The Politics of Punishment across Time and Space: A Pooled Time-Series Analysis of Imprisonment Rates*

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Abstract

Despite considerable theoretical interest, little is known about the political determinants of punishment. This study uses a pooled time-series design to fill this gap by examining political and other determinants of state imprisonment rates. The presence of Republican elected officials is used to assess the strength of the law-and-order political party. Ethnic threat theories suggest that imprisonments will be more likely in jurisdictions with the most blacks or Hispanics, while economic threat theories suggest that the imprisoned population will be greater where economic stratification is most pronounced. After controlling for social disorganization, religious fundamentalism, political conservatism, and violent crimes, the results show that Republican strength and minority threat lead to higher imprisonment rates. Statistical interactions support predictions that these relationships became stronger after greater Republican stress on law and order. The latter findings confirm theoretical expectations that these relationships are historically contingent.

What factors account for shifts in the imprisoned population? Theoretical explanations for the incidence of punishment are fundamental to an understanding of social order. Many theorists (Collins 1975; Goode 1972; Lenski 1966; Tilly 1992; Weber 1968) claim that sanctions must be used to insure internal stability even in

the most progressive societies. As Weber pointed out long ago, the crucial defining element of the state is its ability to punish domestic dissidents. Although domestic order in advanced societies is based on more than the state's ability to sanction, without this power governments cease to exist.

The classical theorists who followed Hobbes and asked how social order is possible went to great lengths to account for the amount of formal punishment under various conditions. Durkheim and Weber help us understand how states employ legal sanctions to control their populations, while recent scholars have used neo-Marxist perspectives to explain the incidence and severity of punishment. A better understanding of the determinants of imprisonments should increase our knowledge about the punitive foundations of social order.

The popular wisdom views fluctuations in imprisonments as a natural response to changes in crime. Yet there is little recent correspondence between yearly shifts in the crime and imprisonment rates in the U.S. From 1947 until the early 1970s the imprisonment rates remained almost constant (see Figure 1). After 1980 the total crime rates stopped growing and after 1990 these rates fell, yet Figure 1 shows that the proportion of the population that was imprisoned grew dramatically throughout this period. The absence of a positive relationship between the crime and the imprisonment rates suggests a need for other explanations.

Many theorists see incarceration as intensely political (Chambliss 1994; Foucault 1977; Garland 1990; Savelsberg 1994). Garland (1990:134) justifies a political approach when he writes that punishment should be seen "not in the narrow terms of the "crime problem" but instead as one of the mechanisms for managing the underclass" Because the punishments administered by the state are a fundamental component of political authority (Foucault 1977; Garland 1990), a well developed political sociology of sanctions should deepen our understanding about how this authority is created and sustained. A better understanding of these relationships also should help explain the puzzling discrepancy between the stability or the recent reduction in street crime and the sharp growth in the imprisonment rates. All of these considerations suggest that research that focuses on the political determinants of imprisonments should be theoretically productive.

Only a few quantitative studies examine the political determinants of punishment. Beckett (1997) holds both the crime rates and media coverage constant and finds that statements of national politicians magnify public perceptions about the salience of street crime. Her results suggest that the law and order appeals that some politicians use to gain office amplify public anxieties and resentments about these offenses. Jacobs and Helms (1996) analyze yearly shifts in prison admissions and find that growth in the strength of the law-and-order Republican party produces a subsequent expansion in these rates. Caldeira and Cowart (1980) find that in contrast to Democrats, Republican presidents since Truman increased spending on corrections and other criminal justice programs.

FIGURE 1: Shifts in the Prison Population per 100,000 and Crime Rates Using Differing Scales
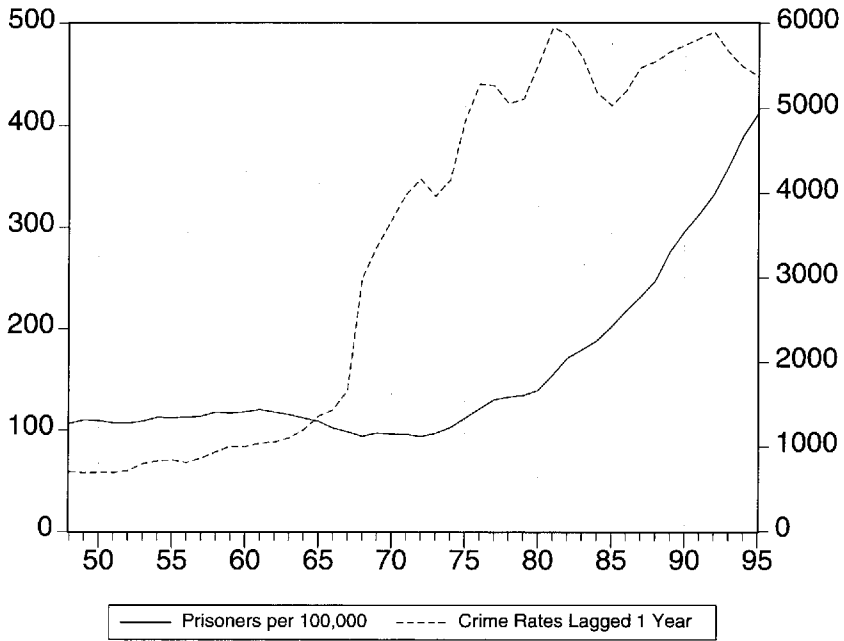
Note: Crime rates graphed on right axis

Although state officials are responsible for most of the decisions about sentencing, time served, and prison capacity, these three longitudinal investigations of yearly outcomes in the entire U.S. could not isolate state-level effects. Because the available pooled time-series studies of state imprisonment rates (Carroll & Cornell 1985; Greenberg & West 1998; Parker & Horwitz 1986; Wallace 1981) have not given much attention to political explanations, we employ this useful estimation procedure to assess the influence of politics on state incarceration rates.

A panel design offers many advantages. Perhaps the influence of the political or other determinants of imprisonments is historically contingent. Some explanations may be more influential in one period than others (Isaac & Griffin 1989). We therefore use a panel design that is sensitive to historical shifts in the strength of relationships. Such an estimation approach is advantageous because it takes into account both cross-sectional variation and changes in imprisonments

over time. Yet single factor explanations are suspect, so we must examine many hypotheses in this analysis, but this means the next section cannot focus on just a few accounts.

State agencies have the most influence on correctional outcomes. It would be surprising if these administrative units are not governed by the same political factors that account for the behavior of other state bureaucracies. While we do not believe that politics is the only explanation for jurisdictional differences in imprisonment rates, various considerations suggest that these underresearched explanations should be subjected to additional study.

Theoretical Explanations for Imprisonments

Three approaches inform our understanding of state behavior. The most recent, exemplified by Evans et al. (1984), suggests that parochial interests determine political outcomes. Office holders and candidates strategically chose issues that increase their support at the expense of their rivals. A second tradition treats public policy as the result of external social forces that act on the state. This explanatory approach leads to hypotheses about the effects of social divisions on political outcomes. A third approach stresses ideology because citizen political values may best determine which policies they will support.

We first present explanations for imprisonments based on the strategic behavior of political entrepreneurs. We go on to discuss the effects of minority and economic threat. After presenting rationales for political ideology and fundamentalist religious beliefs, we present the logic for the controls used in this analysis.

