THE POLITICAL SOCIOLOGY OF THE DEATH PENALTY: A POOLED TIME-SERIES ANALYSIS

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Despite the interest in the death penalty, no statistical studies have isolated the social and political forces that account for the legality of this punishment. Racial or ethnic threat theories suggest that the death penalty will more likely be legal in jurisdictions with relatively large black or Hispanic populations. Economic threat explanations suggest that this punishment will be present in unequal areas. Jurisdictions with a more conservative public or a stronger law-and-order Republican party should be more likely to legalize the death penalty as well. After controlling for social disorganization, region, period, and violent crime, panel analyses suggest that minority presence and economic inequality enhance the likelihood of a legal death penalty. Conservative values and Republican strength in the legislature have equivalent effects. A supplemental time-to-event analysis supports these conclusions. The results suggest that a political approach has explanatory power because threat effects expressed through politics and effects that are directly political invariably account for decisions about the legality of capital punishment.

WHY IS THE death penalty present in some jurisdictions but not in others? No other contemporary punishment is more severe, yet the literature is almost silent about the social and political influences that affect the legality of this punishment. A few informative case studies about attempts to change death-penalty provisions in particular states have been published (Galliher and Galliher 1997; Haines 1996; Koch and Galliher 1993), but general tests of theoretically derived hypotheses about this issue do not seem to exist. This gap in the literature is puzzling because other aspects of the death penalty have been intensely investigated. Many studies assess the racial and other determinants of death sentences (Paternoster 1991). The literature on deterrent effects is equally substantial (Paternoster 1991). Yet little is known about the social and political forces that make capital punishment legal.

In part because executions are such vivid demonstrations of state power, we focus on the political sociology of this punishment. Garland (1990) reveals some of the conceptual promise of a political sociology of punishment when he writes, "Penal law, at base, concerns itself with social authority and the governing claims of those with power. It reinforces these claims by coercive sanctions as well as symbolic displays" (p. 123). Both Foucault (1977) and Garland (1990) see executions as symbolic rituals that magnify political authority by forcibly reminding the populace of the immense coercive power behind the law. This theoretical interest in the links between politics and punishment sug-

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gest that a void can be filled by using concepts borrowed from political sociology to explain the presence or absence of the death penalty.

The Weberians and the neo-Marxists, who stress coercive explanations for order, often see the criminal law and the state agencies that administer it as primarily serving the interests of the privileged. In this political view, an important (but not the only) use of the law is to maintain control over the "dangerous classes" who threaten social order (Chambliss 1964). If criminal sanctions are partially shaped by the need to control an underclass with much to gain from violence that reallocates resources, enhanced state coercion can be expected where this threat is most intense. The death sentence therefore should be legal in jurisdictions with substantial racial or economic cleavages. We test claims that the legality of the ultimate punishment is shaped by such social divisions by estimating the relationships between underclass threat and the presence of the death penalty after controlling for other plausible explanations.

Punishment also may respond to completely political influences. Conservatives and political parties that are more conservative than their rival parties are more likely than their opponents to support harsh sanctions. Jacobs and Helms (1996) analyze changes over time in prison admissions and find that increases in the political strength of the law-and-order Republican Party produced a subsequent growth in these rates. Sutton (2000) extends this finding by showing that expansions in the strength of conservative political parties lead to larger prison populations in five advanced nations. These results suggest that the strength of the more conservative Republican Party or the strength of conservative public sentiments will influence decisions about the legality of capital punishment.

A related question we can begin to address concerns the primary source of political influence. In many democracies the abolition of the death penalty was imposed on a reluctant public by political leaders (Zimring and Hawkins 1986). A British lawmaker comments that "in the case of capital punishment, legislators lead from the front" (Buxton 1974:245). But the United States is an exceptional democracy—with frail parties, a weak bureaucracy, and democratically accountable state governments that decide many important criminal justice policies (Savelsberg 1994). These conditions give the U.S. public far more control over social policy than citizens have in more centralized democracies (Savelsberg 1994). In a populist democracy like the United States, a politically inactive public can be aroused by an intensely moral issue like capital punishment (Koch and Galliher 1993; Zimring and Hawkins 1986). The legality of the death penalty, rather than being imposed from above, may depend on public values and citizen pressures that force politicians to act. We examine these explanations by assessing the links between political partisanship, public ideology, and the presence or absence of capital punishment.

Hood (1998) lists many potential accounts, but he finds little evidence about this issue, so we hope that this analysis of the presence of the death penalty will fill an important gap in the literature. Because executions cannot occur in the absence of legalization, we begin to develop a political sociology of the ultimate sanction by focusing on the political and social conditions that give state courts the right to impose this sentence. Results from research designs that assess many hypotheses are more accurate (Johnston 1984, note 10). We therefore present exhaustive analyses, but this strategy means the next section must discuss multiple explanations. To show that conclusions about such an important issue do not depend on method, we present findings based on multiple specifications and different research procedures.

THEORETICAL EXPLANATIONS

Three general perspectives inform our understanding of state behavior. A common approach in political sociology treats public outcomes as the result of external social and economic forces that influence state decisions. This orientation yields hypotheses about the effects of social divisions. We begin by discussing the explanatory power of racial cleavages and economic inequality, partly because these threat explanations are so prominent in the literature. A micro ap-

proach focuses instead on individual belief systems. Political ideology may have strong effects on an intensely symbolic and moral issue like capital punishment. The most recent approach, exemplified by Evans, Rueschemeyer, and Skocpol (1984), suggests that the parochial interests of state officials help determine political outcomes. Because politicians often act strategically and choose issues that increase their political support, we discuss the rationale for accounts that stress partisan tactics. We conclude with the reasons for holding alternative explanations constant.

Racial, Ethnic, and Economic Divisions

Minority threat. Conflicts about race once were and perhaps still are the most salient feature of politics in the United States. Claims that the U.S. criminal justice system is not colorblind have stimulated many studies about the ascriptive determinants of punishment (for a review of literature on race and the death sentence, see Paternoster 1991). We use quantitative methods because no previous studies have used these methods to gauge the general relationships between minority presence and the legality of capital punishment.

Racial or ethnic threat theories suggest that enhanced minority presence leads to repression. Blumer (1958) and Blalock (1967) claim that dominant racial groups are intimidated by large minority populations. Greater minority presence threatens middle- and working-class whites, who respond with efforts to maintain their superior position. Fosset and Kiecolt (1989), Bobo and Hutchings (1996), and Taylor (1998) find that negative feelings about blacks are more common in areas with relatively more black residents. With the crime rate held constant, Liska, Lawrence, and Sanchirico (1982) find that fear of crime covaries with African American presence in cities. Many studies suggest that the perceived threats due to a larger racial underclass lead to enhanced efforts to control street crime. Cities with relatively large minority populations have more police officers (Jackson 1989; Jacobs 1979; Liska, Laurence, and Benson 1981), and higher arrest rates (Liska, Chamblin, and Reed 1984). Time-series work shows that expansions in nonwhite presence lead to increased spending on jails and prisons (Jacobs and Helms 1999).

These findings suggest that whites from all classes successfully demand enhanced criminal punishments in areas with the largest minority populations. Such results, and the added finding that white support for capital punishment is closely associated with prejudice against blacks (Barkan and Cohn 1994), leads to the expectation that: *A legal death penalty should be more likely in jurisdictions with high proportions of African Americans.* In many states Hispanics occupy a minority niche similar to that of blacks, so we also expect that: *The probability of a legal death penalty should be significantly greater in jurisdictions with relatively large Hispanic populations.*

Both theory and empirical findings suggest that relationships between threat and repressive outcomes should be curvilinear. Taylor (1998) shows that many associations between minority presence and the racial attitudes of whites depart from linearity, while Jackson (1989) finds nonlinear relationships between minority presence and police expenditures. Blalock's (1967 p. 128) theory predicts that minority threat will produce relationships between minority presence and repressive political decisions that are positive, but these relationships will have an increasing slope. Blalock claims that such an increasing slope will be more likely when a dichotomous political outcome is at issue. Reed (1972) and Corzine, Creech, and Corzine (1983) study lynchings and find support for Blalock's expectations about increasing slopes.

