous explanations for the prison boom can be roughly divided into five types: *social threat, politics, crime control, crowding*, and *sentencing policy*. More complete descriptions of each are provided elsewhere (e.g., Greenberg and West, 2001; Jacobs and Carmichael, 2001; Smith, 2004), and they are merely summarized here for completeness. A complete list of independent variables used, and their sources, is provided in Table 2.

Table 2. List of variables used

Dependent Variables	
PRISON	Prisoners under state jurisdiction per 1,000 resident population (National Prison Statistics, U.S. Bureau of Justice Statistics)
P.CAPITAL	Real state government spending per capita on capital outlays for corrections (Annual Survey of Governments [ASG], U.S. Census Bureau)
Independent Variables	
Social Threats	
The Economy	
GDP	Real state gross domestic product over previous year (Regional Economic Accounts [REA], U.S. Bureau of Economic Analysis)
WAGE	Average real wage over previous year (REA)
UNEMP	Unemployment rate, averaged over calendar year (Local Area Unemployment Statistics, U.S. Bureau of Labor Statistics)
POVERTY	Proportion of persons under poverty threshold (Current Population Reports [CPR], U.S. Census Bureau)

The Underclass	
DROPOUT	High-school graduates per 17-year-old resident, subtracted from 1 (Digest of Education Statistics [DES], National Center for Education Statistics)
UNWED	Proportion of all births to unwed mothers (National Vital Statistics Reports [NVSR], National Center for Health Statistics)
FOOD	Real state spending on U.S. Department of Agriculture Food Stamp program (REA)
BLACK	Black proportion of resident population (Annual Population Estimates [APE], U.S. Census Bureau)
HISPANIC	Spanish proportion of resident population, any race (APE)
Failing Institutions	
DIVORCE	Divorces per 1,000 resident population (NVSR)
ENROLLED	Proportion of 5–17-year-olds enrolled in public primary and secondary schools (DES)
POLCHANGE	Absolute value of change in Republican proportion in legislature, next year (Klarner, 2003, 2007)
MHPOP	Number of inpatients in mental hospitals per 1,000 resident population (Raphael, 2000; Uniform Reporting System, Center for Mental Health Services, U.S. Substance Abuse and Mental Health Services Administration)
Public Opinion & Politics	
CONSERV	Citizen ideology index (0 = liberal, 100 = conservative) (Berry, Ringquist, Fording, and Hanson, 1998; Fording, 2007)
REPGOV	Republican governor (1 = yes, 0 = no) (Klarner, 2007)
RCONTROL	Republican control of legislature (1 = yes, 0 = no) (Klarner)
MCONTROL	Mixed control of legislature, statehouse (1 = yes, 0 = no) (Klarner)
PCTREP	Percent of legislators who are Republicans (Klarner)
Electoral Cycle	
GOVELEC	Gubernatorial election year (Klarner)
PRESELEC	Presidential election year
Crime	
VIOLENT	Reported violent crimes per 1,000 resident population (Uniform Crime Reports [UCR], Federal Bureau of Investigation)
PROPERTY	Reported property crimes per 1,000 resident population (UCR)
DRUGS	Drug possession and trafficking arrests per 1,000 resident population (UCR)

Prison Crowding	
JAIL	Number of convicted offenders doing time in local jails because of prison crowding, per 1,000 resident population (NPS)
OTHERINST	Number of convicted offenders doing time in institutions other than state prisons and local jails, per 1,000 resident population (NPS)
LITIGATION	State prison system facing litigation to reduce crowding (Levitt, 1996; *ACLU National Prison Project Journal*)
Sentencing Policy	
HABITUAL	Habitual offender ("three strikes") law (1 = yes, 0 = no) (Zimring et al., 2001)
TRUTH	"Truth in sentencing" law (1 = yes, 0 = no) (Sabol et al., 2002)
PRESUMP	Presumptive sentencing guidelines (1 = yes, 0 = no) (Frase, 2005)
MJDECRIM	Marijuana decriminalized (1 = yes, 0 = no) (MacCoun and Reuter, 2001)
Institutional Capacity	
T.SPENDING	Real state government spending per capita on operations and maintenance, capital outlays, and interest payments for all state functions (ASG)
MANDATORY	Real state government spending per capita on interest payments and on operations and maintenance for primary and secondary education, welfare, health, hospitals, and highways (ASG)
P.CAPMA	4-year moving average of P.CAPITAL, real capital spending on corrections, divided by PRISON lagged 1 year

The *social threat* argument states that society is likely to become more punitive when the social fabric is threatened. Threats include a sputtering economy, a growing underclass,[11] or apparent weaknesses in such institutions of formal social control as the family, public schools, government, or mental health system.

11. Previous prison studies have considered poverty and unemployment rates as a measure of the underclass. For most people, however, poverty and unemployment status is temporary, not chronic (Bane and Ellwood, 1986); most of the poor and unemployed are not members of the underclass (Ricketts and Sawhill, 1988). In this sample, a principal components analysis of all economic and underclass indicators produced two factors with eigenvalues greater than 1.0. The economic indicators loaded heavily on one, whereas the race and underclass measures loaded heavily on the other. When these categories are considered as scales, Cronbach's α is substantial for each group (economics α = .77; underclass α = .79); moving any variable from one group to another reduces values of α for each group. Measures of income inequality are associated with both concepts, but inequality measures were not statistically significant or were unavailable for 2005.

Politics provides a simpler explanation for the prison boom: A conservative electorate and Republican-elected representatives are more likely to support prison expansion than others. Timing may also be an issue. Prison populations may be higher or lower during gubernatorial or presidential election years, when elected officials may believe the public is paying greater attention.

Social and political threats extend well beyond the criminal justice system, but three alternative explanations lie closer to home. First, prison officials and policy makers may expand prisons in response to increasing *crime rates*. Because prison populations only increase when more convicted offenders enter the system than leave it, anything that increases the size of the incoming cohort can be expected to increase the prison population. All else equal, a 10% increase in crime should produce something like a 10% increase in the size of an incoming cohort.

Prison populations may also respond to *crowding*, which is measured by the number of convicted offenders who are serving time in local jails (Beck and Gilliard, 1995) or in private prisons, mental institutions, or prisons in other states. It may also respond to federal litigation to reduce overcrowding (Levitt, 1996). Crowding can be expected to decrease prison populations in the short run but increase prison capital spending, which makes larger populations possible in the long run.

Finally, intake and release choices may be constrained by previous *sentencing policy* choices. "Three strikes" and "truth-in-sentencing" laws mandate long sentences for some classes of offenders, which presumably increases prison populations (Turner, Greenwood, Chen, and Fain, 1999). Presumptive sentencing and decriminalization of minor drug offenses may reduce prison growth (Marvell, 1995).

All the independent variables described thus far measure objectives—variables that the state could reasonably hope to affect through prison expansion. Another class of variables measures constraints. No matter how appealing the objective, a state will find it difficult to build and operate more prisons if it has no money. Unfortunately, the simplest measure of financial capacity—state revenues—may be misleading by itself. Particularly in recent years, federal requirements and previously enacted statutes have mandated large expenditures in primary and secondary education, welfare, health care, and highways, all of which may squeeze out the correctional system; debt service is another such category of mandated expenditures.[12] We can expect that total revenues will be positively associated with prison populations and capital spending, but that mandated expenditures will be negatively associated.

These suggestions do not by any means exhaust the possible explanations. For example, some researchers view prisons as a form of economic development, particularly if the potential sites of new prisons are in depressed rural areas (Cherry and Kunce, 2001; King, Mauer, and Huling, 2004). Because prisons are large-scale construction projects, they may be helpful in smoothing over temporary declines in demand for housing and commercial construction (Burns and Grebler, 1984). Criminologists have been arguing for years that crime control and overcrowding can be dealt with by increasing reliance on community corrections, so the number of offenders on, for example, intensive supervision probation or house arrest might be an indicator of how a state has chosen to deal with a particular threat. Nevertheless, the five categories described above cover the most frequently cited (and very likely the most important) explanations.

A Model of Prison Population Choice

To parse these competing explanations, let us proceed in four steps. First, we must determine the proper form of the prison equation. A simple equilibrium model of prison population, which is based on perceived social benefits and costs, fills the bill handily. Because external conditions change faster than prison populations, states approach this long-run equilibrium but never reach it. The second step is to account for disequilibrium through a partial adjustment process. We then consider the effects of prison capacity; increases in capacity may reduce the social costs of putting a particular number of offenders behind bars, but the capacity increases respond to different social, economic, and political conditions than prison populations. Finally, let us consider how best to estimate this model given the statistical characteristics of prison population data.

12. For example, the staff of the Michigan House of Representatives concluded that 98% of spending in primary and secondary education, welfare, health, and highways was required by preexisting entitlements or federal mandates. Only 52% of spending in all other functions was so mandated (Haag, 1999).

Prison Benefits and Costs

The incarceration of each convicted offender provides some benefit to society. Justice is done. Future crimes are prevented through incapacitation and through general and specific deterrence. More generally, the exertion of formal control over the forces of disorder may reassure the public that other threats to the social order will be handled. We can reasonably expect that these benefits should be increasing at a diminishing rate as the number of imprisoned offenders increases.[13] Thus, the marginal benefit of each new offender—the value of the next prison bed—should be decreasing with the number of prisoners.