POLITICAL ENTREPRENEURS AND PUNITIVE OUTCOMES: BRINGING THE STATE BACK IN

State officials often stress issues that help them overcome electoral handicaps. An anti-crime agenda can be extremely useful in this endeavor. Conservative parties generally support economic policies that help the affluent at the expense of the least affluent. Hibbs (1987) and Blank and Blinder (1986), for example, show that conservative parties chose macroeconomic policies that help their prosperous constituents at the expense of the poor. Allen and Campbell (1994) show that regressive tax policies that benefit the rich and hurt the poor are more likely when Republicans hold office. Yet economic resource distributions are skewed so the affluent are in the minority. This makes the electoral base of more conservative parties smaller than the base of moderate left parties.

While their support for regressive policies makes electoral success more difficult for conservative parties, an emphasis on street crime helps to overcome this disadvantage. Conservative candidates use law and order appeals to attract less affluent voters who are more likely to be crime victims and who are more likely to live in or near to areas where violent crime is problematic. Officials in the Nixon

administration have acknowledged that they deliberately used the law and order issue to appeal to anti-minority sentiments (Edsal & Edsal 1991). By emphasizing street crime and other social problems readily blamed on a racial underclass, parties closer to the right can win elections by attracting votes from the less prosperous (Beckett 1997) and still pursue economic policies that help their affluent core supporters (Hibbs 1987).

Instead of highlighting social arrangements that close off law abiding alternatives for the poor, conservatives see reprehensible individual choices as the primary explanation for street crime (Burnham 1970; Thorne 1990). If this view is correct, increases in the expected costs of illegal acts should be effective. Republicans often claim that deterrence is the best remedy for these offenses. The core support for conservative parties comes from the prosperous who are well served by existing arrangements, so appeals for law and order will not offend this constituency. In part because Democrats and their liberal supporters are likely to see street crime as the result of unfortunate social circumstances, it has been more difficult for Democrats to adopt a political strategy that emphasizes deterrence and retribution. In any case, claims that the Democrats are "soft on crime" became a major part of Republican campaigns at both the national and the state levels since the 1964 presidential election (Chambliss 1994; Davey 1998).

After they were elected, Republican incumbents did not disregard their promises. Caldeira and Cowart (1980) find that Republican presidents since 1935 spent greater amounts on various criminal justice policies more than their Democratic counterparts. State level Republican officials also allocated more tax dollars to law enforcement and corrections than their Democratic counterparts (Scheingold 1991; Davey 1998). Both federal and state Republican officials vigorously supported longer sentencing provisions and increased the number of crimes punishable by imprisonment.

If Beckett's (1997) findings are correct and political rhetoric enhances public fear of and resentments against street crime, successful law and order campaigns should increase political support for harsh punishment. Imprisonment rates therefore should be more substantial in jurisdictions where the law and order Republican party has been most successful for two reasons. First, according to Beckett, Republican rhetoric magnifies public demands for severe penalties. Second, after they gain office Republican officials can follow their predilections and make imprisonment and longer sentences more likely by increasing the penalties for various offenses and by expanding prison capacity. Both accounts suggest that *jurisdictions with more Republicans in important political offices and a politically stronger Republican party should have higher imprisonment rates.*

Beckett's findings that claims of national politicians create anxieties about street crime suggest that these relationships may be historically contingent. After a tentative use of this issue in the 1964 election, subsequent Republican presidential candidates increased their emphasis on law and order. Nixon ran on riots and street

crime in 1968, while Reagan used the same rhetoric in 1980 (Beckett 1997; Edsal & Edsal 1991). Yet the Bush campaign against Dukakis in 1988 reached new heights in the weight given to this issue. Bush supporters ran vivid advertisements implying that when Dukakis was governor of Massachusetts, he was responsible for the violent crimes committed by a black inmate on temporary furlough from prison. Davey (1998) provides evidence that these contrasts in Republican and Democratic rhetoric about punitive reactions to crime expanded throughout the 1980s.

Beckett's results about the relationship between political rhetoric about law and order and the salience of crime and Davey's evidence about the growth in partisan differences on this issue lead to a historically contingent hypothesis. If political rhetoric about street crime increases public demands for harsh punishments, *the relationships between Republican political strength and incarcerations should become more substantial during the latter part of the analysis period during a time when both state and national Republicans placed greater emphasis on law and order.* Tests for the presence of interactive relationships between Republican strength and time should show if these expectations about the presence of historically contingent relationships are correct.

SOCIAL DIVISIONS WITH POLITICAL IMPLICATIONS: MINORITY PRESENCE AND ECONOMIC INEQUALITY

Social divisions that are external to the state should explain imprisonment rates as well. The threat posed by a large racial underclass should produce enhanced incarceration rates. Substantial gaps in the economic resources of the rich and the poor may have the same effect because repressive measures may be particularly likely in the most unequal jurisdictions where the affluent feel menaced by an economic underclass with much to gain from violence that takes resources from the rich.

Minority Threat

Blumer (1958) and Blalock (1967) claim that dominant groups are threatened by growth in minority populations. Fossett and Kiecolt (1989), and Bobo and Hutchings (1996) show that negative attitudes about blacks are more likely in areas with the most blacks. Liska, Lawrence, and Sanchirico (1982) hold the crime rate constant and find that fear of crime is greater in cities with more African-Americans. Perceptions about the menace of a potentially violent, expanding racial underclass should increase the likelihood that dominant whites will successfully demand harsh punishments and increased incarceration rates.

Additional findings support these threat expectations about social control. Studies of the determinants of police strength show that cities with more minorities have larger numbers of police officers relative to their population (Jackson 1989; Liska et al. 1981). Jacobs and Helms (1999) find that a growth in the percentage of

nonwhites led to greater spending on prisons and jails. These results suggest that whites successfully demand severe penalties for crime in areas where minority presence is greatest. If this threat hypothesis is correct, *we can expect that imprisoned populations should be larger in jurisdictions with the most blacks*. Because Hispanics occupy a role similar to that of blacks in many states, it is equally plausible that *states with more substantial Hispanic populations will have higher imprisonment rates as well.*

The same logic leads to additional hypotheses about historically contingent relationships. While the relative presence of African-Americans stayed almost constant, the percentage of Hispanics increased substantially during the analysis period. *If the Hispanic version of the ethnic threat account is correct, the relationship between Hispanic presence and imprisonments should become stronger with the passage of time.*

Another historically contingent hypothesis about minority threat seems equally plausible. Perhaps the covert racial elements in the increasingly strident Republican appeals for law and order (Davey 1998) accentuated public fear of violent black criminals. If this hypothesis is correct, we can expect other period contrasts in the results. *If the sharp growth in Republican rhetoric about law and order magnified fears of a violent black underclass, the relationship between African American presence and the imprisonment rates also should become stronger near the end of the analysis period.* We therefore estimate contingent relationships between ethnic threat and imprisonments by testing for interactions between period and the presence of two minorities. This approach will tell us if relationships between ethnic threat and imprisonments became stronger after expansions in political emphasis on law and order.

