An Empirical Examination of the Factors Associated with the Commutation of State Death Row Prisoners’ Sentences between 1986 and 2005

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NOTES

AN EMPIRICAL EXAMINATION OF THE FACTORS ASSOCIATED WITH THE COMMUTATION OF STATE DEATH ROW PRISONERS’ SENTENCES BETWEEN 1986 AND 2005

John Kraemer*

Commutation is usually a death row prisoner’s last hope of evading his or her capital sentence. However, unlike many other stages of the death penalty process, little research focuses on the factors that affect decisions to commute or allow a death sentence to go forward, and that which has been conducted utilizes data which is now nearly a decade old. This paper seeks to examine personal and demographic factors associated with commutation decisions and to resolve inconsistent findings in the prior research. Using the statistical method of multiple logistic regression, this paper finds statistically significant disparities in the odds of commutation by sex (women have an eleven-fold increase in odds of commutation), race (nonwhite prisoners have twice the odds of commutation), geography (southern prisoners have less than one-fifth the odds of commutation), and education (college educated offenders have one-fifth the odds of commutation). After adjusting for other factors, this research does not find evidence that, across the run of cases, criminal history or severity significantly influence commutation decisions. This research, while unable to generate conclusions about any individual case, provides evidence that executives’ commutation decisionmaking is driven more by personal characteristics—some of which are troubling—than by criminal culpability.

I. BACKGROUND

The King may pardon any felon: but it may be objected that if he pardon one felon, he may pardon all, to the damage of the commonwealth; and yet none will doubt but that is left to his wisdom . . . And the wisdom and providence of the King is not to be disputed by the subject; for by intendment they cannot be severed from his person, and to argue . . . that by his power he may do ill, is no argument for a subject.1

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A death row inmate’s petition for clemency is . . . a “unilateral hope.” The defendant . . . appeals for clemency as a matter of grace . . . . Under any analysis, the Governor’s executive discretion need not be fettered by the types of procedural protections sought by respondent.2

Separated by nearly four centuries, these decisions bear striking similarity: the power to commute belongs to the executive and is generally beyond substantive review. In the United States, constraints on the power to commute are controlled by a fractured decision of the Supreme Court in Ohio Adult Parole Authority v. Woodard.3 Four justices found essentially no judicial review of clemency decisions, deeming it instead an act of grace.4 Three others joined Justice O’Connor and would review for minimal procedural safeguards, finding a due process violation if clemency was granted or refused, for example, by flip of a coin or if the prisoner was arbitrarily barred from whatever procedures the state had created.5 Justice Stevens alone argued that the executive could not “ignore the commands of the Equal Protection Clause and use race, religion, or political affiliation as a standard for granting or denying clemency”6 and that clemency procedures, if created, must meet “the most basic elements of fair procedure.”7

Lower courts generally have followed the O’Connor position and found commutation procedures to be acceptable so long as they are not so whimsical as a decision made by the flip of a coin8 or with disregard for procedural requirements

3. Id.
4. Id. at 285 (Rehnquist, C.J.) (holding that “the executive’s clemency authority would cease to be a matter of grace committed to the executive authority if it were constrained by the sort of procedural requirements that respondent urges”). Chief Justice Rehnquist’s opinion was joined by Justice Scalia, Justice Kennedy, and Justice Thomas.
5. Id. at 289 (O’Connor, J., concurring in part and concurring in the judgment). Justice O’Connor’s opinion was joined by Justice Souter, Justice Ginsburg, and Justice Breyer.
6. Id. at 292 (Stevens, J., concurring in part and dissenting in part).
7. Id.
8. See Faulder v. Tex. Bd. of Pardons & Paroles, 178 F.3d 343, 344-45 (5th Cir. 1999) (noting that judicial intervention into clemency decisions might be warranted “where a state officials ‘flipped a coin’ to determine whether to grant clemency”); Duvall v. Keating, 162 F.3d 1058, 1061 (10th Cir. 1998) (noting that the Due Process Clause ensures a death row prisoner that “the clemency decision will not be wholly arbitrary, capricious or based upon whim, for example, flipping a coin”); Alley v. Key, 431 F. Supp. 2d 790, 802 (W.D. Tenn. 2008) (noting that the Constitution ensures “that the executive’s exercise of her clemency power is not grossly arbitrary, as in, for example, ‘flipping a coin’ to determine the fate of the applicant”); Allen v. Hickman, 407 F. Supp. 2d 1098, 1104 (N.D. Cal. 2005) (noting that a clemency decision that is “wholly arbitrary or capricious” violates the Due Process Clause); Workman v. Summers, 136 F. Supp. 2d 896, 899 (M.D. Tenn. 2001) (holding that death row inmate could not establish violation of due process where the clemency decision was not “arbitrary, capricious, or based upon whim”); Bacon v. Lee, 549 S.E.2d 840, 850 (N.C. 2001) (holding that North Carolina state clemency procedures do not violate Woodard because the clemency decision is “not reached by means of a procedure such as a coin toss”).
guaranteed by state law.9 However, one court found that there is no liberty interest in a commutation, so changes to the clemency procedures cannot trigger a due process claim.10 Several courts have found no right to present any particular information during clemency hearings.11 Additionally, several courts have explicitly found that the substantive grounds for commutation are left to the sole discretion of the executive.12 At least one court has suggested equal protection claims cannot be made in clemency proceedings,13 which is troubling in light of this paper’s finding that race and sex appear to be implicated in executive decision making about commutation.

Though commutation is largely unregulated by the courts—or perhaps, because of this—there is a compelling need to consider the factors that influence commutation decisions. The Supreme Court has spoken of executive clemency as “the ‘fail safe’ in our criminal justice system.”14 At the same time, critics have denounced commutation as too arbitrarily and infrequently used.15 This Note quantitatively examines those factors associated with contemporary commutations of state prisoners, updating the empirical literature on this subject by nearly a decade. The results show meaningful disparities in commutation on the basis of sex, race, and geographic region. Controlling for other factors, women are much more likely to be commuted than men,16 as are people of color when compared to whites.17

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9. See Noel v. Norris, 336 F.3d 648, 649 (8th Cir. 2003) (holding that denial of clemency was not unconstitutional because prisoner was not “prohibited . . . from using the procedure that [the state] had established”); Roll v. Carnahan, 225 F.3d 1016, 1018 (8th Cir. 2000) (holding that prisoner’s lawsuit failed to state a claim because he did not contend that the state deliberately interfered with his procedural rights); see also Faulder, 178 F.3d at 344-45; Duvall, 162 F.3d at 1061; Alley, 431 F. Supp. 2d at 802; Allen, 407 F. Supp. 2d at 1103-04; Workman, 136 F. Supp. 2d at 899; Lewis v. State Dep’t of Corr., 139 P.3d 1266, 1269-70 (Alaska 2006); Bacon, 549 S.E.2d at 850.

10. See Snodgrass v. Robinson, 512 F.3d 999, 1003 (8th Cir. 2008) (holding that because Iowa prisoners have no liberty interest in a commutation, there can be no due process violation related to changes in the commutation procedure).

11. See Alley, 431 F. Supp. 2d at 802 (holding that a “potential clemency applicant is [not] constitutionally entitled to what amounts to discovery for the preparation of a clemency application”); Nabelek v. Bradford, 228 S.W.3d 715, 718 (Tex. Ct. App. 2006) (holding that because clemency “need not assure error-free determinations, there is no due process right to present any particular information when seeking clemency”).

12. See Snodgrass, 512 F.3d at 1000 (holding that commutation is subject to the “unfettered discretion vested in the governor”); Roll, 225 F.3d at 1018 (holding that based on Missouri law, “the decision to grant or deny clemency is left to the discretion of the governor”); Workman, 136 F. Supp. 2d at 898 (citing Roll, 225 F.3d at 1018); Bacon, 549 S.E.2d at 850 n.5 (observing that “the decision to grant or deny clemency in any particular case is entirely dependent, at least in North Carolina, on the individual discretion of the executive”).

13. Bacon, 549 S.E.2d at 856 (holding that prisoner’s equal protection claim fails because prisoner could not establish that he “has been, or will be, treated differently for purposes of pursuing clemency than other similarly situated death row inmates”).


15. For a wide-ranging critique of the Court’s reliance on commutation in Herrera, see generally Victoria J. Palacios, Faith in Fantasy: The Supreme Court’s Reliance on Commutation to Ensure Justice in Death Penalty Cases, 49 VAND. L. REV. 311 (1996).

16. See infra Part IV.A.

17. See infra Part IV.B.
Furthermore, prisoners sentenced in a southern state are far less likely to receive a commutation than their counterparts from other regions; the same is true for prisoners with college educations. Perhaps most disturbing, this Note finds no evidence that, across the run of cases, past criminal history or severity of the capital offense are associated with the likelihood of commutation.

This paper’s primary strength over prior research is that it uses a data set nearly a decade newer than any examined previously. This is particularly important because this paper finds evidence of significant changes in commutation behavior since the last papers’ data were collected. Additionally, it includes more nuanced measures for severity of the underlying offense and the offender’s criminal history. However, unlike some of the other research, it does not include an investigation of government structures relating to decision-making, often referred to in the literature as “structural factors.” This exclusion should not, though, impact the associations discerned in this study between the measured variables and commutation.