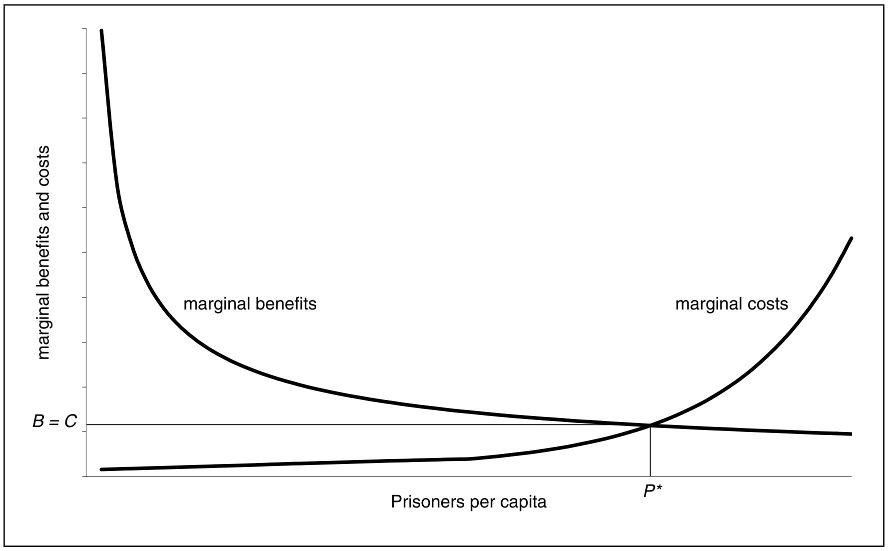
A simple model that fits this description is as follows:

$$MB = \alpha_b \, P^{\beta_b} \, \upsilon \qquad (2)$$

where MB is marginal social benefit per prisoner, P is the number of prisoners, parameters $\alpha_b > 0$ and $\beta_b < 0$, and υ is an error term with $E(\upsilon) = 1.0$ (see Figure 1). We can reasonably expect that α_b will be a function of current and past crime, other threats to the social order, and the degree to which politics are conservative and sentencing policies are stringent; the more of each of these, the higher the perceived benefit of incarcerating any given number of offenders. For now, let us assume that β_b is equal across states and over time and is determined by the variance of dangerousness among the population of convicted offenders.

13. The simplest explanation for this expectation is to consider the incapacitative effects of incarcerating successively larger populations. If we could only incarcerate one offender, then we would choose that offender for whom the social benefits of incarceration would be greatest—a terrorist or a serial killer perhaps. As our capacity grows, we work our way down the list of convicts, incarcerating progressively less dangerous offenders and tallying progressively smaller marginal benefits.

Figure 1. Equilibrium prison population depends on benefits and costs



Incarceration is also costly. Prisoners must be guarded, housed, fed, and clothed; the interest on prison construction bonds must be paid. Most prisoners had jobs in the legitimate economy before incarceration and their income will be lost; welfare payments to their dependents will increase to make up the difference. More generally, the removal of young men (particularly young black men) reduces the ability of all residents of urban neighborhoods to break the cycle of poverty and to achieve financial independence (Western, 2006).

These costs should be increasing but at an increasing rate with the number of prisoners. The simplest explanation for this expectation focuses on the financial costs. The first offenders imprisoned would be sent to those prisons that are the cheapest to operate and maintain; subsequent prisoners would be sent to more costly facilities, as we work our way up to the limits of current capacity. As we fill prisons beyond capacity or farm prisoners out to other states or institutions, we incur both higher financial costs (increased transportation costs, profit, an inconvenience premium) and higher inchoate political and administrative costs (these prisoners remain our responsibility, but we cannot control and take care of them directly). Thus, the marginal cost of each offender—the cost of the next prison bed—should be increasing with the number of prisoners:

$$MC = \alpha_c P^{\beta c} \nu \qquad (3)$$

Here, MC is marginal cost, parameters $\alpha_c > 0$ and $\beta_c > 0$, and $E(\nu) = 1.0$ (see Figure 1). We can expect that α_c will decrease with available capacity (more capacity reduces costs by decreasing crowding and allowing more offenders to be housed within state institutions); it will increase with the number of offenders housed in other institutions (more costly than the state's own prisons). Marginal costs should also depend on opportunity costs: If state revenues are high this year, then we may be giving up little in terms of schools, highways, and hospitals by incarcerating a lot of offenders (opportunity costs are low). If money is short, then the same level of incarceration means giving up a lot of these things, which effectively increases the cost. Again, let us assume for simplicity that β_c is the same for all states and years.

Social benefits and costs are difficult to measure, but we can reasonably assume that the best balance will be given by that point where the marginal benefit and cost curves meet (P^* in Figure 1). At this point, the social benefit of an additional prison bed will be exactly the same as the social cost, and net social benefits are maximized. That is,

$$\alpha_b P^{\beta b} \upsilon = \alpha_c P^{\beta c} \nu \tag{4}$$

which can be restated as follows:

$$\log P = \beta (\log \alpha_b - \log \alpha_c + \log \upsilon - \log \nu) \tag{5}$$

where $\beta = (\beta_c - \beta_b)^{-1}$, which is a constant greater than zero. If each log α is a linear combination of variables, then

$$\log P = \beta (\alpha_0 + \Sigma_i B_i \log X_i - \Sigma_j B_j \log Z_j + u) \tag{6}$$

where X_i are predictors of benefits and B_i are coefficients to be estimated, Z_j are predictors of costs and B_j are coefficients, and $u = \log \upsilon - \log \nu$ with $E(u) = 0$. Although we cannot recover the slopes of the benefit and cost functions from this reduced form, we can estimate the relative effects of the independent variables. Note, incidentally, that this form keeps the signs of all coefficients in the expected directions: More capacity and state resources reduce costs in the supply function and thus have positive coefficients in Equation 6. The greater use of county jails and other institutions as well as higher levels of need for other state functions increase costs and lead to negative coefficients in Equation 6. Let us simplify the following equations by referring to all predictor variables as $\boldsymbol{X}$.

Role of Disequilibrium

Equation 6 gives the long-run values of the relationship between P and its predictors. But prison populations are slow to change. The size of incoming cohorts and the sentences assigned depend on state sentencing mandates and on the behavior of judges and juries distributed throughout the state; prison officials have limited authority to release current convicts before their sentences are completed. In general, we will not achieve the desired equilibrium value in any given year. If the left-hand side of Equation 6 is considered to be the desired value log P^*, then we may reasonably expect that movements over the previous year should close the gap between log P^* and the previous empirical value. That is,

$$\log P_t - \log P_{t-1} = \gamma(\log P_t^* - \log P_{t-1}) + v_t \qquad (7)$$

where γ represents the speed of adjustment. If $\gamma = 1$, adjustment is immediate; if $\gamma = 0$, there is no adjustment, log P_i is a random walk, and the system does not seek long-run equilibrium at all. Substituting Equation 6 for log P_t^* and rearranging terms yields

$$\log P_t = \gamma\beta\alpha + (1 - \gamma) \log P_{t-1} + \gamma\beta\boldsymbol{B} \log \boldsymbol{X} + \varepsilon_t \qquad (8)$$

where $\varepsilon_t = \gamma\beta u_t + v_t$. This equation is an autoregressive distributed lag model. Although β cannot be determined, we can at least recover the $\beta\boldsymbol{B}$ by dividing the empirically derived coefficients by γ.

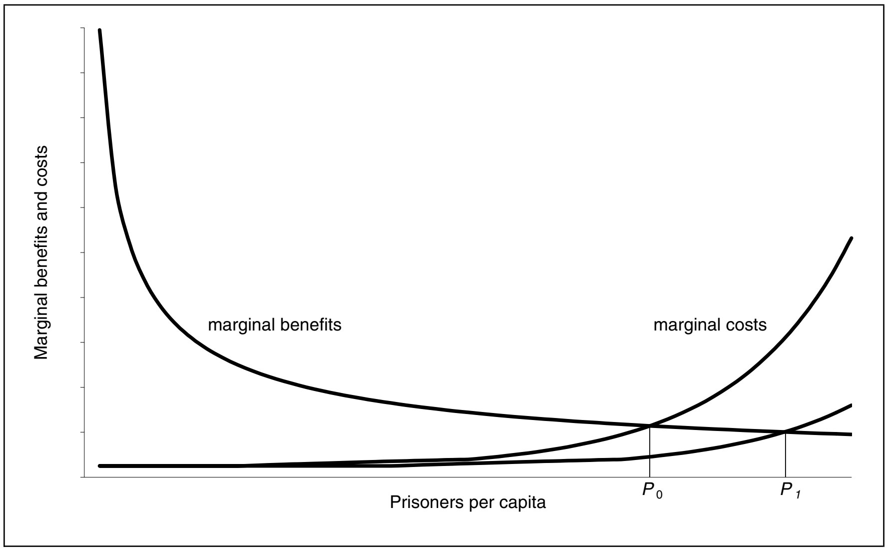
Prison Capital Stock

The model thus far assumes that benefits and costs are independent and that the $\boldsymbol{X}$ are exogenous. This assumption is not true, but it is probably close in most respects. States can take action to reduce social threats: subsidizing economic development and job training, or reforming the welfare system to discourage divorce, for example. They can take action to reduce crime in ways that have nothing to do with prisons: subsidizing increases in local police hiring, improving officer training, or increasing funds for dropout prevention programs. If these programs were both effective and developed in reaction to incarceration levels, we would need to treat some of these variables as endogenous. Nevertheless, given that the crime-control debate has focused on incarceration for the last 30 years, considering them to be exogenous seems a reasonable simplification.[14]

However, we cannot simplify away changes in the prison capital stock. If the supply of state prisons is insufficient by itself to meet demand—specifically, if the cost and benefit curves meet at a point after marginal costs have begun to increase rapidly—then it may be beneficial to increase supply (point P_0 in Figure 2). The increase in supply would reduce marginal costs, which allows for more prisoners at equilibrium (point P_1).

14. Real state expenditures per capita on the criminal justice system, which include prisons, police, probation, parole, and other sanctions, increased by 117% over this period. More than 67% of the increase was in prisons, which makes it by far the biggest state response to the crime problem.