Entirely political considerations also suggest that these relationships will take the nonlinear form stipulated by Blalock. If racial divisions or other social forces explain the presence of the death penalty, these relationships must go through intervening but unmeasured political processes. Yet politicians avoid irreversible policies that cannot be altered incrementally because such issues produce unresolvable conflicts and unhappy constituents (Oberschall 1973). The competitive nature of the agenda-setting process in legislatures provides another reason to think that the relationships between social

threat and the legality of the death penalty will not be linear. Busy representatives are compelled to ignore many important matters that compete for their attention. The forces we study therefore must reach thresholds sufficient to generate enough political pressure to *make* the legislature consider capital punishment. Opposition provides another reason for thinking that these relationships will be discontinuous. Political resistance typically exhibits sharp increases after the actors who seek legislative change accumulate almost enough votes to prevail. For all of these reasons, we follow Blalock (1967) and the empirical findings and expect that the death penalty will be present or absent in a state if most of the forces we assess are above or below the thresholds needed to trigger successful political action.

ECONOMIC THREAT. Chambliss (1964), Chambliss and Seidman (1980), and Garland (1990) argue that punishment is shaped more by the menace of an economic underclass than by a racial or ethnic underclass. 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) write, "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" (p. 33). Because capital punishment is an extreme example of state coercion, the death penalty should be legal in the most economically stratified jurisdictions (Black 1989).

Garland (1990) completes this argument for a relationship between economic inequality and punishment by claiming, "Where social power and authority are structured upon class lines, . . . then punishment will reproduce the forms and figures of class even when its actions appear to transcend class divisions and protect those on the wrong side of the class divide" (p. 134). If Garland is right and punishment helps perpetuate an unequal class-based society, the most economically unequal jurisdictions with the greatest need for state coercion (Chambliss and Seidman 1980) should be more likely to legalize the death penalty. Although the key political actors could be from the middle- rather than the upper-class, Stone (1987), captures most of the reasons for this expectation when he writes, "[T]he criminal law was indeed in the last resort an instrument of the elite to protect their own and other people's lives by the use of selective terror" (p. 250).

The logic of exchange provides another theoretical link between the degree of economic inequality and punitive measures. In societies in which production is coordinated by markets, exchange imbalances will be common. Violence that redistributes resources is one method dependent populations may use to overcome their disadvantaged position in unbalanced exchange relationships (Blau 1964). This possibility should be obvious to prosperous citizens and to their political representatives. In areas in which economic differences between the poor and other groups are substantial, the menace of the "dangerous classes" will be greater and state officials may respond by legalizing the ultimate punishment.

The threat posed by an economic underclass is relational because it involves contrasts between the resources of middle- and upper-income groups and the least prosperous groups (Jacobs 1979). Sharp differences in economic resources should enhance feelings of relative deprivation. Economic inequality therefore produces both the motivation and greater potential rewards for violent acts that redistribute goods. If this economic threat explanation is correct, we can expect that: *Where economic inequality is most pronounced, jurisdictions will respond by making capital punishment legal.* Because disputes about the explanatory power of minority presence or economic inequality remain unresolved, we test both the racial-ethnic and the economic versions of threat theory.

EXPLICITLY POLITICAL EXPLANATIONS: IDEOLOGY AND PARTISAN STRATEGIES

IDEOLOGY. A different political account stresses public belief systems. A legal death penalty may be likely where conservative political ideologies that support harsh punishments are more prevalent. Despite the plausibility of this claim, research that assesses the relationship between conservative public ideologies and the stringency of the criminal codes does not seem to exist.

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

Instead of stressing social arrangements that eliminate lawful alternatives, conservatives believe that reprehensible individual choices are the primary explanation for crime (Burnham 1970; Thorne 1990). If most crime results from unfettered choices, increases in the expected costs of lawbreaking should be effective. This logic leads to the common conservative view that deterrence is the best remedy for lawless behavior. Thus, Molnar (1976) writes, "[I]f 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" (p. 47). Conservatives use the same logic to justify a claim that the threat of the death penalty will save many innocent victims from criminal predation.[1] Because they see human nature as fixed (Thorne 1990), many conservatives believe that the most vicious criminals cannot be reformed. Such pernicious and incorrigible offenders must be executed to ensure that they no longer can harm the innocent.

Liberals are much more optimistic about the potential for rehabilitation and believe that crime is caused by inequitable social conditions (Garland 2001; Thorne 1990). They are skeptical of harsh penalties and view social reform as the most effective and just remedy for lawlessness (Garland 2001; Taylor, Walton, and Young 1973). Studies invariably show that liberal values are closely associated with an aversion to harsh punishments and hostility to the death penalty (Brillon 1988; Langworthy and Whitehead 1986; Taylor, Scheppele, and Stinchcombe 1979; Van Dijk and Steinmetz 1988). Lakoff (1996) contends that this disagreement between liberals and conservatives about the morality of capital punishment is one of the most reliable dividing lines between these ideological camps. Because public support for the death penalty should be most intense where conservative values dominate, we expect that: *A legal death penalty should be more likely where conservative belief systems are stronger than liberal belief systems.*

PARTISAN STRATEGIES. A different set of political explanations focuses on the strategic behavior of politicians. Developments in political sociology suggest that political processes are not simple derivatives of social and economic arrangements (Evans et al. 1984). State managers often act autonomously and support policies that will enhance their parochial interests. Republican candidates can win elections by appealing to lower-middle-class and working-class voters who do not benefit from Republican economic policies (Blank and Blinder 1986; Hibbs 1987) if they campaign on a "wedge" issue—like law and order (Beckett 1997;

[1] Paternoster (1991) confirms the conclusions of virtually all other scholars by writing, "After years of research with different methodologies and statistical approaches, the empirical evidence seems to clearly suggest that capital punishment is not a superior general deterrent" (p. 241). Zimring and Hawkins (1986) give reasons for the public's continued support for capital punishment despite the absence of evidence that it is superior to imprisonment as a deterrent. They explain public reactions after this penalty was abolished in other democracies with the following analogy:

> In reality the death penalty is about as relevant to controlling crime as rain-dancing is to controlling the weather. So long as rain dances continue to be performed, as they have been since time immemorial, the belief that they have some influence on rainfall cannot be tested. When they cease to be performed, and the amount of precipitation remains unchanged in subsequent years, the ritual's influence dissipates. Similarly, as time passes after abolition and increases in violent crime do not materialize, the felt necessity of the capital sanction diminishes gradually. (1986:14–15)

Other scholars (e.g., Hood 1998) have claimed that repeated evidence showing that the death penalty is not a more effective deterrent than imprisonment has little effect on public support for the death penalty.

Edsal and Edsal 1991), which divides the Democratic coalition.

An antistreet-crime agenda lets Republicans covertly appeal to antiminority sentiments and attract less affluent voters who are more likely to be victims of street crime and who are more likely to live in or near areas in which violent crime is problematic. Statements by Nixon administration officials reveal that Republicans deliberately used such law-and-order appeals to gain support from racists (Edsal and Edsal 1991). By emphasizing street crime and other problems readily blamed on a racially distinct underclass, conservatives can win elections by capturing votes from less prosperous citizens who resent the underclass (Edsal and Edsal 1991). Such law-and-order campaign tactics will help conservatives win elections even though conservatives continue to support economic policies that primarily benefit their affluent core supporters (Blank and Blinder 1986; Hibbs 1987). Claims that the Democrats are "soft on crime" therefore became a central part of Republican political campaign appeals after 1964 (Chambliss 1999).

