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Life Terms or Death Sentences: The Uneasy Relationship between Judicial Elections and Capital Punishment

Richard R. W. Brooks

Steven Raphael

LIFE TERMS OR DEATH SENTENCES: THE UNEASY RELATIONSHIP BETWEEN JUDICIAL ELECTIONS AND CAPITAL PUNISHMENT

RICHARD R. W. BROOKS[*] AND STEVEN RAPHAEL[**]

I. INTRODUCTION

One day Louise Harris approached her lover, Lorenzo "Bo Bo" McCarter, with a proposition that led to an agreement to kill her husband.[1] Harris and McCarter paid Michael Sockwell and Alex Hood one hundred dollars to carry out the gruesome killing.[2] Following the killing, Harris, McCarter, Sockwell, and Hood were all convicted of capital murder in separate proceedings. In each case, a jury recommended life in prison without parole. Yet, in two of the four cases, the trial judge declined to follow the jury recommendations, choosing instead to sentence the defendants to death by electrocution. Different sentences following guilty verdicts on the same offense often occur because the "guilty/not–guilty" determination affords only the crudest approximation of culpability. Through sentencing, however, mitigating and aggravating considerations can give countenance to culpability. Still, the juries in the Harris murder cases had access to these considerations when they reached the same sentence recom-

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[1] Harris's husband, a local deputy sheriff, had death benefits worth one quarter of a million dollars, which the co–conspirators allegedly planned to share.

[2] As Deputy Sheriff Isaiah Harris came to a stop sign while driving to work in his 1979 Ford Thunderbird, Sockwell jumped out of the bushes holding a shotgun, pointed it at the deputy's face, and fired at close range. "As a result, the lower half of the victim's face was blown off, leaving his teeth, tongue, and 'matter' from his face blown across the car." Harris v. State, 632 So. 2d 503, 508 (Ala. Crim. App. 1992), aff'd sub nom. Ex Parte Harris, 632 So. 2d 543 (Ala. 1993), aff'd sub nom. Harris v. Alabama, 513 U.S. 504 (1995) (Stevens, J., dissenting) (affirming the Alabama Supreme Court decision sustaining the defendant's conviction and death sentence).

mendation for all four defendants. Why two defendants ultimately received death sentences and two received life sentences is a question that has more to do with the trial judge's temperament and discretion to impose his preferences than with some "objective" balancing of mitigating and aggravating circumstances.[3] In this Article, we attempt to measure the extent to which judicial temperament affects the likelihood of a defendant being found guilty of murder and the impact of specific judges on sentencing. Using detailed historical data on all murders recorded by the Chicago police over a sixty–year period during the late–nineteenth and early–twentieth centuries, we find significant judge–specific effects for both convictions and sentencing outcomes. Our analysis also reveals a consistent pattern of harsher outcomes correlated with the race of the defendant,[4] the race of the victim,[5] and the killing of police officers. Further, we observe a strong relationship between election years for judges and the likelihood that a defendant will receive a death sentence. That is, conditional on being found guilty of murder, criminal defendants were approximately 15% more likely to be sentenced to death when the sentence was issued during the judge's election year.

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[3] Sentencing disparity would result even if it were possible to weigh aggravating and mitigating circumstances objectively among defendants and judges. For example, consider the four traditional motivations for criminal sentencing: rehabilitation, incapacitation, deterrence, and just deserts. When sentencing a particular criminal defendant, judges with a preference for incapacitation will often reach different sentences from judges who place more weight on rehabilitation, even if they weigh the mitigating and aggravating circumstances around the offense equally. "[T]his predictable disagreement causes a troublesome result: two offenders identical in all relevant respects who have committed identical offenses may receive very different sentences." Paul H. Robinson, *One Perspective on Sentencing Reform in the United States*, 8 CRIM. L.F. 1, 5 (1997).

[4] Our research findings are consistent with the long–acknowledged pattern of blacks being convicted and executed at higher rates than whites prior to *Furman v. Georgia*, 408 U.S. 238 (1972). Research on capital cases since *Furman* has revealed a less consistent racial pattern in terms of conviction and execution rates. To explain the change, scholars have pointed to increased minority representation on juries and the end of rape's classification as a capital offense, among other factors. *See* STUART BANNER, THE DEATH PENALTY: AN AMERICAN HISTORY 289 (2002).

[5] Our findings are also consistent with the more recently identified "race of the victim effect." That is, regardless of the defendant's race, murderers of white victims are several times more likely to receive a death sentence. *See* DAVID C. BALDUS ET AL., EQUAL JUSTICE AND THE DEATH PENALTY: A LEGAL AND EMPIRICAL ANALYSIS (1990); Alan Widmayer & James Marquart, *Capital Punishment and Structured Discretion: Arbitrariness and Discrimination after* Furman, *in* CORRECTIONAL THEORY AND PRACTICE 178 (Clayton A. Hartjen & Edward E. Rhine eds., 1992).

A. POLITICS AND CAPITAL PUNISHMENT

In Louise Harris's death sentence appeal before the U.S. Supreme Court, Justice John Paul Stevens remarked that judges may be too responsive "to political pressures when pronouncing sentence in highly publicized capital cases."[6] Given the broad support of capital punishment among Americans, Justice Stevens observed that "judges who covet higher office—or who merely wish to remain judges—must constantly profess their fealty to the death penalty."[7] When up for re-election, most judges simply cannot afford to ignore popular sentiment about the death penalty. Nor, apparently, can many other elected officials.[8] For instance, one recent study found that during gubernatorial election years, states are 25% more likely to execute

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[6] Harris v. Alabama, 513 U.S. 504, 520 (1995) (Stevens, J., dissenting). Challenging the Alabama trial judge's decision to execute Harris notwithstanding the life sentence recommendation of the jury, Justice Stevens observed in his dissent that Alabama judges overturned jury recommendations for life sentences in 47 cases, while reversing juries in only 5 cases where they recommended death. *Id.* at 521. Similar patterns, where judges were more likely to reverse juries on life sentences rather than death sentences, were also identified in Florida and Indiana. *Id.* at 521 n.8. "Not surprisingly, given the political pressures they face, judges are far more likely than juries to impose the death penalty." *Id.* at 521. Of course, judges may disproportionately reverse juries recommending life sentences simply because juries are more prone to make errors in those cases, in comparison to cases where death may appear clearly warranted.

[7] *Id.* at 519. A correlation between political events and judicial sentencing may exist even in states where there is no genuine competition in the electoral process (such as in Mississippi, where very few judgeships are contested in primary or general elections). James Alfini, *Mississippi Judicial Selection: Election, Appointment, and Bar Anointment, in* COURTS AND JUDGES 253, 255-60 (James Cramer ed., 1981). Stephen B. Bright, *Elected Judges and the Death Penalty in Texas: Why Full Habeas Corpus Review by Independent Federal Judges is Indispensable to Protecting Constitutional Rights,* 78 TEX. L. REV. 1805, 1832 (2000) ("[J]udges ignore public attitudes and their political supporters at the peril of losing their positions in the next election.")[hereinafter Bright, *Elected Judges*]; *see also* Stephen B. Bright, *Political Attacks on the Judiciary: Can Justice be Done Amid Efforts to Intimidate and Remove Judges From Office for Unpopular Decisions?,* 72 N.Y.U. L. REV. 308 (1997) [hereinafter Bright, *Political Attacks*]; John D. Fabian, *The Paradox of Elected Judges: Tension in the American Judicial System,* 15 GEO. J. LEGAL ETHICS 155 (2001); *cf.* Shirley S. Abrahamson, *The Ballot and the Bench,* 76 N.Y.U. L. REV. 973 (2001) (arguing for the importance of an electoral system for state judges, while conceding the existence of unwanted political influence over judicial decision-making).