Economic Threat

Many scholars (Garland 1990; Chambliss & Seidman 1980), have argued that rates of punishment are shaped by the menace of an economic rather than a racial or ethnic underclass. Both Weberians and neo-Marxists claim that disparities in economic resources create a potentially unstable social order that must be sustained by repression. Chambliss and Seidman (1980:33), for example, write that "the more economically stratified a society becomes, the more it becomes necessary for dominant groups to enforce through coercion the norms of conduct that guarantee their supremacy."

A growing economic underclass with an interest in the use of violence to redistribute resources and little to lose should threaten economically influential groups. The privileged may respond to increased inequality by calling for heightened punishment. The menace of an economic underclass is best thought of as a relational concept (Jacobs 1979), so increased differences in the resources of the affluent and the least affluent should magnify this threat. If the economic version

of the threat hypothesis is correct, *enhanced economic inequality should produce more substantial imprisonment rates.* Because disputes about the relative importance of minority threat or economic inequality remain unresolved, we test both of these versions of threat theory.

POLITICAL IDEOLOGY AND RELIGIOUS FUNDAMENTALISM

The prior explanations have emphasized explanations that stress political and social arrangements. Yet public belief systems may provide more powerful explanations for punitive outcomes especially in a relatively populistic, direct democracy like the U.S. where public opinion can be such an important determinant of symbolic policies like punishment.

Political Ideology

While expansions in the strength of the more conservative Republican party may lead to higher imprisonment rates because strategically acute Republicans can reap substantial political gains by emphasizing street crime, an equally plausible alternative explanation focuses on ideology. Perhaps the Republican party does best in jurisdictions where conservative values are strongest. Instead of being imposed from above by political leaders, it is equally plausible that higher incarceration rates are the result of demands for harsh punishment by a conservative citizenry.

An emphasis on punishment is consistent with conservative beliefs about individual accountability. Conservatives see criminals as autonomous, rational individuals who are responsible for their acts and therefore deserve to be punished (Lacey 1988). Conservative thought about crime relies on other concepts borrowed from the marketplace. According to this view "punishment should be equivalent to the offense, so that justice consists in a kind of equity or fair trading that exchanges one harmful act for another which equals it" (Garland 1990:113).

Because they believe that individuals deliberately choose to disobey the law (Burnham 1970; Thorne 1990), conservatives often hold that deterrence is the best remedy for lawless behavior. Molnar (1976:47), for example, claims that "if those who deserve it are not appropriately penalized, then the so-far guiltless tend to fall, by a kind of social gravitational pull, to lower levels of discipline and civilization." Studies indicate that respondents with liberal values are far less likely to support punitive reactions to crime (Taylor, Scheppele, & Stinchcombe 1979; Langworthy & Whitehead 1986; Van Dijk & Steinmetz 1988). All of these considerations suggest that *the imprisonment rates should be most substantial in jurisdictions where conservative values are strongest and support for liberal views is least substantial.*

Religious Fundamentalism

In part because they follow their political counterparts and give little weight to environmental factors that mitigate culpability, religious conservatives stress retribution. Historical research suggests that religious views influenced punishment (Erikson 1966; Ignatieff 1978; McGowen 1995). Survey research (Grasmick et al. 1992; Grasmick & McGill 1994; Curry 1996) shows that fundamentalist Protestant values are associated with greater support for harsh reactions to crime. According to this retaliatory outlook, the state is obligated to make those who are guilty of grievous social harms pay for what they have done.

Where such values are prevalent, we can expect increased demands for severity. In states where membership in fundamentalist Protestant churches and traditional religious views are most common, public officials should face heightened pressures to sentence offenders to prison and increase sentence length. Because these demands should increase prison admissions and time served, we expect that *states with the largest membership in fundamentalist Protestant churches will have higher incarceration rates.*

Unemployment and Additional Controls

Most empirical work on imprisonment rates focused on the neo-Marxist hypothesis that imprisonment is used to control the excess supply of labor in capitalist societies (Rusche & Kirchheimer 1939). Many studies have examined the links between unemployment and subsequent imprisonments, but investigators do not find consistent results (Melossi 1989; Michalowski & Pearson 1990). A literature review conducted by sympathetic scholars (Chiricos & Delone 1992) concludes that only 60% of the 147 reported relationships between unemployment and imprisonments are positive. Despite these discrepancies, the attention given to this hypothesis in the literature suggests unemployment should be investigated.

Because we must limit this research to relationships of theoretical interest, the administrative procedures that states use to control the size of their incarcerated population are not at issue. Policies such as determinate sentencing laws can be viewed as part of the dependent variable. Yet there is considerable interest in the influence of these laws, so we assess their independent effects on imprisonments. The standard assumption is that such provisions increase the incarcerated population. In what is probably the most carefully done study, however, Marvell and Moody (1996) find that the great majority of states with determinate sentencing laws have smaller prison populations. Because there is disagreement about the direction of this relationship, we use two-tailed significance tests to evaluate this effect.

Jurisdictions with greater percentages of their population living in larger communities may have higher imprisonment rates because large cities are notoriously difficult to police. Officials in these jurisdictions may be more likely

to invoke severe penalties for street crimes. Studies of public policy often find that the presence of economic resources required to support expensive programs is an important explanatory factor (Dye 1966; Hofferbert 1966), so we see if the tax base matters. Regional effects that capture unmeasured cultural and other differences should be held constant as well. Finally, incarcerations should be most likely in states with the greatest amount of lawless behavior, so we include serious crime rates in all analyses.

Summary

This study focuses on political effects and ethnic or economic divisions that should be reflected in the politics of punishment. Because local Republican officials should be likely to support severe punishments, we see if states where the Republican party is strongest have larger incarcerated populations. Minority threat hypotheses suggest that states with the most blacks or Hispanics will have higher imprisonment rates, while an economic threat hypothesis suggests that economic inequality will produce more incarcerations. We also see if political ideology and religious fundamentalism explain state imprisonment rates, and we include controls for the amount of serious crime, and unemployment as well.

Methods

RESEARCH DESIGN, THE DEPENDENT VARIABLE, AND ESTIMATION

We follow Greenberg and West (1998) and use data from the 50 states largely taken from the 1970, 1980, and 1990 Census. If the analysis is not confined to census years, the values of critical explanatory variables such as the percentage of blacks or Hispanics and economic inequality must be estimated for the nine years between each census. To avoid measurement error in such important explanatory variables and results that would automatically favor some hypotheses over others, we study imprisonment in these three periods and analyze 150 state-years. The sample starts in 1970 because the census does not report Hispanic presence or economic inequality before then.[1]

We analyze prisoners per hundred thousand because this outcome is a more comprehensive indicator of total punitive responses than admission rates or sentences. When admissions or the length of sentences increase, prison officials can use early releases to create space for new inmates. Prison admissions or sentence length therefore could expand even when total imprisonments stay constant or decline. Imprisonment rates are a more exhaustive measure than sentence length or admission rates because they capture the probability of imprisonment, time served, early releases, and the state provisions about parole violation reincarcerations.[2]

Because prison terms often are longer than a year, we borrow a method used in time-series research when investigators are unsure about the time needed for relationships to be completed and use two-year averages of the imprisonment rates. The dependent variable therefore is constructed by calculating the natural log of the mean of per capita imprisonments in 1971-72, 1981-82, and 1991-92 (variables are logged to the base e to create unskewed distributions and multivariate normality). When we analyze prison populations limited to just the first year after each census in unreported equations, the point estimates are similar and the significance tests remain identical to those we report.