Republican officeholders have repeatedly increased the severity of legal sanctions. Multiple findings show that Republican political strength is associated with more repressive criminal justice outcomes (Jacobs and Carmichael 2001; Jacobs and Helms 1996, 1997, 1999). Because capital punishment has been a key issue in many state political campaigns (Constanzo 1997) and because Pierce and Radelet (1990–1991) claim that many politicians support the death penalty for strategic reasons, we expect that: *Jurisdictions with a Republican governor will be more likely to legalize the death penalty.* The same logic suggests that: *A legal death penalty will be more probable in states with substantial Republican strength in the legislature.*

Alternative Accounts: Controls for Social Disorganization and Violent Crime

Garland (2000) claims that enhanced demands for severe punishment are produced by social disorganization, which may account for the presence of the death penalty after serious crime rates have been held constant in the multivariate analyses. Some violence that results from disorganization may not be captured by the violent crime or murder rates, but this violence may lead to greater public anxieties or resentments and enhanced political pressures for harsh punishments (Garland 2001; Tyler and Boeckmann 1997). It is important to hold such factors constant because jurisdictions with relatively large minority populations or enhanced economic stratification may legalize the death penalty, but not due to racial or economic threat. The public anxieties or resentments produced by social disorganization often appear where minority presence and economic stratification is most substantial. If disorganization effects are not held constant, we could not rule out the possibility that citizens are responding to social disorganization rather than to racial or economic threat when they successfully pressure their political representatives to legalize capital punishment.

Sampson (1987) and Sampson and Groves (1989) find that cities with relatively large proportions of female-headed families have relatively high violent crime rates. Substantial unemployment may heighten demands for severe punishments because the prosperous view the unemployed as a threat (Chambliss 1964) or because greater unemployment magnifies resentments against underclass offenders. But states with relatively large populations that live in or near large cities should be less likely to retain the death sentence because rural citizens often hold more punitive views about the most appropriate punishments for serious crime (Hagan 1977).

Geographic mobility interferes with the formation of group ties and solidarity. Outsiders inspire hostility and fear (Hale 1996), while community stability and an absence of strangers strengthens within-group ties and empathic feelings toward one's neighbors (Hale 1996). Citizens in jurisdictions with remarkably few outsiders therefore may be less willing to support executions. It follows that jurisdictions with extremely high proportions of residents who were born in a state should not be as likely to legalize capital punishment.

Finally, it is crucial to hold the violent crime rates constant because greater politi-

cal support for capital punishment should be present where such crimes are more common. A primary justification for the death penalty is its purported deterrent effect on those who are tempted to use illegal force to commit predatory acts. States with relatively high violent crime or murder rates therefore should be more likely to let their courts impose the death penalty. In the analyses that follow we hold each of these control variables constant in at least one model.

METHODS

Research Design, Dependent Variables, and Estimation

Research design. In the primary analyses, we use state-level explanatory variables for 1970, 1980, and 1990 to explain the presence or absence of the death penalty in the 50 states in 1971, 1981, and 1991. Following researchers in public policy we use one-year lags. If the sample 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 and findings that would automatically favor some hypotheses over others, we limit the sample to these 150 state-years.[2] We begin the panel analyses in 1970 because the census did not enumerate Hispanics or calculate income inequality before then.

Dependent variables. In 1971, 41 states had the death penalty, but 9 did not. By 1981, the number of abolitionist states grew to 13; in 1991 the states without the death penalty grew to 14. Oregon legalized capital punishment between 1971 and 1981. New Jersey had the death penalty in 1971, but dropped capital punishment by 1981 only to make it legal again by 1991. Six other states (Kansas, Massachusetts, North Dakota, Rhode Island, New York, and Vermont) abolished capital punishment between 1971 and 1991. Eight states (Alaska, Hawaii, Iowa, Maine, Michigan, Minnesota, West Virginia, and Wisconsin) never legalized capital punishment. All remaining states retained the death penalty during the three periods we analyze.[3]

Four years after its 1972 *Furman v. Georgia* decision (408 U.S. 238 [1972]) that temporarily outlawed capital punishment, the Supreme Court reversed course. In its 1976 *Gregg v. Georgia* (96 Sup. Ct. 2902 [1976]), *Jurek v. Texas* (428 U.S. 262 [1976]), and *Proffit v. Florida* (428 U.S. 242 [1976]) decisions, the Supreme Court upheld the constitutionality of guided discretionary death-penalty statutes if states also added a separate independent procedure to determine sentence after a guilty verdict in capital cases.[4] Not all death-penalty states immediately complied with the substantial alterations to death-penalty law required by the Supreme Court. We present a supplemental event-history analysis of the time it took states to meet the Supreme Court's new requirements for a constitutional death penalty to find out if the factors associated with legalization isolated in the panel analyses explain how quickly the death penalty was "relegalized." If results from these supplemental time-to-event regression analyses support the panel results, these findings

[2] Including periods separated by multiple years in a pooled time-series design reduces serial correlation and the effects of measurement error (Johnston and DiNardo 1997).

[3] The over-time standard deviation in the presence of the death penalty (see Table 1) shows that enough variation is present to use it as the dependent variable in panel analyses. Information about the legality of the death penalty was collected from various editions of *The Sourcebook of Criminal Justice Statistics* and checked with data in Bowers (1984) and Zimring and Hawkins (1986). The death penalty is coded as legal in a state if it is authorized for any criminal act.

[4] Guided discretionary procedures attempt to achieve sentencing equity across courts within a state by requiring that standardized instructions be given to juries. These instructions list various aggravating or mitigating factors that must be considered in a subsequent independent sentencing phase in capital trials (Paternoster 1991; Zimring and Hawkins 1986). The Supreme Court required that states institute a separate sentencing procedure to occur after a guilty verdict in capital cases so defendants would not have to partially admit guilt by introducing evidence about mitigating factors during the part of their trial that determined their guilt or innocence.

should produce much greater confidence in the conclusions.[5]

The presence or absence of the death penalty would not be as compelling if many states with a legal death penalty never used it. States with the death penalty failed to impose death sentences in only 14 of the 114 state-years covered by the death penalty in the five years during and after the time we measure its presence (1971–1975, 1981–1985, 1991–1995). Half of the 14 non-death sentence state-years occurred in the 1971–1975 period, when the anticipated 1972 *Furman* decision, that temporarily outlawed capital punishment, led to sharp reductions in the number of death sentences. From 1991 to 1995, only 2 of 35 death-penalty states did not impose this sentence. The mean number of death sentences in these three five-year periods in death-penalty states was 30.7 (after 1980 and the reversal of the *Furman* decision, this mean rises to 39.4). If states allow the death sentence, their courts use it. The legality of this punishment is not just a symbolic issue.[6]

Estimation. In the primary analyses we use a pooled time-series cross-sectional procedure because it captures both cross-sectional and longitudinal variation. An analysis sensitive to both kinds of variation will produce more efficient estimates. Most of the variation in the presence of the death penalty is cross-sectional, but about a third involves over-time changes in the legality of this punishment (see Table 1). In part because most—but by no means all—of this variation is cross-sectional, we present supplemental Weibull regression analyses of time to compliance with the Supreme Court's 1976 requirements for a constitutional death penalty to check the validity of our pooled time-series cross-sectional findings.