[8] The term "political suicide" is often used to describe the likely consequences when elected officials appear unsupportive of capital punishment. See James R. Acker & Charles S. Lanier, *May God—or the Governor—Have Mercy: Executive Clemency and Executions in Modern Death-Penalty Systems,* 36 CRIM. L. BULL. 200, 210 n.53 (2000) and accompanying text. *See also* BANNER, *supra* note 4, at 276 (recalling the role the death penalty played in the 1988 presidential campaign between George H. Bush and Michael Dukakis (exemplified by Willie Horton), and later in the 1992 campaign between George H. Bush and Bill Clinton (exemplified by Ricky Rector)).

prisoners.[9] Prosecutorial election cycles have also been long known to correlate with the state's willingness to seek death sentences in murder cases.[10] It would be surprising if judges, during their election cycles, were unresponsive to the political pressures confronting their elected counterparts in the governors' and prosecutors' offices.[11] In these offices, the pressure is often relieved through an exercise of gubernatorial or prosecutorial discretion. One might expect that in judicial chambers and courtrooms throughout the country, judicial discretion serves a similar purpose.

B. JUDICIAL DISCRETION AND JUDGE SPECIFIC EFFECTS

Judicial discretion can, of course, be good or bad. Through discretion, judges can tailor punishments to conform to socially desirable objectives. Unfortunately, judicial discretion based on unwarranted considerations, such as re–election prospects or ethnicity, will lead to undesirable variations in sentencing.[12] In response to growing

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[9] *See* JEFFERY D. KUBIK & JOHN R. MORAN, LETHAL ELECTIONS: GUBERNATORIAL POLITICS AND THE TIMING OF EXECUTIONS (Center for Policy Research, Syracuse University, Working Paper, 2001). Although grants of clemency were once a more common practice, in recent decades governors have grown increasingly unwilling to commute death sentences, Illinois' recent mass commutation notwithstanding. "Part of clemency's decline [is] attributable to the growing popularity and salience of the death penalty. A commutation could be political suicide for an elected official . . . ." BANNER, *supra* note 4, at 291. Part of the decline in gubernatorial clemency may also be attributed to the constraints of the Supreme Court's constitutional rulings on the capital punishment beginning with *Furman v. Georgia,* 408 U.S. 238 (1972) and *Gregg v. Georgia,* 429 U.S. 1301 (1976). "Mercy had been banished from the system, replaced by an arcane set of rules that haphazardly selected who would live and who would die." BANNER, *supra* note 4, at 309–10.

[10] BANNER, *supra* note 4, at 288 ("Whether a defendant was charged with capital or noncapital murder depended largely on whether the prosecutor was up for reelection.").

[11] Indeed, even after being elected, scholars and practitioners have also charged that political considerations such as patronage and commitment to campaign platforms are often present when judges appoint counsel to represent indigent capital offense defendants. *See* Bright, *Elected Judges, supra* note 7, at 1806 ("Texas trial judges—some treating the appointment of counsel to defend poor defendants as political patronage and some assigning lawyers . . . to help move their dockets—have frequently appointed incompetent lawyers to defend those accused of capital crimes." (footnotes omitted)); Gerald F. Uelmen, *The Fattest Crocodile: Why Elected Judges Can't Ignore Public Opinion,* 13 CRIM. JUST. 4, 9 (Spring 1998) (observing that "[j]udges who owe their elections to a campaign commitment to enforcing the death penalty will be more likely to countenance lazy and sloppy lawyers").

[12] Numerous studies and anecdotal accounts that have pointed to sentencing variation resulting from such unwarranted factors as the region of the country where the defendant was convicted, the defendant's race, and the defendant's gender, among other factors. *See* Kathleen Daly & Michael Tonry, *Gender, Race, and Sentencing, in* 22 CRIME & JUSTICE: A REVIEW OF RESEARCH 201 (Michael Tonry ed., 1997); Debra A. Curran, *Judicial Discretion and Defendant's Sex,* 21 CRIMINOLOGY 41 (1983); John Hagan & Kristin Bumiller, *Making*

perceptions of widespread variations of this sort, the U.S. Congress passed the Sentencing Reform Act in 1984.[13] This Act came nearly one century after studies first identified systemic sentencing variability due to individual differences among judges.[14] One early analysis of discretion in judicial decision–making was offered by George Everson in this journal in 1919.[15] Everson analyzed the sentencing patterns of approximately forty judges on the New York Magistrates' Court: "With the consent of the Chief City Magistrate the Committee on Criminal Courts went over the record of 155,000 or so cases disposed of in 1914. . . ."[16] Significant variations were found: "The results showing to what extent justice is affected by the personality of the judges were so startling and disconcerting that it seemed advisable to discontinue the comparative tables of the records of the justices."[17] Several years later, Frederick Gaudet and his colleagues examined 7442 randomly assigned cases in one New Jersey jurisdiction and observed stark differences among the sentencing behavior of judges.[18]

To be confident that these observed differences in sentencing were due to the judicial temperament and not other "non–judge" factors that are correlated with particular judges, Everson and Gaudet, *et al.*, relied on random case assignments. Randomly assigned cases will control for these non–judge factors, assuming that the number of cases is sufficiently large. Reliance on randomly assigned cases continues to be a useful methodological device for research on judicial decision–making. For example, Joel Waldfogel recently analyzed

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Sense of Sentencing: A Review and Critique of Sentencing Research, in 2 RESEARCH ON SENTENCING: THE SEARCH FOR REFORM 1 (Alfred Blumstein et al. eds., 1983).

[13] The U.S. Congress observed that sentencing outcomes for similar offenses were "unjustifiably wide" when it created and charged the Sentencing Commission with devising the sentencing guidelines. See Sentencing Reform Act, Pub. L. No. 98–473, 98 Stat. 1837 (1984)(codified in 18 U.S.C. §3551), which mandated the creation of the Federal Sentencing Guidelines. *See also* Martin E. Frankel, *Lawlessness in Sentencing*, 41 U. CIN. L. REV. 1 (1972) (offering an early criticism of sentencing variation among federal judges); Robinson, *supra* note 3; *cf.* Albert W. Alschuler, *The Failure of Sentencing Guidelines: A Plea for Less Aggregation*, 58 U. CHI. L. REV. 901 (1991) (arguing there is too much uniformity, rather than too little, in sentencing).

[14] Francis Galton, *Terms of Imprisonment*, 52 NAT. 174, 174-76 (June 20, 1895).

[15] George Everson, *The Human Element in Justice*, 10 J. CRIM. L. & CRIMINOLOGY 90 (1919).

[16] *Id.* at 96.

[17] Charles Grove Haines, *General Observations on the Effects of Personal, Political and Economic Influences in the Decisions of Judges*, 17 ILL. L. REV. 96, 105 (1922).

[18] Frederick J. Gaudet et al., *Individual Differences in the Sentencing Tendencies of Judges*, 23 J. CRIM. L. & CRIMINOLOGY 811 (1933).

data from federal criminal cases that were randomly assigned to ten judges in the Northern District of California from 1984 to 1987.[19] He too found statistically significant inter–judge disparity. Similarly, James Anderson and his colleagues relied on random assignments over 77,201 federal cases from 1981 to 1993 to show a reduction in interjudge sentencing disparity after the implementation of the Federal Sentencing Guidelines.[20] With a sufficient number of cases, an accurate and straightforward comparison of judicial leniency or severity may be made relying on random case assignments. However, the appropriateness of such comparisons is predicated on the validity of the random assignment mechanism. That is, the case assignments must be truly random in order to draw meaningful conclusions.[21] There are many reasons to be skeptical that such mechanisms exist in actual courtroom practice,[22] especially in Chicago during the time of this present study.[23]

As an alternative to relying on random assignments for valid comparisons, researchers may explicitly control for factors that are correlated with judicial sentencing. One method of controlling for

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[19] Joel Waldfogel, *Does Inter–Judge Disparity Justify Empirically Based Sentencing Guidelines?*, 18 INT'L REV. L. & ECON. 293 (1998).

[20] James M. Anderson et al., *Measuring Interjudge Sentencing Disparity: Before and After the Federal Sentencing Guidelines*, 42 J.L. & ECON. 271 (1999).

