

8-2007

The Determinants of Executions Since 1951: How Politics, Protests, Public Opinion, and Social Divisions Shape Capital Punishment

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Original Citation

Jacobs, D. and Stephanie L. Kent. 2007. "The Determinants of Executions Since 1951: How Politics, Protests, Public Opinion, and Social Divisions Shape Capital Punishment." *Social Problems* 54(3):297-318.

Repository Citation

Jacobs, David and Kent, Stephanie L., "The Determinants of Executions Since 1951: How Politics, Protests, Public Opinion, and Social Divisions Shape Capital Punishment" (2007). *Sociology & Criminology Faculty Publications*. 5.
https://engagedscholarship.csuohio.edu/clsoc_crim_facpub/5

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The Determinants of Executions since 1951: How Politics, Protests, Public Opinion, and Social Divisions Shape Capital Punishment

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This time-series study uses hypotheses derived from a politically refined version of conflict theory to explain both public support for the death penalty and the number of executions. With murders in death penalty states and Supreme Court decisions held constant, tests of hypotheses about lags suggest that public support and Republican strength in the states influence yearly executions by their effects on death sentences rather than the later appeals process. Other dynamic results show that national level Republican strength, presidential elections that emphasize law and order, economic inequality, and higher murder rates increase yearly executions because they affect the extremely influential but later appeals process. Civil rights protests, however, immediately reduce both public support and executions. Although minority threat enhances public support for capital punishment, this contextual factor does not explain executions. These results are unique as no prior studies have assessed the conditions that determine how often the harshest punishment is used. Keywords: execution politics, civil rights protests and executions, death penalty support and executions, social divisions and executions, delayed effects.

What factors explain executions? An execution is the most severe legal penalty, but little research on the factors that influence this sanction exists. Theorists see criminal punishment as intrinsically political (Foucault 1977; Garland 1990; Savelsberg 1994), yet the political forces that alter yearly execution frequencies have largely been ignored. The literature has focused instead on who is given the death sentence and this penalty's deterrent effects. These studies have been useful, but the determinants of executions deserve scrutiny. David Jacobs and Jason T. Carmichael (2002) isolated the factors that make capital punishment legal, but they could not assess the effects of public opinion and mass protests. To fill this void, we use a national level time-series design to detect the social and political forces that explain the yearly number of executions in the last Western society that still uses this punishment. This longitudinal approach offers other advantages as it shows when various factors influence different stages in the long legal process that may end in an execution.

An execution is one of the most vivid displays of state power (Foucault 1977; Garland 1990), but politics matter for more concrete reasons. While juries participate in initial decisions, state and federal officials have more consequential effects. Office holders decide whether this punishment is legal and the laws that alter the likelihood of this sentence. Courts rule on the constitutionality of these decisions and both state and federal judges decide capital appeals. Largely because of the appeals process, only less than *ten* percent of all death sentences result

The authors thank Doug Downey, Jim Moody, Bob O'Brien, and the participants in seminars at the Ohio State law school, the Department of Criminal Justice at the University of Cincinnati, and seminars in economics departments at the University of Oregon and Ohio State for their valuable comments. They thank Craig Jenkins for supplying the civil rights protest data and Dan Tope for his research assistance. Direct comments to: David Jacobs, Department of Sociology, 300 Bricker Hall, 190 N. Oval Mall, Ohio State University, Columbus, OH 43210. E-mail: jacobs.184@osu.edu.

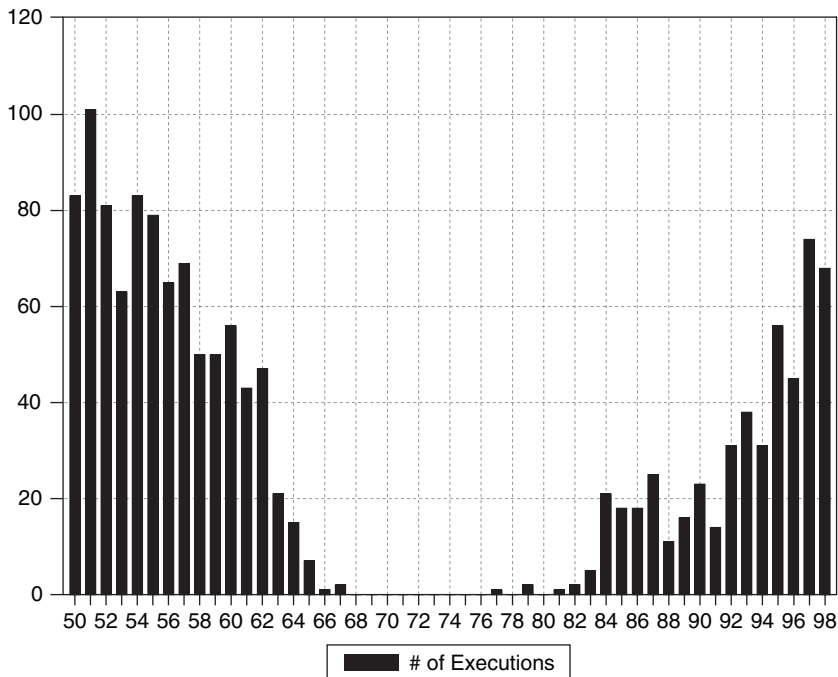


Figure 1 • Yearly Changes in the Number of Executions

in executions (Liebman, Fagan, and West 2000)!¹ Political explanations thus may provide new insights about the behavior of the judges and the other actors who determine how often this ultimate penalty is used.

The theoretical approach we use is grounded in a political sociology informed by neo-Weberian conflict theory (Collins 1975). The racial and economic divisions emphasized in conflict theory and the mass protests that result help shape public and elite views about punishment. Shifts in public opinion driven by these forces should matter as well when such an intensely moral issue must be decided in this directly democratic polity. In this study we test hypotheses about how political institutions, partisan politics, and protests alter both support for the death penalty and executions. In addition to detecting which explanations matter, our time-series approach will provide insights about the stage in this long legal process when explanations are most influential.

Figure 1 shows yearly executions. After a sharp decline that ended with a moratorium on executions from 1968 to 1977, this trend abruptly reversed, but why? Before analyzing executions, we present analyses of public support for this penalty to discover if the same explanations account for both outcomes. The federal courts exercise the most control over this process (Zimring 2003). Their rulings must be modeled with a time-series approach, but

1. Three appeals determine death row outcomes: the first two appeals typically are decided by state courts. After 1973 about 41 percent of state death sentences were reversed in the first state appeal. Nine and half percent of the remainder were successful in their second state appeal. Forty percent of those who exhausted their state appeals then gained relief in the federal courts (Liebman et al. 2000). In 1969 the lower federal courts stopped executions. In 1972 the Supreme Court made the death penalty unconstitutional in *Furman* but in 1976 the High Court reversed in *Gregg* and again made this penalty constitutional.

this fact means we cannot analyze regional differences. In part because so few offenders sentenced to death are executed, we conduct a time-series analysis that explains the latter far more influential stages of this process that is largely controlled by the federal judiciary.

Theory

Hierarchical Social Divisions

Minority Threat. Racial threat theory states that large minority populations jeopardize the dominance of ascendant groups (Blumer 1958; Blalock 1967). Ethnocentric beliefs solidified by the resulting political struggles (Blumer 1958) enhance majority presumptions that they should have exclusive claims over important rights and privileges (Bobo and Hutchings 1996). Such threats produce racist views in areas with the largest black populations (Fosset and Kiecolt 1989; Taylor 1998) and increased votes for racist candidates (Giles and Buckner 1993). Lincoln Quillian and Devah Pager (2001) hold crime constant and find positive relationships between black presence and fear of crime. Racially dominant groups often respond with political demands for punitive measures that target minorities. Support for capital punishment is greater in areas with larger minority populations (Baumer, Messner, and Rosenfeld 2003). The probability of a legal death penalty is greater where black populations are most substantial (Jacobs and Carmichael 2002).