Because the presence or absence of the death penalty is a dichotomy, we estimate most of the pooled time-series cross-sectional models with a pooled complementary log-log procedure. In simplified notation the complementary log-log model takes the following form:

$$\log[-\log(1-P)] = \mathbf{a} + \mathbf{B}_1X + \ldots + \mathbf{B}_kX.$$

This estimator is appropriate when a dichotomous dependent variable is skewed (Agresti 1990; Clayton and Hills 1993; Long 1997; Rotolo 2000). It gives results that are slightly stronger than probit or logit models. To show that the complementary log-log results are not idiosyncratic, we report pooled time-series cross-sectional probit results as well. We use a population-averaged estimation procedure (Liang and Zeger 1986; Pendergast et al. 1996; Prentice and Zhao 1991; StataCorp 2000) that is closely analogous to random-effects.[7] To control for pe-

[5] This supplemental analysis is important because most of the initial state decisions about the legality of capital punishment occurred many years before the information required for a plausible analysis was collected. The 1976 Supreme Court decisions are extremely useful because they forced the states to decide this issue again. If this time-to-event analysis shows that the same effects explain the presence of this penalty and the time it took to comply with the Supreme Court's requirements, objections about historical inheritance should be largely eliminated.

[6] We do not present comparisons of the number of executions to show that the death-penalty states often use this punishment because the federal courts frequently interfered with these state decisions. The delay between death sentences and executions (often longer than 10 years) and the changes in the relevant legal and legislative decisions that occurred during this long period make valid state contrasts using the number of executions impossible. These difficulties also mean that the number of executions in a state cannot be used as a dependent variable in an analysis. The substantial time between sentence and execution creates severe estimation problems. Adequate controls for the shifting influence of the federal courts during the substantial delays between sentence and punishment probably are not possible. The number of death sentences in the states is another potential dependent variable, but the distinct nature of such a complex analysis means that this investigation must be reported in a different paper.

[7] Fixed-effects logit is inappropriate in this case because such an analysis would be restricted to 24 cases. In any event, fixed-effects estimation removes all cross-sectional variation, but we want to capture the factors that produce both cross-sectional and over-time jurisdictional differences. Although the population-averaged procedure we use is almost identical to random-effects, random-effects cannot be used because only less exhaustive models pass quadcheck tests. Random-effects procedures that analyze dichotomies use the Gauss-Hermite quadrature to

riod effects, we include dummy variables for years and dummy variables for region to capture omitted factors like culture.

Explanatory Variable Measurement

Many of the theories we test predict discontinuous effects. When we use dummy variables to capture these threshold effects, we first tested this specification by including both the dummy variable(s) and the explanatory variable in its continuous form in the same panel analyses (not shown). If the coefficient(s) on the dummy variable(s) are significant, but the coefficient on the variable in continuous form is not, we have strong evidence that the relationship in question is discontinuous and that a dummy variable specification is the most appropriate way to operationalize such threshold effects (note 15 describes BIC and AIC tests that assess these dummy variable specifications). Explanatory variables are left in continuous form when this test shows that dummy variables are ineffective. *We avoid multiple runs and over-fitting* by breaking the (ordered) continuous variables into equal fourths and calculating dummy variable thresholds based on a state's quartile score. Note that over-fitting is problematic because it produces ungeneralizable results. If the thresholds we use for these dummy variables are effective largely because they have been over-fit to the presence of a legal death penalty in the pooled time-series cross-sectional panel analyses, they should not explain the alternative outcome in the supplemental time-to-compliance event-history analyses.

We assess *minority presence* with the percentage of blacks and with the natural log of the percentage of Hispanics (some explanatory variables are in natural-log form to correct skewed distributions and ensure multivariate normality). Because the relationship between the percentage of blacks and the presence or absence of a death penalty should be discontinuous and because the test described above shows that a dummy specification will be most appropriate, we use a dummy variable to represent this threat effect. This dummy variable is scored 1 if the percentage of blacks in a state exceeds the median percentage black for all states (or 6.4 percent) and 0 otherwise.

We measure *economic inequality* with the Gini index computed on family incomes by the census. In most analyses we use a dummy variable coded 1 if the value of Gini was greater than the median Gini value (.369) but less than the 75th percentile score (.389), and a dummy variable coded 1 if a state's score on Gini exceeded the 75th percentile for the states.

Residence in large communities (50,000 plus) is measured with the percentage living in metropolitan statistical areas. The *presence of outsiders* is assessed with a dummy coded 1 if a state was in the highest quartile of the percentage of residents born in state (or a score greater than 75.3 percent). We gauge *violent crime* with the natural log of the Uniform Crime Report murder and violent crime rates (alternative crime statistics are not available by region). We assess *family disorganization* with the natural log of the percentage of families with a female head, and we use the census *unemployment rate*.

Berry et al. (1998) view *citizen ideology* as the mean position on a liberal-conservative continuum. To compute a measure that varies over time, they identify the ideological position of each member of Congress with ratings by interest groups (Americans for Democratic Action, Committee on Political Education) of that representative's voting record. They estimate citizen ideology within each state by congressional district with the ideology score for the district's in-

compute the log-likelihood and its derivatives. If random-effects is appropriate, the estimates of the log-likelihood and the coefficients should not diverge by more than 1 percent when different quadrature points are tested. Only restricted random-effects models pass this test, so we use population-averaged estimation instead (although the less exhaustive random-effects models that pass this test give theoretically equivalent results). All standard errors are corrected for heteroskedasticity using White's (1980) method. We estimate with XTGEE complementary log-log and probit routines in Stata version 7 and correct for potential autocorrelation with an AR1 term. This estimator is robust to mispecification because the estimates are corrected for heteroskedasticity.

cumbent representative and with an estimated score for that incumbent's challenger in the last election. Ideology scores for the incumbent are combined with estimated ideology scores for the challenger weighted by district election results to capture within-district ideological divisions. Berry et al. (1998) calculate state scores on liberalism–conservatism using the mean of these congressional district scores. Theory suggests that this variable's predictive power will be greatest when it is left in continuous form.

Republican strength is measured with a dummy variable coded 1 for the presence of a Republican governor and by a dummy variable coded 1 when the percentage of Republicans in a state's legislature exceeds 60 percent. We select this threshold because it is plausible that a dichotomous outcome like the death penalty would be more likely in areas in which the law-and-order party had a substantial legislative majority.

The prior discussion suggests that the coefficients on three explanatory variables should be negative: the percentage living in metropolitan statistical areas, the dummy variable for the percentage born in the state, and the liberalism-conservative index (that gives liberal states a higher score) should be inversely related to the probability of a legal death penalty. We do not predict the sign on the dummy variables for region or period, and we use two-tailed significance tests for these effects. The coefficients on all remaining explanatory variables should be positive.

SPECIFICATION

One of the more general specifications of the panel models therefore is:

$$\begin{aligned} \textit{Death Penalty} = {} & b_0 + b_1 \textit{Percentage Black} \\ & + b_2 \textit{Percentage Hispanic} \\ & + b_3 \textit{Inequality} + b_4 \textit{Born in State} \\ & + b_5 \textit{Murder Rate} + b_6 \textit{Ideology} \\ & + b_7 \textit{Republican Legislature} \\ & + b_8 \textit{Republican Governor} \\ & + b_9 \textit{South} + b_{10} \textit{North East} \\ & + b_{11} \textit{West} + b_{12} 1970 \\ & + b_{13} 1980. \end{aligned} \quad (1)$$

In additional analyses we control for the effects of the percentage of female-headed families, the violent crime rates, the percentage of residents living in a MSA, and the unemployment rate.[8]

ANALYSES

DESCRIPTIVE STATISTICS AND CROSS-TABULATIONS

Table 1 shows the expected signs along with the means and standard deviations across states and over time.[9] These and other results show that there is enough over-time variation in the dependent variable to conduct pooled time-series analyses. The over-time coefficient of variation (the standard deviation divided by its mean) for the death-penalty variable is .249, while the relevant chi-

[8] Inferences about the relationships between the explanatory variables and the presence of the death penalty will be equivalent to those from any random-effects pooled time-series panel analysis that assesses the combination of cross-sectional and longitudinal variation in a dependent variable (see Johnston and DiNardo 1997). The best information about whether a explanatory variable's effects in a panel analysis is mostly longitudinal or mostly cross-sectional can be found in Table 1. If most of the variation in an explanatory variable is cross-sectional, it's covariation with the dependent variable should be largely cross-sectional rather than longitudinal. An argument that these potential policy shifts are largely governed by the inertia of past decisions is plausible, but the supplemental duration analysis of state decisions to "relegalize" capital punishment should eliminate this objection. In effect, the 1976 Supreme Court decisions forced the states that had the death penalty to again make decisions about adopting this punishment. In the panel analyses, what appears as a nondecision by states that kept the death penalty in fact involved significant legal changes. Note that such claims about historical inertia enhance the plausibility of Blalock's (1967) hypotheses about nonlinear relationships. If it is so difficult to alter inherited state decisions about capital punishment, increases in the social forces we assess probably would have to become more extreme to overcome this inertia when political victory is imminent.