[21] *Cf.* Waldfogel, *supra* note 19. Waldfogel supports his use of random assignments by examining whether caseloads for individual judges in his sample differed according to gender. Gender, however, is but one element among many that could systematically generate a biased distribution of an allegedly random process of case assignments. It should be noted that Waldfogel does not rely solely on random assignments in his analysis. He also uses multivariate analysis, controlling for many observable characteristics of the offense and the offender.

[22] Shari Seidman Diamond & Hans Zeisel, *Sentencing Councils: A Study of Sentencing Disparity and its Reduction*, 43 U. CHI. L. REV. 109, 112 (1975).

[23] The assignment of cases by the Chief Justice of the Criminal Court was nominally random, though for much of this period the case handling of the Criminal Court was broadly viewed as given to widespread political corruption. THE ILLINOIS CRIME SURVEY (John H. Wigmore ed., 1929). According to the procedural rules of the Criminal Court, "The Chief Justice [of the Criminal Court] shall each term assign the cases for trial to the several branches in rotation by numbers with such variations there from as in his judgment will tend to expedite the disposition of cases." *See* COOK CTY. CRIM. CT. R. 15, (1920) *as recorded in* GUNTHROP'S LEGAL DIRECTORY OF CHICAGO, 1919–1920, 193 (Chi. Bar Ass'n ed., 15th ed. 1921). Yet, even the selection of the Chief Justice was given to political manipulation: "[t]his is made possible by the fact that those in charge of assignments are elected officials who often stand ready to distribute cases . . . in exchange for political support and judicial favors." ALBERT LEPAWSKY, THE JUDICIAL SYSTEM OF METROPOLITAN CHICAGO 62 (1932). Today, cases are distributed through computer–generated random assignment, which has led to some improvement.

such factors is to place the set of cases being analyzed into subgroups with similar relevant characteristics. Comparisons within subgroups across different judges may then be used to identify judge–specific effects. Edward Green, for instance, employed this procedure in his examination of the Philadelphia criminal court, where he analyzed 1437 cases between 1956 and 1957 among 18 judges.[24] Though Green underestimated his own results, significant disparity among judges was again found.[25] Caution must be exercised when interpreting these results, however, since cases within a subgroup may differ in salient ways that are unobservable to the researchers. Our research is based on simple analysis of variance and multivariate regression techniques, which—for practical purposes—are the same as the subgroup methodology described above, though somewhat more sophisticated. We control for relevant non–judge variables by including them, along with the judge–specific variables, in our regression equations. Unfortunately, just as with the subgroup comparison method, omission of relevant unobserved factors will lead to biased results. Since it is impossible to be certain that all relevant factors are included in the analysis, bias will result if important omitted factors are more likely to occur with a subset of judges.

Bias resulting from omitted variables will be unavoidable in analyses of judicial decision–making unless the researcher is able perfectly to control the variance among the cases and background factors that judges face. Simulated or mock trials, which evaluate the rulings of multiple judges over the same cases, have been used to evaluate differences across judges in a perfectly controlled setting. The problem with this setting is that the simulation itself may omit some pertinent real–world consideration. To overcome the artificiality of simulations, Shari Diamond and Hans Zeisel looked at New York and Chicago sentencing councils, which consist of a panel of judges who read pre–sentencing reports and offer sentencing recommendations to the presiding trial judge.[26] Judges on sentencing councils avoid the contrived nature of experiments because they are aware their sentencing recommendation will affect real defendants. Using

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[24] *See* EDWARD GREEN, JUDICIAL ATTITUDES IN SENTENCING: A STUDY OF THE FACTORS UNDERLYING THE SENTENCING PRACTICES OF THE CRIMINAL COURT OF PHILADELPHIA 15 (1961).

[25] *See* Diamond & Zeisel, *supra* note 22, at 113.

[26]*Id.* at 109 ("[Sentencing councils enable] the sentencing judge, before imposing sentence, to meet with his colleagues in order to learn what sentences they would impose if they were the sentencing judge.").

this thoughtful approach, Diamond and Zeisel were able to identify significant disparity among the panel judges in terms of the types of sentences (*e.g.*, imprisonment or probation) and the length of sentences.

One drawback, however, of using disparity among sentencing councils as a proxy for courtroom variability is that the members of the council are ultimately not issuing the sentence, but rather only making a recommendation from a fairly anonymous vantage point. This anonymity may have implications for sentencing decisions. For example, since sentencing decisions take place in a broader political context than the courtroom, the anonymity of the council may serve to embolden or restrain judicial behavior. On the other hand, trial judges know their decisions often have political resonance, which can affect their job tenure. This consideration led Gaudet and his colleagues, in their 1933 study of New Jersey judges, to speculate that there might be some impact of imminent reappointment on the sentencing behavior of judges.[27] Though they found no significant impact, they suggested that a comparable analysis of a jurisdiction with a judicial elective process, rather than an appointment process, would be compelling.[28] This Article provides that analysis by looking at conviction rates and death sentences during and around judicial election years in Chicago. First, however, the next section presents a brief discussion of Chicago's criminal court structure in the late 1800's and early 1900's.

C. THE CRIMINAL COURT OF COOK COUNTY, 1870 TO 1930

During the period of our study (from 1870 to 1930), criminal cases in Illinois were heard in any of several courts, including the Circuit, City, County, Superior, Municipal, and Criminal courts.[29] Yet, jurisdiction over cases involving more serious offenses, particu-

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[27] Gaudet et al., *supra* note 18, at 815 ("Another question which arose in the authors' minds in working with these data [was] . . . whether the imminence of a reappointment affected the sentencing tendencies of these judges.").

[28] Within an elective framework, the political influence on judges is in some ways abated and in other ways exacerbated. *See* EDWARD M. MARTIN, THE ROLE OF THE BAR IN ELECTING THE BENCH IN CHICAGO 294–99 (1936).

[29] Membership and jurisdiction of these courts often overlapped significantly, producing venue–shopping opportunities for clever defense attorneys and ambitious prosecutors. These courts, however, generally did not have direct jurisdiction over criminal cases. 37 ILL. REV. STAT. 701 (1931). For example, unlike the Circuit Courts of other Illinois counties, the Circuit Court of Cook County had only quasi–criminal jurisdiction. *See* MARTIN, *supra* note 28, at 25.

larly murder, was largely restricted to the Criminal Court of Cook County (Criminal Court).[30] The Criminal Court was staffed by elected judges from the Circuit and Superior Courts of Cook County.[31] These judges were appointed to serve on the Criminal Court for one to two years of their six–year elected terms.[32] Judges generally resisted appointments to the Criminal Court, especially early in their elected terms.[33] However, as their terms came to a close, a Criminal Court assignment could be quite valuable: "judges desire[d] to be assigned immediately before coming up for re–election for the sake of profiting from the greater newspaper public-ity. . . ."[34] For this and other reasons, we hypothesize that judicial

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[30] Judicial manipulation on the Criminal Court of Cook County was broadly acknowl-edged. A comprehensive and critical study of the judicial process in Chicago may be found in THE ILLINOIS CRIME SURVEY, *supra* note 23, which provides a "dismal and disconcerting picture" of judicial administration at that time.

[31] The election cycle for the Circuit Court commenced roughly on the first Monday of June every six years, beginning in 1873 (single vacancies and special elections to expand the Circuit court were held during various off years). Five judges were elected to the Circuit Court in 1873, 1879, and 1885; eleven were chosen in 1891; fourteen in 1897, 1903 and 1909; twenty were chosen every sixth year from 1915 to 1933. Though Superior Court judges also held six–year terms, their election cycle was less transparent. There were, in fact, four separate approximate six–year cycles for the Superior court, labeled series A through D. In election series A and B, voters selected a single judge every sixth year begin-ning in 1873 and 1871, respectively. In series C, voters selected four judges in 1880 and 1886 and six judges every sixth year from 1892 to 1934. Series D elections placed one judge on the bench in 1874, 1881, 1887, and 1893; four judges in 1900 and 1905; ten judges in 1911; twelve in 1917; and twenty in 1923 and 1929. MARTIN, *supra* note 28, at 11–18.