There are two standard ways to estimate panel models with more cases than periods. Fixed-effects models automatically hold constant all unchanging case attributes that are not included in models, so stronger claims can be made that unmeasured effects are not biasing the results. Because the alternative random-effects procedure does not automatically eliminate all case attributes that remain constant over time, the effects of theoretically interesting time-invariant explanatory variables can be estimated with this approach. Each procedure has advantages and disadvantages, so we present results based on both.[3]

We estimate shifts in the strength of relationships at different times by breaking some explanatory variables by year and by including interactions with time and the main effects in the models (Hsiao 1986; McDowell, Singell & Zilliak 1999). This use of period-specific interactions will show how relationships shift over time. In addition to their substantive advantages, such specifications should increase model explanatory power because the coefficients on the periodized variables will not be forced to be equal. To promote comparisons, we report results from both unperiodized and periodized equations, but we include uninteracted period dummies in all models to control for the sharp expansions in imprisonments. The use of these period dummies in all analyses means we use either two-factor fixed-effects or mixed fixed-effects, random-effects estimation.

MEASUREMENT OF EXPLANATORY VARIABLES

We measure Republican strength with an index created by multiplying a dummy coded 1 for the presence of a Republican governor with the percentage of Republicans in state legislatures. Minority presence is assessed with the percentage of blacks and with the natural log of the percentage of Hispanics.[4] We assess economic inequality with a Gini index calculated by the Census on family incomes, while the unemployment rates are taken from the same source. We assess poverty with the log of the percentage of families below the poverty line.

Berry et al. (1998) view citizen ideology as the mean position on a liberal-conservative continuum. To construct a measure that varies over time, they identify the ideological position of each member of Congress from a state with interest group ratings by the Americans for Democratic Action and COPE. These pressure groups

computed each representative's liberalism-conservatism score from their voting record. Berry et al. estimate citizen ideology within each congressional district in a state with the ideology score for district incumbents and an estimated score for each incumbent's last challenger. Incumbent ideology scores are combined with estimated challenger ideology scores weighted by within district vote margins to measure congressional district ideology. Berry et al. then compute state scores on liberalism-conservatism with the mean scores for all congressional districts in a state.

Following a useful innovation in Greenberg and West (1998), we measure religious fundamentalism in the states with the natural log of a scale created by Morgan and Watson (1991) based on state religious affiliations enumerated by Quinn et al. (1982). This variable is measured only in 1980, although Newport (1979) suggests that church membership is extremely stable. Finally, the regional dummies are coded from census definitions. Because religious fundamentalism and the regional dummies do not change from one period to the next, we must use random-effects models to assess their effects.[5]

The degree to which state populations live in large communities of 50,000 or more is measured with the percentage living in MSAs. A state's tax base is operationalized with the log of mean incomes in 1987 dollars. Although we tried other crime rate combinations, the natural log of the violent crime rates is the most effective measure. We follow Greenburg and West (1998) and use a dummy variable coded "1" if a state had determinate sentencing provisions based on the codes in Marvell and Moody (1996).[6]

SPECIFICATION

Except for regional dummies and the political ideology measure (which assigns higher scores to more liberal states), the coefficients on all explanatory variables should be positive. One of the more general specifications of the fixed-effects panel models that do not include interactions with time therefore is:

$$Prison\ population = b_0 + b_1\ Repub + b_2\ Ideology + b_3\ Black + b_4\ Hispanics$$
$$+ b_5\ Inequal + b_6\ Unemployment + b_7\ Metro + b_8\ Violent\ crime + b_9\ Tax\ base$$
$$+ b_{10}\ DSL + b_{11}\ Yr_{1980} + b_{12}\ Yr_{1990} \tag{1}$$

where *Prsion population* is the natural log of the mean number of prisoners per 100,000 population in the two years after a census, *Repub* is a dummy coded 1 if a state had a Republican governor times the percentage of Republicans in the legislature, *Ideology* is the Berry et al. measure of political ideology, *Black* is the percentage of blacks, *Hispanics* is the natural log of the percentage of Hispanics, *Inequal* is a Gini index computed on income, *Unemployment* is the unemployment rate, *Metro* is the percentage of state residents living in metropolitan areas, *Violent*

TABLE 1: The Means and Standard Deviations Over Time and Across States.

	Mean 1970	Mean 1980	Mean 1990	Overall Standard Deviation	Cross State Standard Deviation	Over Time Standard Deviation
Ln Prisoners per capita	4.239	4.840	5.471	.684	.437	.529
Republican Strength	21.520	19.794	17.435	24.637	16.694	18.222
Ln percent Hispanic	.388	.784	.976	1.161	1.125	.316
Percent black	8.778	9.182	9.612	9.200	9.235	.710
Ln violent crime rate	5.384	5.964	6.110	.692	.603	.347
Percent unemployed	4.874	6.786	6.366	1.716	1.270	1.164
Economic enequalty (Gini)	.359	.360	.395	.027	.020	.019
Percent residents in MSAs	61.488	61.364	66.914	22.661	22.525	3.599
Ln percent poor families	.823	.818	.792	.149	.140	.055
Tax base (Ln mean income)	10.289	10.218	10.362	.163	.146	.075
Determinate sentencing laws	.000	.140	.200	.318	.239	.212
Political ideology	44.589	42.406	48.222	15.980	14.761	6.356
Ln fundamentalism	-2.222	-2.222	-2.222	1.295	1.295	.000

crime is the natural log of the violent crime rate, *Tax base* is the natural log of real mean household incomes, *DSL* is a dummy scored 1 if a state had a determinate sentencing law, and *Yr* is a dummy is coded 1 for that year.

In additional fixed-effects models we estimate the period-specific effects of some explanatory variables by testing for interactions between these variables and the two period dummies. A general specification of these models is:

$$
\begin{aligned}
\textit{Prison population} = {}& b_0 + b_1 (\textit{Repub} \times Yr_{1990}) + b_2 (\textit{Repub} \times Yr_{1980}) + b_3 \textit{Repub} \\
& + b_4 \textit{Ideology} + b_5 (\textit{Hisp} \times Yr_{1990}) + b_6 (\textit{Hisp} \times Yr_{1980}) + b_7 \textit{Hisp} \\
& + b_8 (\textit{Black} \times Yr_{1990}) + b_9 (\textit{Black} \times Yr_{1980}) + b_{10} \textit{Black} + b_{11} \textit{Violent crime} \\
& + b_{12} \textit{Unemp} + b_{13} \textit{Inequality} + b_{14} Yr_{1980} + b_{15} Yr_{1990}
\end{aligned} \tag{2}
$$

where all variables are defined as above, and b_3, b_7, b_{10}, b_{14} and b_{15} are the coefficients on all required main effects. Because their scores do not change from one period to the next, we conclude the analyses by using three regional dummies and religious fundamentalism in random-effects models that use specifications otherwise similar to those in equation 2.