[9] The standard deviations across time and space are calculated using the XTSUM procedure in Stata version 7.

Table 1. Predicted Sign, Mean, and Standard Deviation Across States and Over Time for Variables Used in the Analysis: 1971, 1981, and 1991

Variable	Predicted Sign	Mean	Overall Standard Deviation	Cross-State Standard Deviation	Over-Time Standard Deviation
State has the death penalty		.760	.429	.387	.189
Minority Presence					
Percent black in state ≥ state median	+	.500	.502	.496	.095
Percent Hispanic (ln)	+	.716	1.161	1.125	.316
Economic Inequality					
Income inequality in third quartile	+	.247	.433	.231	.366
Income inequality in fourth quartile	+	.247	.433	.268	.341
Residence/Nativity					
Percent in MSA	–	63.255	22.661	22.525	3.599
Percent born in-state in fourth quartile	–	.247	.433	.386	.201
Political Variables					
Liberalism–conservatism score	–	45.072	15.980	14.761	6.356
Republican governor	+	.433	.497	.295	.401
Percent Republicans in legislature ≥ 60 percent	+	.153	.362	.263	.250
Crime Measures					
Murder rate	+	7.359	4.111	3.881	1.428
Violent crime rate (ln)	+	5.820	.692	.603	.347
Family Disorganization					
Percent female-headed families (ln)	+	2.398	.224	.190	.121
Percent unemployed	+	6.009	1.716	1.270	1.164

square test shows that there is sufficient change in this outcome to indicate that panel estimation is the most appropriate procedure.

Figures 1a and 1b show alterations in the distribution of states with and without the death penalty. Figure 1a presents a transition matrix for 1971 and 1981. Eight states did not allow the death penalty in either 1971 or 1981 while 36 states retained this penalty in both years. The top right and bottom left cells show transitions: one state that did not have the death penalty in 1971 legalized it by 1981, while five states that had the death penalty in 1971 had not relegalized it by 1981. The transition matrix for 1981 and 1991, presented in Figure 1b, shows that 12 nondeath-penalty states and 35 death-penalty states did not change this provision during this decade, but two states that had a legal death penalty in 1981 did not in 1991, and one state that did not allow the death penalty in 1981 reinstituted it by 1991.

MULTIVARIATE RESULTS

PANEL ANALYSES. Table 2 shows the first panel results. These initial models gauge threat and social disorganization hypotheses, but they ignore political accounts. Model 1 includes black and Hispanic presence to assess minority threat, and economic inequality to measure the threat posed by an economic underclass. Disorganization indicators such as the dummy variable for the percentage of residents born in state, the murder rates, together with dummy variables for year that control for period differences are included as well. We add regional dummy variables in Model 2. In Model 3 we retain all variables, but the more comprehensive violent crime rates are substituted for the

States with the Death Penalty in 1981

States with the Death Penalty in 1971	No	Yes	Total
No	8 (88.9%)	1 (11.1%)	9 (100%)
Yes	5 (12.2%)	36 (87.8%)	41 (100%)
Total	13 (26%)	37 (74%)	50 (100%)

(a) 1971 Compared with 1981

States with the Death Penalty in 1991

States with the Death Penalty in 1981	No	Yes	Total
No	12 (92.3%)	1 (7.7%)	13 (100%)
Yes	2 (5.4%)	35 (94.6%)	37 (100%)
Total	14 (28%)	36 (72%)	50 (100%)

(b) 1981 Compared with 1991

Figure 1. Transition Matrices of the Presence of Death-Penalty Laws: 1971 to 1981 and 1981 to 1991

murder rates. In Model 4 we add the remaining disorganization variables.[10]

We find persistent evidence for threat effects in these initial analyses. The most economically unequal states and states with the highest percentages of blacks are more likely to have the death penalty, but Hispanic presence is not significant (and this finding does not change if this continuous variable is recoded with dummy variables). Although the percentage born in state is an exception, the other social disorganization variables and either the violent crime rate or the murder rate do not influence this decision, and these conclusions persist when we enter these variables separately (in analyses not shown). But we do not know what will happen when political effects are assessed.

Table 3 presents these findings. Model 1 includes the Berry et al. (1998) liberalism–conservatism index, the presence of a Republican governor, Republican dominance in the state legislature, and all variables that were significant in Table 2. In Model 2 we again add the three regional dummy variables. The ineffective Republican governor variable is dropped in Model 3. Model 4 includes the same variables used in Model 3, but this model is estimated with pooled time-series probit to find out what happens when the best model is estimated with a more conventional statistical procedure.

The presence of blacks and economic inequality continue to explain the legality of the death penalty, but political effects have explanatory power as well. Liberal states are less likely than conservative states to allow the death penalty, and capital punishment tends to be legal in jurisdictions in which the Republican party has legislative majorities.[11]

[10] The number of regressors in these models is appropriate because econometricians claim that exhaustive specifications are preferable. Johnston (1984) 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. (p. 262)

[11] Although leaving the threat and political variables in continuous form is a misspecification (for additional evidence, see note 15), the most theoretically important findings survive this test. Consider the following complimentary log-log equation with all variables but the percentage born in state, region, and year in continuous form:

$$\begin{aligned} \textit{Death Penalty} = {} & -1.81 \underset{(2.69)}{} + .078 \underset{(.033)}{\textit{Percentage Black}} \\ & + 11.3 \underset{(6.45)}{\textit{Gini}} - .901 \underset{(.329)}{\textit{Born in State}} \\ & - .030 \underset{(.011)}{\textit{Ideology}} \\ & + .017 \underset{(.011)}{\textit{Republican Legislature}} \\ & + .578 \underset{(.629)}{\textit{South}} + .859 \underset{(.313)}{(\textit{1970})} + .143 \underset{(.243)}{(\textit{1980})} \end{aligned}$$

This (mis)specification using threat variables in continuous form, however, is not robust when additional controls are introduced.