[32] ILL. CONST. of 1870, art. VI, § 12 (1870), *reprinted in* ILLINOIS CONSTITUTIONS 136 (Emil Joseph Verlie ed., 1919); 37 ILL. REV. STAT. 158–59 (1931); and 23 ILL. REV. STAT. 191 (1931). According to the Rules of the Criminal Court, the chief judges of the Circuit and the Superior Courts had authority over the annual assignment of their members to the Criminal Court. *See* COOK CTY. CRIM. CT. R. (1920), *as recorded in* GUNTHROP'S LEGAL DIRECTORY OF CHICAGO, 1919–1920, *supra* note 25. In practice, however, an executive committee of judges on the Circuit and Superior Courts assumed the role of determining as-signments. *See* LEPAWSKY, *supra* note 23, at 164–76.

[33] *See* JUDGE MICHAEL L. MCKINLEY, *The Fine Art of the Fixer*, *in* CRIME AND THE CIVIC CANCER—GRAFT 22, 22 (The Chicago Daily News Reprints, Series No. 16, 1923) (the then chief justice of the Criminal Court noted that "[t]hough service in the Criminal Court is dis-tasteful to the majority of judges, by the rule of rotation they are sent to the criminal branches toward the close of their elective term of six years."). The executive committees regularly assigned the newly–elected judges to the Criminal Court in the early part of their terms. LEPAWSKY, *supra* note 23, at 171.

[34] *See* LEPAWSKY, *supra* note 23, at 167 (describing how judges on the Criminal Court of Cook County leveraged their position during election time: "An alert judge assigned to a criminal branch preceding an election, if he has a flair for tart comments and slogans about crime, is practically able to assure his re–election.").

sentencing will correlate with election cycles.[35]   Though primarily focusing on the election cycles of judges, we also consider the impact of the elections of other political players who exercised direct or indirect influence over criminal cases, such as the mayor and the state's attorney, the county's principal prosecutor.[36] Prosecutors, of course, had broad discretion in disposing of criminal cases—from the charging to the sentencing stages—but state's attorneys' influence in Chicago extended beyond traditional prosecutorial discretion.[37] For instance, Robert E. Crowe served as the Cook County's State's Attorney for several years during our study, while simultaneously leading the party machinery that controlled Republican judicial nominations in Chicago. Crowe literally hand–picked the judges before whom he would argue his cases.[38]

II. DESCRIPTION OF THE DATA AND THE EMPIRICAL METHODOLOGY

To assess whether judicial discretion impacts the likelihood of

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[35] The ILL. CONST. of 1870, art, VI, §24 (1870), *reprinted in* ILLINOIS CONSTITUTIONS, *supra* note 32, at 140, provided a process for the selection of the chief judge of the Circuit and Superior Courts, but there was no clear directive for the selection of the executive committees or the chief judge of the Criminal Court. In practice, it appears that the chief judge of the Criminal Court voted from the Circuit Court one year and the Superior Court the next year.

[36] Barbara Caulfield, *Access to Information in the Office of the State's Attorney of Cook County, Illinois*, 68 NW. U. L. REV. 336, 336 (1973)("The power of the State's Attorney has been described by the American Bar Association as 'enormous' and virtually 'unreviewable' except for the period check of elections."). In a 2001 article, Ahmed E. Taha provided recent evidence of the impact of prosecutorial power following the implementation of the Federal Sentencing Guidelines. By constraining judicial discretion, Taha found that the guidelines shifted "a great ,deal of sentencing power from judges to prosecutors because prosecutors choose which charges are filed against defendants." *See* Ahmed E. Taha, *The Equilibrium Effect of Legal Rule Changes: Are the Federal Sentencing Guidelines Being Circumvented?*, 21 INT'L L. REV. L. & ECON. 251, 251 (2001); *cf.* William M. Rhodes & Catherine Conly, *Federal Sentencing Guidelines: Will They Shift Sentencing Discretion from Judges to Prosecutors?*, *in* COURTS AND JUDGES, *supra* note 7, at 197.

[37] *See* James R. Kavanaugh, *Representing the People of Illinois: Prosecutorial Power and Its Limitations*, 27 DEPAUL L. REV. 625 (discussing various discretionary mechanisms available to prosecutors from the perspective of the Chief of the Criminal Bureau of the Cook County State's Attorney's Office).

[38] MARTIN, *supra* note 28, at 75–81. The Illinois Crime Commission noted that "[a]fter the municipal election in 1927, the mayor, the state's attorney [Crowe], the coroner, the chief of police, the sheriff of Cook County, and a majority of the judges on the criminal courts were all affiliated with the dominant political faction in the county," leading to inefficiency and corruption. THE ILLINOIS CRIME SURVEY, *supra* note 23, at 17. "The records indicate that literally thousands of felons were being released outright by the prosecutor." *Id.* See also *id.* at 285–331 for a detailed discussion of prosecution of felony cases in Chicago.

conviction and ultimate sentencing outcomes, we make use of de-
tailed historical data on murders occurring in Chicago in the late–
nineteenth and early–twentieth centuries. These data include inci-
dent–level detail on various aspects of murder cases, including in-
formation on trial disposition of arrested defendants and the name of
the trial judge hearing the case.

Our first empirical strategy exploits the fact that the majority of
judges appearing in these murder records are observed trying more
than one case. Specifically, we perform a one–way analysis of vari-
ance (ANOVA) of several trial and sentencing outcomes in an at-
tempt to identify statistically whether judge–specific effects are im-
portant. We analyze three outcomes: the likelihood of a guilty
verdict, the likelihood of receiving a death sentence conditional on a
guilty verdict, and the likelihood of receiving a life sentence condi-
tional on a guilty verdict. To illustrate the basic method, suppose that
we observe K judges (indexed by $k = 1,. . .,K$) who each try N cases
(indexed by $n = 1,. . .,N$). Define the outcome $Guilty_{nk}$ as an indica-
tor variable equal to one if trial n heard by judge k resulted in a guilty
verdict and equal to zero otherwise. The judge–specific conviction
rates are defined by the K equations

$$\overline{Guilty_k} = \frac{\sum_{n=1}^{N} Guilty_{nk}}{N}, \text{ for } k = 1,...,K,$$

while the overall conviction rate is defined by the equation

$$\overline{Guilty} = \frac{\sum_{k=1}^{K}\sum_{n=1}^{N} Guilty_{nk}}{N * K}.$$

Assume for the moment that judges do not affect the likelihood
that a trial results in a guilty verdict (that is, suppose that the null hy-
pothesis of no judge effects is true). Under this assumption, the
overall variance in the variable $Guilty_{nk}$ can be estimated using both
the within–judge variation in this outcome and the variation occur-
ring between judges. The variation occurring within judges is de-
fined by the judge–specific sums of squared deviations about the
judge–specific means. This is given by the K equations

$$SS_k = \sum_{n=1}^{N} (Guilty_{nk} - \overline{Guilty_k})^2, \; for \; k = 1,...,K.$$

This within–judge variation is used to estimate the variance in the guilty indicator variable by calculating the mean square within (MSW), or

$$MSW = \frac{\sum_{k=1}^{K} SS_k}{N*K-K}.$$

Under the null hypothesis, the MSW is a consistent estimate of the variance of $Guilty_{nk}$. An alternative estimate is provided by the equation for the mean square between (MSB), or

$$MSB = \frac{N \sum_{k=1}^{K} (\overline{Guilty_k} - \overline{Guilty})^2}{K-1}$$

which, assuming no judge effects, also provides a consistent estimate of the variance in $Guilty_{nk}$. This latter estimate exploits the fact that sampling variation of the judge–specific means around the overall mean is proportional to the overall variance in the guilty indicator variable.