Figure 2. Value of capacity increase depends on cost function



Prison capacity is notoriously difficult to estimate accurately.[15] We can, however, estimate changes in capacity through the proxy of capital spending on the prison system. Some of this spending will no doubt go into deferred maintenance or prison improvements that neither increase beds nor reduce operating costs, but it makes sense to believe that most of it

15. Federal standards allow states to report prison capacity in any of three ways: (1) *rated capacity*, the expert judgment of an experienced prison official; (2) *design capacity,* the number of inmates the architect planned for in designing the facility; and (3) *operational capacity,* the number of inmates the prison can accommodate while maintaining programs and services at expected levels. The last definition is obviously considerably looser, and operational capacity estimates are generally higher than the other two. Some states report all three figures, whereas some report only one or two; states sometimes change definitions from year to year (for example, Beck and Gilliard, 1995). Experimentation with both capacity and capital spending variables showed that capital spending was consistently a better predictor of prison populations and custody populations than capacity; adding capital spending to a model that already includes capacity reduces the standard error of the residuals and (usually) the Akaike and Schwarz criteria. Adding capacity to a model that includes capital spending decreases none of these. Combined with the results described in the text, the most reasonable conclusion is that capital spending is a more accurate measure of prison capacity than capacity itself.

will go into prison expansion. It may take several years before a project is complete, but eventually most capital spending will shift the cost curve outward, which reduces marginal costs and allows for larger prison populations.

Costs, then, depend on the stock of prison beds available in year 0, plus changes in that stock associated with lagged values of prison capital spending. The number of beds in year 0 is a constant for each state and will be captured by each state's intercept term (that is, the STATE fixed effect). Changes can be measured as the summation of the lagged values of capital spending, $\sum_{t=0}^{t} \boldsymbol{K}_t$, where K_t represents annual prison capital spending in year t. Like prison populations, we can reasonably expect that prison capital spending will respond to values of the other $\boldsymbol{X}$ variables, of the form $K_t = \alpha_0 + \boldsymbol{\alpha}_1 \boldsymbol{X}_\mathrm{t} + u_t$.

Nevertheless, capital spending in any given year probably depends on expectations of future prison benefits and costs, as measured by the expected values of future predictor variables. Suppose our current expectations for future values of X_t (call them X_t*) depend on a simple learning process in which previous expectations are compared with previous actual values. If the actual values are higher than expected, then we adjust future expectations upward; if lower, then we adjust downward. That is,

$$X_t^* - X_{t-1}^* = \lambda\ (X_{t-1} - X_{t-1}^*) \tag{9}$$

or, equivalently, X_t* is a weighted average of previous actual values and previous expectations:

$$X_t^* = \lambda\ X_{t-1} + (1 - \lambda)\ X_{t-1}^* \tag{10}$$

If $K_t = \alpha_0 + \alpha_1\ X_{t-1}^* + u_t$, then it is not difficult to show that

$$K_t^* = \lambda\alpha_0 + \lambda\alpha_1\ X_t + (1 - \lambda)\ K_{t-1} + v_t \tag{11}$$

where $v_t = u_t - (1 - \lambda)\ u_{t-1}$. This equation is the standard adaptive expectations model (Cagan, 1956).

An additional complication is present here, however. Many prison expansion projects take more than 1 year to complete. Our intention is to spend the money necessary to complete the project, K_t*, but because of the complexity of the building process, we can only spend some proportion of that amount in any given year. If that proportion is a constant δ, then it is not difficult to show (Waud, 1966) that Equation 11 can be modified to account for these partial adjustments as follows:

$$K_t = \lambda\delta\alpha_0 + \lambda\delta\alpha_1\ X_t + (2 - \lambda - \delta)\ K_{t-1} - (1 - \lambda)(1 - \delta)\ K_{t-2} + v_t \tag{12}$$

This form is not entirely accurate. Projects are completed in finite time, and spending is likely to be more or less uniform over the project period, not exponentially decreasing. So we should not read too much into coefficients λ and δ (and cannot in any event tell which is which). Nevertheless, Equation 12 accounts for both mechanical “stickiness” caused by multi-year projects and for adaptive expectations of future need. In practice, we take the logs of K and X to stabilize the variance of v and ensure comparability with Equation 6.[16]

We can expect that the critical predictors of capital spending would be those associated with higher marginal costs: the number of state prisoners in county jails and other institutions, or litigation to decrease crowding in current facilities. A Republican-controlled legislature and a conservative electorate may be more willing to spend limited funds on prisons than on other state functions, which reduces opportunity costs. Capital spending should also depend on funding availability (measured by state revenues net of mandatory expenditures) as well as on expected future increases in prison demand (signaled by increases in crime or social threats or by strict sentencing policies that reduce the state’s ability to control prison populations on a short-term basis). Note that capital spending, like the prison population, could also be put in a benefit-cost form; because we are unable to measure benefits and costs, we are limited to the reduced form of Equation 12.

Thus, we need two equations: a capital spending equation and a more general prison population equation that includes accumulated lags of capital spending.[17] The values of predictor variables can affect prison populations directly (through the prison equation) or indirectly (by affecting capital spending, which affects future prison populations through the prison equation). To estimate the full effect of each predictor variable, we need to take both the direct and the indirect effects into account.

16. Use of logs is also likely to improve validity by decreasing the importance of outliers. A purist might reasonably argue that we should be adding values of K_t, not multiplying them together. This function makes the model much more complicated, however.

17. Capital spending has a significant effect on downstream prison populations for up to 7 years, although the effects diminish as the lag increases. Experimentation showed that a 4-year moving average (that is, the current value plus three lags) produced slightly higher and more significant coefficients than longer lags, and it also allowed for a larger sample size and smaller standard errors on the coefficients for the other variables. The length of the moving average does not have a substantial effect on the results, however.

If capital spending levels affect prison populations on a dollar-for-dollar basis, but we measure changes in population on a proportionate change basis, then we will need to account for steadily increasing prison populations in our definition of capital spending in the prison equation. (If we do not, we are forced to assume that a dollar in capital spending has about four times the effect on prison populations in 2005 as it did in 1977.) The simplest way to do this is to divide spending levels by the previous period’s prison population. Thus, when capital spending enters as an independent variable in the PRISON equation, it can be considered as a 4-year moving average of capital spending per inmate.

Estimation

Estimating Equation 8 by ordinary least squares (OLS) is problematic, because log P_t is highly serially correlated and near unit root. As a result, the standard errors of the empirically estimated coefficients for the independent variables are likely to be greater than they seem, and spurious conclusions may be drawn from the results. If we can reasonably restrict the number of lags in the X variables to one (that is, no X_{t-2} is an effective predictor of P_t), then we can instead rely on the error correction form of Equation 8:

$$\Delta\log P_{it} = \beta\alpha_i + \beta\boldsymbol{B}_t\, \Delta\log \boldsymbol{X}_{\mathrm{it}} - \delta\ (\log P_{t-1} - \beta\boldsymbol{B}_{t-1} \log \boldsymbol{X}_{\mathrm{t-1}}) + \varepsilon_t \qquad (13)$$

The most efficient way to estimate Equation 13 is to estimate an equation of the form:

$$\Delta\log P_{it} = \delta\beta\alpha_i + \delta\beta\ \boldsymbol{B}_t\, \Delta\log \boldsymbol{X}_{it} - \delta \log P_{t-1} + \delta\beta\boldsymbol{B}_{t-1} \log \boldsymbol{X}_{t-1} + \varepsilon_t \qquad (14)$$

which is sometimes called the "single equation" form of the error correction equation. In Equation 14, $\beta\boldsymbol{B}_t$ represents the short-run effect of changes in predictor variables on changes in prison populations, $\beta\boldsymbol{B}_{t-1}$ represents the long-run equilibrium effect, and δ remains the speed of partial adjustment. The total effect of a permanent change in independent variable X is equal to $\beta\boldsymbol{B}_t$ (the coefficient on ΔX_t) + $\beta\boldsymbol{B}_{t-1}$ (the coefficient on X_{t-1} divided by δ). Note, incidentally, that capital spending enters the dynamic portion of Equation 14 as a series of recent annual lags: $\Sigma_k\ \beta_k \log K_{t-k}$ (or perhaps as a moving average of annual lags). However, it enters the error correction portion as the sum of all lags from t = 0 to t.

The long-run equilibrium effects are only accurate if the error correction term $P_{t-1} - \beta\boldsymbol{B}_{t-1}\ \boldsymbol{X}_{t-1}$ is stationary. A complete approach to this problem would require use of the Johansen procedure, which involves a search among P and the $\boldsymbol{X}$ for one or more cointegration equations. In fact, several cointegration equations may exist, each of which requires a separate error correction term and adjustment value δ. Also, no stationary equations may be available, which suggests that there is no long-run equilibrium at all and P_t depends only on short-run changes in the predictors. Given the number of predictors and states (and the relatively short time series available in this data set), it makes more sense to assume a single common equilibrium equation and to test the error correction term for stationarity.[18] If we can reject the null hypothesis of unit roots for the error correction term, then we may reasonably conclude that at least one long-run equilibrium equation exists and that $\beta \boldsymbol{B}_{t-1} \boldsymbol{X}_{t-1}$ is a reasonable estimate of it.

Approach to Inference

This tortuous procedure is likely to lead to a messy result. Each of the 30-odd independent variables enters the prison population twice (once in the dynamic portion and once in the error correction or long-run equilibrium portion). Each independent variable also enters the capital spending equation. With more than 100 coefficients to be estimated, some are bound to be Type II errors (insignificant predictors that seem to be significant, just by chance). We can simplify inference and reduce the likelihood of Type II errors by focusing not on individual variables but on classes of competing explanations. Thus, GDP, wage rates, unemployment rates, and poverty rates can all be considered as indicators of a single social threat (a sputtering economy) rather than as separate variables.

To avoid omitted variable bias, let us use a process of backward elimination. All available variables are added to the model; then each category is removed to test the size and significance of the reduction in predictive accuracy. Categories that reduce accuracy the most when removed are presumably the most important predictors. By successively eliminating the least important categories, we can also test the extent to which coefficients for the remaining variables are affected by the stock of variables in the equation, which is a simplified extreme bounds analysis (Leamer, 1983).