Part II of this Note examines prior empirical research on commutation decisions, looking first at demographic factors, and then at crime-specific and political or structural factors. Part III lays out the methodologies used in this Note to identify key factors associated with commutation decisions. The findings of this original research are described in Part IV, along with suggested legal and policy implications.

II. PRIOR EMPIRICAL RESEARCH ON FACTORS ASSOCIATED WITH COMMUTATION

A. Demographic Factors

Only a handful of studies have used empirical evidence to examine the factors associated with commutation of death sentences in the modern judicial system, and many of the reported results are inconsistent. The four primary studies were conducted by William Alex Pridemore, using data from 1974 to 1995; Laura

18. See infra Part IV.D.
19. See infra Part IV.D.
20. See infra Part IV.C.
21. These factors often include such things as the timing of commutation decisions in the election cycle or the political party of the decision-maker. E.g., Michael Heise, Mercy by the Numbers: An Empirical Analysis of Clemency and its Structure, 89 Va. L. Rev. 239, 266 (2003) (describing clemency-making authority, geography, and time period as structural factors).
22. See infra Part II.A.
23. See infra Part II.B.
24. There are a handful of studies on commutation from before the Supreme Court’s 1972 Furman decision. Because they have limited applicability to the contemporary system, they will not be discussed here. A good summary can be found in William Alex Pridemore, An Empirical Examination of Commutations and Executions in Post-Furman Capital Cases, 17 Just. Q. 159, 163-64 (2000).
25. Id. at 161 fig.1.
Argys and H. Naci Mocan, using data from 1977 to 1997;\textsuperscript{26} Jeffrey Kubik and John Moran, using data from 1972 to 2000;\textsuperscript{27} and Michael Heise, using data from 1973 to 1999.\textsuperscript{28} These studies take two broad forms: investigations of how political or structural factors impact commutation decisions\textsuperscript{29} and investigations of the effects of prisoner characteristics on commutation.\textsuperscript{30} Because the newest research is based on data now nearly a decade old, there is a need for examination of more recent data to better understand factors presently associated with decisions to commute or allow executions to go forward. This study both examines more recent data and controls for historical trends in the frequency of commutations. The results of this research are reported in Part IV.

It is well established that female capital defendants are much less likely to be sentenced to death than male capital defendants.\textsuperscript{31} There is also strong evidence that a high proportion of women sentenced to death are not executed.\textsuperscript{32} The Pridemore study estimates that the odds of execution instead of commutation are more than sixty times greater for men than women.\textsuperscript{33} The research focusing on the most contemporaneous data has generally found a statistically significant ten to twenty-fold increase in the odds of commutation for women, when compared to men (and controlling for other factors).\textsuperscript{34} However, precise estimates of the effect of sex on commutation are difficult to obtain because of the relatively small number of women whose sentences of death go to final disposition.

Fairly strong statistical evidence supports the claim that black defendants are more likely to be sentenced to death than similarly situated white defendants.\textsuperscript{35}
Additionally, there is very strong evidence that the race of the murder victim has a substantial effect on the likelihood that an offender will be sentenced to death.\(^{36}\) However, the effect of race on commutation is more uncertain. Heise and Pridemore did not find race to be a significant predictor of commutation.\(^{37}\) On the other hand, Argys and Mocan found that prisoners of black, Hispanic, or other racial or ethnic heritage are more likely to be commuted than white prisoners.\(^{38}\) They also found that state governors who are white are more likely to commute white prisoners’ sentences than minority prisoners’ sentences\(^{39}\) and that Democratic governors are more likely to commute the sentences of minority inmates than whites.\(^{40}\)

It is possible that the relatively small number of observations in all of these studies could mask a subtle race effect in those studies that did not find one. Additionally, if a race effect exists, its interpretation is difficult based on these data. One could plausibly argue that such an effect mitigates racial disparities in sentencing,\(^{41}\) but one might also argue that such an effect reflects impermissible racial considerations in carrying out the sentence. Unfortunately, no studies have investigated the effect of the victim’s race on commutation; if executives weigh the lives of black victims less heavily—and murders are disproportionately committed within race—one would see a greater chance of commutation for black defendants.\(^{42}\)

Research from data collected in the late 1990s has tended to indicate that younger offenders are more likely to have their sentences commuted, but different measures of age have been used in different studies. Pridemore used age at time of final decision to execute or commute and concluded that prisoners under the age of twenty-four were more likely to have their sentences commuted.\(^{43}\) Argys and

\(^{36}\) See Baldus et al., supra note 35, at 1659–60 (noting that in 82% of empirical death penalty studies, race of victim was found to influence the likelihood of being charged with capital murder or receiving the death penalty).

\(^{37}\) Heise, supra note 21, at 281–82; Pridemore, supra note 24, at 171.

\(^{38}\) Argys & Mocan, supra note 26, at 275–76 tbl.5. Additionally, there is some evidence that an impending gubernatorial election has a greater positive effect on the likelihood of execution of black prisoners than white prisoners. Kubik & Moran, supra note 27, at 23.

\(^{39}\) Argys & Mocan, supra note 26, at 277.

\(^{40}\) Id.

\(^{41}\) This is raised as a potential explanation by Argys and Mocan. See id. (explaining that preferential treatment of minorities that emerges during the execution or commutation stage might be a remedy for discrimination during the arrest, trial, conviction, and sentencing phases).

\(^{42}\) This is exactly the argument unsuccessfully raised to support the equal protection challenge to disparities in death sentencing on the basis of the victim’s race in McCleskey v. Kemp. See 481 U.S. 279, 293 n.11 (1987) (noting that the petitioner’s expert testified that there is a higher probability that the defendant will be sentenced to death if the victim is white).

\(^{43}\) Pridemore, supra note 24, at 171–72.
Mocan used age at sentencing. They found that the likelihood of commutation is inversely related to age until the offender is forty-four; after forty-four, the likelihood of commutation increases with age.\textsuperscript{44} Heise found that younger offenders are more likely to have their sentences commuted, but it is unclear how he measured age.\textsuperscript{45} It is possible that the age effect is due to the increasing possibility of a prior criminal record as a defendant ages.\textsuperscript{46} Although all of these studies adjust for whether the offender had one or more previous felony convictions, the crudeness of this measure may allow some confounding to persist.

The geographic region in which the prisoner has been sentenced to death consistently has been found to be a predictor of final outcome. Both Pridemore and Heise found that the odds of commutation are reduced two to four-fold in southern states, compared to non-southern states.\textsuperscript{47} Both found this effect to persist even when controlling for the political party of the state’s governor.\textsuperscript{48}

There are mixed findings regarding the effect of the defendant’s educational attainment on the likelihood of commutation. Pridemore found no significant difference in the likelihood of commutation between prisoners who had achieved at least a high school degree and those who had not.\textsuperscript{49} Heise, on the other hand, found that the likelihood of commutation decreases with greater education.\textsuperscript{50} Argys and Mocan found that, compared with prisoners who had obtained a high school degree (but had no further education), defendants who did not advance past grade school are more likely to receive commutation, and defendants who attended college are less likely.\textsuperscript{51} It is unclear whether any association between education and commutation is causal or whether education is a proxy for other socioeconomic variables.\textsuperscript{52}

Several studies investigate whether the offender’s marital status impacts the likelihood that she will be commuted. Neither Heise\textsuperscript{53} nor Argys and Mocan\textsuperscript{54} found marital status to be a significant predictor of commutation. Pridemore did not find marital status to be a statistically significant predictor of execution when examining all cases after the death penalty was reinstated in the late 1970s; for those cases between 1978 and 1995, however, he found that married offenders have reduced odds of execution. Pridemore was only marginally certain that this

\textsuperscript{44} Argys & Mocan, supra note 26, at 275.
\textsuperscript{45} Heise, supra note 21, at 284-85.
\textsuperscript{46} Id. at 274 tbl.3, 285.
\textsuperscript{47} Id. at 274 tbl.3, 301-02; Pridemore, supra note 24, at 172-73.
\textsuperscript{48} Heise, supra note 21, at 274 tbl.3, 295-96; Pridemore, supra note 24, at 172-73.
\textsuperscript{49} Pridemore, supra note 24, at 172.
\textsuperscript{50} Heise, supra note 21, at 285.
\textsuperscript{51} Argys & Mocan, supra note 26, at 275.
\textsuperscript{52} See Heise, supra note 21, at 285 (“After all, to the extent that a defendant’s education level is one proxy – however imperfect – for intelligence or, more broadly socioeconomic status, one would expect that the clemency process generally would favor more intelligent and more advantaged defendants.”).
\textsuperscript{53} Id. at 288-89 (citing Pridemore, supra note 24, at 172 tbl.3).
\textsuperscript{54} Argys & Mocan, supra note 26, at 276 tbl.5.
was not due to statistical chance.\textsuperscript{55}

\textbf{B. Crime-Specific and Structural Factors}

As one commonly asserted justification for capital punishment is preventing recidivistic violence—including violence in prison—one would expect a history of prior felonies to be associated with a decreased likelihood of commutation. However, neither Pridemore\textsuperscript{56} nor Argys and Mocan\textsuperscript{57} found a history of prior felonies to be associated with the likelihood of commutation. Heise found a statistically significant halving of the odds of commutation for prisoners with prior felonies when he examined all prisoners under sentence of death between 1973 and 1999;\textsuperscript{58} however, when he looked at only those prisoners on death row between 1985 and 1999, this association did not persist.\textsuperscript{59} The quantitative studies of commutations have not included other measures of criminal culpability, which is unfortunate because it seems likely that executives take additional crime-specific factors into consideration. Furthermore, there is some qualitative evidence that factors relevant to the underlying offense and the offender—such as equity in sentencing between co-defendants—play a role in at least some decisions to commute.\textsuperscript{60} In addition to looking at the presence of prior felonies, this Note will examine the effect of multiple homicides and being under sentence at the time of the capital offense on commutation.

