Life “With” Or “Without”? An Empirical Study of Homicide Sentencing

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Life “With” Or “Without”? An Empirical Study of Homicide Sentencing

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Abstract
The number of Americans serving sentences of life without the possibility of parole ("LWOP") has grown rapidly over the past generation and now exceeds 50,000. Yet, little empirical research has been conducted on the determinants of LWOP sentences. The dearth of research on LWOP sentencing stands in sharp contrast to the many dozens of studies that have been conducted on the determinants of death sentences—studies that have consistently found that race, gender, and other questionable factors may influence sentencing outcomes. The present study is the first to employ a similar methodology to identify both case- and county-level variables that are correlated with the imposition of discretionary LWOP sentences. More specifically, we have assessed the relationship between fifty different variables and LWOP decisions in 450 homicide cases in Wisconsin between 2001 and 2018. In our final model, we find seven variables that are correlated with sentencing outcomes. Of
particular note, we find that judge and prosecutor personal characteristics are statistically significant correlates of LWOP decisions. We also find a significantly greater likelihood that LWOP sentences will be imposed in counties that are more Republican. We conclude with a proposal for a new LWOP sentencing process that may help to ensure that this very severe sentence is reserved for the most serious crimes committed by the most dangerous defendants.

I. Introduction

The number of Americans serving sentences of life without the possibility of parole ("LWOP") has grown rapidly over the past generation and now exceeds 50,000. Yet, little empirical research has been conducted on the determinants of LWOP sentences. The dearth of research on LWOP sentencing stands in sharp contrast to the many dozens of studies that have been conducted on the determinants of death sentences—studies that have consistently found that race, gender, and other questionable factors may influence sentencing outcomes. The present study is the first to employ a similar methodology to identify both case- and county-level variables that are correlated with the imposition of discretionary LWOP sentences. More specifically, we have assessed the relationship between fifty different variables and LWOP decisions in 450 homicide cases in Wisconsin between 2001 and 2018. In our final model, we find seven variables that are correlated with sentencing outcomes. Of particular note, we find that judge and prosecutor personal characteristics are statistically significant correlates of LWOP decisions. We also find a significantly greater likelihood that LWOP sentences will be imposed in counties that are more Republican. We conclude with a proposal for a new LWOP sentencing process that may help to ensure that this very severe sentence is reserved for the most serious crimes committed by the most dangerous defendants.

"[D]eath is different," the Supreme Court has declared. The unique irrevocability of the death penalty has been invoked to justify a unique set of constitutional safeguards that are intended to ensure that death sentences are reserved for the proverbial "worst of the worst"—for "those offenders," as the Court has put it, "who commit 'a narrow category of the most serious crimes' and whose extreme culpability makes them 'the most deserving of execution.'" Yet, despite these special safeguards, a large body of empirical research consistently finds that capital sentencing decisions are affected by race and other dubious factors.

Echoing its death-is-different jurisprudence, the Supreme Court has begun to recognize that the sentence of life without the possibility of parole ("LWOP") may also be a distinctly harsh sentence that warrants special constitutional treatment. As the Court has observed:

[L]ife without parole is the second most severe penalty permitted by law. It is true that a death sentence is unique in its severity and irrevocability; yet life without parole sentences share some characteristics with death sentences that are shared by no other sentences. The State does not execute the offender sentenced to life without parole, but the sentence alters the offender's life by a forfeiture that is irrevocable. It deprives the convict of the most basic liberties without giving hope of restoration, except perhaps by executive clemency—the remote possibility of which does not mitigate the harshness of the sentence.

Despite this recognition of LWOP's harshness, the Court has thus far declined to adopt special restrictions on its use except in relation to juvenile defendants.

If, as the Supreme Court has indicated, LWOP is similar to death in its severity, then evidence that LWOP sentencing is similarly influenced by inappropriate or questionable considerations might warrant comparable efforts to minimize those influences, either as a matter of constitutional adjudication or one of statutory reform. Yet, no prior research has subjected adult LWOP sentencing to the same sort of systematic quantitative analysis that has been performed repeatedly on capital sentencing.