These results—and findings that white support for capital punishment is associated with racist views (Barkan and Cohn 1994)—lead to two propositions. *Public support for capital punishment can be expected to expand after a growth in the nonwhite population.* And: *Executions should increase after expansions in nonwhite populations.* Minority threat ought to have greater effects on less affluent and the less educated (Turner 1969; Edsall and Edsall 1991; Bobo and Johnson 2004) who are more likely to support capital punishment and whose opinions have a greater influence on the polls than elites. Yet the most important decisions about executions are made by well-educated judges who are more insulated from minority threat, so this racial menace ought to provide a stronger explanation for public support for this penalty.

Economic Threat. The economic version of conflict theory stresses inequality. Anthony Giddens (1971) and Gerhard Lenski (1966) claim that disparities in economic rewards create potentially unstable social arrangements that must be sustained by punitive threats. William Chambliss and Robert Seidman (1980) thus write that “The more economically stratified a society becomes, the more it becomes necessary for dominant groups to enforce by coercion the norms of conduct that guarantee their supremacy” (p. 33). If unequal societies are so fragile, inequality can be expected to produce a greater need for state coercion. It follows that executions ought to increase after expansions in inequality.

Those who are disadvantaged by unbalanced exchanges may use violence to redistribute resources (Blau 1964). Greater inequality also enhances relative deprivation. Inequality thus increases both the psychological impetus and the rewards for redistributive crimes that victimize the affluent. These possibilities ought to be obvious to the affluent and to their political representatives who control sanctions. And theorists such as Alexis Tocqueville and Emile Durkheim who do not stress conflict claim that as inequality expands, affluent citizens will exhibit reduced empathy for their less fortunate counterparts (Whitman 2003). For these reasons, David Garland (1990) claims that criminal punishments are largely determined by relations between the rich and the poor in part because harsh punishments reinforce the hierarchal order and dramatize state power. Lawrence Stone (1987) writes “the 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). It follows that: *Public support for the death penalty can be expected to expand after increases in economic inequality;* and: *A growth in executions ought to*

occur after economic inequality increases. The empirical merits of the inequality and minority threat hypotheses remain unresolved so we test both.²

Mass Protests. The racial and economic disparities stressed by conflict theorists can produce potent protests. In comparison to the less representative European democracies, the United States is the most direct of all large democracies because governmental policies tend to be more closely tailored to public opinion. In such a democracy in which policy is less insulated from shifts in voter sentiments, reactions to such vivid political protests are extensive, so the resulting changes in governmental policies are not narrowly targeted. Theorists repeatedly assert that influential movements produce unanticipated outcomes (Giugni 1998). For example, "The range of effects far surpasses the explicit demands made by activists in the course of social movements" (Tilly 1999:260). Studies provide evidence for such unintended effects (Paul, Mahler, and Schwartz 1997; Deng 1997; Andrews 2001; Isaac and Christiansen 2002). According to Samuel Walker (1993:64) the civil rights movement significantly enhanced awareness about the effects of discrimination in the legal system.

Inasmuch as the risks of execution are greater for African Americans (Paternoster 1991), the protests that led to increased sympathy for the victims of racism (Turner 1969; McAdam 1982) may have reduced the incidence of this punishment. Although civil rights protestors focused on other issues, the NAACP's Legal Defense Fund has actively pursued anti-death penalty lobbying and litigation (Haines 1996). These protests produced other unintended policy shifts. Kenneth T. Andrews (2001) finds an association between such protests and spending on poverty programs although the goal of the demonstrators was voting rights. Civil rights protests also led to greater progressivity in the tax codes (Jacobs and Helms 2001). Inasmuch as racial equity is one of the most controversial aspects of the death penalty, the demonstrations that altered race relations by enhancing sympathies for African Americans (Turner 1969; McAdam 1982) may have had unforeseen negative effects on public support for executions and on their frequency.

While these protests enhanced resistance to the South, they had powerful effects on the conscience of citizens elsewhere (McAdam 1982).³ If this sympathy for the victims of racism had spillover effects, we can expect that: *Increased civil rights protests led to reduced public support for the death penalty.* Especially if we find evidence for the last hypothesis, it is likely that: *As a result of their effects on elite opinion, civil rights protests should reduce executions.* These protests increased sharply after the early 1960s when elites became more sympathetic to this movement's goals (Turner 1969; Edsall and Edsall 1991; Bobo and Johnson 2004). The association between such protests and executions thus ought to increase after the early 1960s. We therefore assess a contingent relationship (Isaac and Griffin 1989) between such protests and executions.

Political Accounts

Partisanship. Conservative parties face difficulties because they favor the prosperous (Allen and Campbell 1994). Income distributions are skewed, so studies of party identification show that moderate left parties have a larger voter base. But political appeals about declines in

2. Inequality has received less support than race. Jacobs (1979) finds relationships between inequality and police strength in MSAs, but this relationship did not persist when he included smaller MSAs. Greenberg, Kessler, and Loftin (1985) do not find an association between inequality and police size, but the cities they analyzed were far smaller than those in the supportive studies and their methods were suspect (see Kent and Jacobs 2005 for a critique). Arrest rates are higher in unequal cities (Williams and Drake 1980; Liška, Chamlin, and Reed 1985; Gove, Sullivan, and Wilson 1998). The probability of a legal death penalty is greater in unequal states (Jacobs and Carmichael 2002). Sorensen, Marquardt, and Brock (1993) find that inequality explains the police use of deadly force, but Jacobs and O'Brien (1998) do not.

3. *National* effects produced civil rights victories. Martin Luther King's nonviolent protests and the violent Southern reactions to these protests highlighted the injustice of Jim Crow racism to previously unconcerned publics in the North (but not in the South) who then at least tacitly supported the *national* civil rights laws that destroyed Jim Crow.

law abiding behavior and public order can help conservatives overcome this handicap (Zimring 2003). Republicans have repeatedly used such law and order issues to gain votes from less prosperous, nonminority citizens who resent the underclass and who are more likely to live where violent crime risks are greater (Edsall and Edsall 1991; Garland 2001; Oliver 2003:126). Nixon campaign officials admit that they used the crime issue to capture anti-minority voters (Beckett 1997).⁴ After they won elections, greater Republican political strength led to increased imprisonments (Jacobs and Helms 1996; Jacobs and Carmichael 2001) and these victories made legalization of the death penalty more likely (Jacobs and Carmichael 2002).

Franklin E. Zimring (2003) claims that "The influence of any American president on the death penalty is enormous" (p. 192). Presidents use what Roosevelt called the bully pulpit to shape opinion. Federal judges depend on the president to enforce their rulings, so Republican control of the presidency should reduce successful death sentence appeals.⁵ Despite a supposedly independent federal judiciary, Tracy E. George and Lee Epstein (1992) find that *national* Republican strength measured by control of the presidency and Congress was the most important determinant of Supreme Court votes on the death penalty. Their rhetorical resources and their direct influence on the Supreme Court also ought to give presidents authority over the lower federal and state courts. In light of the strong George and Epstein findings, and because *all* decisions about the death penalty are subject to Supreme Court rulings, it is difficult to see how partisan control of the presidency would not matter. The fervent Republican support for the death penalty and the effects of their law and order campaigns documented by Katherine Beckett (1997) suggest that expansions in Republican governors also should lead to increased executions.

Hence: *The presence of a Republican president ought to produce increased public support for the death penalty based on the public attention given to presidential rhetoric.* Second, the federal courts and other actors who make these decisions are likely to respond to presidential preferences. Hence: *The presence of a Republican president will lead to more executions.* Third, since state level law and order rhetoric ought to influence public opinion: *Public support for the death penalty should expand after additional Republican governors are elected.* Fourth: *Increases in Republican governors ought to produce expansions in executions.* Yet this relationship will take a long time to be completed as state level partisanship can be expected to affect the first stages in this long legal process, but governors only rarely influence the final stages of the execution process.