A variety of structural factors that are beyond the scope of this study have also been found to be associated with the likelihood of commutation but are sufficiently informative to be noted. One such structural factor is the party affiliation of the state governor. Pridemore found that, after adjusting for other factors, Democrats were significantly less likely to commute than Republicans between 1978 and 1995.\textsuperscript{61} Argys and Mocan concluded in the opposite direction that Democratic governors are significantly more likely to commute.\textsuperscript{62} Heise found no significant association between the governor’s party affiliation and the odds of commutation.\textsuperscript{63} One possible explanation for the difference in effect found by Argys and

\begin{footnotesize}
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\item \textsuperscript{55} Pridemore, supra note 24, at 172 (reporting a p-value of 0.061, which is generally considered to be of only marginal significance).
\item \textsuperscript{56} Id. at 171-72 tbl.3.
\item \textsuperscript{57} Supra note 26, at 275-76 tbl.5.
\item \textsuperscript{58} Heise, supra note 21, at 274 tbl.3, 287.
\item \textsuperscript{59} Id. at 274 tbl.3.
\item \textsuperscript{60} See Michael L. Radalet & Barbara A. Zsembik, \textit{Executive Clemency in Post-Furman Capital Cases}, 27 U. Rich. L. Rev. 289, 302-03 (1992-93) (listing examples where clemencies were given in response to the status of the offender or issues of equity).
\item \textsuperscript{61} Pridemore, supra note 24, at 177.
\item \textsuperscript{62} Argys & Mocan, supra note 26, at 275-77.
\item \textsuperscript{63} Heise, supra note 21, at 274 tbl.3.
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Mocan is that they did not control for geographic region. So Argys and Mocan’s finding of relatively fewer commutations by Republican governors may in fact be due to the fairly well-established “southern effect.”

Heise investigated the effect of variation in the type of entity that makes commutation decisions on the likelihood of commutation. He determined that commutation is more likely when the decision is made by an appointed administrative board than when made by a governor alone or a combination of the two. This result is somewhat expected, as commutation decisions are often unpopular, so elected officials may be concerned about the political ramifications of granting clemency. This concern may also be reflected in Argys and Mocan’s finding that lame-duck governors are more than eighty percentage points more likely to commute than other governors. Interestingly, though, Heise found no significant increase in commutation at the end of governors’ time in office, so the effect of the lame-duck period on commutation is unclear.

In a similar vein, Pridemore concluded that the odds of execution instead of commutation were increased in gubernatorial election years. However, Heise found no association between the odds of commutation and whether the decision to execute or commute was made in an election year. Similarly, Argys and Mocan found no significant change in the likelihood of commutation in the six months before a gubernatorial election. Kubik and Moran found that executions increase in election years but that this increase is due to factors other than decreases in commutations. Thus, the influence of political pressures on decisions to commute is unclear. At least one court has considered this influence and decided that electoral pressures on a governor facing re-election not to commute do not give rise to a legal cause of action by the prisoner.

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64. Both Heise and Pridemore found a substantial decrease in likelihood of commutation in southern states, and this association persisted after controlling for the party of the state’s governor. Heise, supra note 21, at 274 tbl.3, 301-02; Pridemore, supra note 24, at 172-73.
66. Heise, supra note 21, at 301.
67. Id. at 297-98.
68. See id. at 298-99 (noting that administrative boards are more likely to grant favorable clemency decisions than governors because administrative boards are “more insulated from direct political and popular pressures”).
69. Argys & Mocan, supra note 26, at 278.
70. Heise, supra note 21, at 293.
71. Pridemore, supra note 24, at 172 tbl.3.
72. Heise, supra note 21, at 292.
73. Argys & Mocan, supra note 26, at 278.
75. See Roll v. Carnahan, 225 F.3d 1016, 1017-18 (8th Cir. 2000). Roll alleged that the Missouri governor was a candidate for the United States Senate and “politically restrained from being full and fair in considering [Roll’s] clemency petition.” Id. at 1017. The court held that Roll failed to state a claim because the decision to grant or deny clemency is left to the discretion of the governor. Id. at 1018.
All of the published studies on factors associated with commutation decisions used data ending in 1999 or earlier. There is some substantial evidence that commutation behavior has changed in the years since the Supreme Court reinstated the death penalty. Commutation is much less common in recent years than it was before the Supreme Court’s *Furman v. Georgia* decision temporarily halted all executions. Additionally, Pridemore notes that sentences were commuted much more often in the years immediately following the death penalty’s reinstatement, and Heise found that the odds of commutation between 1985 and 1999 are much lower than between 1973 and 1984.

III. METHODS

Statistical analysis of the factors associated with commutation is performed by first defining the population to be studied. The population is then broken down by relevant characteristics, which comprise the variables studied. These characteristics are then analyzed to determine their effect on commutation and whether the observed effects are likely due to statistical chance. The analysis finds that a number of discrepancies—including those due to race, sex, and geographic location—cannot be explained by chance.

A. Study Population

The data analyzed in this study were compiled from official prison records and made publicly available by the Bureau of Justice Statistics at the United States Department of Justice (DOJ). The original data set can be downloaded from the website of the Inter-University Consortium for Political and Social Research (ICPSR). The data set initially included all prisoners under sentence of death.

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76. 408 U.S. 238 (1972).
77. See Palacios, *supra* note 15, at 347-48 (observing that there has been a dramatic decline in death penalty commutations since *Furman*).
80. The primary audience of this paper is attorneys, many of whom have not had statistical training. Explanation of basic statistical concepts can be found in Part III.B, *infra*. Because more detailed descriptions of the methodologies used in this paper may be of interest to social scientists—a secondary audience for this Note—more detailed explanations are often provided in the footnotes.
81. *See infra* Part III.A.
82. *See infra* Part III.C for variable creation.
83. The methods used to analyze the data are described in Part III.D.
84. Findings are reported *infra* Part IV.
between 1973 and 2005. Because this study seeks to examine the factors associated with decisions to commute death sentences, the only relevant prisoners are those for whom there was a discrete decision either to commute or allow to be executed. Thus, prisoners removed from death row by any other mechanism were excluded from the analysis.

In order to capture contemporaneous decision-making about commutation, this paper only examines decisions to execute or commute made in the twenty-year period ranging from 1986 to 2005 (the most recent year for which data is available). The decision to examine twenty years of data was a pragmatic one, balancing the need to have enough data for effective statistical analysis against the distortions caused by historical trends in the rate of commutations. The years chosen was essentially the period that acceptably met both criteria. Thus, all cases in which a prisoner’s sentence was carried out or commuted before 1986 were excluded from the analysis. Additionally, all commutations from Illinois in 2003 were excluded as being due to the governor’s concerns about the appropriateness of the death penalty and not factors specific to each case. A LexisNexis search did not uncover any other blanket commutations within the time period examined in this study. There may be other instances in which particular sentences were commuted for non-case specific reasons, but ICPSR data use restrictions prohibit any attempt to individually identify cases, so no effort could be made to exclude such cases. Prisoners in the federal prison system and from the District of Columbia were also excluded to isolate decisions made by state executives, which likely have different commutation considerations than the President. After excluding data removed for the reasons described above, the data set included 1053 prisoners, of whom 951 were executed and 102 were granted clemency. Basic characteristics of the prisoners included in the data set can be found in Table 1.

87. Observations in the data set have been stripped of identifying information, and it is publicly available. ICPSR and DOJ rules prohibit any attempt to identify individual cases, and none was made. Thus, human subjects committee approval was not required or sought for this analysis. Information is supplied for basic socio-demographic characteristics and criminal history data. See id.

88. There is some evidence of such a historical trend. Pridemore notes that the proportion of prisoners commuted rather than executed was 4.9 times higher between 1978 and 1983 than between 1984 and 1995. See also Palacios, supra note 15, at 347-49.

89. See Maurice Possley & Steve Mills, Clemency for all: Ryan commutes 164 death sentences to life in prison without parole, Chi. Trib., Jan. 12, 2003, at C1 (explaining that Governor Ryan placed a moratorium on the death penalty after a three-year examination of the state’s death penalty system).