There are plausible reasons to suspect that LWOP sentencing would reflect influences that are similar to those that shape capital sentencing. After all, the death penalty is constitutionally limited to cases of murder, and the great majority of LWOP sentences are also imposed for that same offense. LWOP sentencing dynamics would therefore typically involve many of the same factors as capital sentencing dynamics—a tragic loss of life, bereaved family members, heightened media attention, and so forth. Yet, there may also be reasons to think that LWOP sentencing is systematically different than capital sentencing. Perhaps most importantly, while juries have a constitutionally mandated role in capital sentencing, no such role exists in LWOP sentencing—typically leaving the judge as the key decider. In contrast to a lay jury, a trained, experienced legal professional might plausibly attend more closely to the legally mandated sentencing criteria and more reliably avoid inappropriate influences. On the other hand, given the political realities facing an elected judiciary—especially in high-profile murder cases—the gap between jury and judge sentencing practices might turn out to be less than expected.

Whatever the particular dynamics of LWOP sentencing, there can be no question that the phenomenon has been of growing importance. There are more than 50,000 prisoners currently serving LWOP sentences in the United States, representing a more than four-fold increase since the early 1990s. Strikingly, LWOP was not even an option in most states in 1970, but is now authorized in all but one. The use of LWOP far outstrips use of the death penalty, which has been in long-term decline since the 1990s and is now typically imposed in fewer than 100 cases per year. At last count, 2,703 individuals sat on America’s death rows—amounting to barely 5 percent of the nation’s LWOP population.

Each LWOP sentence imposed today will carry heavy, long-term fiscal and human costs. For instance, taking into account the expenses of caring for elderly prisoners—many of whom suffer from chronic medical conditions—whole-life sentences have been estimated to cost taxpayers about $1 million on average. There are, in short, compelling reasons for judges, lawyers, and policymakers to want to get LWOP sentencing right.

Against this backdrop, we offer the first analytically rigorous quantitative study of discretionary LWOP sentencing. More specifically, we focus on LWOP sentencing in the state of Wisconsin. When an individual is convicted in Wisconsin of first-degree intentional homicide—the state's analog to the offense known as “first-degree murder” in many other jurisdictions—the judge must choose between a sentence of life without the possibility of release and life with a possibility of release. We have gathered data on all cases in which a defendant was convicted of this offense between the start of 2001 and early 2018. Using sentence imposed (i.e., "life with" vs. "life without") as our dependent variable, we have assessed the impact of 50 different independent variables, including characteristics of the defendant, victim, judge, prosecutor, defense lawyer, and county of prosecution.

In our final regression model, we find a statistically significant relationship between the sentence imposed and seven independent variables. Some of these variables are plainly appropriate considerations pertaining to offense severity and criminal history. Notably, we did not find any main or direct effects of race of defendant or race of victim in LWOP sentencing. Nonetheless, some of our statistically significant variables are troubling, particularly those relating to the backgrounds of the judge and prosecutor. Additionally, we find that the partisan political orientation of the county of prosecution also seems to affect sentencing patterns, which may raise concerns about a lack of statewide uniformity in the use of LWOP.

The article proceeds as follows. Section II summarizes the prior empirical research on both capital and noncapital sentencing. Section III details Wisconsin’s sentencing system for first-degree intentional homicide. Although Wisconsin’s system is in some respects unusual, we show it to be closely analogous to that used for first-degree murder in 10 other states, and to present significant similarities to aspects of what is found in more than a dozen others. Section IV describes our data and analytical methodology. Section V presents our results. Finally, Section VI considers implications for policy.
II. Prior Research on Determinants of Sentencing Outcomes

A large body of research produced over several decades has attempted to identify the determinants of capital and noncapital sentencing outcomes by employing the tools of multivariate regression analysis. In this section, we first provide a general overview of this area of research, and then summarize key findings from prior studies. We conclude by considering what findings might be expected in our study based on the extant research.

A Overview: Quantitative Sentencing Research

The quantitative empirical research on capital and noncapital sentencing has proceeded along two parallel tracks, with surprisingly little cross-referencing of studies from one subfield to the other. Nonetheless, the studies in both areas—capital and noncapital—tend to employ similar analytical strategies. First, a dependent variable is identified based on the sentencing outcome of interest. Depending on the study, the dependent variable would be either imposition of a death sentence (yes or no), imposition of an incarceration sentence (yes or no), or the length of an incarceration sentence. Then, several independent variables are identified, typically including variables relating to offense severity, defendant criminal history, and defendant race and gender. In addition to this standard suite of independent variables, most studies include a variety of additional variables reflecting, for instance, the particular analytical interests of the researchers and the availability of different categories of supplemental data in the relevant jurisdiction. Finally, the researchers use statistical modeling techniques to determine whether there is a significant relationship between each independent variable and the dependent variable, statistically controlling for the other independent variables. In most prior studies—and also for our purposes—"significance" is understood to mean that there is less than a 5 percent likelihood that an apparent relationship between variables is due to random variability or chance.