Electoral Effects. If one party emphasizes a popular law and order platform, as the Republicans have since 1968, their political rivals will find that their electoral prospects will be

4. For example, "John Erlichman, special counsel to the president, described Nixon's 1968 campaign strategy. 'We'll go after the racists. That subliminal appeal to the anti-black voter was always present in Nixon's statements and speeches'" (Beckett 1997, p. 42, quoting Erlichman 1982, p. 233). The best example of a use of street crime to gain anti-minority votes occurred in the 1988 Bush presidential campaign against Dukakis: "A Republican group blanketed CNN with an ad declaring that 'Dukakis not only opposed the death penalty, he allowed first-degree murderers to have weekend passes from prison' . . . [as the] clearly black [offender]—Willie Horton stared dully into the camera. Bush supporters [then] released a second advertisement . . . featuring [a victim]. 'Mike Dukakis and Willie Horton changed our lives forever. Horton broke into our home. For twelve hours, I was beaten, slashed and terrorized . . . My wife Angie was brutally raped' " Carter (1996, pp. 76–77). Such racial tactics persist. A 2006 Tennessee campaign spot "showed an attractive white woman, bare shouldered . . . who declares she met [the black Democratic Senate candidate] at 'a play-boy party.' [It] closes with the woman saying, with a wink 'Harold, call me. A . . . specialist in media politics [claimed that this spot] 'makes the Willie Horton ad. look like child's play' " (Toner 2006, pp. A1 and A20).

5. George and Epstein (1992:325) explain presidential influence by discussing the President's representative to the Supreme Court. "[S]olicitors general seem to carry great weight with the justices. Their legal expertise . . . and their embodiment of the interests of the nation are just some of the factors explaining [their] . . . excellent record before the Court." Thus, "[scholars] point to the multifaceted relationship between the Court and the president, one that goes well beyond his power to nominate justices or even to send his solicitor general into the Marble palace" (p. 325). Justices have disappointed the presidents who nominated them but the literature suggests that such disappointments are unusual. A Democrat was president for a total of 20 years in our sample. These presidencies include Truman (1951–52), Kennedy-Johnson (1961–68), Carter (1977–80), and Clinton (1993–98).

reduced unless they counter with equivalent appeals. Such electoral effects enhance federal spending on criminal justice (Caldeira 1983), prison admissions (Jacobs and Helms 1996), and sentence severity (Dyke 2003; Kubik and Moran 2003). Hence: *The enhanced law and order rhetoric before the Presidential elections after 1967 ought to produce greater subsequent support for the death penalty.* And: *Such appeals can be expected to increase executions after these elections as this rhetoric ought to influence judges who decide death penalty appeals.*

Public Opinion

Support for the death penalty may explain federal court decisions about death sentence appeals. Although federal judges never face voters, students of judicial politics claim that these officials avoid challenging public views. "Where clear poll margins exist, three-fifths to two-thirds of [Supreme] Court rulings reflect the polls" (Marshall 1989:192; Flemming and Wood 1997). Federal judges depend on presidents to enforce their decisions (Carp and Stidham 2001), but presidents cannot disregard public opinion because they and their political heirs are elected. To protect the legitimacy of the courts, even these seemingly independent federal judges may be unlikely to defy public opinion particularly when public views about an issue are intense.

But public opinion may have stronger associations with the much earlier state and local decisions about the legality of this penalty and capital sentences. Fervent local supporters of the death penalty are unlikely to vote for indulgent prosecutors or judges and it is unlikely that juries in such communities would be lenient. The elected prosecutors and the mostly elected local judges who decide these questions may be more responsive to public opinion than the unelected federal judges who influence appeals. If lags show that support for this penalty has immediate effects on executions, such findings will suggest that these views influence the later stages of the appeals process. If lags show that it takes a long time for this relationship to be completed, the results instead will suggest that public support only affects the first stages controlled by local office holders and jurors.

It would be surprising if public opinion was inconsequential because views on this issue are so intense. We study this association in the most direct of all large democracies that probably is most responsive to citizen opinions. And the death penalty is likely to be legal where public support for this punishment is greatest (Erickson 1976; Mooney and Lee 2000). Hence: *Shifts in public support for the death penalty ought to influence the number of executions.* The time between first death sentences and executions since 1950 is about 7.5 years. Evidence for such long lags between an explanatory factor and executions will suggest that the factor affects the initial state and local stages. But if lags show that this relationship is completed after a year, the results will show that public support influences the appeals process rather than the initial local stages. Our time-series design ought to fill an important gap in the literature by providing dynamic information about when the forces at issue influence executions.

Alternative Explanations

This penalty should covary with the number of capital crimes in death penalty states, but there is long delay between such crimes and executions.⁶ But a growth in murder *rates* may lead to increased executions via greater media coverage and increased political rhetoric about street crime. This threat is best modeled with rates because the media focus on rates. The threat captured by the murder rates need not be delayed by as long as the effects of the number of murders. Although murder counts must be lagged so these offenses can affect death sentences,

6. Death sentence data that begin early enough to allow the necessary eight-year (or more) lags (the time between sentence and execution is about 7.5 years) are unavailable (these figures begin in 1956), so we use the number of murders in death penalty states to gauge crimes subject to this penalty. In one analysis we introduce a control for the number of death row inmates, which should better capture persons at risk of an execution than death sentences.

the threat captured by the murder *rates* could influence the later stages in the execution process. And this relationship between murder rates and executions may not be constant over time because street crime became so politicized after 1967, so we test contingent associations between murder rates and executions. The number of murders may be a weak control for incidence, so we also capture the supply of capital offenders by using the number of death row inmates. Inasmuch as presidential power over judges may be based on nominations, we include Republican appointments to the federal bench in some models as well.

In 1972 in *Furman*, the Supreme Court held that the death penalty was unconstitutional. The resulting political outcry was intense (Epstein and Kobylka 1992; Zimring and Hawkins 1986:38–41). These fierce reactions probably enhanced public support for capital punishment (Beckett 1997). We expect a positive relationship between this decision and public support, but *Furman* could not affect executions as the national moratorium began four years before this decision (see Figure 1). But in 1976 in *Gregg* the Supreme Court reversed and upheld the death penalty (see note 3). This relegalization should produce an acceleration in executions. And finally, perhaps public views about this punishment are influenced by how often it is used. To assess this intriguing relationship, we also include the lagged number of executions in one model.

In sum, civil rights protests ought to reduce both public support and execution frequencies, but public support for capital punishment should have a positive relationship with executions. Republican presidencies, governorships, law and order presidential elections, and inequality ought to enhance both public support and executions. Yet although racial threat is likely to explain public support, it may not have an relationship with executions. Finally, we expect that the *Furman* decision that briefly made executions unconstitutional will explain public support but the *Gregg* decision that relegalized this penalty will explain executions. We must analyze public opinion because the findings will show that some explanations go *through* public support for this penalty to influence execution frequencies.

Methods

Research Design, The Dependent Variable, and Estimation

Design. We use national data because the legal process that leads to an execution is controlled by both state and federal officials, but federal law dominates (Zimring 2003). For example, on the basis of a correct expectation that the Supreme Court soon would rule on this issue, the lower federal courts stopped executions from 1968 until the Supreme Court's 1972 *Furman* decision (Zimring 2003). Other examples of federal supremacy include appeals. Since *Furman*, the federal courts required that at least 21 percent of those given a death sentence by state courts be retried, yet just 12 percent of these death sentences were upheld in the second state trial (Liebman et al. 2000). And federal rulings forced state appellate courts to overturn many death sentences. The entire appeals process is crucial because it is responsible for so many death row exits (Liebman et al. 2000). The preeminence of federal law, the fact that less than ten percent of all death sentences result in executions largely because of the lengthy appeals process, and the seven to nine year delay between sentencing and execution since 1951 clearly make a national level time-series design a useful way to learn more about this process.