91. infra pp. 1400.
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<td>88.5</td>
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<td>55</td>
<td>8.2</td>
<td>619</td>
<td>91.8</td>
<td>674</td>
</tr>
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<td>40</td>
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<tr>
<td><strong>Multiple Murders</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
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<td>10.0</td>
<td>893</td>
<td>90.0</td>
<td>992</td>
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<td>3</td>
<td>4.9</td>
<td>58</td>
<td>95.1</td>
<td>61</td>
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<td><strong>Status at First Capital Offense</strong></td>
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<td></td>
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<td></td>
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<td>Not under sentence and no charges pending</td>
<td>71</td>
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<td>560</td>
<td>88.7</td>
<td>631</td>
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<td>Under sentence, parole / parole, escaped, or charges pending</td>
<td>31</td>
<td>7.3</td>
<td>391</td>
<td>92.7</td>
<td>422</td>
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<td><strong>Education</strong></td>
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<tr>
<td>Less than high school degree</td>
<td>61</td>
<td>11.4</td>
<td>474</td>
<td>88.6</td>
<td>555</td>
</tr>
<tr>
<td>High school degree</td>
<td>33</td>
<td>9.2</td>
<td>325</td>
<td>90.8</td>
<td>358</td>
</tr>
<tr>
<td>At least some college</td>
<td>3</td>
<td>3.2</td>
<td>90</td>
<td>96.8</td>
<td>93</td>
</tr>
<tr>
<td>Unknown</td>
<td>5</td>
<td>7.5</td>
<td>62</td>
<td>92.5</td>
<td>67</td>
</tr>
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<td><strong>Age at Arrest</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Under 25</td>
<td>49</td>
<td>11.5</td>
<td>377</td>
<td>88.5</td>
<td>426</td>
</tr>
<tr>
<td>25 to 34</td>
<td>33</td>
<td>8.0</td>
<td>382</td>
<td>92.0</td>
<td>415</td>
</tr>
<tr>
<td>35 to 44</td>
<td>14</td>
<td>8.9</td>
<td>143</td>
<td>91.1</td>
<td>157</td>
</tr>
<tr>
<td>45+</td>
<td>6</td>
<td>10.9</td>
<td>49</td>
<td>89.1</td>
<td>55</td>
</tr>
<tr>
<td><strong>Marital Status at First Capital Imprisonment</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Married, widowed, never married, unknown</td>
<td>86</td>
<td>10.4</td>
<td>737</td>
<td>89.6</td>
<td>823</td>
</tr>
<tr>
<td>Divorced / Separated</td>
<td>16</td>
<td>7.4</td>
<td>214</td>
<td>93.0</td>
<td>230</td>
</tr>
<tr>
<td><strong>Location of Incarceration</strong></td>
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<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Not southern state</td>
<td>52</td>
<td>20.6</td>
<td>200</td>
<td>79.4</td>
<td>252</td>
</tr>
<tr>
<td>Southern state</td>
<td>50</td>
<td>6.2</td>
<td>751</td>
<td>93.8</td>
<td>801</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td>102</td>
<td>9.7</td>
<td>951</td>
<td>90.3</td>
<td>1053</td>
</tr>
</tbody>
</table>
B. Explanation of Technical Terms and Methods Used in this Paper

This section of the Note will provide a basic understanding of the terms and methods used in this paper for those without training in statistics. Additionally, explanatory illustrations are included in the notes, as appropriate.

The key statistical concept in this paper is the odds ratio (which will often be abbreviated in this paper as “OR”). Odds ratios express the strength of an association between two variables, similar to the more familiar correlation score. In this study, for example, odds ratios express whether factors that might play a role in commutation decisions—such as nonwhite race or female sex—are related to the outcome of interest (commutation). They describe the extent to which the odds of commutation change for varying values of a variable. Consider race. In this study, a person is classified as either white or nonwhite, and this study finds that the odds ratio of commutation for a nonwhite offender is equal to about 2.0. This means that, when compared to a white offender, a nonwhite offender has twice the odds of commutation. If the odds ratio of commutation had instead been

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92. For those readers who desire a more general explanation of statistical and scientific methods as applied to the law, see volume nine of the Kansas Journal of Law and Public Policy, which contains several articles transcribed from a symposium at the University of Kansas on this topic. 9 KAN. J.L. & PUB. POL’Y 1 (1999-2000).
0.5, it would have indicated that a nonwhite offender had only half the odds of commutation compared to white offenders. Lastly, if the odds ratio had been equal to 1.0, it would indicate that nonwhites had no different odds of commutation than white offenders, which would suggest that there is no association between race and commutation. Thus the odds ratio for a characteristic can only be interpreted in relation to another characteristic; in the previous example, the odds ratio tells us how much greater or smaller the odds of commutation are for nonwhite offenders in relation to those for white offenders. The comparison group is often called a reference group, and is marked in this Note’s data tables as “Ref.” The greater an odds ratio is from 1.0 (either above or below it) the greater the association between the characteristic and commutation.93

The second important concept for understanding the results reported in the paper is that of statistical significance. This paper reports p-values when an odds ratio is presented. P-values are a measure of the likelihood that the odds ratio observed in this research is simply caused by chance. For example, this study includes 593 white offenders and 460 nonwhite offenders. This sample is used to try to understand the relationship between race and commutation for the thousands of people who could potentially be subject to a commutation decision. It is possible that the people included in the study vary from each other with respect to the odds of commutation not because of anything about race but simply by chance. The reported p-value tells what the likelihood is that chance accounted for the odds ratio that is observed instead of anything truly different about the populations to

93. Odds ratios can be difficult to grasp conceptually because they describe increases or decreases in the odds of an occurrence (commutation, in this paper), and outside of statistics classes or horse tracks, odds are rarely used. People tend to think of chance in terms of probabilities. If this study included 100 prisoners, 10 of who were commuted, one would say that the chance of commutation was 10%—or a probability of 0.1. Odds are a related but distinct measure of chance, which are more difficult to understand intuitively. The odds of an event’s occurrence are equal to the probability of that event, divided by one minus the probability. Thus in the previous example, in which 10 of 100 prisoners are commuted, the odds of commutation are equal to the probability of commutation (0.1) divided by one minus the probability of commutation (0.9), which equals 0.11. As long as the event of interest does not occur terribly frequently, the odds and probability of its occurrence will have similar values. However, if an event is more likely, the odds and probability will diverge substantially. For example, suppose that 40 of the hypothetical 100 prisoners were commuted, for a probability of commutation equal to 0.4. In this case, the odds would be equal to 0.67. Odds ratios represent a multiplier of the odds of the occurrence. If white offenders were (hypothetically) to have odds of commutation equal to 0.1, and the odds ratio comparing non-white offenders to white offenders is 2.0, nonwhite offenders have odds of commutation equal to 0.2. Though more counter-intuitive than probabilities, odds have mathematical features that are superior for the sort of statistical analysis undertaken in this paper.

This all gets particularly confusing because a doubling of odds does not correspond to a doubling of probabilities, meaning that the effect on the probability of commutation for an odds ratio of 2.0 will vary depending on what the underlying odds are. Consider the following example. An odds ratio of 2.0 could mean that the odds in the reference group are 0.1 and 0.2 in the comparison group. These odds would correspond to probabilities of 0.09 and 0.17, the latter is nearly twice the former. Alternatively, it could mean that the odds in the reference group are 0.7 and 1.4 in the comparison group. These odds would correspond to probabilities of 0.41 and 0.58; the latter is not nearly twice the former. Because of this feature of odds ratios, when an odd ratio is reported in this paper, a footnote will translate that odds ratio’s effect on various levels of probability.
which the research is generalized. In general, if the p-value is less than 0.05 (5%), one can be reasonably assured that the association is unlikely due to chance. If it is between 0.05 and 0.10, an inference that the association is not due to chance may be appropriate, but such conclusions are usually made with caution. If the p-value is greater than 0.10 (10%), social scientists usually are not persuaded that the association is not because of random chance.  

To better understand this concept, consider an example in which fifty children—twenty-five boys and twenty-five girls—flip a coin one time each. This process is clearly random; there is no reason why boys in general would be expected to come up with heads more often than girls. However, suppose the boys come up with heads fifteen times, and the girls only ten times. For the boys in this example, the odds of flipping heads is 1.5; for the girls the odds are only 0.67. Thus, this study has observed an odds ratio of flipping heads of 2.25, comparing boys to girls, which is a fairly strong association between sex and the outcome of the coin flip. However, if one statistically calculated the likelihood that a difference of this great or greater would be observed when there are only twenty-five flips per group, one would find that the likelihood is about 16%, which is sufficiently likely that one would not be convinced that the difference in heads and tails was related to the sex of the coin flip.  

Everything else being equal, the p-value will fall as the sample size increases; if the exact same odds ratio had been observed with 100 boys and 100 girls flipping coins, the likelihood that it was attributable to chance is only 0.35%.

The third important concept is that of confounding. A confounding variable is one that is associated with the explanatory variable of interest and independently causes the outcome being studied. For example, rainy days increase the number of car accidents. They also increase the amount of umbrella use. If one were (for whatever odd reason) studying the relationship between umbrella use and car accidents, one might erroneously infer that increased umbrella use causes increased car accidents, if one does not account for rain as a confounding factor. Failure to address confounding makes it impossible to make a causal inference about the effect of variables on the outcome of interest.