A central objective of much of the research has been to determine whether and, if so, to what extent sentencing outcomes are associated with certain considerations of particular concern. Broadly speaking, the factors that drive sentencing outcomes may be divided into three categories. First, there are factors that are generally recognized as appropriate. These include the defendant's criminal history and the severity of the harm that was caused or intended by the defendant. Second, on the other end of the spectrum, are factors that are generally recognized as inappropriate, including race, gender, ethnicity, and religion. Finally, there are a host of factors of questionable appropriateness. These are factors that do not relate clearly and directly to the blameworthiness of the offense or the future dangerousness of the defendant, but that might nonetheless be rationally justified as considerations at sentencing. Reasonable minds can and do differ as to their appropriateness. For instance, one much discussed example would be mode of conviction. Defendants who are convicted at trial normally receive longer sentences than otherwise similar defendants who plead guilty. Mode of conviction has no strong, clear relationship to offense severity or defendant dangerousness, but might arguably be appropriate to take into account at sentencing so as to encourage guilty pleas and enhance system efficiency. On the other hand, the practice also has its critics, who argue, among other things, that excessive "trial penalties" can induce even innocent defendants to plead guilty. In any event, while empirical research alone cannot resolve the normative questions about how much weight, if any, should be given to such questionable sentencing factors, such research can helpfully inform the debate by uncovering the realities of current practice.

One final aspect of the existing knowledge base bears emphasis: the great majority of the existing studies are single-state studies. Although such studies are able to estimate the impact of county-level variation within the state, for example, variation as to county crime rates or voting patterns, they are unable to assess the impact of state-level variation, for example, variation in sentencing laws or judicial selection methods. Moreover, the research is concentrated in a small number of states, based largely on the availability of relevant data in those states, while many other states have been the subject of few, if any, high-quality, published empirical studies.
Great care must be taken when attempting to generalize from single-state studies. While the total volume of empirical sentencing research is large, there remains a great need for replication in the many states that have not been subject to careful study in this area in recent years—including, we may note, Wisconsin.

B Capital Sentencing

Many researchers have explored the impact of defendant demographics—particularly gender, race, and age—on decisions to seek or impose the death penalty in cases of murder. The most well-established finding relates to gender, specifically, that female defendants are less likely to receive the death penalty than male defendants in cases that otherwise appear similar. For instance, a study of the outcomes in 205 death-eligible cases in Connecticut found that the second-most important factor (behind only place of prosecution) was whether the defendant was a woman, which reduced the likelihood of capital punishment by more than 15 percentage points. Likewise, a study of 5,320 homicide cases in Ohio found that male defendants were about three times more likely to receive the death penalty than female defendants.

There is less evidence to support the view that defendant race, in and of itself, affects the death penalty once accounting for other variables. For instance, out of 36 studies completed in the late 1990s and early 2000s, only four found that defendant race had an impact on outcomes after controlling for other variables.

Research also indicates that defendant age may sometimes affect capital sentencing. Age has not been studied as extensively as race and gender, but a few studies do find that younger defendants are less likely to be sentenced to death than are older defendants. Indeed, one study of North Carolina found that each additional year of the defendant's age increased the odds of a death sentence by about 3 percent on average. This finding suggests that younger defendants may be viewed as less culpable or as more amenable to rehabilitation than older defendants.

There seems even stronger evidence that victim demographics matter. As to race, many studies find that defendants are more likely to receive the death penalty when the victim is white in comparison with other racial categories. Likewise, much research indicates that capital punishment is more frequently imposed when the victim is female. Some research also suggests that the death penalty may be more likely when the victim is young, although this finding is not as well-established in the literature.

Researchers consistently find that place of prosecution—typically assessed at the county level—can matter a great deal in capital murder cases. For instance, in Connecticut, one researcher using a number of different regression models found that "the single most important influence from 1973 to 2007 explaining whether a death-eligible defendant would be sentenced to death was whether the crime occurred in Waterbury." Indeed, while only 2.6 percent of the death-eligible cases outside Waterbury resulted in a death sentence, the comparable figure in Waterbury was more than an order of magnitude higher at 33.3 percent.

While the strength of the "Waterbury effect" in Connecticut may be exceptional, studies in other states have revealed geographic disparities to be a "pervasive" feature in the administration of the death penalty. Researchers have attempted to unpack these disparities and identify the county-level political and demographic variables that are especially associated with higher death-sentencing rates. In particular, some studies find a significantly higher likelihood of a death sentence in counties that are more rural, more politically conservative, and less racially diverse. Such findings, however, are far from uniform.