TABLE 2: Fixed-Effects Estimates of the Determinants of U.S. State
Imprisonment Rates[a]

	1	2	3	4
Intercept	2.8268***	2.4537*	-.7324	-2.6174
	(.7782)	(1.0406)	(3.8786)	(3.8437)
Republican srength[b]	.1369	.1428	.1407	.1236
	(.0879)	(.0889)	(.0891)	(.0869)
Political ideology	-.0047*	-.0045*	-.0047*	-.0056*
	(.0027)	(.0028)	(.0028)	(.0027)
Ln percent Hispanic	.1164	.1224	.1080	.0736
	(.0807)	(.0817)	(.0835)	(.0823)
Percent black	.0455*	.0399	.0392	.0378
	(.0262)	(.0283)	(.0283)	(.0275)
Ln violent crime rate	.1693	.1588	.1541	.1430
	(.1116)	(.1137)	(.1140)	(.1108)
Percent unemployed	.0199	.0174	.0155	.0160
	(.0200)	(.0206)	(.0208)	(.0202)
Percent residents in MSAs	.0023	.0029	.0025	.0037
	(.0064)	(.0065)	(.0066)	(.0064)
Economic inequalty (Gini)	—	1.2187	1.5820	2.9084
		(2.2457)	(2.2891)	(2.2868)
Tax base (Ln mean income)	—	—	.3047	.4471
			(.3573)	(.3518)
Determinate sentencing laws	—	—	—	-.2067**
				(.0827)
1 if 1980	.3948***	.4040***	.4370***	.4912***
	(.0924)	(.0943)	(.1021)	(.1015)
1 if 1990	.9881***	.9516***	.9343***	.9420***
	(.1084)	(.1280)	(.1296)	(.1260)
R^2	.795***	.803***	.804***	.807***
(N = 150)				

Note: Standard errors are in parentheses.

[a] State fixed effects are not included in the R^2

[b] To facilitate comparisons, all coefficients on Republican Strength have been multiplied by 100.

* p ≤ .05   ** p ≤ .01   *** p ≤ .001 (one-tailed tests except for determinate sentencing)

TABLE 3: Fixed-Effects Models of the Relationships between Explanatory Variables and U.S. State Imprisonment Rates that Test for Time-Varying Relationships[a]

	1	2	3	4
Intercept	3.6107***	3.6453***	3.8325***	3.6126***
	(.6847)	(.7289)	(1.0111)	(.9805)
Republican strength $_{1990}$	.5823***	.5866***	.5920***	.6088***
	(.1837)	(.1871)	(.1892)	(.1829)
Republican strength $_{1980}$	.4065*	.4155*	.4215*	.4403**
	(.1726)	(.1844)	(.1868)	(.1806)
Republican strength [b]	-.1269	-.1302	-.1359	-.1544
	(.1230)	(.1258)	(.1282)	(.1241)
Political ideology	-.0103***	-.0103***	-.0103***	-.0109***
	(.0032)	(.0032)	(.0033)	(.0032)
Ln percent Hispanic $_{1990}$	.0599*	.0597*	.0638*	.0729*
	(.0328)	(.0330)	(.0366)	(.0356)
Ln percent Hispanic $_{1980}$	.0071	.0070	.0075	.0189
	(.0356)	(.0358)	(.0360)	(.0351)
Ln percent Hispanic	.0986	.1004	.0974	.0713
	(.0774)	(.0788)	(.0801)	(.0780)
Percent black $_{1990}$	.0149**	.0151**	.0151**	.0150**
	(.0050)	(.0052)	(.0052)	(.0050)
Percent black $_{1980}$	.0153***	.0155**	.0153**	.0151**
	(.0049)	(.0051)	(.0051)	(.0049)
Percent black	.0129	.0125	.0151	.0131
	(.0254)	(.0256)	(.0275)	(.0266)
Ln violent crime rate	.1786	.1755	.1789	.1725
	(.1080)	(.1107)	(.1120)	(.1083)
Percent unemployed	—	-.0029	-.0017	.0004
		(.0202)	(.0208)	(.0201)
Economic inequality (Gini)	—	—	-.6364	.2043
			(2.3686)	(2.3106)
Determinate sentencing laws	—	—	—	-.2013**
				(.0759)
1 if 1980	.2036*	.2070*	.2035*	.2289*
	(.1107)	(.1137)	(.1151)	(.1116)
1 if 1990	.7652***	.7685***	.7826***	.8016***
	(.1094)	(.1124)	(.1246)	(.1206)
R^2	.852***	.850***	.851***	.862***
(N = 150)				

Note: Standard errors are in parentheses.

[a] State fixed effects are not included in the R^2

[b] To facilitate comparisons, all coefficients on Republican Strength have been multiplied by 100.

* p ≤ .05   ** p ≤ .01   *** p ≤ .001 (one-tailed tests except on determinate sentencing)

Analyses

SUMMARY STATISTICS AND THE FIXED-EFFECTS RESULTS

Table 1 shows the means and the standard deviations over time and across states.[7] Because they provide an informative contrast with subsequent results, Table 2 presents the findings from fixed-effects models that ignore period-specific interactions. In addition to Republican strength and political ideology, the first model includes conventional sociological indicators such as Hispanic and black presence, the percentage living in MSAs, plus the unemployment and the violent crime rates. We add economic inequality in model 2 and assess two policy measures by including the tax base measure in model 3 and determinate sentencing in the final model.

The results support expectations that conservative states have higher imprisonment rates, but the relationship between racial threat and imprisonments is more inconsistent. The percentage of blacks matters only in the first analysis while the percentage of Hispanics never has a positive influence on the size of the incarcerated population. We find that states with determinate sentencing laws have reduced incarceration rates, but the amount of unemployment and violent crime in the states does not predict these rates.

These initial analyses suggest that more conservative views produce larger prison populations, but the remaining relationships are negligible. Yet we do not know what will happen if these relationships are not forced to remain constant over time. The next section describes findings from fixed-effects models that test for historically contingent associations.

Estimating Period-Specific Relationships

Table 3 shows the results when the coefficients on three explanatory variables are not forced to be equal across periods. We test Republican strength, Hispanic presence, and the percentage of blacks for interactions with time, since (in unreported analyses that are available on request) *we find no differences when we test for period interactions between imprisonments and all remaining variables.* Recall that period interaction terms are created by multiplying the explanatory variables in question with dummies coded 1 for year and then including these interacted explanatory variables and all main effects in models. If an interaction between an explanatory variable and time is significant after main effects are held constant, we can conclude that a particular explanatory variable had different relationships in different periods.