The Dependent Variables and the Sample. In the primary analyses we model the number of yearly executions from 1951 to 1998.⁷ But we also assess the determinants of public support for the death penalty using the percentage of respondents who answered yes when they were

7. We follow Kaminski and Marvell (2002) and use their counts taken from the Center for Disease Control data on executions rather than Bureau of Justice counts as Kaminski and Marvell claim that the CDC data are more accurate. This choice, however, does not affect the results.

asked three slightly different questions in the Gallup, the General Social Survey (GSS), and the Harris polls.⁸ Linear interpolation is used to calculate missing values in the few years when at least one of these three surveys did not ask a death penalty question. The reliability of this three-item scale is .94. Explanatory variables are available from 1947 to 1997, but lags, first-differencing, and corrections for serial correlation remove initial cases, so analysis samples are reduced to less than 50 years.

Estimation. Count estimation is unsuitable because corrections for serial correlation are not well developed (Cameron and Trivedi 1998). And differencing to correct unit root problems cannot be used as count estimators cannot handle a dependent variable with any negative values. Least squares, if an outcome has many zero minimum scores will produce erroneous estimates, but, to deal with unit root problems, we must difference executions (Enders 1995).⁹ Some of these differenced dependent variable scores inevitably will be negative. This rules out count estimation, but it means that this differenced dependent variable will *not* have zero minimum scores. Statistical tests show that both execution counts and their residuals are normally distributed after we follow a methodological recommendation from Jacob Cohen and associates (2003:245) and place these counts into square root form before they are differenced. It follows that a least squares estimator clearly is most suitable.

Measurement of Explanatory Variables and Model Specification

We use the public opinion index we constructed from Gallup, GSS, and Harris poll data with both long and short lags to explain executions. This variable is in two-year moving average form.¹⁰ Moving averages are employed because many explanatory variables ought to have multi-year cumulative effects and because the time between first sentence and execution is not constant. The mean lag between sentence and execution from 1951 to 1998 is about 7.5 years, but it increases to more than 9 years after the 1978 resumption in executions. We chose lags and moving averages on the basis of theory and these delays (and in one equation we force most to be equal).¹¹ Civil rights protests are assessed with counts (Jenkins and Eckert 1986) extended by J. Craig Jenkins to 1998. Less contentious events such as meetings, litigation, press conferences, and rallies, as well as protests, are included because acts that enhanced sympathy for blacks (Turner 1969) should explain executions with a one-year lag.

8. The Gallup question is "Are you in favor of the death penalty for persons convicted of murder?" The GSS question was "Do you favor or oppose the death penalty for persons convicted of murder?" (before 1974 the GSS used the Gallup question). The Harris question is "Do you believe in capital punishment, that is the death penalty, or are you opposed to it?"

9. The Phillips-Perron and Dickey-Fuller unit root tests applied to the two dependent variables show that these variables must be first-differenced. We first-difference all explanatory variables save the two Supreme Court case dummies because unit root tests give the same results for most of these variables and because differencing a dependent variable and the explanatory variables produces coefficients that have the same interpretation if these series were left in level form (see note 18). Finally, because serial correlation is eliminated and all explanatory variables are lagged, simultaneity bias should not be present.

10. The series 1, 3, 7, 6, 11 would be equal to 2, 4, -1, and 5 after it was differenced; if the original series was transformed with a two-year moving average it would be equal to 2, 5, 6.5, and 8.5. Note that both transformations remove the first case. A three-year moving average would remove the first two cases and so on.

11. Moving averages are used to capture cumulative relationships that take more than a year to be completed. We choose lags on the basis of theory, but theory is uninformative about multiyear cumulative effects. Johnston (1984) provides a remedy: "Economic theory is mostly about *equilibrium* situations and contains little in the way of systematically developed dynamic theory. Thus it cannot be expected to yield strong insights about lag(s) . . . [such theoretical gaps] inevitably lead to a certain degree of interaction between theory and data" (p. 501, emphasis in original). Since sociological theory says little about multi-year effects, we choose moving averages on the basis of theory and empirical reasons. The only statistics we could find on the time between death sentences and executions are sometimes in median and sometimes in mean years, so these durations are approximate.

A first-differenced dummy coded “1” if a Republican is president is lagged by one year to assess national level partisan strength.¹² But Republican control over the presidency may work through Republican appointments to the federal courts. This explanation is assessed with the number of these appointments in square root form (Cohen et. al 2003:245), and it is lagged by two years and transformed with a two-year moving average. We measure state level partisanship with the proportion of Republican governors. As it is likely that Republican strength in the states only will affect the early stages in this process, we lag this variable by ten years and place it in two-year moving average form. To capture the effects of law and order campaign rhetoric, we use a dummy coded “1” for Presidential election years after 1967. Findings (Caldeira 1983; Jacobs and Helms 1996) suggest that this variable ought to be lagged by a year.

We assess minority threat with the percentage of nonwhites lagged by a year (statistics on blacks are unavailable until 1962). After differencing, this measure’s coefficient of variation is .448, so it has sufficient variance. We measure economic inequality with a Gini index calculated on family incomes lagged by one year. We gauge the supply of capital offenses with a two-year moving average of the number of murders in those states with a legal death penalty lagged by eight years (complete information on death sentences is unavailable). We measure the murder rates using Robert J. Kaminski and Thomas B. Marvell’s (2002) counts. This threat effect may influence the intermediate stages of the execution process over multiple years. We lag this variable by four years and transform it with three-year moving averages to assess multi-year affects and shifting lags. We sometimes split this indicator by period to capture an enhanced association between the murder rates and executions after the intense politicization of street crime.

Perhaps as a result of the sharp objections to the 1972 *Furman* decision enumerated by Epstein and Kobylka (1992) and Zimring and Hawkins (1986), this ruling that made executions unconstitutional probably accelerated public support for the death penalty (Zimring and Hawkins 1986). We use a dummy variable coded “1” for years from 1972 until 1976 (when *Gregg* superseded *Furman*) in the public opinion analyses. A dummy variable coded “1” from 1977 on is included in the execution models to capture the expected positive acceleration in executions after the 1976 *Gregg* decision that relegalized this punishment. This control is lagged by two years. In contrast to all other variables, these two are not differenced. If a dependent variable is differenced, explanatory variables that are not differenced test for the acceleration effects we anticipate. Theory suggests positive signs on all coefficients but civil rights protests.

Specification. One general specification of the supplemental analysis that models the percentage who support the death penalty therefore is:

$$\begin{aligned}
 d(\text{PUBLIC SUPPORT}) = & \mathbf{b}_0 + \mathbf{b}_1 d(\text{CIVIL RIGHTS})_{t-1} + \mathbf{b}_2 d(\text{REPUBLICAN PRES.})_{t-1} \\
 & + \mathbf{b}_3 d(\text{REPUBLICAN GOV})_{t-1} + \mathbf{b}_4 d(\text{PRES. ELECTION AFTER 1967})_{t-1} \\
 & + \mathbf{b}_5 d(\% \text{NONWHITE})_{t-1} + \mathbf{b}_6 d(\text{INEQUALITY})_{t-1} + \mathbf{b}_7 d(\text{MURDER RATE})_{t-1} \\
 & + \mathbf{b}_8 \text{FURMAN} + \mathbf{b}_9 d(\text{N EXECUTIONS}) + \epsilon
 \end{aligned} \tag{1}$$

where d is the first difference operator and all variables are defined as above, but civil rights protests and the percentage of nonwhites are in log (to the base e) form to capture the modest nonlinear relationships between these variables and public support. To assess relationships with combined lagged and immediate effects, we transform Republican governors with a two-year moving average and executions with three-year moving average.

12. Absent expectations about accelerating relationships, dummy variables must be differenced because the dependent variable is differenced (Kennedy 1998). Note that sophisticated time-series studies in macroeconomics often employ different lags on explanatory variables.