Table 2. Coefficients from Pooled Time-Series Complementary Log-Log Models Predicting the Presence of Death-Penalty Statutes in 1971, 1981, and 1991: Selected Threat and Socioeconomic Variables

Independent Variable	Model 1	Model 2	Model 3	Model 4
Intercept	−.591	−.987*	−1.476	−1.053
	(.320)	(.430)	(2.016)	(2.596)
Percent black ≥ state median	.965**	1.345***	1.182**	1.679***
	(.342)	(.443)	(.396)	(.438)
Percent Hispanic (ln)	−.066	−.097	−.189	−.057
	(.133)	(.194)	(.242)	(.225)
Income inequality in third quartile	.550**	.472*	.474*	.359
	(.207)	(.217)	(.224)	(.242)
Income inequality in fourth quartile	1.034***	1.050**	.962**	1.168**
	(.326)	(.364)	(.359)	(.426)
Percent in MSA	—	—	—	−.014
				(.009)
Percent born in state in fourth quartile	−.730**	−.787**	−.791**	−1.029**
	(.288)	(.285)	(.311)	(.336)
Murder rate	−.004	−.038	—	−.023
	(.036)	(.052)		(.055)
Violent crime rate (ln)	—	—	.076	—
			(.369)	
Percent female-headed families (ln)	—	—	—	.665
				(1.292)
Percent unemployed	—	—	—	−.134
				(.086)
Region				
South	—	.937	.824	.891
		(.605)	(.566)	(.535)
Northeast	—	.226	.278	.245
		(.459)	(.470)	(.488)
West	—	1.085*	1.088	1.171
		(.626)	(.652)	(.651)
Year				
1970	.900***	.858**	.803	.864*
	(.262)	(.308)	(.413)	(.397)
1980	.545**	.580*	.489*	.484
	(.222)	(.264)	(.223)	(.272)
Chi -square test	31.67***	47.21***	56.13***	57.59***

Note: Numbers in parentheses are standard errors corrected for heteroskedasticity. N = 150 state-years.
*$p \leq .05$ **$p \leq .01$ ***$p \leq .001$ (one-tailed tests; two-tailed tests for region and year variables)

When the same ineffective measures of social disorganization or crime were entered in models (not shown) that include both political and threat variables, the theoretical implications persist, and these disorganization indicators remain nonsignificant. It is important that chi-square tests for combined effects show that the period dummy variables always are statistically significant. Such results show that there is sufficient over-time variation in the dependent variable to conduct pooled time-series cross-sectional analyses.[12]

[12] These panel findings hold if the percentage divorced, the percentage of state residents aged

Table 3. Coefficients from Pooled Time-Series Complementary Log-Log and Probit Models Predicting the Presence of Death-Penalty Statutes in 1971, 1981, and 1991: Selected Threat, Socioeconomic, and Political Variables

Independent Variable	Model 1	Model 2	Model 3	Model 4[a]
Intercept	1.814*	1.838	1.687	1.734
	(.791)	(1.066)	(.932)	(.952)
Percent black ≥ state median	1.381**	1.760***	1.709***	1.570***
	(.464)	(.462)	(.467)	(.452)
Income inequality in third quartile	.378*	.374	.432*	.402
	(.213)	(.240)	(.201)	(.278)
Income inequality in fourth quartile	.938***	.938**	1.017**	.961*
	(.294)	(.326)	(.331)	(.429)
Percent born in state in fourth quartile	−.895**	−.914**	−.952**	−.886**
	(.327)	(.356)	(.352)	(.395)
Liberalism–conservatism score	−.049***	−.068***	−.067***	−.059***
	(.013)	(.018)	(.017)	(.015)
Republican governor	−.137	−.141	—	—
	(.190)	(.229)		
Percent Republicans in legislature ≥ 60 percent	.638**	.867**	.854**	.726*
	(.242)	(.319)	(.303)	(.315)
Region				
South	—	.964	.969	.897
		(.564)	(.562)	(.553)
Northeast	—	1.144*	1.161*	1.215*
		(.520)	(.528)	(.549)
West	—	1.272*	1.261*	1.349*
		(.524)	(.493)	(.543)
Year				
1970	.952**	1.050**	1.066**	1.156**
	(.362)	(.389)	(.366)	(.437)
1980	.213	.156	.177	.236
	(.244)	(.283)	(.273)	(.363)
Chi-square test	56.99***	50.88***	52.25***	75.33***

Note: Numbers in parentheses are standard errors corrected for heteroskedasticity. N = 150 state-years.

[a] Model 4 is estimated with pooled time-series probit.

*$p \leq .05$ **$p \leq .01$ ***$p \leq .001$ (one-tailed tests; two-tailed tests on region and year variables)

DURATION ANALYSES. An important advantage of the period we study is the natural experiment conducted by the Supreme Count on the constitutionality of capital punishment that occurred in the first decade of the sample period. Recall that in 1976 the Supreme Court reversed its 1972 *Furman* decision and let the states use the death penalty if they made significant alterations to their death-penalty statutes. Retentionist states differed in the time they took to comply, and

15 to 25, the four separate violent crime rates (assault, murder, rape, and robbery rates), on the percentage below the poverty line are included in the models. The largest correlation between any two explanatory variables in the best models (Models 3 and 4 in Table 3) is .60, and the maximum variance inflation score (VIF) is 3.06. The highest VIF score for *any* model is below 5 (VIF = 4.59 for Model 4 of Table 3), or the value that the most conservative statisticians use to indicate multicollinearity. The highest correlation between two explanatory variables in any model is .626—between the percentage of female-headed

four death-penalty states had not altered their statutes by 1995. If the mostly cross-sectional panel results provide an accurate picture of the forces that produce a legal death penalty, similar factors should explain the time it took for states to conform to the Supreme Court's altered requirements. We employ Weibull regression to reexamine the determinants of support for capital punishment with this different research procedure.

We use 1970 explanatory variables, omit the states that did not have the death penalty, and analyze the years between the relevant court decisions in 1976 and state compliance up through 1995. The four death-penalty states that had not met the Supreme Court's requirements by 1995 are treated as censored. These analyses use specifications identical to those in the pooled time-series cross-sectional analyses except that we drop the nonsignificant Republican legislature variable and use instead the percentage of votes for the Republican presidential candidate to assess the political strength of the law-and-order party. Nixon ran on a law-and-order platform (Beckett 1997; Edsal and Edsal 1991), so this measure should provide an equally valid indicator of the partisan-strength explanation for the legality of this punishment.

In Table 4 we again begin with a simplified model and include the dummy variables that gauge the presence of blacks and economic inequality. Identical measures of political ideology and regional effects, along with the new indicator of Republican strength are included in this model as well. The unemployment rates are added in Model 2. In Model 3 we drop unemployment and add the percentage of Hispanics and the murder rate. Model 4 includes the violent crime rate and the percentage of state residents living in large cities, but the percentage of Hispanics and the murder rate are eliminated. The signs on explanatory variables should be the opposite of those in the panel analyses because a longer time to compliance with Supreme Court requirements indicates diminished support for the death penalty.

These duration findings largely duplicate the panel results. The same threshold measures of black threat and economic inequality explain time to compliance, and the same ideology measure again has an association in the predicted direction. The factors that are nonsignificant in the panel analyses of presence of the death penalty do not explain this duration outcome, and the same four theoretically important factors with explanatory power in Model 1 of Table 4 continue to matter after various controls are introduced in the last three models in Table 4.

The only notable contrast between the findings in the panel analyses and the duration analyses concerns the way partisanship is measured. Republican legislative strength explains the presence of the death penalty in the panel analyses, but this indicator has no effect on time to compliance with the Supreme Court's requirements. Instead, we find that public support for a law-and-order Republican presidential candidate explains this interval.[13]

families and the violent crime rate. To see if collinearity is masking these nonsignificant effects, we conduct a test for the joint significance of both variables, but this chi-square test is negative. Such tests on other correlated but nonsignificant pairs of variables give the same results. The stability of the coefficients also suggests that collinearity is not present. If we increase lags by analyzing death-penalty presence two years after each census, all panel findings except Republican legislative strength persist.