Under the null hypothesis of no effect of judicial discretion on outcomes, these two variance estimates should be similar. Alternatively stated, under the null hypothesis the ratio MSB/MSW should be equal to one. If the null hypothesis is false, however, between–judge variation should exceed the variation that one would expect to result from sampling variation alone. In other words, the MSB should be larger than the MSW, and the ratio of the two should exceed one. Hence, a simple test for an effect of judicial discretion is a test of the null hypothesis

$$H_0: MSB/MSW = 1$$

against the alternative hypothesis

$$H_1: MSB/MSW > 1.$$

This ANOVA test compares the unadjusted variation occurring between judges to the variation occurring within judges and tests whether the between–judge variation is too large relative to the within–judge variation to be consistent with no role for judicial discretion. One problem with this test concerns the fact that this simple empirical tool does not account for systematic variation in the types of cases that are handled by the judges observed in the sample. For example, it may be that over the course of their careers, certain judges receive cases that, on average, involve more heinous circumstances than the cases heard by other judges. To the extent that this is the case, some judges will have higher conviction rates on average than others. These differences, however, would reflect variation in the average circumstances of the cases heard rather than differences in the manner with which the judge managed the trial and sentencing proceedings.

One method of addressing these concerns would be to test for the statistical significance of judge–specific effects on trial and sentencing outcomes in the context of a multivariate regression. Specifically, define the variable $Guilty_{nk}$ as above and let X_{nk} be a vector of characteristics of the defendant, the victim, and the circumstances of the *nth* murder case heard by the *kth* judge. Using ordinary least squares (OLS), we could estimate the linear regression equation

$$Guilty_{nk} = \alpha + \alpha_k + \beta' X_{nk} + \varepsilon_{nk},$$

where α is a common intercept term, α_k is a judge–specific intercept term that is defined for K–1 judges, β is a vector of coefficients corresponding to the control variables included in X_{nk}, and ε_{nk} is a normally–distributed error term with a mean of zero. The regression–adjusted test for judicial discretion would test this model with variable intercepts against a constrained regression model with a single intercept for all judges. In other words, a test for judge effects is a test of the joint statistical significance of the K–1 judge effects that are included in the regression specification. Below, we present both tests for judge effects that do not account for systematic variation in the types of cases heard as well as tests for judge effects that adjust for observable covariates.

Our second empirical strategy for assessing whether judicial dis-

cretion played an important role in determining our three trial and sentencing outcomes is to assess whether the likelihood of each outcome differs when the murder occurs during a judicial election year. To the extent that judges benefited politically from stiff outcomes in an election year and if judges have some discretion over outcomes, one might expect differential outcomes in election years relative to non–election years.

To test this hypothesis, we exploit the timing of judicial elections during the time period covered by our sample. For Circuit court judges, elections occurred every six years.[39] Using the names of each judge as reported in the murder records we researched historical records in order to identify those judges serving on Circuit courts. We then restrict the sample to those murders that were tried by Circuit court judges.[40] With this restricted sample, we estimate the model

$$Guilty_{nk} = \alpha + \alpha_k + \gamma Elect_{nk} + \beta' X_{nk} + \varepsilon_{nk},$$

where all of the variables are defined as above and the variable $Elect_{nk}$ is a dummy variable equal to one if the offense occurs during an election year. We estimate several variants of this model (without other control variables, controlling for a host of defendant, victim, and incident characteristics, and controlling for these covariates plus judge–specific fixed effects) for each of the three trial and sentencing outcomes discussed above. We interpret positive and statistically significant coefficients on the election year dummy as evidence of judicial discretion impacting outcomes.

We use data from the Northwestern University School of Law Project for the Study of Homicide in Chicago. The database provides detailed information on all murders occurring in the city of Chicago between 1870 and 1930 that were recorded by the police. Researchers on the Homicide project took handwritten reports summarizing details of specific homicides (such as characteristics of the murder, victim and defendant characteristics, whether arrests were made, and post arrest trial outcomes) and coded these details into a uniform set of variables amenable to statistical analysis. The database includes

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[39] For the time period covered in our sample, Circuit court elections occurred in 1891, 1897, 1903, 1909, 1915, 1921, 1927, and 1933.

[40] Identifying election years for Superior court judges is considerably more difficult since there were four separate Superior court election cycles and the historical records do not clearly indicate on which cycle on each Superior court judge was elected.

such information on over 10,000 murders occurring during this period.

Given the nature of the inquiry, we impose several sample restrictions to arrive at our final sample for analysis. First, since we are interested in studying the role of judges in determining trial outcomes, we restrict the sample to those murders where an arrest is recorded, where there is information on the trial outcome, and where the judge hearing the trial is explicitly identified. Furthermore, since our simple ANOVA test and the regression–adjusted test for judge effects require that there be at least two trials per judge, we restrict the sample to observations where there is at least one other trial heard by the same judge. These combined sample restrictions reduce the size of the final sample used to analyze the determinants of guilty verdicts to 2631 murder trials. These murder trials are heard by 139 separate judges. Of these, 1302 murder trials were heard in Circuit courts. Hence, the sub–sample used to test for an election year effect is approximately half the size used to test for judge effects.

For our analysis of the determinants of the likelihood of receiving a death or a life sentence, we must further restrict the sample to observations where there is complete information on the ultimate sentence handed down to those defendants found guilty. This additional restriction reduces the sample size for the analysis of these outcomes to 851. These sentencing proceedings are handled by 97 separate judges. In the regression models that adjust for observable aspects of the crime, we further restrict the sample to those observations with complete information on the additional explanatory variables. For all three outcomes, this additional restriction reduces the sample size by about one–third. Of these outcomes, approximately half were tried in Circuit courts.

Figures 1 through 3 graphically depict the degree of between–judge variation in the three outcomes that we analyze. The figures are constructed as follows: For Figure 1, we first calculated judge–specific conviction rates by calculating the mean of the dummy variable indicating a guilty verdict for each judge. The figure then plots the distribution of these judge–specific conviction rates. Figures 2 and 3 perform the similar calculations for a dummy variable indicating a death sentence and a dummy variable indicating a life sentence. There is considerable variation for all three outcomes. The distribution of conviction rates in Figure 1 is dual–peaked, with a spike at conviction rates of 0 and conviction rates falling in the 0.36 to 0.45 category. The dispersion around this central category is substantial.
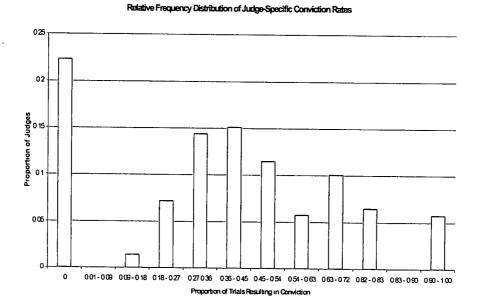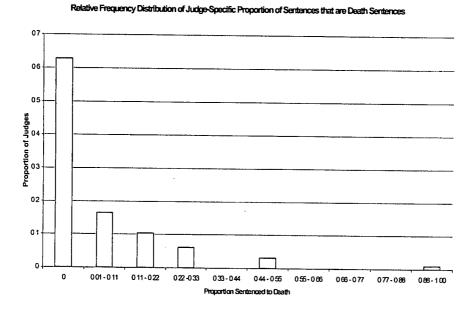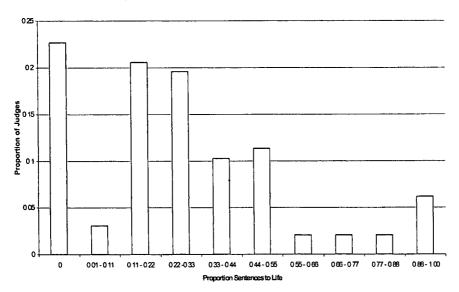
Figure 1

Relative Frequency Distribution of Judge-Specific Conviction Rates

Figure 2

Relative Frequency Distribution of Judge-Specific Proportion of Sentences that are Death Sentences

Figure 3

Relative Frequency Distribution of Judge-Specific Proportion of Sentences that are Life Sentences

Proportion Sentences to Life

The dispersion in death sentences shown in Figure 2 is considerably less. The modal judge–specific rate is 0, with over 60% of judges presiding over sentencing proceedings that never resulted in a death penalty. Nonetheless, there are some judges that have consistently higher rates, with approximately 15% falling in the 0.01 to 0.11 category, 11% falling in the 0.11 to 0.22 category, and 6% falling in the 0.22 to 0.33 category. The rates at which sentencing results in life sentences are considerably more disperse, with the distribution in Figure 3 resembling the distribution of conviction rates in Figure 1. Again, there appear to be two peaks, one at zero and one in the 0.11 to 0.33 range, with a fair degree of dispersion around the more central peak.