We again begin with a simple specification. Model 1 includes the violent crime rates, Republican strength interacted with the 1990 and the 1980 year dummies and the relevant main effects (the year dummies and Republican strength in all years), political ideology, percent Hispanic interacted with the 1990 and the 1980 dummy plus its main effects, and the percentage of blacks interacted with these

TABLE 4: Random-Effects Models — Relationships between Variables and Imprisonment Rates[a]

	1	2	3	4
Intercept	2.9292***	3.6932***	2.9011***	2.8170***
	(.3153)	(.6937)	(.3330)	(.2965)
Republican strength $_{1990}$	.4794**	.4989**	.4992**	.4921**
	(.1717)	(.1734)	(.1743)	(.1683)
Republican strength $_{1980}$	.4007**	.4104**	.4388**	.4145**
	(.1627)	(.1685)	(.1670)	(.1626)
Republican strength	-.0605	-.0873	-.0649	-.0643
	(.1103)	(.1131)	(.1112)	(.1076)
Political ideology	-.0073***	-.0075***	-.0076***	-.0076***
	(.0021)	(.0022)	(.0022)	(.0020)
Ln percent Hispanic $_{1990}$	.0609*	.0756*	.0613*	.0814**
	(.0324)	(.0346)	(.0327)	(.0324)
Ln percent Hispanic $_{1980}$	.0098	.0131	.0106	.0297
	(.0339)	(.0343)	(.0342)	(.0338)
Ln percent Hispanic	.0161	.0204	.0126	.0059
	(.0341)	(.0351)	(.0381)	(.0322)
Percent black $_{1990}$	.0119**	.0116**	.0123**	.0109**
	(.0044)	(.0045)	(.0045)	(.0043)
Percent black $_{1980}$	.0157***	.0145***	.0163***	.0144***
	(.0045)	(.0046)	(.0046)	(.0045)
Percent black	.0020	.0044	-.0006	.0008
	(.0055)	(.0061)	(.0063)	(.0051)
Fundamentalism	.0903**	.1038***	.0835*	.0902***
	(.0300)	(.0342)	(.0411)	(.0278)
Ln violent crime rate	.3390***	.3263***	.3472***	.3565***
	(.0636)	(.0728)	(.0647)	(.0596)
Percent unemployed	—	.0081	—	.0094
		(.0152)		(.0142)
Percent residents in MSA	—	.0000	—	—
		(.0019)		
Economic inequality (Gini)	—	-1.9878	—	—
		(1.6200)		
North East	—	—	-.0229	—
			(.1209)	
Midwest	—	—	-.0534	—
			(.0959)	
South	—	—	.0522	—
			(.1177)	
Determinate sentencing laws	—	—	—	-.2377***
				(.0618)
1 if 1980	.1505*	.1481	.1335	.1524*
	(.0868)	(.0918)	(.0884)	(.0852)
1 if 1990	.7427***	.7926***	.7341***	.7589***
	(.0816)	(.0937)	(.0828)	(.0805)
R^2	.891***	.894***	.894***	.905***
(N = 150)				

[a] To facilitate comparisons, all coefficients on Republican Strength have been multiplied by 100.
*p ≤ .05   ** p ≤ .01   *** p ≤ .001 (one-tailed tests except on determinate sentencing)

two years plus its main effects. In model 2 we add the unemployment rate. In model 3 we add economic inequality, while in the last model we add the presence of determinate sentencing laws to the prior explanatory variables.

These periodized results are far more interesting than the time invariant findings reported in Table 2. Significant interactions between Republican strength and period after the sharp increase in Republican rhetoric about law and order appear in all four models. We find historically contingent evidence for threat effects as well. After the substantial growth in the Hispanic population, the percentage of Hispanics had more substantial relationships with the imprisonment rates in 1990. The percentage of blacks now matters in all four analyses, but the evidence from these superior specifications shows that this explanatory variable's relationship with the incarceration rates also becomes stronger with the passage of time. When the coefficients on these explanatory variables are not forced to be identical in different periods, we find substantial increases in the explanatory power of the models.

The controls in these fixed-effects models are extensive. As a result of these more extensive controls, a comparison of the coefficients across rows in Table 3 shows that the point estimates are similar, while the results of the significance tests are virtually identical. Because unmeasured time-invariant state differences and over-time variation are held constant in these two-factor fixed-effects models, it is not easy to argue that these findings are spurious.

Compared to the prior analyses that ignored historical contingency, these results offer much stronger support for theoretical predictions based on political perspectives about punishment.[8] In accord with expectations, after the increased emphasis on law and order by the Republican party, the relationship between imprisonments and Republican strength becomes much stronger. Period-specific interactions also suggest that the relationship between the presence of two minority groups and the incarceration rates intensifies with the passage of time. Yet we still do not know if religious fundamentalism or region explain punitive reactions to crime.

ESTIMATING THE INFLUENCE OF FUNDAMENTALISM AND REGION WITH RANDOM EFFECTS

Random-effects panel models can assess the effects of time-invariant explanatory variables. This estimation procedure will let us estimate the influence of religious fundamentalism and region even though their scores do not vary over time. When these time-invariant indicators are included in random-effects models, the Hausman test for differences in the coefficients estimated by random- and fixed-effects procedures can tell us if these time-invariant explanatory variables capture the omitted effects that were automatically held constant by the state dummies in the fixed-effects models.

The random-effects analyses reported in Table 4 use specifications that are almost identical to those employed in the fixed-effects models in Table 3. In model 1

we enter the same interactions and main effects used in model 1 of Table 3, but we now can include religious fundamentalism. In model 2 we again add the unemployment rate, economic inequality, and the percentage living in MSAs. In model 3 we drop ineffective controls and add dummies for region. In the last model we revert to the baseline specification in model 1, but we add the dummy scored 1 for the presence of determinate sentencing legislation.

The findings are similar to those reported inTable 3 with one theoretically important exception. The results invariably suggest that imprisonment rates are higher in jurisdiction where religious fundamentalism is most prevalent. Model 2 shows that this result and the other findings persist after three standard controls are added. In model 3 we find no evidence that regional differences matter. Chi-square tests show that the joint effects of these regional dummies are insignificant. If we instead use eight dummies to represent the nine census subregions, we find the same negligible relationships. The last model shows that sentencing legislation reduces the relative size of the incarcerated population. The findings in these analyses persist if other mixes of the explanatory variables are used. For example, adding the three regional variables to the longer list of variables in model 2 or using the determinate sentencing indicator in other (unreported) models leaves the theoretical implications intact.

Hausman tests that gauge whether there are significant differences between coefficients from fixed- and random-effects models suggest that the coefficients estimated by these two statistical procedures do not differ *as long as religious fundamentalism is included in the random-effects models*. These repeated findings give us reason to believe that the religious fundamentalism variable captures most of the time-invariant state attributes that are automatically controlled by state dummies in the fixed-effects models.[9] ·

We again find that Republican strength and minority group presence explain the imprisonment rates with relationships that become stronger over time. Yet these random-effects analyses produce one additional theoretically interesting relationship that could not be investigated with a fixed-effects approach. The findings reported in Table 4 suggest that greater religious fundamentalism leads to higher incarceration rates.

ADDITIONAL METHODOLOGICAL CONSIDERATIONS

We tried other variables in unreported analyses. Including the tax base measure in the models that test for statistical interactions does not alter the results. State divorce rates, density, the percentage of residents 15 to 24, and the poverty rates or separate black and white poverty rates do not matter, and their inclusion does not alter the theoretical implications. Replacing violent crime rates with murder rates or with combined murder and robbery rates as well as using the total crime or the property crime rates does not change the findings, but these alternative crime measures reduce explanatory power. When we use the presence of self-identified Republicans

in the equations, this common measure of partisanship in political science (Miller 1991) has no discernable effects. Narrow majorities in the legislature also do not matter.