One of the more general specifications of the execution models is:

$$\begin{aligned}
 d(\text{N EXECUTIONS}) = & \mathbf{b}_0 + \mathbf{b}_1 d(\text{PUBLIC SUPPORT})_{t-7} + \mathbf{b}_2 d(\text{CIVIL RIGHTS EARLY})_{t-1} \\
 & + \mathbf{b}_3 d(\text{CIVIL RIGHTS LATE})_{t-1} + \mathbf{b}_4 d(\text{REPUBLICAN PRES.})_{t-1} \\
 & + \mathbf{b}_5 d(\text{PROP. REPUBLICAN GOV.})_{t-9} + \mathbf{b}_6 d(\text{PRES. ELECTION AFTER 1967})_{t-1} \\
 & + \mathbf{b}_7 d(\text{REPUBLICAN JUDGES})_{t-2} + \mathbf{b}_8 d(\% \text{NONWHITE})_{t-1} + \mathbf{b}_9 d(\text{INEQUALITY})_{t-1} \\
 & + \mathbf{b}_{10} d(\text{MURDER RATE})_{t-4} + \mathbf{b}_{11} d(\text{N MURDERS})_{t-9} + \mathbf{b}_{12} \text{GREGG}_{t-2} + \mathbf{e}. \quad (2)
 \end{aligned}$$

where all variables are defined as above. All effects save executions, when it is used as an explanatory variable, and the intercepts are assessed with one-tailed tests (see the Appendix for the data and see note 19 for two-tailed test results). Table 1 reports the correlations for the execution analyses. These relationships are modest and the correlations (not shown) between variables in the supplemental analysis of public support for this penalty are weaker (all findings not shown are available on request).¹³

Analyses

Analyses of Public Support for Capital Punishment

Model 1 in Table 2 assesses the effects of murder rates, the percentage of nonwhites, a dummy variable for the 1972 *Furman* decision, and a dummy variable coded "1" to ascertain the effects of Republican presidents on public support for this penalty. In Model 2, we add law and order elections, civil rights protests, and Republican governors. Economic inequality replaces the ineffective Republican president measure in Model 3. In Model 4, we add lagged number of executions to discover if prior executions altered public support.

The findings in the first model show that murder rates, minority threat, and the sharp political outcry about the *Furman* decision have positive relationships with death penalty support, but Republican presidents do not matter. These results hold in Model 2, but these findings suggest that the law and order campaign appeals in presidential elections after 1967, civil rights protests, and Republican governors also influenced public support for this punishment. In Model 3 the results show that public support for capital punishment increases after a growth in inequality. When we add executions in three-year moving average form in Model 4, it is interesting that the findings suggest that additional executions *reduce* public support for this penalty.

In addition to the effects of the *Furman* decision and the murder rates, other factors influence support for this punishment. As the literature suggests (Jacobs and Helms 1996; Dyke 2003; Kubik and Moran 2003; Caldeira 1983), support for the death penalty expands after law and order elections and after increases in Republican governors. Minority and economic threat also matter, but increased civil rights protests reduce support for this penalty. With the particularly useful presidential election and civil rights findings in mind, we now analyze executions.

The Determinants of Executions

In Table 3 we again start with a restricted model and include public support for capital punishment lagged by ten and by one year, murders in death penalty states, and the *Gregg* decision. The murder rates, two measures of Republican strength, and economic inequality are added to Model 2, but we drop the ineffective public opinion indicator with a one-year lag. In Model 3 minority threat is added to the prior explanatory variables. In Model 4 we include a dummy coded "1" for the Clinton years. Clinton's enthusiastic support for the death

13. Differencing means that zero scores when there were no executions or the zero scores produced when there was no change in the yearly execution counts will not be problematic. After differencing, all zero values are located close to the midpoint of the dependent variable.

Table 1 • Correlation Matrix and Descriptive Statistics for Variables in the Execution Analyses

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1. # Executions (sqr.)	1.000											
2. % Supporting the death penalty $t-10^a$.339	1.000										
3. 1 if <i>Gregg</i> $t-2$.353	.436	1.000									
4. # Murders in dth pen states $t-8$.003	-.296	-.035	1.000								
5. Murder rate $t-4^a$.187	.000	-.211	-.187	1.000							
6. 1 if Republican president $t-1$.098	-.095	-.085	.047	-.143	1.000						
7. Prop. Republican governors $t-10^a$.069	.128	-.064	-.001	.016	-.230	1.000					
8. Inequality (Gini) $t-1$.160	.154	.277	.150	.027	-.012	-.145	1.000				
9. # Republican federal judge appointments (sqr.) $t-2$.196	.178	-.121	.020	.054	.410	-.277	.278	1.000			
10. % Nonwhite $t-1$.407	.267	.681	.192	.077	-.014	-.117	.287	-.020	1.000		
11. 1 if pres. election yr. after 1967 $t-1$.195	-.017	.000	-.093	-.005	.000	.000	-.206	.074	.072	1.000	
12. # Civil rights protests $t-1$	-.102	-.174	-.039	-.112	-.189	.004	.038	.020	-.053	-.092	.053	1.000
Mean	-.018	.009	.417	234	.091	.000	.002	.001	.000	.143	.000	-.115
Standard dev.	1.04	.215	.498	710	.330	.000	.053	.007	1.67	.064	.583	55.3

Notes: Except for the *Gregg* decision, all variables are in difference form. $N = 48$ years.

^aProportion of Republican governors and public support are in two-year moving average form while the murder rates are three-year moving averaged.

Table 2 • Time-Series Least Squares Analyses of Public Support for the Death Penalty with All Variables but Furman in Difference Form^a

Explanatory Variable	Model 1	Model 2	Model 3	Model 4
Ln murder rate _{t-1}	2.0847* (.9400)	1.6880* (.8358)	1.5092* (.7871)	1.1947* (.6539)
Ln % nonwhite _{t-1}	26.7232** (10.6715)	21.0738* (9.2636)	17.3878* (8.8806)	30.5518*** (8.4955)
1 if <i>Furman</i> (1972–1976)	.3724** (.1424)	.4003** (.1275)	.4106*** (.1203)	.4146*** (.0999)
Republican president _{t-1}	-.0878 (.0987)	-.1400 (.0917)	—	—
1 if Presidential election after 1967 _{t-1}	—	.1145* (.0544)	.1410** (.0566)	.1725** (.0557)
# Civil rights protests _{t-1}	—	-.0013* (.0006)	-.0012* (.0006)	-.0017** (.0005)
Prop. Republican governors _{t-1}	—	1.2215* (.5964)	1.3284* (.5752)	1.3297** (.4927)
Inequality (Gini) _{t-1}	—	—	8.9710* (4.8566)	14.0337** (4.8347)
# of executions	—	—	—	-.0171** (.0056)
Intercept	-.3132* (.1151)	-.2605* (.0993)	-.2336* (.0935)	-.3940*** (.0932)
R ² (corrected)	.303***	.425***	.442***	.531***
D.W. statistic	1.999	2.034	2.045	2.009
N (yrs.) & sample	49 (1950–98)	49 (1950–98)	49 (1950–98)	49 (1950–98)
AR terms at lags of:	1 year	1 year	1 year	1 year

* $p < .05$ ** $p < .01$ *** $p < .001$ (standard errors are in parentheses; one-tailed tests)

^aThe number of civil rights protests and the proportion of Republican governors are two-year moving averaged, the percentage of nonwhites and the number of executions are three-year moving averaged, while the murder rates are in four-year moving average form.

penalty during his campaign suggests that his administration may have facilitated capital punishment with the same fervor as prior Republican administrations. We again enter public support lagged by a year in Model 4 to find out if this immediate association is present in a model that is more exhaustive than Model 1.

In Model 1, public support for capital punishment with a ten-year lag explains executions, but it has no effect when it is lagged by a year. This contrast suggests that public support affects the initial stages, but it does not influence the later appeals. The next model shows that increases in national level Republican strength produce more executions. Growth in murder rates and economic inequality along with the *Gregg* decision that made capital punishment legal again also produced more executions. Yet the addition of the nonwhite presence variable in Model 3 does not increase the plausibility of a racial threat explanation for executions. And Model 4 gives no support for claims that the Clinton administration encouraged executions or that the two political parties are converging on this issue. These results again suggest that an *immediate* relationship between public support for this penalty and executions does not exist. But it remains to be seen if civil rights protests matter or if executions expand after the law and order presidential elections. We also should discover if presidential partisanship effects are based on judicial appointments rather than the indirect and informal links found by George and Epstein (1992).