The three figures reveal considerable visual dispersion in the rates of conviction, sentences to death, and sentences to life, when murder trials are grouped by the judges hearing the cases. What is left to be seen is whether this dispersion is a statistically significant departure from the case of equal rates across all judges. We now turn to the results from such empirical test.

III. EMPIRICAL RESULTS

A. RESULTS FROM THE UNADJUSTED ONE–WAY ANALYSIS–OF–VARIANCE

Table 1 presents the results from an unadjusted analysis–of–variance for our three trial and sentencing outcomes. Panel A presents results for variation in a dummy variable indicating a guilty verdict, Panel B presents results for variation in a dummy variable indicating a death sentence, while Panel C presents results for variation in a dummy variable indicating that the defendant received a life sentence. Each panel reports the standard ANOVA results: the first column presents the degrees of freedom for the between–judge, within–judge, and total variation calculations, the second column presents the between, within and total sum of squares, the third column presents estimates of the MSB and the MSW, while the fourth column presents the ratio, MSB/MSW.

Recall that under the null hypothesis of no judge effects, this ratio should be equal to one. Under the alternative hypothesis, this ratio should exceed one. For relatively large samples, this ratio has an F–distribution, which thus allows us to test whether the departure from one is statistically significant. The final column presents the p–value (the likelihood of observing a ratio at least as large as the test statistics under the null hypothesis) from such a test for each panel. Specifically, the figure provides the area under the tail of the F–distribution to the right of the constructed ratio. Small values indicate that deviation from one at least as large as that which is observed is relatively unlikely.

Beginning with the results for conviction rates, the between judge variation is large relative to the within–judge variation, with the MSB nearly double the MSW. The probability of observing such an outcome when the no judge effects null hypothesis is true is very unlikely (the p–value is 0.0001). Hence, the results in Panel A strongly indicate an effect of judicial discretion on the likelihood of conviction.

Similarly, the results for the variable indicating that the convicted defendant received a death sentence indicate that the between–judge variation in death sentence rates is too large relative to the amount of within–judge variation in this variable to be consistent with the null hypothesis of no judge effects. The MSB is approximately 30% greater than the MSW. Moreover, the departure of the ratio of these two variables from one is statistically significant at the

5% level of confidence (the p–value is 0.0326). Hence, these results also indicate a significant role of discretion in determining who received the death sentence.

Table 1

Initial Analysis of Variance of Trial and Sentencing Outcomes: Do Judges Matter?

Panel A: Variation in Trials that Result in Convictions

	Degrees of Freedom	Sum of Squares	Mean Square	F-Statistic	Prob (F > Test Statistic)
Between Judge	138	64.52	0.47	2.01	0.0001
Within Judge	2,492	578.41	0.23	-	-
Total	2,630	642.92	-	-	-

Panel B: Variation in Convictions that Result in Death Sentences

	Degrees of Freedom	Sum of Squares	Mean Square	F-Statistic	Prob (F > Test Statistic)
Between Judge	96	9.52	0.10	1.31	0.0326
Within Judge	754	57.22	0.08	-	-
Total	850	66.74	-	-	-

Panel C: Variation in Conviction that Result in Life Sentences

	Degrees of Freedom	Sum of Squares	Mean Square	F-Statistic	Prob (F > Test Statistic)
Between Judge	96	22.93	0.24	1.20	0.1021
Within Judge	754	149.82	0.20	-	-
Total	850	172.75	-	-	-

The weakest evidence of an effect of judicial discretion is observed in Panel C. Again the between–judge variation is large relative to the within–judge variation, with a ratio of the mean squares equal to 1.2. However, the F–test indicates that values at least as large as that which we observe would occur at least 10% of the time under the null hypothesis. Hence, there is some evidence that discretion is important for this outcome, yet the observed result is only weakly significant.

An alternative way of gauging the importance of judicial discretion in determining these outcomes is to analyze the proportion of variation in these outcomes that can be attributed to between–judge variation. This figure can be calculated by dividing the sum of squares between by the total sum of squares (both figures are presented in the second columns of the individual panels). For conviction rates, approximately 10% of the overall variation in this outcome is attributable to between–judge variation, while for death sentence and life sentence rates, approximately 14% and 13%, respectively, is attributable to between–judge variation in these outcomes.

The ANOVA tests presented in Table 1 do not account for possible variation in the circumstances of murder incidents that may explain inter–judge variation in conviction rates and sentencing outcomes. For instance, it may be that certain judges, by chance, received trials that were clear convictions. To account for this possibility, we extracted several additional variables from the Northwestern database that more fully described the circumstances of each incident and that may be related to the probability of being convicted and the severity of the sentence. Table 2 presents the means of these additional explanatory variables for the analysis sample stratified by the values of the three dependent variables. Specifically, the table presents the means of these variables for trials resulting in guilty verdicts and not guilty verdicts, for convictions with death sentences and convictions without death sentences, and for convictions with life sentences and convictions without life sentences.

Table 2

Mean Values of Explanatory Values by Trial and Sentencing Outcomes

	Guilty Verdict	No Guilty Verdict	Death Sen-tence	No Death Sen-tence	Life Sen-tence	No Life Sen-tence
Black Defendant	0.25	0.17	0.23	0.25	0.26	0.24
Black Victim	0.26	0.25	0.16	0.28	0.24	0.28
Male Defendant	0.94	0.86	1.00	0.93	0.98	0.92
Male Victim	0.73	0.84	0.72	0.70	0.71	0.70
Victim 0 to 5 years	0.14	0.07	0.19	0.15	0.09	0.18
Victim 6 to 10 years	0.01	0.01	0.00	0.01	0.01	0.00
Victim 11 to 20 years	0.07	0.07	0.05	0.07	0.07	0.06
Victim 21 to 40 years	0.58	0.63	0.44	0.57	0.54	0.57
Victim older than 40	0.20	0.22	0.32	0.20	0.29	0.19
Victim Police	0.06	0.04	0.23	0.06	0.12	0.06
Defendant Police	0.01	0.02	0.00	0.01	0.01	0.01
Victim/ Defendant Related	0.19	0.14	0.14	0.20	0.14	0.21
Multiple Victims	0.05	0.03	0.13	0.06	0.10	0.05
Multiple De-fendants	0.28	0.25	0.56	0.29	0.40	0.28
Multiple Arrests	0.27	0.23	0.54	0.26	0.38	0.25

Year of the Murder						
1890-1900	0.12	0.04	0.11	0.14	0.09	0.16
1901-1910	0.26	0.15	0.28	0.22	0.22	0.23
1911-1920	0.22	0.25	0.23	0.22	0.27	0.21
1921-1930	0.41	0.57	0.39	0.41	0.42	0.41
N	755	1,017	57	525	162	420

Samples restricted to observations with complete information on all explanatory variables.

The additional covariates include indicator variables for whether the defendant is African–American, whether the victim is African–American, and for the gender of the defendant and victim, several indicator variables for the age of the victim, variables indicating whether the victim or defendant is a police officer, and a dummy variable indicating whether the victim and defendant are related. We also constructed dummy variables indicating whether there are multiple victims, multiple defendants, and multiple arrests. For the descriptive purposes of Table 2, we present the year distributions for the sample using ten–year intervals. In the regression models that follow we control for a complete set of year dummy variables for the period from 1890 to 1930.