We reestimated the models in Table 4 with a population averaged approach (Liang & Zeger 1986). This procedure lets us correct for potential heteroskedasticity with White's (1980) adjustment, but the results do not change. AR1 corrections for autocorrelation when the models are estimated this way give results with identical implications. Correcting the standard errors for within region interdependence and spatial propinquity produces only minor changes in the significance tests, so we conclude that spatial autocorrelation probably is not biasing the results.

The results remain stable when we use diverse specifications and different estimation procedures.[10] Because we include dummies for both time and states in the fixed-effects models and hold constant multiple explanations, it is difficult to believe that omitted variables are distorting the findings. These considerations suggest that we have isolated the major social and political processes that produce state differences in imprisonment rates.

Discussion

FINDINGS

With many explanations held constant, the results show that expansions in the strength of the Republican party and stronger conservative values produce subsequent increases in the prison population. Despite the theoretical interest in this relationship, few statistical findings show a connection between politics and formal punishment. The stronger relationships between Republican strength and incarcerations in different periods provide added support for expectations based on the history of recent political events.

While this analysis focuses on direct measures of political effects, we find evidence for alternative explanations about ethnic divisions that are indirectly political. These threat explanations are political because effective demands for harsh punishments that result from shifts in minority presence must be directed at the political officials who can alter the criminal codes or sentence offenders. As threat theorists would expect, the results show that states with the largest black populations have higher incarceration rates after violent crime and other explanations have been held constant. The tests for period-specific relationships confirm expectations that the increased political emphasis on black street crime led to stronger relationships between African American presence and the incarceration rates.

The period specific relationship between Hispanic presence and imprisonments provides added evidence for such indirect political threat explanations. In contrast to blacks, the Hispanic population increased substantially during the analysis

period. If ethnic threat theories are correct, the recent sharp growth in Hispanic presence should lead to stronger relationships between the percentage of Hispanics and the imprisonment rates during the period near the end of the analysis. The results invariably support this expectation, but they also suggest that black threat has a more substantial influence on U.S. imprisonment rates. Such contrasts in minority threat effects should not be surprising given the intense political disputes about race throughout the history of the U.S. While these threat effects are robust, they do not appear to be as strong as the relationships between the strength of the Republican party and state imprisonment rates.

We also find that states where membership in fundamentalist churches is greatest are likely to imprison larger percentages of their population. It is unfortunate that the only available measure does not capture shifts in religious fundamentalism over time. Yet researchers claim that church membership is largely inherited (Newport 1979), so changes in fundamentalist strength in large aggregates like states should be modest. Our results suggest that findings about the relationship between fundamentalist views and punitive attitudes (Curry 1996; Grasmick et al. 1992; Grasmick & McGill 1994) have wider consequences. Religious fundamentalism evidently provides an important macro level explanation for punitive outcomes as well.

The results show that determinate sentencing legislation reduces the size of incarcerated populations. This finding should not be surprising for several reasons. First, the history of this legislation shows that these provisions often were deliberately adopted to control costs by limiting prison populations (Marvel & Moody 1996). Second, even though Marvel and Moody use a different pooled-time series design than the one used in this study, they find equivalent negative relationships. Studies of public policy often reveal unintended effects. Once a determinate sentencing law is in place and not readily altered, the evidence in both this and the Marvel and Moody study suggests that such laws place limits on the forces that would have produced higher incarceration rates if such laws had not been present. Perhaps the limits on incarcerations imposed by determinate sentencing laws prevail even in jurisdictions where officials did not intend this result when these laws were enacted.

Prior research based on a time-series analysis of national data shows that increased Republican strength at the national level led to expansions in prison admissions (Jacobs & Helms 1996), but these relationships were not examined in the states where most of these decisions are made. Table 1 shows that variation in the imprisonment rates across states and over time is almost equal, so these substantial contrasts in cross jurisdictional incarceration rates should not be ignored.

Their highly aggregated time-series data did not let Jacobs and Helms detect the effects of two kinds of ethnic threat. This analysis of national level data also obscured the theoretically interesting historically contingent shifts in the strength of political and ethnic relationships, and this choice meant that Jacobs and Helms

could not assess the influence of political ideology or religious fundamentalism. The absence of these disadvantages together with the inferential benefits conferred by fixed-effects estimation and the larger number of cases in this study probably make the findings in this investigation more definitive.

WIDER IMPLICATIONS

Claims that political factors influence punishments are common in the theoretical literature (Foucault 1977; Garland 1990), but little empirical work has been done on this issue. As one would expect from this emphasis and from the Republican party's increasing stress on law and order, our findings suggest that growth in the political strength of this party produce higher incarceration rates. A claim that Republican political strength leads to harsh reactions to street crime is supported by additional findings. One objection to this conclusion runs that Republican political strength may be based on preexisting conservative views that are the primary cause of punitive reactions. Yet we find that the strength of the Republican party continues to explain the imprisonment rates after citizen ideology and membership in fundamentalist Protestant churches have been held constant.

Such results help us understand the puzzling sharp increases in imprisonments during a time when the crime rates stayed constant or fell. More conservative political parties in two party systems face a persistent dilemma because their electoral base is smaller than their rival's. Because the less affluent outnumber the prosperous, parties whose economic policies primarily help the prosperous (Blank & Blinder 1986; Hibbs 1987) will find that electoral success is problematic. One solution is to stress social issues like law and order to capture increased support from working and lower middle class voters who have greater reasons than the affluent to resent street crime. The results suggest that the Republican party's reliance on such tactics explains a substantial part of the great expansion in imprisonments from the early 1970s until 1990.

Repeated findings that Republican political strength produces increased incarceration rates after indicators of public conservatism have been held constant support a strategic explanation. While a law and order strategy does not contradict Republican views, the persistent relationship between Republican strength and incarceration rates after public ideology is controlled implies that this law and order emphasis was not imposed on Republicans by the public. Instead, perhaps Republican political strategists found that campaigns based on law and order would create a wedge issue that could be used to break the rival Democratic voting coalition apart (Edsal & Edsal 1991).

It is plausible that in order to win elections, Democrats have been forced to match the Republican emphasis on law and order. Yet such a matching effect would reduce the coefficients on Republican strength. If differences between Republicans and Democrats in the emphasis they placed on law and order substantially

diminished toward the end of the years in our sample, we should *not* find that Republican strength is an increasingly strong predictor of the incarceration rates especially after so many other effects have been held constant, yet that is exactly what we find. Claims that the Democrats have matched Republican tendencies to emphasize harsh reactions to street crime have not been supported in this study. A national analysis by Jacobs and Helms (2001) suggests that after 1990 some important Democrats began to adopt Republican tactics and emphasize punitive reactions to crime, but we find no evidence for this before 1990.

More generally, the findings show that political relationships that explain incarceration rates operate on three levels. The Republican political strength results support state theories about the entrepreneurial activities of political officials. Because two ethnic divisions account for differences in the incarceration rates, we find that more conventional hypotheses that stress social cleavages explain the imprisonment rates as well although these political threat effects are not as strong as the direct political relationships. Finally, the influence of political ideology and religious fundamentalism suggests that differences in public values also help account for jurisdictional differences in the proportion of the population that is incarcerated.