Model 5 in Table 4 includes civil rights protests, a presidential election (after 1967) dummy variable, and the number of Republican appointments to the federal bench. We also

Table 3 • Time-Series Least Squares Analyses of the Number of Executions with All Variables but Gregg in Difference Form^a

Explanatory Variable	Model 1	Model 2	Model 3	Model 4
Support for the death penalty _{t-10}	1.2218** (.4917)	.7813** (.3043)	.7821** (.3077)	.7546* (.3416)
1 if Gregg (1977 on) _{t-2}	.4794** (.1656)	.6497*** (.1449)	.6094** (.2288)	.6690* (.3054)
# of murders _{t-9}	.0002 (.0001)	.0001 (.0000)	.0001 (.0001)	.0002 (.0001)
Murder rate _{t-4}	—	.8519*** (.1772)	.8307*** (.2019)	1.0049*** (.2776)
Republican president _{t-1}	—	1.1355*** (.2323)	1.1327*** (.2353)	.7595** (.3152)
Prop. Republican governors _{t-10}	—	1.8492 (1.2561)	1.8747 (1.1616)	.0417 (1.4590)
Inequality (Gini) _{t-1}	—	22.6810* (11.2914)	23.1531* (11.6124)	38.8352** (14.6097)
% nonwhite _{t-1}	—	—	.3940 (1.7195)	-.0828 (2.0577)
1 if Clinton years _{t-1}	—	—	—	-1.6761* (.8035)
Support for the death penalty _{t-1}	.6122 (.4191)	—	—	.0063 (.3373)
Intercept	-.2971** (.0966)	-.4247*** (.0974)	-.4599* (.1827)	-.4260 (.2685)
R ² (corrected)	.477***	.690***	.682***	.668***
D.W. statistic	2.073	1.868	1.853	2.031
N (yrs.) & sample	46 (1953–98)	47 (1952–98)	47 (1952–98)	46 (1953–98)
AR terms at lags of:	1 & 4 yrs.	1 & 3 yrs.	1 & 3 yrs.	1, 2, 3, & 4 yrs.

* $p < .05$ ** $p < .01$ *** $p < .001$ (standard errors are in parentheses; one-tailed tests)

Note: Ns vary due to different corrections for serial correlation.

^aProportion Republican governors, Republican president, and public support are in two-year moving average form while the murder rates are three-year moving averaged.

break the murder rates by period to test a contingent relationship between this threat and executions. Model 6 retains all prior variables, but we now break civil rights protests by period to see if later protests were increasingly influential. In Model 7 we restrict all moving averages to two years, and we only use ten-year lags on the variables that affect the earliest stages of this process. In Model 8 we add the number of prisoners on death row to discover if the supply of capital offenders produces additional executions.

All factors with prior explanatory power continue to matter in these models. The first two models suggest that executions expand after law and order presidential elections, but a growth in civil rights protests reduces executions. And these findings show that taking contingencies into account increases the explanatory power of the murder rates and civil rights protests. Republican judicial appointments, however, do not matter. Instead, consistent with the George and Epstein findings, Republican control of the presidency continues to explain executions. Model 7 shows that the findings persist when moving averages and long lags are forced to be equal, so the conclusions probably do not depend on these measurement decisions. Finally in the last model we add the number of death row inmates. Because data for this variable begins in 1953, lags are constrained by degrees of freedom. We compromised and used a five-year lag and a two-year moving average, but this factor does not matter.

Table 4 • Additional Time-Series Least Squares Analyses of the Number of Executions with All Variables but Gregg in Difference Form^a

<i>Explanatory Variable</i>	<i>Model 5</i>	<i>Model 6</i>	<i>Model 7^b</i>	<i>Model 8</i>
Support for the death penalty _{t-10}	.5536* (.2450)	.6603*** (.1838)	.4090* (.1772)	.4660* (.2148)
1 if Gregg (1977 on) _{t-2}	.6253*** (.0835)	.6172*** (.0672)	.6106*** (.0742)	.7969*** (.1199)
# of murders _{t-8}	.0001 (.0001)	.0001* (.0000)	.0000 (.0000)	.0001* (.0000)
Republican president _{t-1}	1.0241*** (.2330)	.9912*** (.1798)	.9051*** (.1403)	.9395*** (.2418)
Prop. Republican governors _{t-9}	1.5159* (.7581)	1.4277* (.6061)	2.5870*** (.7168)	2.1367*** (.6715)
Inequality (Gini) _{t-1}	32.9386** (10.7651)	28.1559** (8.9817)	42.4839*** (9.4661)	28.7112* (13.6948)
1 if presidential election after 1967 _{t-1}	.3015* (.1454)	.2788* (.1171)	.2594* (.1213)	.2423* (.1144)
# Republican fed. judge appointments (sqr) _{t-2}	.0109 (.0393)	-.0117 (.0310)	.0475* (.0278)	.0018 (.0378)
Murder rate before 1975 _{t-4}	-.1637 (.1986)	-.0518 (.1487)	-.1291 (.1540)	.3596 (.2508)
Murder rate after 1974 _{t-4}	1.1205*** (.1225)	1.1141*** (.0974)	1.1518*** (.1097)	1.1686*** (.1030)
# Civil rights protests _{t-1}	-.0017* (.0009)	—	—	—
# Civil rights protests before 1962 _{t-1}	—	.0010 (.0009)	.0009 (.0009)	.0010 (.0011)
# Civil rights protests after 1961 _{t-1}	—	-.0017** (.0006)	-.0020*** (.0006)	-.0015* (.0006)
# Death row prisoners	—	—	—	-.0003 (.0005)
Intercept	-.4220*** (.0431)	-.4192*** (.0346)	-.4036*** (.0353)	-.5384*** (.0639)
R ² (corrected)	.766***	.837***	.828***	.849***
D.W. statistic	1.778	1.921	1.839	2.184
N (yrs) & sample	48 (1951–88)	48 (1951–98)	47 (1952–98)	37 (1962–98)
AR terms at lags of:	1 & 2 yrs	1 & 2 yrs	1 & 3 yrs	1 & 2 yrs

* $p < .05$ ** $p < .01$ *** $p < .001$ (standard errors are in parentheses; one-tailed tests)

Notes: Ns vary due the elimination of start or end points.

^aProportion Republican governors, Republican president, and public support are in two-year moving average form while the murder rates are three-year moving averaged. In Model 7 to show what happens when lags and moving averages are constrained to be similar, presidential partisanship is not moving averaged and all variables with long lags are lagged at ten years.

Sensitivity

We used the number of murders to measure the supply of capital crimes, but some rapists who did not kill were executed before 1968. All attempts to include rapes destroyed the explanatory power of capital crimes in the one model in which it was effective (probably because of the errors in the Uniform Crime Report data on rapes). Census data on unemployment, black unemployment, and data from Isaac and Kelly (1981) on riots, or data from the statistical abstract on House or Senate partisanship do not explain these outcomes. The findings persist when the initial or final years are removed. Although unit root tests show that variables in level form will

produce incorrect estimates, execution results (not shown) indicate that the results do not depend on differencing.¹⁴ Other statistical tests suggest that these specifications are accurate.¹⁵ The similar findings across different models, the multiple controls, the diagnostic test results, and the explanatory power despite differencing combine to support a claim that these analyses have captured the primary factors that explain both public support and executions.¹⁶

Discussion

The Results

The racial findings suggest that expansions in the nonwhite population produce greater public support for capital punishment, but this growth does not produce additional executions. Yet inequality leads to both greater public support and increased executions. This contrast is plausible as nonelites whose views have far greater statistical weight in the polls are more likely than elites to live where the risk of street crime is greater and, compared to educated elites, nonelites are less sympathetic to the plight of minorities (Turner 1969; Bobo and Johnson 2004). An expectation that the educated judges who rule on appeals are threatened by larger minority populations therefore is not as plausible as an expectation that nonelite anxieties and resentments will be triggered by expansions in minority presence. Hence minority threat ought to explain public support for this penalty, but it should not account for executions, and that is what the results show.