Results

Prison Capital Spending Depends on Politics and Resources

Table 3 shows the results of regressions that use all independent variables to predict prison capital spending (that is, estimates of Equation 12). Although the basic model assumes that capital spending will adapt to expected future requirements and only partially adjust from year to year, it is simpler to report short-term effects (that is, $\lambda\delta\alpha$s, not αs). As described, it is not possible to distinguish between expectations effects

18. Pedroni (2001) has shown that cointegration in a single-equation error correction model can be tested through a simple unit-root test of the residuals of the error correction term, which is applied to each state in the sample. One appropriate test in the panel context is the cross-sectionally augmented Dickey-Fuller test (Pesaran, 2005), which accounts for spatial autocorrelation across states.

(given by λ) and adjustment effects (given by δ), but it is possible to determine the product of the two.[19] The estimated value of λδ is relatively precise and varies little among all the specifications considered. Because the long-term effects are the (reported) short-term effects divided by λδ, the long-term effects of a permanent change in any of these variables should be a large multiple (approximately 7.5) of the effects shown.

Table 3. Predictors of P.CAPITAL

	OLS		Specification Limits	
	Coefficient	*Standard error*	*Minimum*	*Maximum*
Economic Threat				
GDP [–1]	**1.534**	.871	1.504	2.073
WAGES [–1]	3.921	2.605	2.876	3.903
UNEMP [–1]	**.539**	.256	.478	.627
POVERTY [–1]	.099	.133	.079	.130
Underclass Threat				
DROPOUT [–1]	**.452**	.271	.397	.486
UNWED [–1]	–.083	.501	–.311	.106
FOOD [–1]	.464	.292	.419	.550
BLACK [–1]	–.098	.259	.101	–.035
HISPANIC [–1]	—	—	—	—
Institutional Threat				
DIVORCE [–1]	–.018	.276	–.071	.010
ENROLLED [–1]	1.994	1.680	1.507	2.727
POLCHANGE	–.722	.861	–.777	–.603
MHPOP [–1]	—	—	—	—
Public Opinion & Politics				
CONSERV	**.368**	.131	.368	.444
REPGOV*	–.006	.056	–.029	.004
RCONTROL*	**.325**	.136	.315	.339
MCONTROL*	**.156**	.082	.136	.163
PCTREP	.020	.085	.005	.043

19. In Equation 11, only the coefficients on K_{t-1} and K_{t-2} provide information on λ and δ. Specifically, if α_1 is the coefficient on K_{t-1} and α_2 the coefficient on K_{t-2}, then $\alpha_1 = (2 - \lambda - \delta)$ and $\alpha_2 = -(1 - \lambda)(1 - \delta) = \lambda\delta - \lambda - \delta - 1$. Solving both equations for λ and setting them equal to one another leads, eventually, to $0 = \delta^2 + (\alpha_1 - 4)\,\delta + (1 - \alpha_1 - \alpha_2)$, which is a simple quadratic function. Standard errors for λδ were estimated through a Monte Carlo procedure.

	OLS		Specification Limits	
	Coefficient	*Standard error*	*Minimum*	*Maximum*
Electoral Cycle				
GOVELEC*	–.037	.071	–.055	–.021
PRESELEC*	**.384**	.162	.312	.387
CRIME				
VIOLENT [–1]	**.603**	.283	.534	.645
PROPERTY [–1]	–.752	.684	–.845	–.622
DRUGS [–1]	.041	.060	.036	.047
Prison Crowding Conditions				
JAIL	**.044**	.021	.038	.050
OTHERINST [–1]	**.212**	.119	.150	.262
LITIGATION*	.173	.107	.154	.202
Sentencing Policy				
HABITUAL*	**–.180**	.092	–.234	–.137
TRUTH*	–.015	.089	–.028	.003
PRESUMP*	.160	.109	.146	.184
MJDECRIM*	**–.241**	.090	–.253	–.208
Current Spending				
T.SPENDING	**5.006**	1.211	4.973	6.286
MANDATORY	**–3.040**	.850	–3.597	–3.015
Adjustment & Expectations				
P.CAPITAL [–1]	.635	.034	.629	.660
P.CAPITAL [–2]	–.067	.030	–.087	–.065
Lagged effects ($\lambda\delta$)	.134	.018	.130	.143
STATE Fixed Effects				
F (49, 1192)	2.253	<.001		
YEAR Fixed Effects				
χ^2 (24)	67.744	<.001		
R^2	.614	—	—	—
Standard error of residuals	.723	—	—	—

Notes. This table shows coefficients and heteroskedasticity- and autocorrelation-consistent standard errors. All continuous variables are logged and differenced; dummy variables are denoted by an asterisk. The coefficients for continuous variables are elasticities; the coefficients for dummy variables show proportionate changes in corrections capital spending associated with the presence of characteristic. Statistically significant coefficients ($p < .05$, one-tailed test) are shown in **bold**. Specification limits show the minimum and maximum coefficients obtained over 18 regressions with one or more categories deleted. All models use 1,300 observations and include fixed YEAR and STATE effects. Cases are not weighted.

Predictors of prison capital spending are generally consistent with expectations. States spend more when violent crime rates increase (but not property crimes or drug arrests) and when many prisoners are held in county jails and other institutions.[20] Capital spending is lower for states that have decriminalized marijuana and (rather perversely) those that have adopted habitual offender statutes. Presumptive sentencing and truth-in-sentencing policies seem to have little effect on capital spending, however. Some evidence suggests, then, that legislatures respond to an apparent need for prison expansion by increasing capital spending.

Yet other predictors may be more important. Capital spending increases when the electorate is conservative and the legislature is controlled by Republicans (and, to a lesser extent, when one party controls each house). Spending also increases more during presidential election years, when the public presumably is paying more attention. By far, the largest effects are associated with resources. Capital spending increases dramatically with total state spending, and it decreases as more spending is required for education, welfare, and other mandatory functions.

Little evidence is available to suggest that other social threats matter much. None of the underclass and institutional threat variables is significant, and the significant increase in spending associated with higher unemployment rates is offset by an increase when GDP increases. The long-term expectations of resource availability may be more at issue here than responding to an economic threat. As shown in the last two columns, none of these coefficients changed much as the specification changed, which suggests that they are also robust with respect to different specifications.[21]

20. Although crime rates and prison populations seem to be simultaneously determined (see Levitt, 1996; Spelman, 2005; and footnote 24), there is no reason to believe that crime rates and capital spending are simultaneous. Equation 12 and the results shown in Table 3 assume that legislative decisions to increase prison capacity are based on the previous year's changes in crime rates, not on the current year's; prison capital spending only translates into capacity increases after a lag. When instruments for violent and property crime rate changes are used in place of the actual changes, the coefficients do not change appreciably.

21. Bounds are based on the following sets of regressions. First, each category of variables was eliminated, one at a time, and the coefficients of the remaining variables were measured. Second, a backward elimination procedure was used. Starting with the complete specification, categories of variables were eliminated in order of the likelihood that any variable in the category was significantly related to capital spending (that is, the $p(F)$ of the category as a whole). The backward procedure ended when elimination of the next category would increase the value of the (very stringent) Bayesian information criterion. Note that this results in the elimination of some statistically significant categories. The idea here is that if a category absolutely must be included in the specification, it would be misleading to measure the effects of removing that category

As noted, prison capital spending seems to respond to expectations of future variables (λ seems to be greater than zero), but expectations of future need for prisons do not seem to be particularly important here. The key categories are politics and state spending—not crime, crowding, or sentencing policy. The key expectations, then, are that more prisons will remain good politics, and that the resources needed to pay for them will continue to be available. This finding calls into question the claim that prison capacity increases in response to downstream need for prisons. More on this theory is discussed below.

Long-Run Prison Populations Depend Mostly on Crime and Policy

The prison population model described in this article only holds if a long-term equilibrium relationship exists between PRISON and some combination of the independent variables. This theory is only true if the residuals of the error correction term in Equation 14 are stationary with roots less than 1.0. When Equation 14 is estimated from these data and cross-sectionally augmented Dickey-Fuller tests are applied to these residuals from the 50 states, the average root is .71—the result is moderately high but is significantly less than 1.0.[22] Because we can reject the unit-root hypothesis, we may reasonably conclude that the error correction equation represents a long-term equilibrium. Equation 14 thus provides information on both the short-term and long-term effects of each of the independent variables.

on the other variables. Note that the bounds do not measure the effects of changing the variables within categories. Accordingly, more attention should be given to the importance of each category as a whole rather than to the coefficient of any particular variable.

22. More specifically, cross-sectionally augmented Dickey-Fuller tests (Pesaran, 2005) were conducted on the residuals of the error correction term in each state. The average value of the test statistic, $\tau = -1.765$, with a standard deviation across states of .735. The null hypothesis of unit root could only be rejected in 2 of the 50 states. Nevertheless, the unit-root test results are homogeneous. That is, they are consistent with the hypothesis that the root for all residuals is the same; $\chi^2_{50} = 41.232$, $p(\chi^2) = .807$. Thus, a panel test (Im, Pesaran, and Shin, 2003) is appropriate. The panel test result, $Z = -3.423$, $p(Z) < .001$. The residuals are significantly different from unit root, and the error correction term may be construed as an estimate of the long-run equilibrium relationship between the independent variables and PRISON.

Table 4 shows the estimates. The first two columns show the short-term effects ($\beta\boldsymbol{B}_t$, the coefficients on the ΔX_t variables) and their standard errors. Columns 3 and 4 show the long-term effects ($\beta\boldsymbol{B}_{t-1}$, the coefficients on the X_{t-1} variables divided by $-\delta$, the coefficient on PRISON$_{t-1}$) and their standard errors. Columns 5 and 6 show the total effects ($\beta\boldsymbol{B}_t + \beta\boldsymbol{B}_{t-1}$) and standard errors. As it happens, the distinction between short-term and long-term effects is important for some of these variables.[23]

Some variables have a significant impact on PRISON in the short run but not in the long run. Previous capital spending (our proxy for increased prison capacity) and current state spending (net of spending on mandatory functions) clearly affect annual changes in prison populations in the expected (positive) direction. However, they do not seem to have long-term effects. Thus, capacity and funding affect the timing of prison population increases: Populations only increase when the beds and the money are available. But sooner or later, they will be available.