A few researchers have assessed the impact of electoral pressures on death sentences. For instance, in Nebraska, researchers found no relationship between the imminence of a district attorney's reelection date and the likelihood that a death sentence would be sought. By contrast, in a study of 26 state supreme courts, a significant relationship was found between the imminence of a justice's reelection date and the likelihood that...
the justice would vote to affirm a death sentence. The researchers also found that Republican justices were significantly more likely to affirm.

Some researchers have also explored attorney variables. For instance, in the Nebraska study, a correlation was found between the likelihood of a death sentence and the prosecutor's number of prior capital trials. By contrast, the researchers found no significant relationship between death sentences and the experience level of the defense lawyer. However, in a study of Harris County (Houston), Texas, a significant relationship was found between the likelihood of a death sentence and whether the defense lawyer was court-appointed, as opposed to privately retained.

As we will see in the next section, much of the research on noncapital sentencing attempts to assess the impact of offense severity on sentencing outcomes. In the capital context, it is more difficult to identify and analyze appropriate variables that differentiate cases by offense severity, since all the cases under review involve what is often seen as the most serious offense—first-degree murder or the like. A variety of offense variables have been found in some studies to have a statistically significant relationship with the death penalty, but findings in this area tend not to be replicated with consistency across jurisdictions and time periods. By contrast, several studies do find a significant link between sentencing and a defendant's criminal history. This relationship is especially pronounced when the defendant has prior convictions for violence.

In sum, the research evidence strongly suggests that capital sentencing has been influenced by a number of factors that are often regarded as inappropriate, including defendant gender, victim race, and victim gender. Additionally, the research also consistently points to a significant role for another factor that is of at least questionable appropriateness: county of prosecution. Finally, a relatively small number of studies point to some possible impact of electoral pressures and lawyer variables.

C Noncapital Sentencing

A large, parallel body of research has identified a wide range of factors that are associated with sentencing outcomes in noncapital cases. In many respects, the findings echo those that we have considered in the area of capital sentencing, although the case sets differ in some important ways. For instance, as already noted, the noncapital cases encompass a much wider range of offense severity, including all manner of violent, property, drug, and other offenses. Additionally, victim variables are normally not considered in the noncapital studies, both because many of the crimes (e.g., drug crimes) do not have victims in a conventional sense and because, even in cases that do involve clear victimization, victim data tend not to be as readily available in relation to thefts, robberies, sexual assaults, and so forth as in relation to homicides. Finally, it should be recalled that while capital sentencing decisions are normally made by juries, noncapital sentencing is typically in the hands of judges, which may increase the relevance of judge-level variables, such as political ideology or years of experience on the bench.

As in the death-penalty area, many empirical studies of sentencing attempt to isolate the unique contributions of defendant race, gender, and age in explaining variation across sentencing. Indeed, with respect to race, one recent review of the research noted: "No issue has received more attention in the scholarly literature on sentencing than whether nonwhite defendants are treated more harshly than similarly situated whites." The authors summarized the current state of the research as follows:

[A]fter accounting for offense severity and criminal history, there is often a small but significant punishment gap between black and white defendants in the decision to imprison, to the detriment of blacks... We can draw no firm conclusions about the sentencing of Native Americans. Evidence from federal courts indicates that Asian defendants do not receive harsher sentences than whites and for some offense types may receive more lenient sentences. [Additionally], sentencing disparities are often
conditional on other defendant attributes. Punishments are often most severe for young, minority men.55

The authors further observe that black-white disparities are most well-established with respect to the "in/out" decision (i.e., whether to sentence a defendant to incarceration vs, community supervision), but are less consistently found with respect to the length of prison sentences.56 Finally, they note evidence that the magnitude of racial disparities in sentencing has declined over time.57

Meanwhile, with respect to gender, "a persistent finding ... is that female defendants are treated more leniently than male defendants."58 As with race, gender seems to play a more important role as to the in/out decision than as to sentence length.59

As to age, the available research presents a paradox. Although most studies find that age correlates with sentencing outcomes, there is no consistency as to the direction of age effects—some studies find that younger defendants are the ones who are disadvantaged, while others, as in the capital context, find that older defendants tend to receive harsher outcomes.60

As noted above, victim variables have been studied much less often in the noncapital than the capital context, but a few studies do suggest that such variables may play a significant role in the sentencing of noncapital victimizing crimes. For instance, research in Texas has found longer sentences for defendants who commit violent crimes against women, in comparison with those who violently victimize men.61 Other research finds longer sentences on average in cases involving vulnerable victims, such as the very young or the disabled,62 and shorter sentences on average in cases in which the victim had a criminal record.63 However, not all studies have found victim variables to be significant.64