There are several interesting patterns evident in Table 2. For example, those trials that end in a guilty verdict are disproportionately comprised of cases where the defendant was African–American. There also appears to be a relationship between the murder victim's race and the likelihood that the convicted defendant receives the death sentence (with murders of black victims considerably less likely to result in a death sentence). Other interesting patterns include the large difference in the proportion of murders where the victim is a police officer between convicted murderers receiving the death sentence and convicted murderers that do not, and the apparent effect of a relationship between the defendant and victim on the likelihood of receiving either a death or life sentence. To the extent that some of these factors differ among judges, such relationships may explain the significant judge effects evident in the unadjusted analysis of variance presented in Table 1.

Table 3
Judge Effects on the Likelihood of a Guilty Verdict Adjusting for Observed Explanatory Variables

	(1)	(2)	(3)
Black Defendant	-	0.215 (0.054)	0.213 (0.054)
Black Victim	-	0.054 (0.039)	0.046 (0.039)
Black Defendant*Black Victim	-	-0.119 (0.072)	-0.128 (0.072)
Male Defendant	-	0.271 (0.039)	0.272 (0.039)
Male Victim	-	-0.158 (0.030)	-0.156 (0.030)
Victim 0 to 5 years	-	0.011 (0.051)	-0.084 (0.056)
Victim 6 to 10 years	-	-0.034 (0.121)	0.006 (0.120)
Victim 11 to 20 years	-	0.032 (0.049)	0.015 (0.049)
Victim 21 to 40 years	-	0.007 (0.029)	0.005 (0.029)
Victim Police Officer	-	0.106 (0.057)	0.137 (0.058)
Defendant Police Officer	-	-0.100 (0.095)	-0.057 (0.095)
Victim/Defendant Related	-	0.095 (0.034)	0.100 (0.035)
Multiple Victims	-	0.143 (0.060)	0.141 (0.061)

Multiple De-fendants	-	0.001 (0.067)	0.001 (0.067)
Multiple Ar-rests	-	0.085 (0.067)	0.077 (0.068)
Judge Dummies	Yes	Yes	Yes
Year Dummies	No	No	Yes
F-Statistic[a] (P-value)	1.882 (0.0001)	1.827 (0.0001)	1.226 (0.049)
R^2	0.128	0.193	0.223
N	1,772	1,772	1,772

Standard errors are in parentheses. All regression include a constant term.
a. F-statistic and P-Value for tests of the joint significance of the judge dummy variables.

B. TESTING FOR JUDGE EFFECTS HOLDING CONSTANT OBSERVABLE ASPECTS OF THE INCIDENTS

Tables 3 and 4 present regression estimation results that account for the influence of the variables listed in Table 1. Table 3 presents three regression specifications where the dependent variable is an indicator variable equal to one when a trial results in a guilty verdict and zero otherwise. Note, the size of the sample used to estimate the regressions is somewhat smaller than the size of the sample used in the unadjusted ANOVA in Table 1 (for the analysis of guilty verdicts, 1772 observations versus 2631 observations in Table 1). The reduction in sample size is due to the additional restriction that there be complete information for all of the explanatory variables listed in Table 2. To facilitate comparison with the unadjusted ANOVA results in Table 1, the first regression in column (1) includes an intercept and K–1 (where K is the number of judges) judge dummy variables only. The F–statistic from the test of the significance of the regression is equivalent to the F–statistic from the ANOVA tables presented above. This F–statistic along with p–value of the test is presented at the bottom of the table. Hence, the first regression provides an unadjusted test for judge effects for the restricted sample that is comparable to the results in Table 1 for the larger sample. The second regression in Table 3 adds the control variables listed in Table

2 while the third specification adds year dummy variables to the specification in column (2). Again, the F–statistic at the bottom of the table is the test statistic from a test of the joint significance of the K–1 judge dummy variables and presents the regression–adjusted equivalent to the unadjusted ANOVA test for judge effects presented above.

The results in the first column of Table 3 basically confirm the findings in Table 1. The between–judge variation in conviction rates is nearly 80% greater than the variance estimate using the within–judge variation. The p–value on the test of the significance of this departure is 0.0001. Hence, the concordance between these results and those in Table 1 indicates that the additional sample restriction is not affecting the basic pattern. Adding the controls to the specification in column (2) does not appreciably affect the main result. The F–statistic from the test of the joint significance of the judge dummies is still considerably larger than 1 and statistically significant at the 0.0001 level of confidence. Adding year dummies to the regression specification does indeed reduce the amount of residual between–judge variation in conviction rates, as is evidence by the reduction in the F–statistic. Nonetheless, the judge dummy variables are still statistically significant at the 5% level of confidence. Hence, the results in Table 3 provide strong confirmation of the results presented in Panel A of Table 1.

In addition to the evidence concerning judge effects, there are some very stark patterns evident in the partial effects of the explanatory variables on the likelihood of conviction. There is an enormous effect of the race of the defendant on the likelihood of a guilty verdict. In both specifications (2) and (3), black defendants are 21.5 percentage points more likely to be convicted of murder than are white defendants. These estimates are both significant at the 1% level of confidence. The effect of the race of the defendant is mitigated somewhat when the victim is black. The interaction term between black victim and black defendant is negative and marginally significant in both regressions. The magnitude of the interaction term indicates that relative to murder cases where the victim and defendant are both white, cases where the victim and defendant are both black are approximately 10 percentage points more likely to result in a conviction while cases where the defendant is black and victim is white are approximately 21.5 percentage points more likely to result in a conviction. There is also a large positive effect of the defendant being male on the likelihood of conviction and a large negative effect of the victim being male. Finally, there are statistically significant

positive effects on the likelihood of conviction when the victim is a police officer and when the incident involves multiple victims.

Table 4 presents comparable results for the sentencing outcomes. The first three regressions present results where the dependent variable is equal to one if the convicted defendant was sentenced to death while the next three regressions present results where the dependent variable is equal to one if the convicted defendant received a life sentence. Starting with the death sentence results, the estimation results in the first specification confirm the unadjusted findings in Table 1. The F–statistic and p–value at the bottom of the table indicate that the judge effects for the restricted sample are statistically significant at the 2% level of confidence. Adding the variables to the specification in column (2) weakens the significance of the judge effects with a new p–value of 0.077. However, adding year dummy variables to the regression yields a test statistic for the significance of the judge effects that is larger and statistically significant at the 3% level of confidence. Hence, the significant judge effects on the likelihood that a convicted offender received the death sentence survive the addition of controls for the circumstances of the incident.

For the dependent variable indicating a life sentence, there is no evidence of significant judge effects. The test of the significance of the judge dummies in the base case with no controls (column (4)) fails to reject the hypothesis of no judge effects at a reasonable level of significance (the p–value is 0.179). Recall that the ANOVA test in Table 1 using the larger sample was just barely significant. Adding control variables in columns (2) and (3) completely eliminates all evidence of significant judge effects for these outcomes (as is evident by the F–statistics that are essentially equal to one). Hence, for this final outcome, there is little evidence of a statistically significant role of judicial discretion in sentencing.

Unlike the results in the regression models for the likelihood of a conviction, there are few independently significant effects among the explanatory variables included in the regression specifications. One variable which exerts a consistent positive and statistically significant effect for both dependent variables is the dummy variable indicating that the victim is a police officer. In the death sentence models, murderers of police officers are 10 to 15 percentage points more likely to receive the death sentence and 8 to 15 percentage points more likely to receive life sentences.