There are statistical methods through which one can remove the effect of potential confounding variables. This Note employs a couple of methods to do so. The primary is a method called multiple logistic regression, by which the independent contribution of multiple explanatory variables to the odds of the

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94. It should be noted that these cut-offs are somewhat arbitrary, and rigid adherence to a 5% or 10% rule does not make much sense.
95. Thus, if this result were being reported in an article, one would write something along the following lines: “Boys were observed to have greater odds of flipping heads than girls, but this association is not statistically significant (OR=2.25, p=0.16).”
outcome (commutation) can be determined.\textsuperscript{97} In this Note, Table 2\textsuperscript{98} reports the effect of each variable without removing the effect of potential confounding. Table 3\textsuperscript{99} gives the isolated effect of each variable with confounding removed. Thus, there is no category in the data tables that states whether confounding occurs; rather, the effect of measured confounding variables is removed in Table 3. Inferences are based on the adjusted results, but the unadjusted results are included because they may be of interest to some readers.

\textit{C. Variables}

Variables were created from data in the set downloaded from ICPSR to examine predictors of commutation. The outcome of interest in the study reported in this paper is commutation and a variable that can take two values—either commutation or execution—was created to represent it. In addition, the ICPSR data set contains a variety of demographic, socioeconomic, and criminal history variables. The demographic variables include the prisoners’ race/ethnicity, sex, state in which convicted, and years of birth, arrest, conviction, and removal from death row (regardless of whether by execution or commutation). The original race and ethnicity variables categories were combined to yield two broad categories, with prisoners classified as either white (non-Hispanic) or nonwhite.\textsuperscript{100} For increased accuracy of analysis, states were not individually examined; instead, they were categorized in one of two regions: southern or non-southern geography.\textsuperscript{101} An age at arrest variable was constructed by subtracting the year and month of birth from the year and month of arrest.\textsuperscript{102} If no date of arrest was available (198 observations), the median time from arrest to conviction (one year) was subtracted from the date of conviction and assumed to be the date of arrest. Age at arrest was then categorized\textsuperscript{103} into four classes: under twenty-five years of age, twenty-five to

\textsuperscript{97} Such models are more generally referred to as multivariate analysis, as contrasted with univariate analysis, in which only a single explanatory variable is employed.

\textsuperscript{98} \textit{Infra} p. 1407.

\textsuperscript{99} \textit{Infra} p. 1408.

\textsuperscript{100} The nonwhite category includes people of Black, Hispanic, Asian, Pacific Islander, Native Hawaiian, American Indian, Alaskan Native, and other racial identifications.

\textsuperscript{101} The states classified as southern are: Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, Oklahoma, South Carolina, Texas, and Virginia.

\textsuperscript{102} Exact dates of birth, arrest, and conviction are not available to prevent identification of individual prisoners, so all dates were assumed to be the middle of the month. If a year but not a month was available, the date was assumed to be exactly halfway through the year. Thus, it is possible that minor misclassification of age may have occurred at this stage of the construction of this variable, but any misclassification should be non-differential and should not bias the results.

\textsuperscript{103} Other studies keep age as a continuous variable. However, that was rejected in this study because the relationship between the log odds of commutation and a one-unit increase in age appears to vary at different ages, and the nature of the variation did not appear amenable to the use of linear splines, which facilitate modeling a linear relationship which changes at specific cut-points.
thirty-four, thirty-five to forty-four, and forty-five and over. Three criminal history/offense characteristics are available: whether the capital offense consisted of multiple murders, the prisoner’s status at the time of offense, and his or her record of prior felonies. The multiple murder variable has two levels: either the prisoner committed a single murder or more than one murder. History of prior felonies has three categories: one or more prior felonies, no prior felonies, or unknown. Last, a variable was created which categorizes the status of the prisoner at the time of the capital offense. The prisoner was determined to have been either not under sentence with no pending charges or under sentence/facing charges. Greater information about criminal history and the specifics of the capital offense would be desirable but is unavailable without re-identifying prisoners in the study.

Two additional socioeconomic characteristics are available: educational attainment and marital status at the time of first capital imprisonment. Prisoners were categorized into one of two marital status groups: divorced or separated versus married, widowed, never married, or unknown status. The variable was primarily included to determine whether decision makers may treat divorced female prisoners differently than those who are not divorced. There are four categories of educational attainment: less than a high school degree, high school degree, at least some college, and unknown.

Lastly, a variable for the year in which the prisoner was either commuted or executed was included. The inclusion of this variable allows for historical trends in commutation decision-making to be examined. This variable is continuous. However, because a visual inspection of the relative rates of commutations and executions between 1986 and 2005 suggests a dramatic shift after 1999, a spline term was added at 1999 to allow for different linear relationships between each

104. Analysis of death row prisoners who committed murder at an older age would be interesting, but too few death sentences are conferred on elderly defendants to make analysis possible. Between 1986 and 2005, fewer than ten death row inmates at the execution versus commutation stage were fifty-five years old or older at the time of their arrest.

105. There were too few cases of triple murder (or greater) to make separating double murders from triple murders useful.

106. There were forty observations for which a history of prior felonies could not be ascertained; the unknowns were retained out of concern that classifying them into either of the other categories might differentially bias the results. A variable with more detailed categorization of prior felonies was initially created, but not ultimately used so as to preserve statistical power. When multivariate logistic regression was conducted, models with the more and less detailed variables were compared using Akaike’s information criterion and the model with the less detailed categories was judged to be superior. Akaike’s information criterion is a method of evaluating a model that balances the simplicity of the model against its predictive value.

107. A binomial variable was chosen again to maximize statistical power, as some of the more detailed categories would have contained few observations, and there was no logical way to collapse several of the originally available values.

108. It is unclear from the ICPSR data codebook whether “widowed” includes prisoners who are widowed by their murder of a spouse.

109. See fig.1 supra p. 1401.
increasing year and the odds of commutation in each time period.\textsuperscript{110}

\textbf{D. Statistical Analyses}

Two methods of analysis were conducted—univariate and multivariate. Univariate analysis looks one at a time at the contribution of variables and does not remove the effect of confounding. On the other hand, the multivariate analysis, controls for measured confounding variables. All results are reported as odds ratios. An odds ratio above 1.0 indicates greater odds of commutation for the value taken by a variable than the reference value; an odds ratio below 1.0 indicates decreased odds of commutation.\textsuperscript{111} P-values, describing the likelihood that the reported observation was due to random chance, are reported for all odds ratios.\textsuperscript{112} Multivariate analysis was conducted using multiple logistic regression.\textsuperscript{113} Statistical analyses were performed on statistical software.\textsuperscript{114}

\textbf{IV. RESULTS AND DISCUSSION}

\textbf{A. Sex}

Consistent with all other studies on this topic, the prisoner’s sex was found to be significantly associated with the likelihood of commutation. Adjusting for other characteristics, women on death row between 1986 and 2005 had nearly eleven times the odds of clemency (odds ratio, OR = 10.8) than men,\textsuperscript{115} and this association is highly significant (probability of a chance association, \(p < 0.001\)). This effect of sex is slightly weaker than that reported in other studies, but nonetheless, still the strongest association observed in this study between any factor and commutation. Statistical analyses can be conducted to determine whether the

\textsuperscript{110} Spline terms allow linear regression lines to “bend.” When a spline is added, the odds ratio of commutation per one-year increase can be different before the spline and after.

\textsuperscript{111} Odds are a measure of likelihood related to probability but distinct from it. If the probability of an event is labeled \(p\), the odds of that event occurring are equal to \(p / (1-p)\). Thus, a 25% probability is equal to odds of 1/3, and a 50% probability is equal to odds of one. Odds ratios can thus take any value from positive to negative infinity, and an odds ratio of two does not necessarily represent a doubling of the probability of commutation.

\textsuperscript{112} All \(p\)-values reported are two-sided, and all confidence intervals are at the 95\% level. For the univariate analysis, \(p\)-values were calculated using Fisher’s exact test and confidence intervals are exact, with the exception of the continuous year of final disposition variable, which was investigated using univariate logistic regression. Exact \(p\)-values and confidence intervals are most appropriate when many 2x2 table cells are small, as is the case for some of the calculations using this data.

\textsuperscript{113} Dummy variables were created for each value of categorical variables and used in the multiple logistic regression. Dummy variables are created by making a separate variable for each category of a variable (thus, there were four dummy age variables, one for each age group) and allow investigation of the independent contribution of each category.

\textsuperscript{114} The specific software used was Stata 9.2. The program is produced by Statacorp of College Station, Texas.