As in capital cases, research shows that place of prosecution can have an important impact on noncapital sentencing outcomes. A 2012 review of the literature concluded that "substantial evidence exists that what kind of sentence one gets, and the factors that predict why one gets it, in significant part depend on where one is sentenced."65 More recent studies have done nothing to cast this conclusion into doubt.66 The research points to a variety of place-based considerations that may affect sentencing decisions, but—as with the research on capital sentencing—few are consistently found to be statistically significant across studies.67

By contrast, researchers do consistently find offense type, offense severity, and defendant criminal history to be important factors in sentencing. Indeed, it has been asserted that "there is near unanimous agreement that offense severity and prior record are the primary determinants of sentencing outcomes."68

Several studies have examined whether sentencing outcomes are affected by type of defense counsel, that is, privately paid versus court-appointed. However, results have been mixed, with no clear advantage established for one type of lawyer over the other.69

Some studies also indicate that the prosecutor may matter for sentencing outcomes. Indeed, one study in the federal system suggests that the identity of the prosecutor may have a greater impact on cases than the identity of the judge.70 Controlling for various case and defendant variables, the researchers found that sentences could vary by about a full year based on the prosecutor.71 As for electoral considerations, one study in Florida found a statistically significant increase in prison admissions in years in which a local incumbent state’s attorney was up for reelection—but, interestingly, a decrease in the average length of prison terms for white defendants.72

More of the sentencing research has focused on judges than on defense lawyers or prosecutors. Some studies suggest that somewhere in the range of 6 to 14 percent of the variation in sentencing outcomes may be attributable to the judge.73 On the question of whether minority judges sentence more leniently—either generally as to all defendants or specifically as to defendants of their own racial or ethnic group—results have
been inconsistent.\textsuperscript{74} As to gender, there is some evidence of a "chivalry effect," that is, male judges sentencing female defendants more leniently than female judges do.\textsuperscript{75} When it comes to electoral pressures, some studies find that judges tend to impose longer sentences as their reelection date looms.\textsuperscript{76} For instance, one recent study in Washington State concluded that "sentencing of serious offenses becomes more severe as elections approach: sentence lengths increase by about 10 percent between the beginning and end of a judge's political cycle."\textsuperscript{77}

Another variable that has been heavily studied is the mode of conviction: trial versus guilty plea. Researchers regularly find that defendants who are convicted after a trial receive longer sentences than otherwise similar defendants who plead guilty. One recent review of the literature concluded: "The trial penalty is consistently found across jurisdictions, offense types, and over time, and it is among the most robust findings in the empirical sentencing literature."\textsuperscript{78} Studies typically find that defendants who go to trial face a two- to six-fold increase in the odds of imprisonment and sentence lengths that are 15 to 60 percent longer.\textsuperscript{79}

In sum, the noncapital research, like the capital research, raises concerns about race and gender influences at sentencing. Notably, however, the evidence of such influences is much stronger for in/out decisions than sentence-length decisions. This suggests that in relatively low-level, routine cases, sentencing judges may be more likely to fall back on race and gender stereotypes, but that in more serious, higher-stakes cases, in which probation is not a viable sentencing option, judges may rely less on stereotypes and attend in a more carefully individualized way to offense severity and defendant risk. It is thus uncertain whether we would find clear, convincing race or gender effects in our data given our focus on murder cases.

The noncapital research also troublingly points to the influence of judge and prosecutor identity. Since there seems no principled reason why one defendant should receive a longer sentence than another based simply on the judge or prosecutor assigned to the case, we believe that judge and prosecutor variables should be regarded as inappropriate sentencing factors.

Finally, the noncapital research also consistently highlights the importance of two notable \textit{questionable} variables: place of prosecution and mode of conviction.

D Expectations for Discretionary LWOP Sentencing in Noncapital Murder Cases

Our focus in this article is on discretionary judicial LWOP sentencing in noncapital murder cases. By "discretionary," we mean that LWOP is not a mandatory minimum—that is, the sentencing judge has the authority to impose a lesser sentence.

On the one hand, it might be expected that sentencing practices in our cases would mirror capital sentencing practices. After all, our cases seem analogous to capital cases insofar as they involve the most serious, high-profile type of crime, murder, and a potential sentence that is also extremely severe—one that, as noted earlier, the Supreme Court has found to be closely comparable to the death penalty itself.\textsuperscript{80} In light of the capital sentencing research discussed above, the capital-LWOP analogy would suggest that LWOP sentencing might be marked by victim race and gender disparities, defendant gender disparities, and geographic disparities.