Table 4

Judge Effects on the Likelihood of a Receiving a Death or Life Sentence Conditional on Being Convicted

	Dependent Variable=Death Sentence			Dependent Variable=Life Sentence		
	(1)	(2)	(3)	(4)	(5)	(6)
Black Defendant	-	0.052 (0.051)	0.052 (0.051)	-	0.057 (0.079)	0.051 (0.082)
Black Victim	-	-0.007 (0.045)	0.001 (0.045)	-	0.032 (0.069)	0.038 (0.073)
Black Defendant*Black Victim	-	-0.062 (0.073)	-0.067 (0.073)	-	-0.074 (0.113)	-0.057 (0.117)
Male Defendant	-	0.091 (0.052)	0.069 (0.053)	-	0.074 (0.081)	0.137 (0.085)
Male Victim	-	-0.031 (0.032)	-0.017 (0.032)	-	-0.066 (0.050)	-0.073 (0.052)
Victim 0 to 5 years	-	0.032 (0.053)	-0.016 (0.057)	-	-0.147 (0.082)	-0.111 (0.092)
Victim 6 to 10 years	-	-0.103 (0.150)	-0.092 (0.148)	-	0.175 (0.234)	0.268 (0.238)
Victim 11 to 20 years	-	-0.012 (0.057)	-0.027 (0.057)	-	-0.039 (0.089)	-0.013 (0.092)
Victim 21 to 40 years	-	-0.032 (0.033)	-0.037 (0.033)	-	-0.102 (0.052)	-0.099 (0.053)
Victim Police Officer	-	0.101 (0.049)	0.153 (0.050)	-	0.150 (0.077)	0.081 (0.081)
Defendant Police Officer	-	-0.111 (0.141)	0.027 (0.147)	-	0.188 (0.219)	0.107 (0.236)
Victim/ Defendant Related	-	0.042 (0.037)	0.065 (0.037)	-	-0.082 (0.058)	-0.084 (0.060)
Multiple Victims	-	0.054 (0.055)	0.041 (0.055)	-	0.133 (0.085)	0.171 (0.089)
Multiple Defendants	-	0.008 (0.069)	0.053 (0.072)	-	-0.022 (0.108)	-0.043 (0.114)

Multiple Arrests	-	0.097 (0.070)	0.026 (0.072)	-	0.104 (0.108)	0.161 (0.116)
Judge Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year Dummies	No	No	Yes	No	No	Yes
F-Statistic [a] (P-value)	1.393 (0.0148)	1.244 (0.077)	1.350 (0.026)	1.150 (0.179)	1.001 (0.4812)	1.027 (0.421)
R^2	0.208	0.259	0.365	0.178	0.217	0.284
N	582	582	582	582	582	582

Standard errors are in parentheses. All regression include a constant term.
a. F-statistic and P-Value for tests of the joint significance of the judge dummy variables.

To summarize the results, we find strong unambiguous evidence that the judge trying the case is a statistically significant predictor of the likelihood of a conviction and of the likelihood of receiving a death sentence conditional on a conviction. These patterns are evident in the unadjusted data as well as in models that control for observable aspects of the murder incident. We find little evidence that judicial discretion plays a role in the likelihood that convicted murderers received a life sentence.

C. TESTING FOR AN IMPACT OF ELECTION YEARS

As outlined in the methodology section, our second empirical strategy tests for an impact of the homicide trial occurring during an election on the three trial and sentencing outcomes analyzed in this study. Recall that for this exercise, we further restrict the sample to those homicide trials that were heard by Circuit court judges.

Table 5 presents the results of these model estimates. For each outcome, the table presents the regression coefficients on the election year dummy from linear regressions of the outcome indicator on the election year variable. Concerning other covariates, three specifications are estimated for each outcome. The first specification controls for the election year dummy only and hence provides a base estimate of the difference in means between election and non–election years for conviction rates, death sentence rates, and life sentence rates. The second specification adds all of the control variables listed in Table 2 to the first specification (with the exception of the year indicators). The final specification adds a complete set of judge dummies to the

second specification. The election year effects by outcome are organized by column, while each row corresponds to one of the three specifications of the right hand side of the regression models.

Table 5

Estimates of the Effect of the Offense Occurring During a Circuit Court Election Year on the Likelihood of the Trial Outcomes

Guilty Verdict	Death Sentence	Life Sentence
-0.011 (0.036)	0.097 (0.038)**	0.002 (0.060)
0.031 (0.044)	0.139 (0.051)***	-0.063 (0.073)
0.041 (0.046)	0.145 (0.057) **	-0.106 (0.084)

Standard errors are in parentheses. The coefficients presented are the coefficients from a regression of the trial outcomes on a dummy variable indicating that the offense occurred during a Circuit-court election year. Specification (1) regresses the outcome on the election year dummy only. Specification (2) adds all of the explanatory variables (with the exception of the year variables) listed in Table 2 to the model specification. Specification (3) adds a complete set of judge dummy variables to the model in specification (2).
** Statistically significant at the 5% level of confidence.
***Statistically significant at the 1% level of confidence.

The results for the guilty verdict outcome indicate that there is no statistically significant difference in the proportion of trials resulting in a guilty verdict between election and non–election years. This pattern is consistent across all three specifications.

For the death sentence outcome, on the other hand, the proportion of murders resulting in a death sentence (conditional on a conviction) is larger and statistically distinguishable from the comparable proportion in non–election years. In the model omitting other covariates, this difference is approximately 10 percentage points and is significant at the 5% level of confidence. Adding the controls in specifications (2) and (3) actually increases the point estimate to between 14 and 14.5 percentage points. Both estimates are also statistically significant.

Finally, there is no evidence that the propensity to give out life sentences increases in election years. The point estimate of the election year effect is not stable across specifications. Moreover, none of the point estimates are statistically significant.[41]

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[41] Regressions with mayoral and prosecutorial election years revealed no significant patterns with convictions and election years.

IV. CONCLUSION

Using historical data on murders in Chicago from 1870 to 1930, this Article examined the impact of judge–specific effects on the likelihood that a defendant would be found guilty of murder and the likelihood of the defendant receiving various sentences. We observe significant judge–specific effects for both convictions and sentencing outcomes. Additionally, we observe a strong relationship between election years for judges and the likelihood that a defendant will receive a death sentence. Defendants in our sample were approximately 15% more likely to be sentenced to death when the sentence was issued during the trial judge's election year. The size and significance of correlation between judicial election years and death sentences is in some ways surprising because Chicago judges generally did not have discretion to order death sentences. That is, if a defendant did not waive his or her right to a jury trial, then the jury decided both the guilty verdict and the sentence.[42] Of course, even in a jury trial, judges were still able to influence the juries' decisions in various ways, particularly through jury instructions. During the period of this study, the electoral incentives for trial judges might have encouraged them to facilitate a death sentence, only to later privately request clemency from the governor. Through private communications with the governor, trial judges were often the most influential voice advocating or challenging the worthiness of the defendant's request for clemency.[43]

A more contemporaneous examination of the connection between judicial elections and capital sentencing would constitute a valuable line of future research. Particularly useful would be an analysis that compares a jurisdiction where juries decide on death sentences with a jurisdiction where judges have full discretion to im-

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[42] *See* Act of Mar. 5, 1867, 1867 Ill. Laws 90, 90 (establishes law according to which juries in capital cases would decide both guilt and punishment). Around the beginning of the data set used in this analysis, many states, including Illinois, departed from laws requiring a mandatory death penalty for defendants found guilty of certain offenses to a system where the jury was given the discretion to impose death. Juries were given discretion in many states because lawmakers feared juries would not convict clearly guilty defendants for whom they did not view a death sentence to be appropriate. BANNER, *supra* note 4, at 214–16.

[43] We are grateful to Stuart Banner for bringing this point to our attention. The governor may then have to pay the political price the judge avoided in guiding the jury toward a death sentence. Governors may have been more insulated from the political consequences of granting clemency than they are today simply because clemency was more frequently ordered. Governors during that period commuted death sentences to imprisonment at rates between 25% and 50%, which are much higher rates than those seen today.

pose death sentences, such as in Alabama.   In these jurisdictions, where ultimate responsibility for a life or death sentence cannot be placed on the jury, a politically–minded elected judge may be more responsive to popular opinion about capital punishment.