Yet the historically contingent relationships uncovered in this study probably are the most theoretically noteworthy. Because theorists claim that a political emphasis on crime increases punishment (Garland 1990; Savelsberg 1994), we see if the political relationships are strengthened after Republican candidates increased their rhetoric about law and order (Davey 1998). The results support these contingent expectations. The associations between Republican strength and imprisonments become stronger at the end of the analysis period. Such results do not contradict findings (Beckett 1997) that pronouncements by public officials about law and order create a political climate that encourages harsh measures. The results also suggest that those who have criticized statistical research because it ignores the contingent nature of historical process may have overstated their case.

But there may be limits on the generality of these findings. In comparison to the European democracies, the U.S. has an exceptional political system with comparatively frail parties, nominations determined by primary elections, and a relatively weak bureaucracy. Such conditions probably give U.S. voters greater influence over decisions that are largely made by bureaucratic experts in other advanced democracies. Perhaps the political factors that explain imprisonment rates so well in a populistic democracy like the U.S. have less effect on this outcome in the more bureaucratic European states (Savelsberg 1994; Windlesham 1998).

This study nevertheless has isolated historically contingent associations that lead to two theoretically informative generalities about the determinants of the imprisonment rates in one advanced democracy. First, the results support those theorists who claim that incarceration is one method the modern state uses to manage latent political conflicts created by racial and ethnic divisions. Second, we

find robust evidence that conservative shifts in political climate are likely to strengthen punitive reactions to crime. More generally, the findings suggest that research that makes a concerted effort to assess diverse political and economic explanations will provide a better understanding of how historical forces combine to shape the primary punishment used in advanced states.

Notes

1. In addition to its other advantages, using periods separated by multiple years in a pooled time-series design reduces serial correlation and the biases created by measurement errors in explanatory variables (Johnston & DiNardo 1997). We acknowledge, however, that the decennial census years we are forced to use may not exactly correspond to peaks in Republican rhetoric about law and order.

2. Northern and less populated Midwestern states had the fewest prisoners relative to their population. In 1971-1972 the five states with the lowest imprisonment rates were North Dakota with the least, New Hampshire, Massachusetts, Hawaii, and Minnesota. In 1991-92 states with the fewest prisoners (again starting with the lowest score) were North Dakota, Minnesota, West Virginia, Maine, and Vermont. States with the most prisoners tended to be in the South or West. In the early 1970s the five states with the highest incarceration rates were Florida, Texas, Oklahoma, North Carolina, and Georgia with the highest rate. In 1991-92 Arizona had the most prisoners per capita followed by Oklahoma, Nevada, Louisiana, and South Carolina. It remains to be seen if region accounts for these contrasts after we use theoretically derived hypotheses to explain incarcerations.

3. Fixed-effects models automatically correct for any unmeasured state attributes that do not vary over time by including dummies for all states but one in the model. This makes estimates from fixed-effects models robust when unmeasured time-invariant case attributes that are correlated with explanatory variables influence the dependent variable. Random-effects models assume normal error terms and correct for the reduced information given by scores for the same state at different times by weighting with residuals produced by fixed-effects and between regressions (or OLS models estimated using the mean of each variable's three yearly scores for each state). Estimates from random-effects models are more efficient than fixed-effects counterparts, and random-effects estimates are more robust when measurement error is present. Both procedures require standard assumptions about the absence of omitted variables and correlated errors, but this claim is more likely to be true when relationships are estimated with fixed-effects (for more discussion see in rough order of difficulty, Pindyck & Rubinfeld 1998; Johnston & DiNardo 1997; Baltagi 1995; Greene 1997). We use the fixed- and random-effects routines in Stata version 6, but Limdep version 7 produces theoretically equivalent results.

4. We use separate indicators for these concepts because these two minority groups differ substantially in their presence in different states and because adding these two measures together would require the implausible assumption that the coefficients on both indicators are identical.

5. The information required to assess additional theoretically interesting explanations either is unavailable for the entire analysis period or it cannot be obtained at all. For example, an attempt to isolate determinants of state imprisonments of African-Americans must be limited to the years after 1980. The primary goal of this analysis is to see if political explanations matter. Because the three periods we analyze are best suited for this purpose, but information about black prisoners is not available in the first period, the determinants of black incarcerations should be the subject of a different study perhaps undertaken after the necessary information in the 2000 census becomes available.

6. Some researchers try to assess the influence of drug crimes with arrests for drug offenses. Yet research suggests that drug arrest rates in a city fluctuate substantially after shifts in internal departmental directives about the crimes that should be given the most attention (Rubenstein 1973). Drug arrest rates seem to respond to both the political environment of law enforcement agencies and the resulting pressures on officers as well as the number of drug crimes in a city, but the exact weighting of these factors is unclear. Because we do not know what drug arrest rates measure, they should not be included in an analysis like this.

7. Percentage change scores calculated on the two minority presence variables contrast sharply. The mean percentage of blacks across states grew by only 4.6% from 1970 to 1990, but the unlogged percentage of Hispanics expanded by 47.9% during this period. Across state means for percent Hispanic before it is logged are: 1970-3.56%, 1980-4.30%, and 1990-5.27%. The yearly means for Republican strength are well below 50%, but many states are scored "0" on this index because they did not have a Republican governor. The mean tax base diminished in 1980 because across-state mean income *corrected for inflation* fell from its value in 1970. The standard deviations across time and space are calculated with the XTSUM procedure in Stata version 6.

8. The contrasting results in these two tables let us make another important distinction. In an inventive paper that used a similar research design to address related issues, Greenberg and West (1998) report results much like those in Table 2. It is unfortunate that Greenberg and West did not use interactions that let their coefficients vary by period. Other methodological differences between this study and theirs include their use of a less comprehensive measure of Republican strength and a political ideology indicator that is time-invariant and measured after 1977. The latter difficulty meant Greenberg and West had to assume that state ideologies did not change. Greenberg and West apparently find that only African American presence is a consistent predictor of state imprisonment rates, but the improved design used in this study lets us detect many other theoretically important relationships. We nevertheless acknowledge an important dept to Greenberg and West because they provide such a useful literature review and some innovative methods that helped us resolve vexing problems.

9. Sociologists may question the number of regressors in these models, but Johnston (1984) states that exhaustive specifications are preferable, while Blalock (1979) says we should dramatically increase the number of regressors in our equations. The ratio of explanatory variables to cases in many econometric studies is far greater than it is in this analysis. Johnston (1984:262) is worth quoting on this issue. He says "it is more serious to omit relevant variables than to include irrelevant variables since in the former case the coefficients will be biased, the disturbance variance overestimated, and

conventional inference procedures rendered invalid, while in the latter case the coefficients will be unbiased, the disturbance variance properly estimated, and the inference procedures properly estimated. This constitutes a fairly strong case for including rather than excluding relevant variables in equations. There is, however, a qualification. Adding extra variables, be they relevant or irrelevant, will lower the precision of estimation of the relevant coefficients." It follows that inclusive specifications will lead to more conservative significance tests.

10. This stability suggests that collinearity does not seem to be distorting these findings. The similarity of the coefficients and the identical significance test results for the models within each table despite extreme changes in the specifications supports this conclusion.

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