But, on the other hand, it may be that noncapital sentencing makes for a closer comparison. Most importantly, it could be that sentencing practices in the capital area are tied to the unique role of the jury in administering the death penalty. Since we focus here on \textit{judicial} LWOP sentencing, we might reasonably expect our cases to be more reflective of the prior research on the predictors of the severity of noncapital prison sentences, as to which, for instance, judge and prosecutor factors often seem to play an important role.
III. Wisconsin’s Sentencing System for First-Degree Intentional Homicide: Content and Context

There is no crime formally labeled "murder" in the Wisconsin criminal code, but much of the criminal conduct that would be classified as murder in other jurisdictions is encompassed within Wisconsin's offenses of first-degree intentional homicide and first-degree reckless homicide. These offenses roughly correspond to first- and second-degree murder, respectively, in the traditional classification system. We focus in this article on first-degree intentional homicide, which is treated as a Class A felony. In Wisconsin, conviction of a Class A felony results in a mandatory life term, but the sentencing judge has discretion to determine whether the defendant will be eligible to petition for release some time before death. The specifics of the system changed a few times in the 1980s and 1990s, but, for all killings that occurred on or after December 31, 1999—which account for 96 percent of our dataset—the sentencing judge has three options: (1) life without any possibility to petition for release; (2) life with the possibility to petition the sentencing court for release after serving 20 years in prison; or (3) life with the possibility to petition the sentencing court for release after serving some amount of time greater than 20 years.

In comparison to what is found in other states, the Wisconsin system is unusual in some respects, but the unique features tend to be more in the nature of semantics than substance. Indeed, as more and more states abolish the death penalty, it is likely becoming increasingly common nationally for the LWOP-or-less choice to be presented when murder cases advance to sentencing.

In all, 22 states and the District of Columbia have eliminated capital punishment. Out of those 23 jurisdictions, nine currently require a discretionary choice between LWOP and some lesser punishment for adults convicted of intentional murder. In addition to Wisconsin, these jurisdictions include the District of Columbia, Illinois, Maine, Maryland, New York, North Dakota, Rhode Island, and Vermont. For instance, in the District of Columbia, a defendant who is convicted of first-degree murder faces a sentence of 30 years to LWOP, which leaves the D.C. judge with a decision that corresponds reasonably closely in practical effect to the Wisconsin judge's decision of a sentence somewhere between life with eligibility for release at 20 years and life with no possibility of release. Two other states, Alaska and Connecticut, functionally create a similar LWOP-or-less choice insofar as they establish a maximum sentence that precludes parole release for at least 50 years, which might fairly be characterized as de facto LWOP.

Additionally, it should be noted that 13 of the death-penalty jurisdictions also have an LWOP-or-less choice that must be made if a death sentence is not imposed. Finally, even in states in which there is not an LWOP-or-less choice for adult murderers, there may be such a choice for juvenile murderers. Indeed, this may be constitutionally required in some states as a result of the Supreme Court's Eighth Amendment cases, specifically, Roper v. Simmons, which prohibited the death penalty for juveniles, and Miller v. Alabama, which prohibited mandatory LWOP for juveniles. The constitutional need for a sub-LWOP option for all juvenile defendants is now embodied in the murder sentencing statutes of several states.

In short, despite the unique features of the Wisconsin system, a majority of other states now also require LWOP-or-less sentencing choices to be made in at least some subset of murder cases.

IV. Data and Methodology

A Sources of Data

The data for this study come from several sources, but primarily the Wisconsin Department of Corrections (DOC) and the Wisconsin Consolidated Court Automation Program (CCAP). An online DOC database tracks current inmates in the Wisconsin state prison system, while CCAP tracks all civil and criminal case filings in Wisconsin.
since the program began in 1987. The DOC data included much of the information we required on inmate offenses and sentences served, but CCAP was necessary for information on court adjudication processes. Starting with the DOC data, our initial sample included all current inmates in the Wisconsin state prison system as of August 17–21, 2018. That yielded 24,298 individuals. We then restricted the sample to all inmates who were serving a life sentence and were convicted under Wisconsin Statutes Section 940.01 (first-degree intentional homicide) in 2001 or later and processed into the DOC system by August 2018. We selected 2001 as the first year of observation because in that year there was a significant technological upgrade to merge two court system computer departments. In our experience, CCAP data before 2001 can be less reliable and rife with missing values on key covariates.

