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 detailed historical data on murders occurring in Chicago in the late–nineteenth and early–twentieth centuries. These data include incident–level detail on various aspects of murder cases, including information 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 variance (ANOVA) of several trial and sentencing outcomes in an attempt to identify statistically whether judge–specific effects are important. 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 conditional 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 indicator 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

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

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 hypothesis 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 occurring between judges. The variation occurring within judges is defined by the judge–specific sums of squared deviations about the judge–specific means. This is given by the K equations

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

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

$$H_1\text{: MSB/MSW} > 1.$$

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 discretion 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

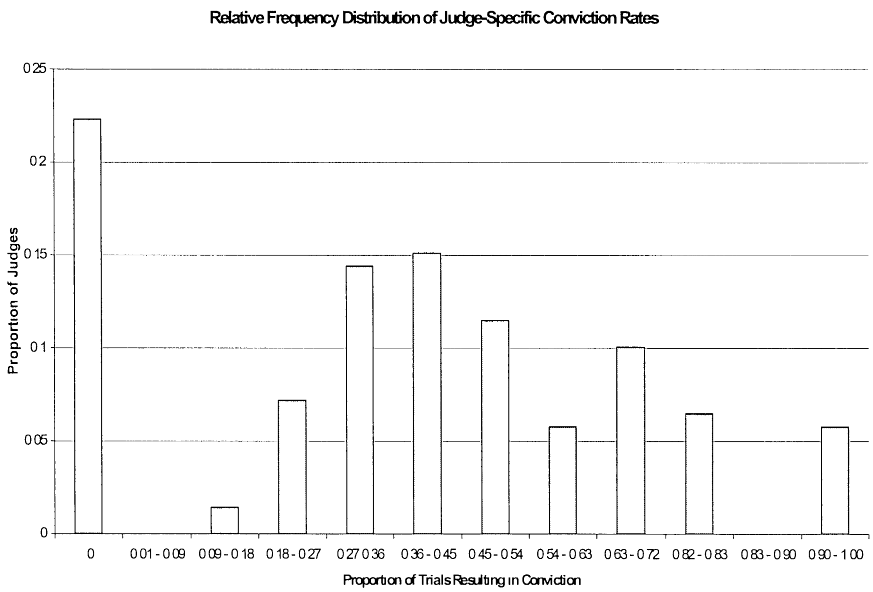


Figure 2

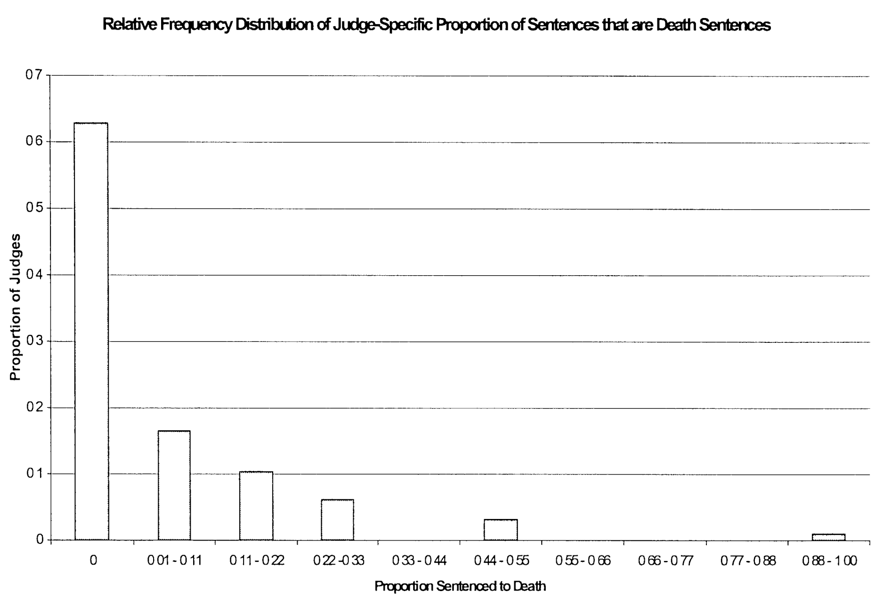
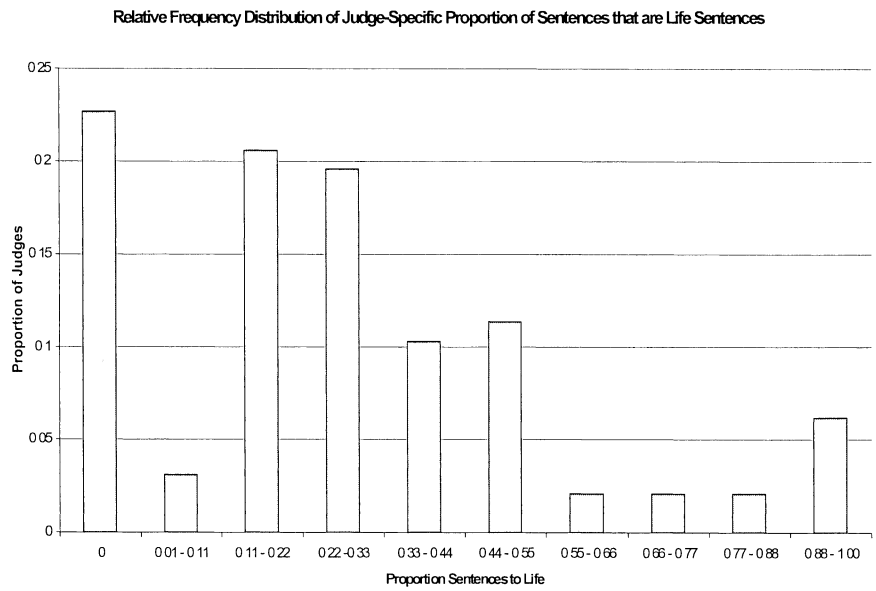


Figure 3



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 Sentence	No Death Sentence	Life Sentence	No Life Sentence
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 Defendants	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 Defendants	-	0.001 (0.067)	0.001 (0.067)
Multiple Arrests	-	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]

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