Other variables affect prison populations in both the short run and the long run. Crime rates are a clear example. In the short run, prison populations seem to increase as drug arrests increase and property crimes decrease. This result is unexpected, but it can be explained by the countervailing effects of prison on crime rates. We can be sure that prisons reduce crime, at least to some extent (cf. Levitt, 1996; Western, 2006). If increases in crime also lead to increased prison populations, the apparent short-run effects of violent and property crime rates as well as (perhaps) drug arrests would be a mixture of these two effects. The figures shown in Table 4 are consistent with this hypothesis.[24] If the effects of prison on crime are

23. When the equation is restricted to short-term effects only, eliminating the error correction term, the short-term effects are very similar to those shown in Table 4. Only for one variable does the restricted equation provide an estimate of short-term effects that differs by as much as one standard error from the short-term estimate provided by the unrestricted equation. That variable, HISPANIC, is marginally significant in the restricted equation ($\beta\boldsymbol{B}_{\mathrm{t}} = -.108$, one-tailed $p \approx .042$) and not significant in the error correction model.

24. The best way to account for simultaneity is to replace the simultaneous independent variables with instruments. In this context, these instruments are predictions of what violent and property crime rates would have been if prison populations had not increased (and presumably reduced crime rates through incapacitation and deterrence). To obtain these instruments, we need to find variables that predict crime rates but do not have any effect at all on prison populations and can be excluded from the PRISON equation.

Instrumental variable analyses are always controversial, but a simple analysis suggests that simultaneity may in fact be the cause of the unexpected short-run coefficients in Table 4. Crime depends not only on the number of motivated offenders but also on the number of criminal opportunities—easy and lucrative targets (Cohen and Felson, 1979; Hindelang, Gottfredson, and Garofalo, 1978; Sparks, 1981). It is not difficult to show that crime rates can be predicted by the number of people who run especially low risks

Table 4. Predictors of PRISON

	Immediate		Long-Run Equilibrium		Total Effects		Specification Limits	
	Coefficient	*Standard error*	*Coefficient*	*Standard error*	*Coefficient*	*Standard error*	*Minimum*	*Maximum*
Economic Threat								
GDP	–.024	.071	.165	.440	.140	.464	.083	.419
WAGES	**–.265**	.143	–.322	.623	–.588	.657	–1.640	–.438
UNEMP	.016	.020	.008	.162	.024	.177	–.011	.084
POVERTY	.003	.012	–.093	.114	–.096	.122	–.166	–.044
Underclass Threat								
DROPOUT	.002	.020	–.084	.165	–.083	.175	–.112	–.005
UNWED	–.028	.047	.044	.308	.015	.334	–.046	.080
FOOD	.000	.024	.087	.149	.088	.155	.047	.157
BLACK	.015	.024	.202	.207	.217	.221	.157	.292
HISPANIC	.021	.062	–.031	.113	–.010	.133	–.049	.062
Institutional Threat								
DIVORCE	.016	.021	.204	.184	.220	.192	.188	.273
ENROLLED	.097	.159	–.101	1.046	–.004	1.103	–.426	.359
POLCHANGE	–.052	.056	.004	.747	–.048	.787	–.181	.045
MHPOP	.001	.001	.029	.021	.029	.022	.016	.062

	Immediate		Long-Run Equilibrium		Total Effects		Specification Limits	
	Coefficient	*Standard error*	*Coefficient*	*Standard error*	*Coefficient*	*Standard error*	*Minimum*	*Maximum*
Public Opinion & Politics								
CONSERV	–.002	.013	–.085	.124	–.087	.133	–.142	–.067
REPGOV*	.000	.005	–.015	.040	–.015	.041	–.034	–.003
RCONTROL*	–.001	.011	.094	.104	.092	.110	.080	.128
MCONTROL*	–.002	.006	–.010	.061	–.012	.065	–.015	.008
PCTREP	.036	.024	–.002	.083	.034	.091	–.036	.110
Electoral Cycle								
GOVELEC*	.006	.004	.062	.050	.069	.052	.067	.080
PRESELEC*	.004	.008	.032	.137	.037	.144	–.022	.078
Crime								
VIOLENT	.024	.028	**.355**	.134	**.380**	.190	.380	.535
PROPERTY	**–.098**	.043	.378	.269	.280	.379	.035	.280
DRUGS	**.009**	.005	**.065**	.036	**.075**	.038	.073	.125
Prison Crowding Conditions								
JAIL [–1]	.000	.002	.021	.015	.021	.015	.017	.025
OTHERINST [–1]	.018	.024	.080	.116	.098	.129	.078	.141
LITIGATION*	.030	.022	–.068	.131	–.098	.149	–.151	–.058
Sentencing Policy								
HABITUAL*	.010	.011	.064	.084	.074	.086	.061	.107
TRUTH*	.008	.009	**.119**	.066	**.127**	.070	.108	.135
PRESUMP*	**–.033**	.011	**–.227**	.073	**–.260**	.075	–.287	–.250
MJDECRIM*	**–.049**	.012	**–.188**	.111	**–.238**	.118	–.314	–.157
Previous Capital Spending								
P.CAPMA	**.009**	.003	.021	.061	.030	.059	–.008	.036

	Immediate		Long-Run Equilibrium		Total Effects		Specification Limits	
	Coefficient	*Standard error*	*Coefficient*	*Standard error*	*Coefficient*	*Standard error*	*Minimum*	*Maximum*
Current Spending								
T.SPENDING	**.250**	.082	–.172	.486	.078	.549	–.158	.381
MANDATORY	**–.116**	.070	.446	.480	.331	.519	.144	.621
Partial Adjustment								
PRISON [–1]	**–.120**	.019	—	—	—	—	–.121	–.105
Fixed effects								
STATE: F (49, 1160)	2.838	<.001	—	—	—	—	—	—
YEAR: χ^2 (23)	109.968	<.001	—	—	—	—	—	—
Long-Run Equilibrium								
F (33,1160)	2.538	<.001	—	—	—	—	—	—
R^2	.367	—	—	—	—	—	—	—
Standard error of residuals	.053	—	—	—	—	—	—	—

Notes. This table shows coefficients and heteroskedasticity- and autocorrelation-consistent standard errors. All continuous variables except P.CAPMA are logged and differenced; P.CAPMA is logged but not differenced. Dummy variables are denoted by an asterisk. The coefficients for continuous variables are elasticities; the coefficients for dummy variables show proportionate change in prison population associated with presence of characteristic. Statistically significant coefficients ($p < .05$, one-tailed test) are shown in **bold**. Specification limits show the minimum and maximum coefficients obtained over 19 regressions with one or more categories deleted. All models use 1,300 observations and include fixed YEAR and STATE effects. Cases are not weighted.

mostly short run in nature, however, we can expect these countervailing effects to be much less apparent in the long-run estimates. Indeed they are: In the long run, prison populations increase as violent and property crime rates as well as drug arrests increase (although only the violent and drug effects are statistically significant). The net result of the short-run and long-run effects is positive.

Sentencing policies also seem to have both immediate and long-run effects in the expected directions. Presumptive sentencing and marijuana decriminalization reduce prison populations in both the short run and the long run. Truth-in-sentencing laws have little immediate effect but a substantial long-run effect. This analysis makes sense: Truth-in-sentencing laws increase time served and reduce the number of offenders released in future years; the full effect would only be observed after prisoners sentenced under the old regime are replaced by those sentenced under the new law. Habitual offender laws seem to have little effect at all, perhaps because they only affect the sentences of a small number of offenders (Zimring, Hawkins, and Kamin, 2001).

(the proportion of the population who are less than 15 years old or older than 65) and especially high risks (military personnel, recent migrants to the state, tourists), and, for property crimes, the extent of social cohesion (proxied by the suicide rate). When these variables are used to predict crime rates, the increase in R^2 is highly significant (F for violent crimes is about 14, for property crimes about 12, both with $p < .001$), but these variables are not significant predictors of prison populations $F(7,1185) = .905$ in the capital spending equation and $F(7,1181) = .986$ in the prison equation, neither of which approaches statistical significance). When the instruments are used in place of actual violent and property crime rates, the short-run coefficient in the prison equation changes from .024 [.028] to .211 [.133] for violent crimes, and from –.098 [.043] to –.023 [.264] for property crimes. Neither of these coefficients is statistically significant, but they change in the expected direction and are reasonable in size. Using instruments for short-run crime effects also has very little effect on the short-run effects of drug arrests or on any of the long-run coefficients. We may reasonably conclude that violent crimes have a substantial, positive effect on prison populations in the long run and may also have a positive effect in the short run, and that property crime rates have essentially no short-run effect on prison populations, although they may have a substantial long-run effect. Thus, the negative coefficient for property crimes in Table 4 is probably caused by simultaneity, and not by any real effect of crime rates on prison populations.

Although Table 4 shows the natural variable estimates rather than the instrumental estimates, the analysis of Table 5 is based on the instruments. This result has the practical effect of reducing the apparent importance of crime rate changes. Using actual crime rates and not instruments leads to a higher change in R^2 when crime is eliminated from the equation. Thus, the finding that crime rates are among the most important predictors of future prison populations is robust with regard to any assumptions about simultaneity.

The most important results here may be the dogs that do not bark. A conservative electorate and a Republican legislature are more likely to increase capital spending and subsequent availability of prison space, but they do not seem to have any direct effect once capital spending has been taken into account. Crowding behaves much the same way: It affects populations only through capital spending. Prison populations, like capital spending, do not seem to respond at all to economic threats, underclass threats, or institutional threats.

The autoregressive term (the coefficient for PRISON_{t-1} in Table 4) is very low at –.120, which means that only a small fraction of the deviations from long-run equilibrium is corrected in any given year. If none of the independent variables changed at all, it would take 5 or 6 years for prison populations to move even 50% of the way toward the long-run equilibrium value, depending on the specification. It would take 12 to 15 years to move 80% of the way. This result is consistent with the limited response of prison populations to the crime decrease of the 1990s. The crime decrease was offset by increases in use of truth-in-sentencing laws, and the movement toward the lower, long-run equilibrium value has been very slow.