The foregoing steps yielded a set of 455 individuals who were convicted of first-degree intentional homicide between the start of 2001 and about mid-2018. Since individuals who are convicted of this crime must serve at least 20 years, we can be reasonably confident that this set of prisoners in mid-2018 was close to the full set of individuals who were convicted of first-degree intentional homicide in Wisconsin in our 2001–2018 timeframe.

The next step was to match the DOC data to CCAP data. Unfortunately, CCAP does not include DOC identifiers, which made linking the two datasets a challenge. We did manage to connect them by conducting manual searches of CCAP using defendant name, county of conviction, and case number—all of which were included in the DOC data.

The CCAP data revealed that a small number of the 455 cases arose from killings that occurred prior to 2000, presumably reflecting delays in the discovery of the offense or the apprehension or prosecution of the perpetrator. In most cases, these time lags did not present a problem for our analysis. However, offenses that took place before 1988 were sentenced under a different homicide law that did not include judicial discretion for possible release. Therefore, we decided to remove all five of the pre-1988 cases, leaving a total final sample of 450 cases.

We also relied on a few additional sources of information for certain covariates, as detailed in Section IV.C.

**B Dependent Variable**

We label our dependent variable Life With, reflecting the sentencing judge's binary decision in cases of first-degree intentional homicide to permit or not permit the defendant to seek release before death. In slightly more than half our cases (54 percent), the judge granted Life With. To be sure, even when a judge leaves open the possibility of release before death, defendants may still be denied when they apply for release and ultimately end up serving a whole-life sentence. Still, consistent with the Supreme Court's recognition of the distinctive hopelessness of the LWOP sentence, we assume that the judge's decision on eligibility for potential release would be regarded as highly consequential by most defendants. Additionally, given the near-even split in outcomes in our cases, it seems that judges also regard the decision as meaningful and not one to be made reflexively in one direction or the other. In any event, we coded cases in which Life With was granted as "1" and cases in which it was denied as "0."

**C Covariates**

We employ a large and diverse grouping of covariates. We divide them into four broad categories of measures: (1) case-severity and case-processing factors, (2) court-actor characteristics, (3) defendant/victim personal characteristics, and (4) county-level covariates. The case-severity and case-processing factors include the following variables:

- Number of homicide victims in the current case;
• Whether the defendant was charged as a "party to a crime," that it, as a participant in a crime that included multiple actors (yes = 1; no = 0);  
• Whether the state sought a "dangerous weapon" sentence enhancement (yes = 1; no = 0);  
• Whether the state sought a "habitual criminality" enhancement (applies when defendant had at least one prior felony conviction, or at least three prior misdemeanor convictions, in the past five years) (yes = 1; no = 0);  
• Whether the state sought a "domestic violence" enhancement (yes = 1; no = 0);  
• Whether the state sought a "hate crime" enhancement (yes = 1; no = 0);  
• Whether the defendant pled guilty or no contest, as opposed to going to trial (yes = 1; no = 0);  
• Whether the defendant was convicted through a jury trial, as opposed to a bench trial or a plea (yes = 1; no = 0);  
• Number of earlier or contemporaneous criminal cases in which the defendant was also convicted;  
• Whether the defendant had an earlier or contemporaneous conviction for another Class A or Class B felony (yes = 1; no = 0).

Most of the information for these variables comes from CCAP, except that the criminal history variables come from DOC and, in most cases, the number of victims was derived from a Supplemental Homicide Report (SHR) compilation. SHRs are prepared by police agencies in connection with homicide investigations. We used an SHR compilation covering our time period that was provided by the Wisconsin Department of Justice. The second grouping of covariates captures the characteristics of the key court actors in the case: judge, defense counsel, and prosecutor. CCAP included the names of the judge and defense lawyer(s) at the time of sentencing in all cases. It also provided the name of the prosecutor at sentencing in a little over two-thirds of the cases. In the remaining cases where CCAP was unclear, we relied primarily on published news media reports of the case to identify the prosecutor. For judges and district attorneys, we obtained information about the proximity of reelection dates and whether the official's last election was contested from the Wisconsin Election Commission. We also include measures for whether the defense counsel was appointed by the court (yes = 1; no = 0), which were taken from CCAP. We also examined the role of judge and lawyer gender. There was one defendant who declined counsel and represented himself. This case was removed from the data set when defense-counsel variables were analyzed.