\textsuperscript{115} As noted above, odds ratios can be difficult to interpret, and an example might be helpful. At the reported odds ratio of 10.8, if a male prisoner has a 10\% chance of being commuted, a female prisoner with identical characteristics would have approximately a 55\% chance of commutation. If the male prisoner has a 30\% chance, the identical female prisoner would have an 82\% chance.
Table 2. Unadjusted odds ratios of commutation by various death row prisoner characteristics, 1986-2005

<table>
<thead>
<tr>
<th>Characteristic</th>
<th>Odds Ratio</th>
<th>95% Confidence Interval</th>
<th>p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Race / Ethnicity</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, not Hispanic</td>
<td>Ref.</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Nonwhite</td>
<td>2.06</td>
<td>(1.33, 3.20)</td>
<td>0.001</td>
</tr>
<tr>
<td>Sex</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Male</td>
<td>Ref.</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Female</td>
<td>6.93</td>
<td>(2.18, 20.6)</td>
<td>0.001</td>
</tr>
<tr>
<td>Prior Felonies</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>Ref.</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Yes</td>
<td>0.683</td>
<td>(0.434, 1.08)</td>
<td>0.086</td>
</tr>
<tr>
<td>Unknown</td>
<td>1.92</td>
<td>(0.712, 4.65)</td>
<td>0.130</td>
</tr>
<tr>
<td>Multiple Murders</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>Ref.</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Yes</td>
<td>0.467</td>
<td>(0.092, 1.48)</td>
<td>0.264</td>
</tr>
<tr>
<td>Status at First Capital Offense</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Not under sentence and no charges pending</td>
<td>Ref.</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Under sentence, probation / parole, escaped, or charges pending</td>
<td>0.625</td>
<td>(0.389, 0.989)</td>
<td>0.043</td>
</tr>
<tr>
<td>Education</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Less than high school degree</td>
<td>Ref.</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>High school degree</td>
<td>0.789</td>
<td>(0.488, 1.26)</td>
<td>0.318</td>
</tr>
<tr>
<td>At least some college</td>
<td>0.259</td>
<td>(0.051, 0.823)</td>
<td>0.015</td>
</tr>
<tr>
<td>Unknown</td>
<td>0.627</td>
<td>(0.189, 1.63)</td>
<td>0.411</td>
</tr>
<tr>
<td>Age at Arrest</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Under 25</td>
<td>Ref.</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>25 to 34</td>
<td>0.665</td>
<td>(0.405, 1.08)</td>
<td>0.103</td>
</tr>
<tr>
<td>35 to 44</td>
<td>0.753</td>
<td>(0.372, 1.44)</td>
<td>0.453</td>
</tr>
<tr>
<td>45+</td>
<td>0.942</td>
<td>(0.384, 2.31)</td>
<td>1.00</td>
</tr>
<tr>
<td>Marital Status at First Capital Imprisonment</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Married, widowed, never married, unknown</td>
<td>Ref.</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Divorced / Separated</td>
<td>0.641</td>
<td>(0.343, 1.13)</td>
<td>0.130</td>
</tr>
<tr>
<td>Location of Incarceration</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Not southern state</td>
<td>Ref.</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Southern state</td>
<td>0.256</td>
<td>(0.165, 0.398)</td>
<td>&lt;0.001</td>
</tr>
<tr>
<td>Year of execution or commutation†</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Before 2000</td>
<td>0.878</td>
<td>(0.831, 0.927)</td>
<td>&lt;0.001</td>
</tr>
<tr>
<td>2000 and after</td>
<td>1.06</td>
<td>(0.926, 1.20)</td>
<td>0.418</td>
</tr>
</tbody>
</table>

† This odds ratio reflects the linear change in odds per one-year increase. The difference in the odds ratio of commutation per unit increase in year before 2000 and after is statistically significant (p=0.03). The reported p-value in this table tests whether the linear trend for each time segment differs significantly from no effect, not whether the time periods differ from one another.
Table 3. Adjusted odds ratios of commutation by various death row prisoner characteristics, 1986-2005

<table>
<thead>
<tr>
<th>Characteristic</th>
<th>Odds Ratio</th>
<th>95% Confidence Interval</th>
<th>p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Race/Ethnicity</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White, not Hispanic</td>
<td>Ref.</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Nonwhite</td>
<td>2.01</td>
<td>(1.26, 3.21)</td>
<td>0.004</td>
</tr>
<tr>
<td>Sex</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Male</td>
<td>Ref.</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Female</td>
<td>10.8</td>
<td>(2.85, 40.7)</td>
<td>&lt;0.001</td>
</tr>
<tr>
<td>Prior Felonies</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>Ref.</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Yes</td>
<td>0.714</td>
<td>(0.414, 1.25)</td>
<td>0.225</td>
</tr>
<tr>
<td>Unknown</td>
<td>1.90</td>
<td>(0.709, 5.11)</td>
<td>0.202</td>
</tr>
<tr>
<td>Multiple Murders</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No</td>
<td>Ref.</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Yes</td>
<td>0.347</td>
<td>(0.093, 1.30)</td>
<td>0.116</td>
</tr>
<tr>
<td>Status at First Capital Offense</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Not under sentence and no charges pending</td>
<td>Ref.</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Under sentence, probation/parole, escaped, or charges pending</td>
<td>0.984</td>
<td>(0.571, 1.69)</td>
<td>0.954</td>
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<tr>
<td>Education</td>
<td></td>
<td></td>
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<tr>
<td>Less than high school degree</td>
<td>Ref.</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>High school degree</td>
<td>0.729</td>
<td>(0.443, 1.20)</td>
<td>0.214</td>
</tr>
<tr>
<td>At least some college</td>
<td>0.177</td>
<td>(0.050, 0.621)</td>
<td>0.007</td>
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<tr>
<td>Unknown</td>
<td>0.408</td>
<td>(0.144, 1.15)</td>
<td>0.091</td>
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<tr>
<td>Age at Arrest</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Under 25</td>
<td>Ref.</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>25 to 34</td>
<td>0.750</td>
<td>(0.443, 1.27)</td>
<td>0.284</td>
</tr>
<tr>
<td>35 to 44</td>
<td>1.20</td>
<td>(0.585, 2.47)</td>
<td>0.617</td>
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<tr>
<td>45+</td>
<td>1.50</td>
<td>(0.517, 4.37)</td>
<td>0.454</td>
</tr>
<tr>
<td>Marital Status at First Capital Imprisonment</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Married, widowed, never married, unknown</td>
<td>Ref.</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Divorced / Separated</td>
<td>0.864</td>
<td>(0.468, 1.60)</td>
<td>0.640</td>
</tr>
<tr>
<td>Location of Incarceration</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Not southern state</td>
<td>Ref.</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>Southern state</td>
<td>0.179</td>
<td>(0.111, 0.289)</td>
<td>&lt;0.001</td>
</tr>
<tr>
<td>Year of execution or commutation†</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Before 2000</td>
<td>0.859</td>
<td>(0.808, 0.913)</td>
<td>&lt;0.001</td>
</tr>
<tr>
<td>2000 and after</td>
<td>1.11</td>
<td>(0.970, 1.28)</td>
<td>0.127</td>
</tr>
</tbody>
</table>

† This odds ratio reflects the linear change in odds per one-year increase. The difference in the odds ratio of commutation per unit increase in year before 2000 and after is highly significant (p=0.004). The reported p-value in this table tests whether the linear trend for each time segment differs significantly from no effect, not whether the time periods differ from one another.
difference between commutation odds for men and women vary by different classifications; for example, it is possible that there might be a different effect of sex for people with a criminal history and those without. When it occurs, this phenomenon is called interaction. Tests for interaction between sex and race, marital status, and criminal offense variables were conducted, and none turned up a significant interaction (which is not unexpected given the small number of women in the dataset).

How should this result be interpreted? It is possible that greater propensity to commute women’s sentences reflects, at least in part, differences in the sort of crimes that women commit compared to men, or mitigating factors that women are more likely to possess (such as a history of being abused by the person they killed). This possibility cannot be ruled out, as it is possible that unmeasured confounding mitigating or aggravating factor may account for the observed sex effect on commutation. However, this is unlikely a major cause. If women’s crimes were less severe, or if they possessed greater mitigating factors, one would have expected to see a weakening of the association between sex and commutation when variables relating to socio-economic status and the prisoner’s criminal offense were included; instead, the association was strengthened after controlling for these factors.

There are additional reasons to doubt that the sex-commutation association is confounded by particulars of the case. Heise notes that women are less likely to be sentenced to death, indicating that women are likely to have to commit more serious crimes than their male counterparts to receive a death sentence. If this is true, women should be less likely to receive commutation if men and women are treated the same with respect to their criminal and personal histories. Additionally, Argys and Mocan investigated the specific crimes for which women were granted clemency before 1997, and concluded that such offenses were not more deserving of clemency than customary male offenses.

The data support a conclusion that women receive preferential treatment in commutation decisions. Even if confounding by unmeasured or imprecisely measured variables plays a role in the observed association, its strength is sufficient that, even if such hypothetical variables were considered, a significant

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116. See Lorraine Schmall, *Forgiving Guin Garcia: Women, the Death Penalty and Commutation*, 11 Wis. WOMEN’S L.J. 283, 315-16 (1996-97) (noting that DOJ studies indicate lesser criminal histories for female capital murder defendants); see also Rapaport, *supra*, note 31, at 979-80 (suggesting that women death row inmates present cases with less severe aggravating factors and milder histories of violence).


118. See id. at 277-78 (observing that “there is no strong evidence that females were more deserving of clemency on the basis of the merits of their cases”).

119. No such investigation could be undertaken in this study without violating the ICPSR’s data use condition against identifying individual prisoners in the dataset.

120. Argys & Mocan, *supra* note 26, at 277-78.
association would likely still exist. Explanations for a causal connection run the gamut from naked sexism to governors’ receipt of less political benefits from execution of women than men. Qualitative research on specific cases would be useful to investigate this question further. However, considering the strong evidence that men are disadvantaged at every other stage of the capital prosecution process, it is troubling that commutations appear to exacerbate, and not reduce, sex differences in capital punishment.