Crime and State Spending Are the Best Predictors of Prison Population

Judging from the coefficients alone, the story looks fairly complicated. Many variables—which represent a wide variety of social, political, and economic explanations—seem to be related to prison capital spending or populations. It is difficult to tell which are most important. One way to estimate the relative importance of each of these explanations is to observe what happens when we remove variables from the model, one category at a time. R^2 values will decrease, of course; we can measure the significance of this reduction (and thus the value of including each category in the complete model) through an F statistic. We can also compare the reductions in R^2 across categories, shrinking the values toward zero to reduce capitalization on chance (Box and Tiao, 1972). The proportion of the total R^2 accounted for by each category provides a rough estimate of that category's importance. This estimate can be misleading if two or more categories are collinear; in this case, eliminating one category may affect the size and significance of the variables in the remaining categories. If the coefficients do not change by much, we can be fairly sure that the change in R^2 is measuring the effect of the removed category.

Table 5 shows the results of this thought experiment. For prison capital spending, economic threat, the electoral cycle, and crowding are all statistically significant, but 67% of the variance is explained by politics and current spending. For prison populations, sentencing policy and current spending are statistically significant, but crime and previous capital spending—more or less, current prison capacity—seem to explain more than the others. In no case did removal of a category appreciably affect the remaining coefficients, so we can be fairly certain that these estimates are untainted by collinearity across categories.[25]

Capital spending is an intervening variable in this analysis. It does not itself cause prison populations to increase, but it makes future increases possible. Thus, the other variables affect prison populations in two ways: directly (in the prison equation) and indirectly (by affecting capital spending, which affects future prison populations). When direct and indirect effects are combined, eliminating current capacity as an explanation, the critical categories are crime and state spending, followed by sentencing policy, politics, and crowding. That is, the principal drivers of prison populations are the apparent need for more prisons and the ability to pay for them. Politics matters, too, but not as much. Other social threats do not matter at all.

25. The size of the differences were measured in three ways: (1) the absolute value of the difference in coefficients between the complete model and the removal model, divided by the standard error of that coefficient in the complete model; (2) the number of coefficients that change by as many as one standard error when some category of variables is removed; and (3) the number of coefficients that change in statistical significance (that is, from insignificant to significant or vice versa). The removal of several categories changed the significance of the remaining coefficients because several variables in the complete model were marginally significant or marginally insignificant. However, only two coefficients changed by as much as a single standard error. (The long-run equilibrium coefficients on WAGES and MHPATIENTS in the PRISON equation increased by 1.60 and 1.50 standard errors, respectively, when crime was removed from the model. This anomaly is both difficult to explain and small enough to ignore.) The average absolute change across all models was .101 for capital spending and .165 for prison population.

Table 5. Relative importance of independent variables in explaining prison capital spending and population

Prison Capital Spending

	k vars	R^2	F	$p(F)$	Shrunk ΔR^2	% total	Average Δ coefficient
Include all covariates	32	.6143	—	—	—	—	—
Remove:							
Economic threats	4	.6093	3.848	.004	.004	12.8%	.145
Underclass threats	5	.6126	1.046	.389	.000	.3	.067
Institutional threats	4	.6136	.484	.748	.000	.0	.043
Opinion and politics	5	.6013	8.002	<.001	.011	39.3	.213
Electoral cycle	2	.6121	3.251	.039	.001	5.2	.018
Crime	3	.6125	1.816	.142	.001	2.7	.063
Prison crowding	3	.6106	3.768	.010	.003	9.3	.128
Sentencing policy	4	.6123	1.522	.193	.001	2.3	.082
Current spending	2	.6055	13.533	<.001	.008	28.1	.148
					.034	100.0%	

Prison Population

	K vars	R^2	F	$p(F)$	Shrunk ΔR^2	% total	Average Δ coeff
Include all covariates	66	.3667	—	—	—	—	—
Remove:							
Economic threats	8	.3638	.653	.733	.003	12.8	.090
Underclass threats	10	.3647	.361	.963	.000	.0	.155
Institutional threats	8	.3639	.627	.755	.000	.0	.122
Opinion and politics	10	.3625	.764	.664	.000	.0	.140
Electoral cycle	4	.3656	.508	.740	.000	.0	.039
Crime	6	.3452	6.556	<.001	.018	31.8	.386
Prison crowding	6	.3619	1.454	.191	.001	4.6	.150
Sentencing policy	8	.3576	2.076	.035	.005	14.5	.240
Previous capital spending	2	.3564	9.399	<.001	.009	28.2	.220
Current spending	4	.3577	4.117	.003	.007	21.0	.108
					.040	100.0%	

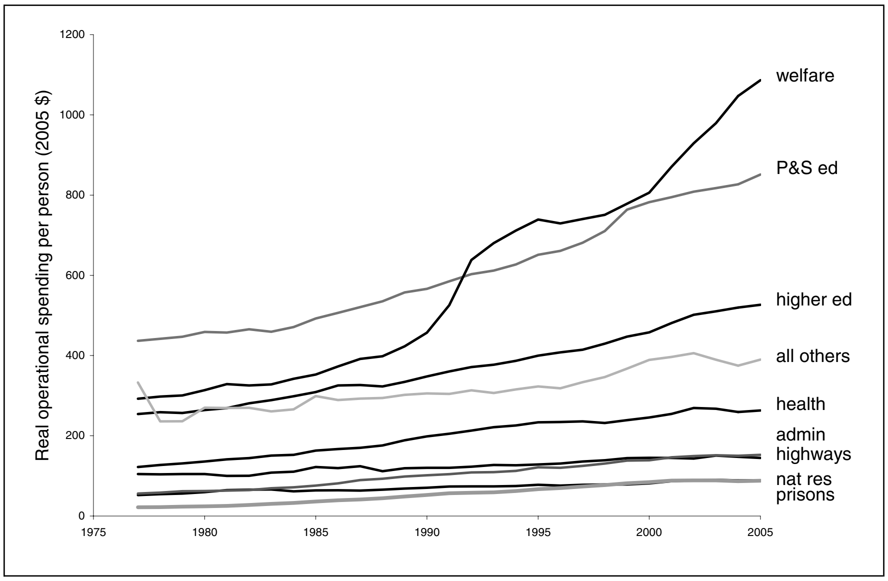
Notes. This table shows the effects of removing one category at a time from the complete model. Shrunk ΔR^2 = (1 – 1/F removal)(R^2 complete – R^2 removal). "% total" is an approximate share of explanatory power associated with each category. "Average Δ coeff" is an average difference between complete and restricted model for remaining coefficients, measured in standard errors.

These results reinforce the principal results in Table 4. Crowding, politics, state resources, and prison capacity all affect the timing of prison population increases. New prison capacity is most likely to be made available when prisons are crowded, the electorate and the legislature are politically conservative, and money is available to pay for prison construction. In the long run, however, the critical factors are increased crime rates and state policies that increase time served.

Discussion

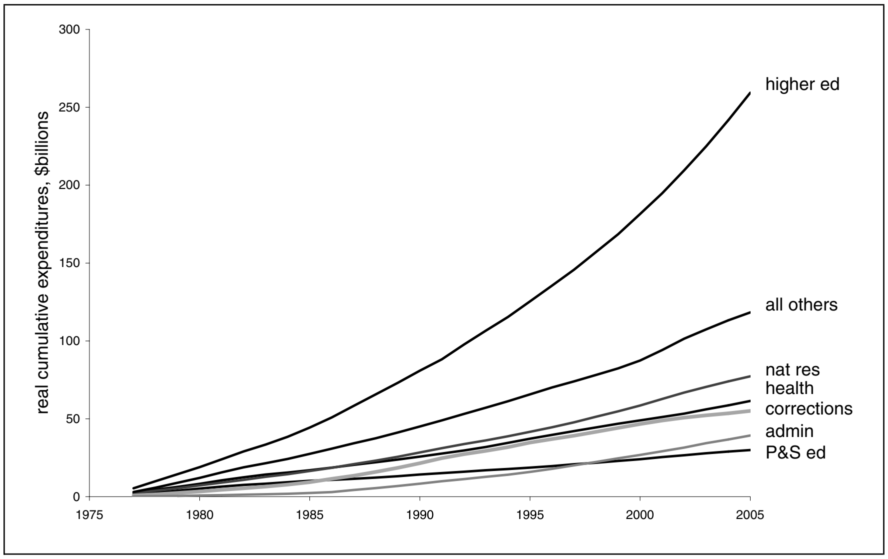
Some of these findings are expected. It comes as no surprise to find that prison populations increase with prison capacity or crime rates, or in response to more or less punitive sentencing policies. However, the importance of state financial resources—explaining nearly 30% of the total variation in prison populations—may be surprising. How can money be a cause of our problem? Wouldn't this affect everything else that states do, too? Of course it would. It did. As Figure 3 shows, spending increased for all state functions between 1977 and 2005. The prison increase was higher than for most other functions: Spending on prison operations quadrupled (as did prison populations); operational spending on primary and secondary education, higher education, and health only doubled. Nonetheless, all boats rose with the tide of cash.

Figure 3. State operational spending, per capita, by function



The capital spending pattern suggests why prison operational spending increased faster than the others. Figure 4 shows cumulative capital spending by function. Higher education tops the list, with $259 billion in real spending, nationwide, across the period.[26] States spent $77 billion on parks and natural resources, $61 billion on health and hospitals, and another $30 billion on primary and secondary education. Viewed in this context, the $55 billion we spent on the correctional system—most of it on new prisons—is hardly exceptional. We built a lot of new prisons, but we built a lot of roads, hospitals, and parks, too.