[13] In this duration analysis we ignore purely legislative changes and code only the time it took states to comply with the Supreme Court's directives. We analyze 42 states because Oregon legalized the death penalty after 1971. We use Weibull regression because it is the most appropriate (and frequently used) estimator for such duration models (Greene 1993). The findings nevertheless persist if Model 1 is estimated with either exponential or Gompertz regression. Attempts to use two-period means calculated on explanatory variables collected in 1970 and 1980 instead of data collected in 1970 failed due to collinearity, but this problem is *not* present in the reported duration models since the maximum VIF in Model 1 is 4.15. The highest VIF is 4.94 for Model 3, a score that remains below 5. Most states responded quickly to the Supreme Court's requirements, so the lag between when the explanatory variables were measured and these legislative changes should not be problematic. Other explanatory variables included in the panel analyses do not predict this time-to-compliance outcome, and entering each of the control variables alone rather than in pairs does not alter these

Table 4. Coefficients from Weibull Regression Analyses Predicting the Time between New Supreme Court Requirements for a Constitutional Death Penalty and State Compliance: Selected Threat, Socioeconomic, and Political Variables

Independent Variable	Model 1	Model 2	Model 3	Model 4
Intercept	2.148**	2.665**	2.125*	.047
	(.760)	(1.017)	(1.014)	(2.290)
Percent black ≥ state median	−1.074*	−1.098*	−1.288**	−1.490**
	(.578)	(.550)	(.490)	(.438)
Percent Hispanic (ln)	—	—	.136	—
			(.133)	
Income inequality in fourth quartile	−.474*	−.435*	−.468*	−.394*
	(.240)	(.256)	(.252)	(.232)
Percent in MSA	—	—	—	.001
				(.010)
Liberalism-conservatism score	.040**	.043**	.044**	.041**
	(.015)	(.017)	(.016)	(.016)
Percent voting Republican	−.041**	−.056**	−.044**	−.036**
	(.017)	(.018)	(.015)	(.016)
Murder rate	—	—	.021	—
			(.062)	
Violent crime rate (ln)	—	—	—	.394
				(.461)
Percent unemployed	—	−.098	—	—
		(.175)		
Region				
South	−.529	−.554	−.490	−.463
	(.616)	(.633)	(.642)	(.650)
North East	−.559	−.613	−.677	−.659
	(.990)	(.993)	(.895)	(.883)
West	−1.426**	−1.329**	−1.765**	−1.719***
	(.506)	(.562)	(.590)	(.490)
Log-likelihood	−53.18	−53.02	−52.52	−51.93
Chi-square test	41.77***	44.56***	43.93***	45.71***

Note: Numbers in parentheses are standard errors corrected for heteroskedasticity. Coefficients are corrected for unmeasured heterogeneity (or overdispersion). N = 42 states.

*$p \leq .05$ **$p \leq .01$ ***$p \leq .001$ (one-tailed tests; two-tailed tests for region variables)

The duration results eliminate a potential objection to the panel analyses. If the panel findings somehow are incorrect because they assess contemporary factors that could not influence choices largely made in the distant past, these factors should not explain what happened when the Supreme Court forced the death-penalty states to redecide their capital punishment laws. Yet we find that the same threat and similar political hypotheses predict both the legality of the death penalty and time to compliance with the Supreme Court's demands for significant revisions to state death-penalty statutes. These similarities in the results from such different research procedures provide additional reasons

time-to-compliance results.

We use the 1968 vote for Nixon because he ran on a law-and-order platform and won by a narrow margin in that election. The alternative Nixon victory in 1972 against McGovern was not a meaningful election because Nixon won by an extremely large margin and received votes from many people who would not have supported him if the Democrats had nominated a more viable candidate.

to think that political decisions about the legality of capital punishment respond to racial and economic threat, conservative values, and the strength of the law-and-order Republican party.

ADDITIONAL METHODOLOGICAL ISSUES. We conducted sensitivity tests by removing, in separate panel analyses, each of the states that abolished or added the death penalty. The results persist in all eight trials (in one trial the dummy variable representing economic inequality in the third inequality quartile became nonsignificant). Correcting the standard errors for within-region interdependence produces only trivial changes in the results, so spatial autocorrelation probably is not biasing the findings. Tests for statistical interaction were negative, and we find little support for hypotheses about intervening effects.[14] Different combinations of the variables entered in the reported models also do not alter the findings.

Late 1970 state-level indicators of self-identification as a conservative or the presence of religious fundamentalism derived from surveys can be used to determine if alternative indicators of public ideology produce the same results as those reported. Equivalent pooled time-series cross-sectional panel models (not shown) restricted to the 1980 and 1990 periods indicate that the death penalty is likely to be legal in states with relatively more self-identified conservatives, but these findings suggest that religious fundamentalism does not matter, and the other findings persist in these two-period analyses.

When we use dummy variables to assess the threshold effects suggested by theory, these specifications were tested by including both the dummy variable(s) and the continuous version of the explanatory variable in unreported analyses. Additional Akaike information criterion (AIC) and Bayesian information criterion (BIC) tests show that the models that use dummy specifications have stronger effects than models using explanatory variables in continuous form.[15] Such results are plausible because threat explanations, political explanations, and prior research findings indicate that these relationships should be discontinuous. The largely equivalent duration findings that are based on the same dummy explanatory variables

[14] Path analysis, which is normally used to gauge indirect effects, is inappropriate in this study. Indirect effects cannot be assessed with mixed estimators by using complementary log-log estimation to assess the path to a dichotomous outcome like the death penalty and panel regression to estimate the paths to intervening continuous variables. Continuous indicators like ideology therefore cannot be specified as intervening variables. A more justifiable tactic involves dropping explanatory variables to see if the other coefficients increase in size. If we remove economic inequality, the coefficient on the black dummy variable goes from 1.7 to 1.8, a 6-percent shift that is well within confidence bounds. If we remove Republican legislative strength, the coefficient on political ideology goes from .067 to .071, a 6.9-percent shift that again is within confidence bounds. Political ideology explains Republican legislative strength, but the total indirect relationship from ideology through Republican legislative strength to the death penalty is modest, and we find no evidence that race or economic inequality account for Republican strength.

[15] We compared models with and without dummy codes using the AIC and BIC tests. Because these tests cannot be used to compare the explanatory power of population-averaged models, we estimated with a maximum likelihood complementary log-log procedure that is not corrected for the panel structure of the data, but we adjusted for the interdependent within-state scores (or scores for the same state at different times) with a less efficient cluster approach. All variables in the null model of these model contrasts are in continuous form. In the second model of each pair, we substitute a variable coded using three dummy variables for the top three quartiles that represents each of the variables coded with dummy variables in the reported analyses. In each of these contrasts, the results of the BIC and AIC tests strongly suggest that the variables in dummy form are the superior specification. We repeated these tests with the collapsed dummy variables used in the reported models and again find the same results. Because the three regional dummy variables are not significant in this less efficient nonpanel cluster-corrected estimation procedure, we control only for the South in these models. The point estimates from these analyses give us additional reasons to believe that threshold effects are present because the values of the coefficients on the dummy variables show the upward or downward trends that would be expected if threshold effects are present.

provide added reasons to believe that these specifications are generalizable because they account for different measures of support for capital punishment. Attempts to use dummy variables to assess the effects of the nonsignificant variables expressed in continuous form were *not* successful. Because the log transformations we use to eliminate skewed distributions do not alter the conclusions, the results do not appear to be distorted by arbitrary coding decisions. If we adjust for the panel structure of the data in a different way, the last three panel models in Table 3 and all four event-history models in Table 4 pass the link test (Pregibon 1980) for specification error.[16]

The predictive accuracy of the pooled time-series cross-sectional panel models is substantial. The best model gives 14 incorrect predictions compared with 136 correct predictions yielding an prediction rate of 90.7 percent. The strong bivariate relationships, the stability of the results despite multiple controls, and their persistence when subjected to multiple tests that use different specifications, estimation procedures, and research designs all suggest that these analyses have captured the most important state-level factors that influence political decisions about the legality of capital punishment.

CONCLUSIONS

SUMMARY AND DISCUSSION

The results corroborate threat and political explanations for jurisdictional decisions about the death penalty. As racial threat theorists like Blalock (1967) would expect, we find that states with the largest black populations are more likely to retain capital punishment after the amount of violent crime and many other explanations are held constant, but we find no evidence that Hispanic presence matters. Because we find discontinuous effects, the racial threat results support Blalock's (1967) theoretical suppositions about threshold effects. Perhaps the explanatory power of black threat compared with Hispanic threat should not be surprising in light of the horrific conflicts about race that occurred throughout U.S. history.