B. Race/Ethnicity

Prisoners of black, Hispanic, or other racial/ethnic heritage have slightly over twice the odds of commutation (OR=2.01) compared to their white counterparts after removing the effect of other factors. This association is highly unlikely to have been caused by chance (p=0.004). There was no significant difference in the odds of commutation between blacks and Hispanics in the adjusted model. The finding of a significant race effect is consistent with the results reported by Argys and Mocan but inconsistent with the finding of no association by Heise and Pridemore. Statistical tests for interaction between nonwhite status and female sex, southern geography, and the criminal history/offense variables were conducted, and none were statistically significant (meaning that there is no evidence that the effect of race varied for people from the South and North, for example).

This finding is likely to be controversial, and caution is warranted in interpreting it. One possibility is that the observed race effect is an artifact of bias earlier in the capital prosecution process. If minority defendants are more likely to be sentenced

121. Furthermore, given that inclusion of the potentially confounding variables available actually strengthened the association between sex and commutation, it is unlikely that including unmeasured variables would weaken it. And, if anything, imprecision in the variables for which association was adjusted should result in an observed association weaker than the true one.
122. See Rapaport, supra note 31, at 979-80 and Streib, supra note 30, at 466 for a range of possibilities. One explanation floated by Rapaport, however, is inconsistent with the results of this study. She suggests that women’s sentences may not actually be commuted more often; rather, it might be that higher rates of commutation may be an artifact of quicker review of women’s sentences. Rapaport, supra note 24, at 979. This study includes only those prisoners who came to a final disposition of their capital sentence, so if the association were simply an artifact of quicker review, it would not have been found in this study.
123. See Streib, supra note 30, at 434 (noting that female offenders are unlikely to be arrested for murder, only very rarely sentenced to death, and almost never executed).
124. Assuming that a white prisoner has a 10% chance of commutation, a nonwhite prisoner with identical characteristics on the features examined in this study would have a 19% chance of commutation. If the white prisoner has a 30% chance, the identical black prisoner has 47% chance.
125. When the multivariate regression model is run using blacks as the reference group, the odds ratio of commutation for Hispanics compared to blacks is 1.48 but not statistically significant (p=0.321).
126. Supra note 26, at 275-77.
127. Supra note 21, at 281-82.
128. Supra note 24, at 171.
to death for less severe crimes than white defendants, then executives who desire consistency in the severity of offenses for which they execute offenders would be expected to commute minority sentences more often. Because the variables which control for criminal history and the particulars of the offense are incompletely and somewhat imprecisely measured in this study, there is the possibility that the observed race effect is confounded by imperfect controls for the nature of the offense. This is not likely, however. When the effect of race was adjusted for the severity of the offense and prior history variables that were available—multiple homicides, legal status at the time of the offense, and history of prior felonies—the odds ratios for commutation comparing the two racial categories was not altered; such an alteration would be expected if criminal offense severity was confounding the race effect, and there is no reason to believe that other aggravating circumstances would have an effect while these do not.

The converse is also a possibility. It may be that race is confounded by unmeasured mitigating circumstances. For example, race may be associated with wealth, adequacy of counsel, or a range of other race-neutral reasons why a governor might feel that commutation is in the interests of justice. Only a few personal history or socioeconomic status variables were available in the Bureau of Justice Statistics dataset, so this is a possibility that cannot be ruled out.

An additional interpretation of the observed association between race and commutation is that it serves as a statistical leveling of the field. If equally situated nonwhite defendants are more likely to be sentenced to death than their white counterparts, a governor might disproportionately commute the sentences of nonwhite prisoners to try to counteract this effect. To the extent that racial bias in sentencing can be detected statistically but not applied to any particular individual, such “reverse arbitrariness” might be one way for a governor who does not wish to commute all sentences to try to limit racial bias in the executions carried out. If this explanation were to be established (and it cannot from the results of this study), one should be troubled by the implicit recognition by governors of bias in the sentencing processes, which disproportionate commutations could not equitably reverse.

129. This assertion is supported by relatively strong but not certain evidence. See, e.g., Baldus et al., supra note 35, at 1713-14 (concluding that among unanimously decided cases, race-of-defendant effects were statistically significant and demonstrate differential treatment); Eberhardt et al., supra note 35, at 384-85 (concluding that defendants whose appearance was perceived as more stereotypically black were more likely to receive a death sentence than defendants whose appearance was perceived as less stereotypically black).

130. Heise notes that we should expect to see this in the absence of adjustment for severity of the offense. Heise, supra note 21, at 282.

131. See Zoe Fawell et al., The Impact of Residual and Unmeasured Confounding in Epidemiologic Studies: A Simulation Study, 166 AM. J. EPIDEMIOLOGY 646 (2007), for a good discussion on the effects of residual confounding and confounding by unmeasured variables.

132. Argys and Mocan’s proposed remedy for discrimination earlier in the capital prosecution process is one explanation for the race effect that they observed. Argys & Mocan, supra note 26, at 277.

133. See Baldus et al., supra note 35, at 1654-55 (arguing that racial bias in sentencing is systemic).
Another potential cause of the observed race effect is that it may be confounded by the race of the victim. As noted above, race of the victim is a well-established predictor of whether one will receive a death sentence; those who kill whites are more likely to be sentenced to death than those who kill blacks.\textsuperscript{134} It may also be associated with whether one receives commutation—no study of commutation decisions to date has been able to include a race of victim variable, so this is presently unknown.\textsuperscript{135} Black defendants are more likely than white defendants to have killed a black victim.\textsuperscript{136} Thus, disproportionate commutation of nonwhite prisoners’ sentences might reflect greater valuation of the lives of white victims than black victims.\textsuperscript{137}

Although there is no definitive explanation for the observed association between race and commutation, it is an interesting and particularly controversial observation. Further research is warranted, particularly to examine what effect, if any, the victim’s race has on the likelihood that a prisoner’s sentence will be commuted. Additionally, closer qualitative examination of governors’ decision-making processes might shed light on this association.

\textbf{C. Criminal History and Crime Severity}

This study was able to investigate the severity of the offense and the prisoners’ criminal history to a greater extent than prior research. In addition to examining the effect of previous conviction for felonies (which was available in previous studies), this research also investigates the effect of the prisoner’s legal status at the time of the offense and whether the offense for which the offender was sentenced to death involved a multiple murder. The commission of multiple murders was associated with a three-fold decrease in the odds of commutation, but the association is not statistically significant (OR=0.347, p=0.116). Furthermore, neither a history of prior felonies nor a legal status other than being free from sentence or charges at the time of offense was significantly associated with decreased odds of commutation. The lack of association between prior felonies and commutation is consistent with the work by Pridemore,\textsuperscript{138} Argys and Mocan,\textsuperscript{139} and Heise (when he examined data from 1985 to 1999),\textsuperscript{140} but inconsistent

\textsuperscript{134} See id. at 1658-60.
\textsuperscript{135} Argys and Mocan also note this deficiency when discussing the race effect observed in their study, and they propose it as a possible explanation. Argys & Mocan, supra note 26, at 277.
\textsuperscript{136} Baldus et al., supra note 35, at 1656-57.
\textsuperscript{137} See Argys & Mocan, supra note 26, at 277 (“Given that white death row inmates are more likely to have perpetrated crimes against white victims and minority inmates were more likely to victimize other minorities, discrimination based on the race of the victim would suggest a harsher treatment of white inmates or higher commutation probabilities of minority inmates.”).
\textsuperscript{138} Supra note 24, at 171-72.
\textsuperscript{139} Supra note 26, at 275-76.
\textsuperscript{140} Supra note 21, at 274 tbl.5, 286-87.
with Heise’s findings when examining earlier data.\textsuperscript{141} It should be noted that both of these variables had to be combined so that there would be large enough groups to study with any precision, so it is possible that an effect was obliterated by collapsing the variables. Additionally, because prisoners with prior felonies are more likely to have been incarcerated at the time of offense, any association between priors and commutation might be washed out to some extent by the inclusion of the variable measuring legal status at the time of the offense; however, even after excluding this variable, there was no significant association between a history of felonies and commutation.

One additional measure of criminal culpability was examined. A criminal severity index was created as a composite of the three variables discussed above (not reported in the data table). Low severity was defined as no (or unknown) prior felonies, not facing charges or under sentence, and having committed only a single murder. Highest severity was defined as having any one of the following: multiple murders; a legal status at the time of offense of being imprisoned, escaped, on probation or parole, or under other sentence; or a prior history of homicide. Any other combination of the three variables was defined as moderate severity. When this variable replaced the three separate offense/history variables in the model, neither level of elevated severity showed a significant change in the odds of commutation compared to the lowest level. Additionally, a statistical test was performed to see if there was a trend of increasing odds of commutation as one went up the severity index; none was found.

Tests for interaction between race and both the severity index variables and the three original variables were conducted. None were significant. Thus, there is no evidence that governors treat criminal history and specifics of the offense differently for white and nonwhite offenders.