Figure 4. Cumulative capital expenditures by function



An important difference must be examined here. Some roads and parks are congested, but many are not; thus, the annual changes in lane miles and state parkland acreage are not very highly correlated with the changes in vehicle miles traveled and the numbers of park visitors. However,

26. The real chart-topper here is highways, with $930 billion in accumulated capital expenses from 1977 to 2005. But much of these expenditures were for highway maintenance, not new highways. To ensure an apples-to-apples comparison (and to make the chart easier to read), highway capital spending was removed.

prison beds are typically filled to capacity soon after they come online.[27] Since capacity estimates became available in 1984, nationwide custody populations have run at about 107% of nationwide capacity, with a range of 97% (in 2005) to 118% (in 1994). Prison populations quadrupled because capital spending went into capacity increases, which were used almost immediately after they came online.

The apparent importance of prison capacity can be explained in two ways. The first is simple and direct: Prison populations are largely driven by available capacity; when the money was available to increase capacity, policy makers spent it; when the beds were available, criminal justice agencies filled them. Thus, prison populations increased by more than the numbers of park visitors in large part because small changes in prison release decisions—entirely under centralized control and easy to change quickly—can keep prisons filled, even if police, prosecutors, and judges do not change their behavior at all (which they may). In contrast, demand for other state functions depends on the slowly changing habits of millions of actors. New highways may eventually persuade people to drive further, but new highways are rarely filled to capacity within a year or two of construction.

An alternative explanation is less consistent with the data presented here. When making decisions about prison capacity, state officials may attempt to meet future demands; in addition to posing an immediate problem, increases in crime, prison crowding, and the like send a signal that the demand for prison space is increasing. If the signal is accurate, then capacity increases may not be a cause of subsequent prison populations at all. They did not come because we built it; we built it, knowing they were on the way, and they came right on schedule. Under this explanation, capital spending is irrelevant, except to explain the timing of capacity increases that were eventually more or less inevitable. This explanation would lead

27. College capacity is difficult to measure, but we can easily compare changes in the outputs of road construction (highway lane miles) and parks (acreage) to changes in usage rates (vehicle miles traveled and visitors). Since 1970, the correlation between annual changes in urban lane miles nationwide and annual changes in total vehicle miles traveled is .19; for all highway miles, urban and rural, the correlation is .05. The correlation between state parkland acreage and state park visitors is –.14. In contrast, the correlation between the annual change in nationwide prison capacity and the annual change in nationwide prison population is .60. The only state function that seems to behave similarly to prisons is hospitals. Since 1970, the correlation between annual changes in hospital beds nationwide and annual changes in average daily patients is .53, which is similar to the prison figure. Doctors, like prison officials, seem to take advantage of available beds soon after they become available.

us to interpret the results of Table 5 differently. Capital spending would no longer be interpreted as a cause. The apparent indirect effects are not causes, either. Prison crowding and conservative politics are now merely signs of increasing demand.

Both of these explanations are probably valid to some extent. Whether it is possible at all to distinguish between the two, it will certainly be difficult to do so in the absence of some kind of experiment.[28] Nevertheless, the second explanation suffers from some anomalies that the first explanation does not. If capital spending decisions were made largely with expected growth in mind, we could reasonably expect that sentencing policy variables would have similar effects on capital spending as they do on prison populations. They do not. When recent arrest rates and prison populations are included in the capital spending equation—the most obvious bases for prison population forecasts—they do not approach statistical significance.[29] More generally, we would expect crime rates and prison crowding to be better predictors of capital spending than politics and state spending. These predictors are not even close. Perhaps most telling is the response to the crime reductions of the 1990s: The number of prison beds continued to increase, for a few years at least; the definition of "demand" was sufficiently elastic to ensure that they were more or less filled to capacity. Like nature, correctional systems nationwide abhorred a vacuum.

28. The problem here is simultaneity: Spending may cause future prison populations, but (expected) future prison populations may cause spending. As with the simultaneous relationship between crime and prison populations, the usual solution is to use instruments. In this case, these variables would be closely associated with capital spending and can be excluded in advance from the prison equation. Tables 3 and 4 suggest that several variables can predict P.CAPITAL but not PRISON, which include a conservative electorate, Republican control of the legislature, and prison crowding. Using these variables as instruments would satisfy the usual statistical tests (the F value in the P.CAPITAL equation would be large, and a Wu test of overidentification in the PRISON equation would not approach statistical significance), but it would be very difficult to argue that these variables could be excluded in advance on theoretical grounds. A similar argument could be made for virtually any variable that affects capital spending. In the absence of valid instruments, the problem is unsolvable (cf. Blumstein, Cohen, and Gooding, 1983; Carlson, 1980).

29. Specifically, seven new variables were added to the P.CAPITAL equation, in addition to the variables shown in Table 3: proportionate changes over the previous 5 years in prison population, violent and property crime rates, adult arrest rates for violent and property crimes, as well as juvenile arrest rates for violent and property crimes. None of these variables were statistically significant; collectively, $F(7,1229) = .546$ and $p(F) = .800$. Three-year and one-year lagged rates were even further from statistical significance.

This analysis by no means proves that capital spending and resulting prison capacity are important drivers of prison populations, but it does make it seem more plausible.

Although we probably cannot obtain a definitive answer to the causation question, we can test its effects on the remaining findings. Specifically, suppose capital spending is entirely caused by expectations of future prison populations. In this case, none of the apparent capital spending effects and none of the indirect effects that work through capital spending would be causal. Alternatively, it may be that capital spending is completely independent of expectations, and all the apparent indirect effects are causal. The most likely case is somewhere in between; let us set bounds of 25% and 75% (that is, between 25% and 75% of the apparent effects are causal).

As Table 6 shows, these assumptions have a limited effect on the conclusions. No matter how much of the relationship between capital spending and population is causal, current state spending explains 29% of the explainable variance. Crime explains 32% to 44%, sentencing policy explains 15% to 20%, and prison crowding explains 6% or 7%.

Table 6. Effect of assumptions of causality on relative importance of independent variables

	Proportion of variance in P.CAPITAL explained by expected future prison			
Category	***All***	***75%***	***25%***	***None***
Economic threats	.0	1.1	2.9	3.6
Underclass threats	.0	.0	.1	.1
Institutional threats	.0	.0	.0	.0
Public opinion and politics	3.5	9.0	11.1	—
Electoral cycle	.0	.5	1.2	1.5
Crime	44.3	40.5	34.8	32.5
Prison crowding	6.4	6.6	7.0	7.2
Sentencing policy	20.2	18.6	16.1	15.1
Current spending	29.2	29.1	29.0	28.9
Total	100.0	100.0	100.0	100.0

Notes. This table shows the percentage of explicable variance that is explained by direct and indirect effects of each category of variables, under four assumptions about the proportion of variance in correctional capital spending that can be explained by expected future prison populations.

The other key finding from Tables 5 and 6 is that some variables do not seem to matter. Prison populations may respond to social threats, but all of these effects combined explain no more than 4% of the explicable variance. Prison populations did not increase because society was worried about the economy, the underclass, or the family. We were worried about crime. Getting a handle on prison populations requires that we develop new responses to the crime problem.

Although it may be simple, this explanation accounts for two of the most puzzling aspects of the prison boom: why it started when it did and why it lasted so long. State revenues increased throughout the 1950s, but crime rates were largely flat. When the increase in crime became a serious issue in the 1960s, prisons filled and legislatures began spending money to construct more. Prison populations increased as new beds began to come online in the early 1970s. The lag between external conditions and public policy response continued on the back end, as well. Crime started to decrease in the 1990s; it took a while for legislators to conclude that this decrease was permanent and not just a temporary blip. Eventually, capital spending was reduced and—a few years later—prison capacity and populations flattened out.[30]

These findings also suggest what may happen if crime rates begin to increase again: Prison officials will seek, and legislatures will authorize, yet more prisons to be constructed and filled. So long as incarceration remains our primary (state-level) response to crime, we will continue ratcheting up prison populations whenever crime rates increase, and the boom will start all over again.

Effects of the 1994 Crime Bill

The 1994 Crime Bill underscores the need to develop alternative responses. The Clinton administration touted the bill for funding 100,000 new police officers, and this objective seems to have been met (Koper, Moore, and Roth, 2002). But to get the bill passed, the White House and congressional Democrats had to accept another proviso: The federal government would provide $10 billion to states for prison construction

30. The lag also explains why Blumstein and Cohen's (1973) argument for a constant level of punishment—accurate from 1930 to 1970—did not play out in the 1970s and beyond. The first public policy response was to make do with the capacity available by shortening sentences and diverting convicted offenders from prison throughout most of the 1960s. New prison beds were only constructed when the limits of this response became clear. Although it is impossible to be sure, it seems likely that any future lag between crime rate increases and additional prison construction will be considerably shorter. The ratchet wrench is still close at hand, and prison officials and state legislators have not forgotten how to use it.

between 1995 and 2000, but the money would only be available to those states that passed truth-in-sentencing laws that eliminated most "good time" provisions and required convicted offenders to serve 85% of their prison sentence.

The effect on state policy was almost immediate. The number of states with truth-in-sentencing statutes went from 4 in 1992, to 17 in 1995, to 27 in 1998. As shown in Table 4, states with truth-in-sentencing statutes will eventually have prison populations about 13% larger than those without them, even after controlling for capital spending. Thus, the crime bill increased prison populations two ways: by making it cheaper for states to build new prisons and by making it more difficult to control populations downstream. This result, of course, was the intent.

The fact that many states did not adopt truth-in-sentencing allows us to use Table 4 to estimate, very roughly, the effect of this requirement on prison populations. Truth-in-sentencing was first adopted by Washington State in 1984, but diffusion was slow. By 1994, a new state was adopting it every 4 years or so, and it is reasonable to expect that this would have continued in the absence of the crime bill's capital funding provision. Let us also assume that all the capital funding provided to states as part of the crime bill simply offset funds that states would have spent anyway. That is, assume the crime bill did not increase prison capacity at all.[31] Now suppose that, instead of using the promise of funding to promote truth-in-sentencing, the funds had been made available to all states on three alternative bases:

- *No policy requirement*—all states were invited to obtain funding on an equal basis, regardless of sentencing policy.
- *Presumptive sentencing requirement*—only states that had adopted presumptive sentencing would be eligible.
- *Presumptive sentencing and marijuana decriminalization requirement*—in addition to presumptive sentencing, states had to decriminalize possession of 1 ounce or less of marijuana.