The findings show that civil rights protests reduce public support for capital punishment. The immediate negative relationship between these protests and public support for this penalty suggests that such protests have similar effects on the educated and more sympathetic (Turner 1969) elites who decide appeals, so findings that these protests reduced executions ought not be surprising. This interpretation is strengthened by evidence that mass protests have important unforeseen effects (Deng 1997; Andrews 2001; Jacobs and Helms 2001; Isaac and Christiansen 2002). Particularly in the most direct of all large democracies, it is unlikely that the powerful movement that eliminated the Jim Crow caste system would produce only narrowly targeted responses. The sympathy for the victims of racism inspired by those protests instead affected other intensely moral policies including the harshest criminal punishment that is so often viewed in racial terms (Barkan and Cohn 1994).

Of course, other explanations for this relationship can be put forward. But our joint findings that civil rights protests reduced *both* public support for the death penalty *and* executions diminish the plausibility of alternative explanations such as minority appeasement. If this negative relationship between civil rights protests and executions is present because the judicial elites who control death penalty appeals sought to mollify protesting African Americans, why did these protests also reduce public support for this penalty? Note also that such relationships

14. Although unit root tests indicate that variables must be differenced, when they are not, the results (not shown) are theoretically identical to those in the reported execution models, but serial correlation must be corrected at lags of 1, 2, 3, and 4 years producing a net reduction in cases. The public opinion results are similar to those reported, but when they are estimated in level form, serial correlation could *not* be corrected. Because unit root tests show that these series must be differenced, such problems should not be surprising.

15. For readers who prefer two-tailed significance tests even after theoretical justification for sign has been provided, we list coefficients that reached one-tail significance but did not reach the two-tailed .05 level: In Table 2 in Models 3 and 4 the coefficients on the murder rate do not reach this threshold and in Model 3 the coefficient on the percentage of non-whites does not reach this level; in Table 4 the number of murders does not reach this level in Models 6 and 8. In Model 5 the proportion of Republican governors and in Model 7 the number of federal judge appointments do not reach the two-tailed .05 level either.

16. The Chow forecast test and the last models restricted to different periods suggest that explanatory effects do not differ before or after the moratorium on executions. Sociologists may wonder about the number of regressors, but Johnston (1984, p. 262) states that exhaustive models are most accurate. Almost all explanatory variables are significant, and there are strong reasons to include the few that are not.

only can be assessed with a national level analysis. Clearly there were and are substantial state differences in executions, but current data limitations rule out any subnational research design that might explore the association between civil rights protests and executions because state level data on these protests is currently unavailable.

The findings show that greater Republican political strength in the states leads to increased public support for the death penalty, but it takes a long time before this variable explains executions probably because a law and order state climate has its greatest effects on death sentences rather than appeals. But the immediate relationship between Republican presidents and executions suggests that Republican control of this powerful office reduces the probability that death row inmates will win appellate relief. These results are supported by George and Epstein's (1992) findings that a strong association between Presidential partisanship and Supreme Court votes on the death penalty exists mostly based on presidential rhetorical powers. Franklin Zimring's (2003) repeated claims about the importance of the presidency when the death penalty is at issue give added support for this explanation about the effects of presidential influence.¹⁷

It is unfashionable in the disciplines that study politics to investigate the effects of partisan differences (Bartels and Brady 2003). Left leaning political sociologists often assume there are only trivial differences between U.S. parties, so such contrasts are not worth investigation. Yet these and other findings (George and Epstein 1992; Sutton 2000; Jacobs and Carmichael 2002) and the vivid quotes in note 6 suggest that partisan differences on criminal justice issues should have considerable explanatory power.

Another national political effect matters. The public opinion results suggest that support for this punishment grows after national law and order elections. In accord with the studies showing that such elections produce increases in other criminal punishments (Jacobs and Helms 1996; Dyke 2003; Kubik and Moran 2003), we find that national law and order elections also generate additional executions because such campaign appeals influence public opinion and probably the judges who decide appeals. These findings suggest that national politics shape these outcomes even though presidents have little direct control over criminal punishments. Other results showing that national political rhetoric heightens the public salience of crime (Beckett 1997) enhance the plausibility of this indirect causal story. And similar national partisan and electoral explanations have equivalent effects on prison admissions (Jacobs and Helms 1996), even though incarceration differs sharply from capital punishment in part because the federal courts rarely intervene.

Clearly combined research that assesses both national, state level, and individual factors would be optimal, but data availability means that state level research must ignore theoretically important effects that can be assessed with a national time-series design. In this analysis we assess the explanatory power of factors that only can be measured with national data, but in another investigation (Jacobs et al. forthcoming) we analyze the state level contextual and individual determinants of executions. Yet that study cannot assess the effects of shifts in public support for the death penalty or civil rights protests because the necessary data is not available.

Public attitudes about the death penalty have been well researched, but no findings seem to be available on the effects of public opinion on executions. Public support for this penalty evidently matters, but we find no evidence that the appellate courts attend to public opinion as lags show that this variable does not affect the last stages of the appeals process. The long delay between shifts in public opinion and executions instead suggests that public opinion only influences the first state and local decisions about this sentence. This is plausible as the politicians who decide this sanction's legality and most local court officials are elected, but federal judges are not. Hence this federal appeals process—that may have the most important effects on executions—is less subject to democratic control than state decisions about the legality of the death penalty (Erickson 1976; Jacobs and Carmichael 2002). Although the constitutional separation

17. In addition to the crucial influence of the federal court system on executions that must be modeled with national data, shifts in both support for and the use of the death penalty evidently are due to national level political processes (see Savelsberg 1994 and Chambliss 1994 for added claims about the importance of national politics).

of powers gives no warrant for this, our results and those reported by George and Epstein (1992) on how the President affects Supreme Court votes on the death penalty suggest that presidents influence these supposedly apolitical decisions about who lives and who dies.

These rich findings have multiple theoretical implications. They support the earlier conflict theorists who emphasized social divisions (Turk 1969). Minority threat explains public support for the death penalty and inequality accounts for public support and execution frequencies. But we also find that politics matters. Executions increase when the law and order party controls the presidency. And law and order election campaigns produce both enhanced support for capital punishment and increased executions. Another important refinement highlights the added explanatory power of this political emphasis. Civil rights protests lead to reductions in both public support for capital punishment and executions. Such parallel results showing that law and order elections after 1967 and civil rights protests have similar relationships with public support *and* with execution frequencies substantiate claims that these two factors influence executions as a consequence of their effects on public and probably elite opinion.

Broader Implications

This longitudinal approach has produced new insights. Lags show when various factors affect different stages in this process. In macro studies, it often is necessary to make assumptions about unmeasured effects that intervene between measurable explanatory factors and the outcome at issue, but this national time-series design provides a way to assess one important intervening factor. In support of Zimring's (2003) claim that capital sanctions "were always matters of federal law and the ultimate judgment was that of the federal courts" (p. 9), we find that public support for capital punishment only affects the initial sentencing stages, but these views have no discernable influence on the later and probably more critical appeals process.

These findings show that the directly democratic U.S. political institutions (Sutton 2000) in this racially divided society encourage criminal justice policies ostensibly designed to achieve vengeance. And they help explain why the United States is the last western nation to use the death penalty. According to James Q. Whitman (2003): "A relatively weak state, like the American one, is much more prey to a harsh retributive politics than continental states are" (p. 201). In this most direct of all large democracies conservatives have repeatedly used the death penalty to gain votes. But in Europe "punishment is a technical matter, not a matter of social revenge . . . [in these nations the] tepid bureaucratic routinization of the criminal law is an important barrier to the kind of overheated democratic retributivism that has come to America" (Whitman 2003, p. 200). Because their comparatively elitest democratic institutions remove decisions about punishment from the voters, the autonomous European bureaucratic experts who control criminal justice policy in these racially homogeneous societies can focus on the social reintegration of criminals rather than their incapacitation by isolation or by execution.