This paper takes no position on whether the death penalty is just. However, if the death penalty is to be retained, it should be conducted in an equitable manner, so it is disconcerting that race and sex appear to play a role in the likelihood that a prisoner’s sentence but that severity of the offense does not appear to drive commutation denials. Fundamental fairness should dictate that those who commit the least severe crimes or those who demonstrate the least likelihood of offending again are most deserving of mercy. However, across the run of cases, they do not appear to receive it. Furthermore, evidence that decisions are based on sex or race at any point in the death penalty process is alarming. These two findings, when taken together, should give pause to lawmakers as they evaluate states’ death penalty regimes, particularly given the Supreme Court’s espoused reliance on commutation as a safety valve for the death penalty process.\textsuperscript{142}

\textsuperscript{141.} \textit{Id.}
\textsuperscript{142.} See Herrera v. Collins, 506 U.S. 390, 415 (1993) (“Executive clemency has provided the ‘fail safe’ in our criminal justice system.”).
Nonetheless, it is important to define the scope of this study’s interpretation: it provides strong evidence that certain factors play a role in executives’ decisions to commute death sentences, across the run of cases. However, it cannot provide proof of the factors used in the decision whether or not to commute any particular prisoner’s sentence.143 Those seeking to extrapolate these results to a particular decision have an uphill battle, particularly given the Supreme Court’s hesitance to accept statistical data.144 Furthermore, as noted above, the extent to which commutations are subject to judicial review for due process and equal protection review is minimal at best. Thus, the primary value of the findings on race, sex, and criminal severity, as well as the other findings in this Note, is likely their value to inform the policymaking process. State policymakers should take quantitative data seriously when evaluating whether the death penalty is implemented equitably in their states.

D. Southern Geography

Prior research consistently has found that commutation is substantially less likely if the prisoner is under sentence in a southern state.145 This research is consistent with previous findings, though it observes a slightly stronger southern effect. Adjusting for other variables, a prisoner in the South has less than one-fifth the odds of commutation than a prisoner sentenced in a non-southern state (OR=0.179), and this association is highly significant (p<0.001).146 There was no interaction between the race/ethnicity and southern geography variables, indicating that there is no variation in the odds of commutation of people who are nonwhite in the South, compared to the rest of the country.

There are multiple interpretations of this finding. On the one hand, it is troubling that prisoners committing the same offense have markedly different chances of being executed based on their geographic location. On the other hand, the American legal system has long valued decentralized prioritization in what constitutes a crime and what punishment is appropriate, so that punishment is attuned to local values. Both of these values are important in our system of law, but substantial regional variation in punishment with the magnitude and finality of the death penalty is at least cause for greater investigation into what the causes of this variation are. If, for example, it were found that death penalty prisoners are less

143. See Baldus, et al., supra note 35, at 1654, for an explanation of the proof of factors.
144. See, e.g., McCleskey v. Kemp, 481 U.S. 279, 308-09 (1987) (noting that statistics only show a likelihood that a factor entered into a particular decision).
145. Heise, supra note 21, at 274 tbl.3, 301-02; see also Pridemore, supra note 24, at 172-73, 177 (noting that region is strong predictor of commutation).
146. See Heise, supra note 21, at 274 tbl.3, 301-02, for a more detailed analysis of the southern effect. If a southern prisoner has a 10% chance of commutation, a prisoner in a non-southern state with identical characteristics on the variables measured in this study would have approximately a 38% chance of commutation. If the southern prisoner had a 30% chance of commutation, the identical non-southern prisoner would have approximately a 70% chance of commutation.
likely to receive a commutation hearing in southern states, this would be cause for great concern.

E. Education

Attending at least some college is associated with a slightly greater than five-fold decrease in the odds of commutation compared to those whose education ceased before attainment of a high school degree (OR = 0.177, p = 0.007), adjusting for other variables. There was no significant difference between those whose education stopped before the receipt of a high school degree and those who received a high school degree (p = 0.214). Excluding those prisoners for whom educational status is unknown, a significant trend was observed between increased education and commutation (p = 0.04, results not reported in the data table above), meaning that as education increased across the various educational levels, so did commutation. This is consistent with previous findings by Heise and Argys and Mocan; Pridemore only examined those who had received at least a high school degree versus those who had not. No significant interactions were observed between the education variables and the race/ethnicity variable. In the absence of greater socioeconomic status data, it is unclear whether educational status is a proxy for social disadvantage, or whether governors look less favorably on offenders with greater education for that reason independently; both could plausibly play a role in governors’ decision-making.

F. Age

No age category had significantly different odds of commutation than the reference group (offenders who were under twenty-five years of age at arrest). No evidence for a trend of changing odds of commutation with increasing age was detected, either using the age categories reported above (p = 0.147) or when breaking arrest age into five groups of equal size (p = 0.416). Exclusion of offenders arrested before they were eighteen years old does not result in any significant trend for either type of age variable.

Prior research has tended to find that younger offenders have a better chance of

147. See Heise, supra note 21, at 274 tbl.3, 285-86. Thus, if a prisoner who attended college has a 10% chance of commutation, a prisoner whose education stopped before attaining a high school degree with identical characteristics on the variables measured in this study would have approximately a 39% chance of commutation. If the prisoner who attended college had a 30% chance of commutation, the identical prisoner without a high school degree would have approximately a 70% chance of commutation.

148. See id.

149. Argys & Mocan, supra note 26, at 275; see also Heise, supra note 21, at 285 (noting the effect of potentially confounding variables was controlled for using the Mantel-Haenszel method).

150. See Argys & Mocan, supra note 26, at 275 (observing increased education levels resulted in higher commutation rates); see also Heise, supra note 21, at 285.

151. Pridemore, supra note 24, at 170.

152. Heise, supra note 21, at 284-85.
commutation than their older counterparts, which is inconsistent with the results of this research. Several possible explanations for this exist. In this research, if age at arrest was missing, it was estimated by subtracting one year from the age at conviction. This likely resulted in some degree of misclassification, which may have weakened the observed association between age and commutation. However, this is unlikely to account for much of the substantial non-significance of the observed association. Another possibility is that the age effect noted by other research was present in older data but not in the newer data examined in this research, or that the observed age effect was an artifact of unmeasured confounding variables in previous research, such as criminal history, which are more completely measured in this study. Additionally, there are various forms an age variable could take, such as age at removal from death row or at sentencing; it is possible that some of the differences between studies relate to different ways of conceptualizing age.

G. Marital Status

Being divorced or separated was not significantly associated with commutation (OR=0.863, p=0.640). An alternative form of this variable, married versus any other marital status, also was not significantly associated with commutation (OR=0.970, p=0.905). Marital status was retained in the model primarily to investigate whether women who have been divorced are treated differently than women who have not been; no evidence of this was observed when using either form of the marital status variable. This finding is consistent with the weight of the evidence from prior research, and there is little reason to include a marital status variable in future research.

H. Year of Disposition

The year in which an offender was executed or commuted was significantly associated with the odds of commutation. Between 1986 and 1999, each passing year was associated with nearly a 15% decreased odds of commutation, compared to the year before it (controlling for other factors). However, after 1999, the odds of commutation increased by 11% each year. The difference between the two time periods is highly significant (p=0.004). This provides strong evidence that

153. See id. (observing death row inmates with more education were less likely to receive clemency); see also Pridemore, supra note 24, at 171-73 (noting older prisoners are less likely to receive commutation). But see Argys & Mocan, supra note 26, at 275 (finding non-linear relationship between age and commutation, with likelihood falling with age at sentencing until 45, then increasing after 45).

154. One last possibility relates to the way in which the studies included the age variable in their models; the other research treats age as a continuous variable, which may capture a non-linear association between age and commutation as a weak linear trend.

155. Heise and Argys & Mocan found no association. See Argys & Mocan, supra note 26, at 276; see also Heise, supra note 21, at 288-89. Contra Pridemore, supra note 24, at 172 (finding no association when examining all cases from 1973 to 1995 but finding a marginally significant association for cases between 1978 and 1995).
executives’ general propensity to commute changed markedly over the time period studied. Early on, they were increasingly unlikely to commute with each passing year, but in the last five years, this ceased. It should be noted, though, that even with the reversal in trend seen after 1999, the odds of commutation were much lower in 2005 than in the late 1980s and early 1990s.

The reason for these trends are unclear, but could be due either to changing norms about the use of commutation or alterations in concern about homicide levels. There is some evidence that Americans have become increasingly uneasy about the imposition of the death penalty since the mid-1990s. Around the same time, violent crime levels in the United States began to fall sharply. Declining imposition of the death penalty may be related to these factors.

V. CONCLUSIONS

This paper substantially updates the research investigating factors associated with decisions whether or not to commute a capital sentence. It provides strong evidence that women’s sentences are substantially more likely to be commuted than men’s, and that prisoners incarcerated in southern states or who have attended college are much less likely to receive a commutation. Both of these findings are consistent with previous research, and the cumulative evidence in favor of these associations is quite strong. This Note additionally provides some degree of evidence that prisoners who are nonwhite are significantly more likely to receive a commutation than their white counterparts. Lastly, this paper finds no evidence that criminal history or severity is associated with commutation, nor can it explain the decision-making employed in any particular case. However, it suggests that troubling factors are considered during executives’ commutation decisions, and it fails to provide evidence that what is arguably the most acceptable factor—the nature of the criminal offense—make much of a difference in whether a death row prisoner is granted clemency.

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156. The Supreme Court began more actively to reign in the imposition of the death penalty in the early 2000s. See Roper v. Simmons, 543 U.S. 551 (2005) (invalidating imposition of death penalty on offenders who were below eighteen years of age at time of crime); Atkins v. Virginia, 536 U.S. 304 (2002) (invalidating imposition of death penalty on offenders who are mentally retarded).