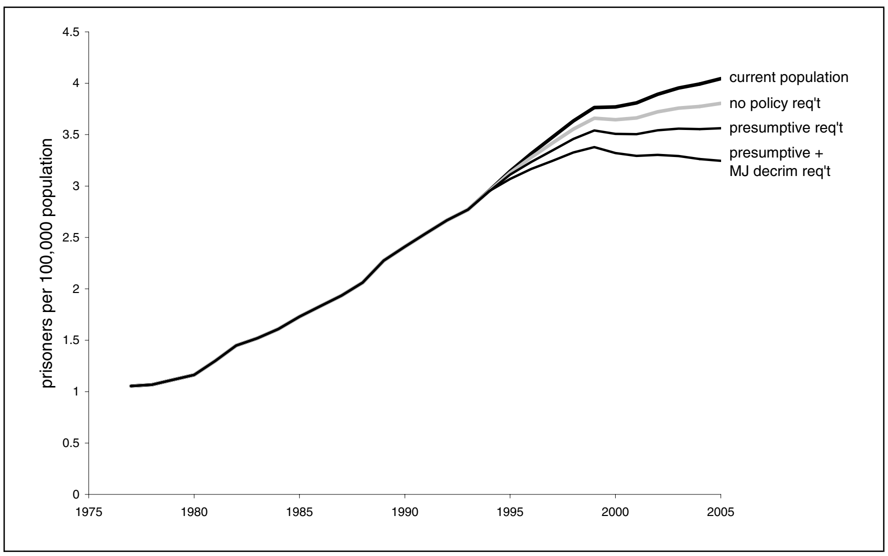
The alternative bases are not at all far-fetched. Early versions of the crime bill included no sentencing policy requirements (Greene, 2002). Presumptive sentencing is intended to make sentencing policy fairer and more even across cases, courtrooms, and counties within a state; mandating such levels of fairness in exchange for federal funding is hardly unreasonable. Popular opinion on the merits of marijuana decriminalization was about evenly split in 1994 (Pastore and Maguire, 2008), but it would not have been unreasonable for the federal government to have insisted that its

31. This is actually a reasonable assumption. Between 1990 and 1994, the states spent $2.4 billion each year on prison construction; between 1996 and 2000, the states spent $2.1 billion.

funds be reserved to reduce crimes with clear victims and social costs. In 1994, 12 states had adopted presumptive sentencing and 12 had decriminalized marijuana; let us assume that all states that adopted truth-in-sentencing between 1995 and 2000 would have adopted presumptive sentencing or decriminalized marijuana to obtain access to federal prison construction funding.

Figure 5 shows the results of this thought experiment. Between 1994 and 2005, the state prison population increased by 372,000. Had the crime bill not required truth-in-sentencing legislation, the population would still have increased, but only by 305,000. If presumptive sentencing had been required instead of truth-in-sentencing, the population increase would have been 238,000 and the incarceration rate per capita would have been essentially flat since 1999. If the crime bill had also required decriminalization of marijuana, the increase would have been only 155,000 and the incarceration rate would have peaked in 1999.

Figure 5. Effects of 1994 Crime Bill policy requirements



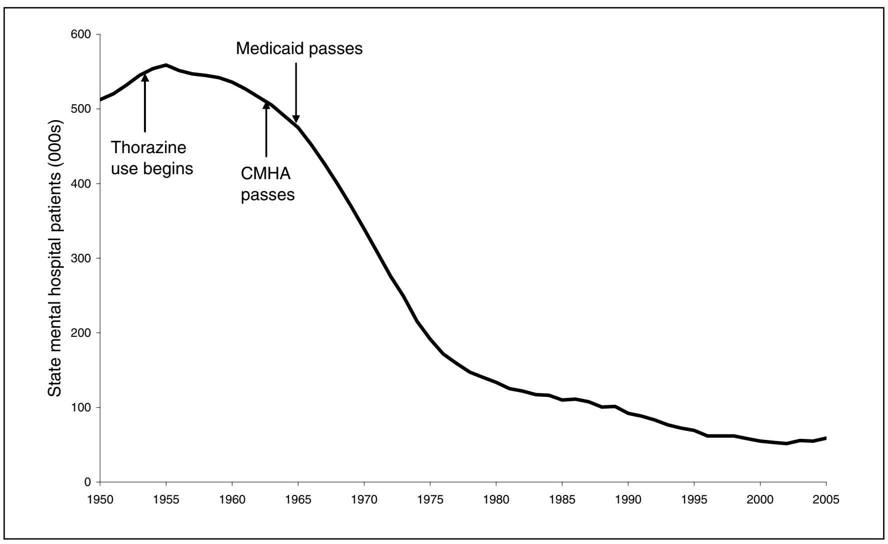
These figures are obviously back-of-the-envelope calculations, but the basic point is clear enough: The effect of sentencing policies on the prison population was large enough that changes in policy could have reversed the prison growth of the last few years. It is also not difficult to make the case that by focusing scarce prison resources on the most violent and dangerous criminals, presumptive sentencing, decriminalization, or similar policies would have improved the benefit-cost ratio of incarceration, regardless of the prison population.

Value of Financial Incentives

The findings described in this article provide no evidence that the deinstitutionalization of the mentally ill had any effect on prison construction or on prison populations. Nevertheless, the case is instructive because of what caused states to deinstitutionalize: the availability of alternatives and a substantial federal funding incentive.

In 1955, the state mental hospital population was 559,000—nearly as large on a per capita basis as the prison population is today. Beginning in the late 1950s, states began to let patients out of mental hospitals. As Figure 6 shows, the decrease was precipitous. By 1972, the number of patients had decreased by 50%, and by 2000, the number had decreased by 90%.

Figure 6. State mental hospitals emptied in the 1960s and 1970s



As usual, the reasons are complex, but most observers of the subject identify two critical explanations. First, the development of drugs such as reserpine and Thorazine relieved some patients of the symptoms of mental illness. This result reduced the need for physical restraints and, after a lengthy period of local experimentation, led to an increase in early discharge programs. Perhaps more importantly, it shifted public attitudes toward the problem, "promot[ing] psychiatrists to physicians in the eyes of some of their colleagues, and the insane to the status of patients in the eyes of many members of the public" (Roberts, 1967:25). Even if the mentally ill could not be cured, they could be made relatively harmless to themselves and others.

Although Thorazine and its kin made community treatment possible, treatment remained a state responsibility until the 1960s. This rule changed in 1963 with passage of the federal Community Mental Health Act. The act provided grants to local governments (but not to states) to create community alternatives to state hospital systems. By the mid-1970s, 650 community mental health centers were in operation, which served almost 2 million patients per year (Koyanagi, 2007).

The real financial impetus for deinstitutionalization came in 1965 with the passage of Medicaid. Through this program, the federal government paid for in-patient care of the mentally ill in local hospitals and nursing homes, but not in state mental hospitals. For the first time, states could offload financial responsibility for caring for the mentally ill. Some mental patients were simply shifted from one long-term institution to another. However, most patients were released into the community (Rochefort, 1984).

Of course, it is highly unlikely that prison deinstitutionalization could ever be so complete. Even if alternatives could be found for property, drug, and public order offenders, something like 40% of prison inmates nationwide were convicted of violent offenses, many for the second or third time. It is very likely that the public will insist on some measure of incapacitation for most of them. Nevertheless, most of the U.S. public supports alternative sanctions for nonviolent and first-time offenders (Farkas and Gutmann, 1992; Langer, 2002). Reducing the number of prisoners by 30% or 40%—roughly the amount needed to obtain a benefit-cost ratio close to 1.0—is not out of the question.

The mental hospital case suggests that the availability of publicly acceptable alternatives to incarceration may not by itself be sufficient to reduce prison populations. Like the mental hospitals of the 1950s, prisons are operated and supported by a large, well-entrenched, and politically adept bureaucracy. Alternatives, like community mental health until the mid-1960s, have no such political backing. Despite the periodic fiscal crises of the 1980s and 1990s, states have found the money to increase funding for all state functions. If this continues—and particularly if more states adopt laws that increase sentence lengths—more growth in prison populations is nearly certain.

That is, unless we can remove the financial incentives. A federal program of funding for alternatives to incarceration—but not prisons—would very likely have the same effects as Medicaid did on mental hospital populations, and for that matter as the 1994 Crime Bill had on prison populations. State legislatures are likely to respond to financial incentives by offloading the responsibility for drug users, property offenders, and first-time violent offenders. If the alternatives are available and well managed, the result could be a more cost-effective and sensible corrections system.

Conclusion

All of this discussion suggests that the prison boom may be largely over, as long as crime rates stay flat. If they begin to increase again, in the absence of persuasive alternatives, states will once again do the only thing they know how to do to solve the problem. This result also suggests what it might take to reverse the buildup: a dramatic improvement in the infrastructure for delivery of alternative sanctions, combined with substantial federal funding for these alternatives (but not for institutions). This formula succeeded in reducing the institutional population of the mentally ill in the 1970s and 1980s; appropriately retooled, it could reduce the institutional population of criminal offenders in the 2010s.

Regardless of what instrument we choose, the more basic finding is that nothing was inevitable about prison buildup in the United States. It was motivated by concern about crime and (to an extent) prison crowding—narrowly framed problems that can be dealt with through alternative means. Whatever symbolic value prisons may hold responded to the market for funding in the same way as all other state functions. It is fully under control of public policy makers. If it continues, it is because we have failed to heed Cassius' advice: "The fault, dear Brutus, is not in our stars, but in ourselves."[32]

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32. William Shakespeare, *Julius Caesar,* Act 1, scene 2.

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