The primary story therefore is political. Partisanship at the state and particularly at the national level, national election campaigns about law and order, and vivid political demonstrations for racial equity helped produce shifts in public support for capital punishment and shifts in the number of executions. Such findings suggest that those who wish to test explanations for criminal justice outcomes should take the new theoretical emphasis on politics (Garland 1990; 2001; Savelsberg 1994; Whitman 2003) seriously. The findings also show that a politically informed version of conflict theory offers greater explanatory power than the earlier versions that largely ignored politics and focused only on social divisions.

These results also may provide some insights about the future. First, expansions in executions seem to reduce public support for capital punishment. Yet the long delay before shifts in public opinion affect executions suggests that this negative relationship between additional executions and public support is not the best news for death penalty opponents.¹⁸ Second, potent

18. Recall that it is unlikely that simultaneity bias is present because the explanatory variables are lagged and the effects of serial correlation are removed.

social movements that convincingly show publics that the least privileged are treated inequitably may spill over to reduce harsh punishments. Third, these results, Zimring's (2003) comments, and the quotes in note 6 suggest that abolitionists will be more likely to succeed in times when Republicans are politically weakest. Yet the most important finding concerns the influence of a remarkable social movement. The civil rights protests that altered the conscience of a nation and changed one of the most pernicious social institutions in U.S. history (Myrdal 1944) evidently had unanticipated effects. These intensely moral protests also helped shape perhaps the most vexing and still unresolved ethical issue about punishment in the United States.

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Appendix

Year	# Ex.	Pub. Supt.	Gregg	# Murdr.	Rep. Pres.	Rep. Gov.	Ineq.	Elt. 67+	# Rep. Judgs.	Murd. Rate	# Civ. Rigs. Priss.
1948	117	-.0161	0	8209.5	.0	.135	.376	0	0	5.221	34
1949	120	.1840	0	7975.5	.0	.250	.371	0	0	5.069	36
1950	83	.2031	0	7731.5	.0	.375	.378	0	0	5.507	23
1951	101	.2221	0	7122	.0	.396	.379	0	0	5.913	17
1952	81	.2412	0	6544.5	.0	.417	.363	0	0	6.083	24
1953	63	.2602	0	6894.5	.0	.458	.368	0	0	5.776	18
1954	83	.2793	0	8004	.5	.521	.359	0	0	5.501	26
1955	79	.2983	0	8581.5	1.0	.510	.371	0	4.5	5.146	20
1956	65	.3174	0	8451.5	1.0	.479	.363	0	27	5.056	38
1957	69	.3365	0	8189.5	1.0	.500	.358	0	32.5	4.907	139
1958	50	.3555	0	7897	1.0	.510	.351	0	21	4.875	84
1959	50	.3746	0	7623	1.0	.448	.354	0	21	4.662	63
1960	56	.3936	0	7672	1.0	.396	.361	0	18	4.577	67
1961	43	.4127	0	7745.5	1.0	.458	.364	0	24.5	4.477	354
1962	47	.4318	0	7596	.5	.521	.374	0	22	4.476	272
1963	21	.4508	0	7490.5	.0	.573	.362	0	5.5	4.500	193
1964	15	.2102	0	7437	.0	.615	.362	0	0	4.581	499
1965	7	-.2901	0	7534.5	.0	.521	.361	0	0	4.648	571
1966	1	-.7904	0	7618	.0	.438	.356	0	0	4.729	664
1967	2	-1.2907	0	7867.5	.0	.417	.349	0	0	4.792	385
1968	0	-1.4773	0	8150.5	.0	.396	.358	0	0	4.995	433
1969	0	-1.3503	0	8343	.0	.348	.348	1	0	5.222	277
1970	0	-1.2232	0	8654.5	.5	.310	.349	0	0	5.565	242
1971	0	-1.2169	0	8834	1.0	.320	.353	0	12.5	6.058	200
1972	0	-1.3312	0	9032.5	1.0	.310	.355	0	43.5	6.659	147
1973	0	-1.4456	0	9634.5	1.0	.310	.359	1	66	7.236	126
1974	0	-1.5599	0	10366.5	1.0	.320	.356	0	49.5	7.723	72
1975	0	-1.8631	0	11587	1.0	.330	.355	0	25	8.300	67
1976	0	-2.1211	0	12973.5	1.0	.350	.357	0	28	8.873	116
1977	1	-2.0021	0	13881	1.0	.430	.358	1	26	9.353	52

(continued)

(Continued)

Year	# Ex.	Pub. Supt.	Gregg	# Murdr.	Rep. Pres.	Rep. Gov.	Ineq.	Elct. 67+	# Rep. Judgs.	Murd. Rate	# Civ. Rgts. Prists.
1978	0	-1.7402	0	14865.5	.5	.510	.363	0	23	9.684	102
1979	2	-1.4782	1	14952.5	.0	.560	.363	0	14.5	9.854	55
1980	0	-1.3949	1	14996	.0	.620	.365	0	0	9.624	82
1981	1	-1.3711	1	15937	.0	.530	.365	1	0	9.301	91
1982	2	-1.5020	1	17704.5	.5	.410	.369	0	0	9.071	30
1983	5	-1.3069	1	19062	1.0	.390	.38	0	21	9.422	49
1984	21	-.6539	1	18220	1.0	.370	.382	0	44.5	9.953	35
1985	18	-.4473	1	17615.5	1.0	.310	.383	1	39.5	10.321	36
1986	18	-.2656	1	17387.5	1.0	.260	.389	0	36.5	10.191	40
1987	25	-.0107	1	18183.5	1.0	.250	.392	0	62	9.510	21
1988	11	-.0234	1	20057.5	1.0	.240	.393	0	64.5	8.874	45
1989	16	-.1125	1	20800.5	1.0	.300	.395	1	44.5	8.445	29
1990	23	-.2166	1	20509	1.0	.370	.401	0	41	8.583	26
1991	14	-.0595	1	19116.5	1.0	.420	.396	0	26.5	8.688	24
1992	31	.2636	1	17994.5	1.0	.460	.397	0	33.5	8.904	16
1993	38	.5481	1	17146	1.0	.380	.404	1	55.5	8.982	15
1994	31	.6217	1	17136	.5	.300	.429	0	60	9.409	15
1995	56	.7061	1	17458	.0	.310	.426	0	31	9.909	47
1996	45	.7322	1	17375.5	.0	.320	.421	0	0	10.148	28
1997	74	.4995	1	17940.5	.0	.400	.425	1	0	10.183	28
1998	68	.6270	1	18975.5	.0	.470	.429	0	0	9.873	20

Sources: Data on public opinion comes from The Gallup Organization. Various Years: The Gallup Brain Online Database: <http://brain.gallup.com/>. Roper Center for Public Opinion Research. Various Years National Opinion Research Center's General Social Survey, Cumulative Data File: CD-ROM. Harris Poll. Various Years. Table 1. Believe in Capital Punishment. Harris Interactive www.harrisinteractive.com/harris_poll. Data on other variables comes from: [unemployment rate] Bureau of Economic Analysis. GDP and Other Major NIPA Aggregates, Survey of Current Business Online; www.bea.gov/. [%nonwhites] United States Department of Commerce, Statistical Information Office, Population Division. Various Years. Washington DC: U.S. Government Printing Office. [Gini measure of income inequality] U.S. Bureau of the Census. Historical Income Tables, Families: Table F-4. Gini Ratios for Families, by Race and Hispanic Origin of Householder: 1947-2005. [party affiliation in the House and Senate] Statistical Abstract of the United States. Various Years. Washington DC: U.S. Government Printing Office. [rape rates] Federal Bureau of Investigation Crime in the United States. Various Years. Uniform Crime Reports U.S. Government Printing Office. Data on Homicides and executions were taken from the Kaminski and Marvell (2002) web site listed at the end of this article. See www.mmavell.com/justec.html. See their data appendix for their sources.

Notes: Except for executions, variables have been appropriately lagged, moving averaged, and put in square root form, but not differenced.