The results provide strong support for the less prominent economic version of threat theory. We find that the death penalty is more likely to be present in jurisdictions in which economic inequality is most pronounced. Such findings corroborate theory-based claims (Chambliss and Seidman 1980; Garland 1990) that extreme economic differences between the affluent and the poor increase the probability that capital punishment will be legal, although these economic divisions are not as visible as social divisions based on race. Both economic and racial threat explanations for repressive measures like a legal death penalty have an unmeasured intervening political component. Successful demands for harsh punishments that result from these threats must be directed at the public officials who can change the criminal codes.

The results show that ideology helps to account for the presence or absence of capital punishment. Greater public support for liberal policies leads to a reduced likelihood that the death sentence will be available to the courts. Some results suggest that jurisdictions in which the strength of the Republican party in state legislatures has passed a threshold will be more likely to allow death sentences, but the party of the governor has no effect on this outcome. The latter finding is plausible. Governors decide the last appeal before an execution is carried out, so Republican governors have much stronger reasons to be ambivalent about the death penalty than Republican representatives who need not make the terrible final decision about an execution (Zimring and Hawkins 1986).

Republican legislative strength, however, does not account for time to compliance with the Supreme Court's revised requirements for a constitutional death penalty, but this duration outcome is explained by support for a Republican law-and-order presidential candidate. One reason for this mild contrast con-

[16] In these models, the standard errors are estimated by assuming that the three-period state scores are interdependent. Although this cluster correction results in estimates are less efficient than the panel estimates, the results are equivalent to those in Model 4 of Table 3. The link test cannot be used to test the specification of population-averaged models, so we again employ this alternative non panel estimator instead.

cerns public influence. Our findings on the delay between the Supreme Court's 1976 decisions and the alterations to state death-penalty laws suggest that this timing was influenced more by public support for law-and-order, but the presence of the death penalty is better explained by the partisan strength in the legislature. In any case, these complementary findings are noteworthy because few results show links between politics and the punitive content of the criminal codes.

Although there is little research on the factors that influence the legality of the death penalty, studies of the politics of formal social control are starting to accumulate. Jacobs and Helms (1996) find that national expansions in Republican strength account for yearly increases in U.S. prison admissions. Cross-national results suggest that this finding has wider applicability: An increase in the strength of the most conservative political parties in five advanced democracies produces growth in the proportion of the population that is incarcerated (Sutton 2000).

Evidence for threat effects seems to depend on how outcomes are decided. Neither riots nor expansions in minority presence explain prison admissions (Jacobs and Helms 1996), but both factors lead to increased spending on corrections (Jacobs and Helms 1999). These contrasts are plausible because as Garland (1990, 2001) claims and Balbus (1973) finds, decisions about imprisonment are relatively insulated from the public, in part because they are often made by appointed officials. Yet spending on corrections is determined by elected representatives who face costs if they ignore public demands for harsh measures. The legality of the death penalty is decided by the same elected representatives. The links between race, economic inequality, and public values and the presence of the death penalty should therefore be stronger than the links between these factors and imprisonments for identical reasons. In a direct democracy like the United States, elected officials who decide the legality of the death penalty must be more responsive to the public than the mostly unelected officials who control the size of prison populations.

The same distinction between political and bureaucratic-legal decision making may provide some insight about the factors that lead to executions. For methodological reasons, a plausible state-level analysis of executions is not feasible (see note 6). If this research were possible, the findings might show that forces external to governments such as threat or public sentiments do not explain executions as well as they explain the legality of capital punishment. Although governors decide the final appeal, the decision to execute ultimately depends far more on the insulated workings of the legal system. After the death penalty has been legalized, the elected representatives in the legislature, who find it difficult to disregard public views, have little effect on executions. In contrast to their influence on the legality of capital punishment, it is reasonable to expect that social threat and public ideology should not have such strong effects on executions.

Most social disorganization hypotheses do not explain the legality of capital punishment, but the absence of migration is an exception. The results show that jurisdictions with the most residents born in state are less likely to have the death penalty. High migration rates reduce community solidarity and break interpersonal ties. The effects of this control variable probably can be attributed to hostility toward strangers (Hale 1996) and to a related disinclination to invoke the ultimate penalty against people who are more likely to be regarded as one's neighbors in jurisdictions with few outsiders. It is more surprising that our results offer no support for the plausible hypothesis that states with the most violent crime or murders will be more likely to retain the death penalty.

IMPLICATIONS

Investigations that employ aggregate data to investigate punitive outcomes can detect previously hidden relationships, and they can be used to link theoretical conceptualizations about the nature of society to punishment. This approach lets us use theories borrowed from core subdisciplines such as political sociology or stratification to investigate the criminal sanctions administered by the state. In this study, we find evidence for three core perspectives in political sociology: Partisan tactical considerations, politi-

cal ideology, and social divisions influence political decisions about the legality of the death penalty.

As might be expected from the recent theoretical emphasis on the politics of punishment and the Republican party's tactical emphasis on law and order, some findings suggest that states with strong Republican parties are more likely to legalize capital punishment. One objection to a conclusion that Republican political strength leads to harsh punishments concerns ideology. The political strength of the Republican Party may be based on preexisting conservative views that are the underlying cause of successful demands for the ultimate sanction. Yet the panel results show that sufficient Republican strength in state legislatures explains the presence of the death penalty after measures of political ideology have been held constant. Although it is difficult to make definitive comparisons about explanatory power, particularly since both effects account for this outcome, the combined results suggest that the relationships between political ideology and the presence or absence of the death penalty are more robust than the associations between Republican Party strength and this sanction.

These findings begin to suggest a tentative answer to a question we posed in the introduction. At least in a populist direct democracy like the United States where social policy is less likely to be decided by unelected experts (Garland 2001; Savelsberg 1994), politicians do not seem to lead from the front when the death penalty is at issue, despite Buxton's (1974) claim. Instead, in contrast to more politically centralized European democracies (Savelsberg 1994), public views seem to be more influential in the United States when the death penalty is at issue. Such results suggest that those who seek increased democracy and find the death penalty to be abhorrent may be supporting policies that have contradictory implications.

Social divisions that are external to the state are important as well. An economic cleavage that works through politics affects the decision to legalize the death penalty. Garland (1990) approvingly quotes Rusche ([1933] 1998) who writes, "The history of the penal system is . . . the history of relations (between) the rich and the poor" (Garland 1990:92). If this claim is correct, jurisdictions with the most substantial economic differences between middle- or upper-income groups and the least affluent should be more likely to let their courts use the ultimate punishment. Our findings support the economic version of threat theory because they show that capital punishment tends to be present where economic inequality is most pronounced.

But our most important results concern race. Findings that document a close relationship between the presence of African Americans in a state and the presence of the death penalty may not be surprising, but they show how important the politics of racial division and the racial foundations of punishment continue to be in the United States. Our results imply that the legality of the death penalty is not decided by colorblind political processes. The persistence of this link between race and capital punishment suggests that the destructive effects of the racial conflicts that have divided the United States since its inception have not yet been transcended, although the effects of race on punishment probably are less conspicuous now than they were in the past.

More generally, findings that political factors influence the legality of the death penalty show that the recent thrust in the theoretical literature is correct. Such results confirm claims by theorists that punishment responds to a set of political acts that often have much to do with how majorities and their political representatives decide to manage the threat posed by the "dangerous" classes. Another important implication concerns the possibilities for integrating research on punishment with core sociological theories. Our findings show that explanations borrowed from other sociological areas provide important insights about the factors that shape the ultimate punishment, at least in one advanced but still racially divided society.

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