We included another variable for all three court actors that may require some discussion. We identified the law schools that granted the law degrees for all the judges, defense lawyers, and prosecutors included in this study. The State Bar of Wisconsin has a publicly accessible on-line database with information about all licensed attorneys in the state, including law school attended and graduation year. Searching by name, we manually collected information about all the judges and lawyers in our dataset. Not surprisingly, the two most prevalent law schools by far were Wisconsin’s only two in-state schools: Marquette University and the University of Wisconsin. We thus initially recoded the law school variable into the three groups: Marquette, Wisconsin, and out of state. After preliminary analysis, however, we found consistent bivariate relationships between the dependent variable and judges and prosecutors who received their law degrees from Marquette. To account for this pattern, we recoded the law school variable so that, Marquette = 1 and all other law schools = 0.

This second grouping of covariates also included:

• Law school graduation year, which was converted into a years-out-of-law-school variable;  
• Number of years of judicial experience;  
• Whether the judge was up for reelection within 12 months of case sentencing (yes = 1; no = 0);  
• Whether the judge’s last election was contested (contested = 1; uncontested = 0);
Whether the judge had been a prosecutor before becoming a judge (yes = 1; no = 0); whether the judge had previously been a public defender (yes = 1; no = 0); number of years since law school for defense counsel and prosecutor; and whether the local district attorney was up for reelection within 12 months of sentencing (yes = 1; no = 0).

The third category of variables captures the personal characteristics of defendants and their victims. Information on defendants came from the DOC data and includes race, which was recoded into a binary variable (black = 1; all others = 0); gender (1 = female; 0 = male); and age at the time of offense. We coded black as the reference category to detect for discrimination against black defendants, which prior sentencing research has identified as an area of concern, as discussed in Section I. We were able to secure information on victim demographics from the SHR, which allowed us to include the following covariates: victim race (white = 1; nonwhite = 0); victim gender (female = 1; male = 0); victim age; and whether the victim was a current or former intimate partner of the suspect (yes = 1; no = 0). In cases, where there was more than one victim, we coded race and gender so that if any one of the victims was white or a woman, the value would be 1.

The final covariates are county-level characteristics. Unlike some other states, Wisconsin's judicial districts are organized along county lines, which makes county-level variables the appropriate means to study the effect of geographic variation. To detect whether such variation coincides with the likelihood of a judge granting Life With, we included several variables. Demographic county-level data were taken from the Wisconsin Department of Health Services, which relies on U.S. Census population estimates. We pulled unemployment data from the U.S. Department of Labor Unemployment Statistics Map of Wisconsin. We also included violent crime rate by county based on data from the Wisconsin Department of Justice. To link these aggregate measures to the case at hand, we used the year that the sentence was given as the year from which to draw county-level estimates. We also collected information on county-level estimates for the year before the sentence in order to calculate change scores. This step allowed us to observe whether changes in racial composition, the unemployment rate, and/or violent crime are tied to sentencing outcomes. Finally, we included a county-level political context variable that captures the average percent in a given county that voted for Republican presidential candidates in the years 2000, 2004, 2008, 2012, and 2016.

D Analytic Approach

As previously stated, our analytic approach for this study focuses on the Life With versus Life Without (LWOP) decision. Since this is a binary variable, we rely on analytic techniques reserved for categorical and limited dependent variables. We first begin our analysis with a discussion of the sample distributions and means. We proceed by discussing the results of the difference in means tests and chi-square cross-classification tests of significance. Lastly, we present and discuss the results of our regression models.

We also pare down the number of covariates in our final models to avoid problems with overfitting. With a sample of 450, we avoid these potential issues as our final model includes only 23 covariates. We excluded most variables that did not share a bivariate relationship with Life With, especially if they did not have a clear theoretical justification for inclusion. It was also necessary to selectively omit specific county-level measures in the final models due to issues with multicollinearity. Most of Wisconsin’s nonwhite population is concentrated in just a few counties located in the southeast portion of the state, which tend to be the same counties that have the highest concentrations of inequality, unemployment, and crime. The close relationship between these variables creates problems with analysis when they are included in a single model. To counter these potential problems, we focus on violent crime rate in the year of the sentence and the two-year change in violent crime.
rate, in addition to the aggregate measure of political conservatism. This decision was also informed by bivariate analysis, which revealed a relationship between Life With and the violent crime measures.135

V. Results and Findings

A Descriptive Statistics

We first discuss the descriptive statistics presented in Table 1. The demographic composition of the defendants in these cases is consistent with broader patterns for homicide offenders. For example, the vast majority of individuals currently incarcerated for intentional homicide in these data are men (96 percent).136 The racial composition of these prisoners is also comparable to national trends with 46 percent of the offenders being identified as black; 49 percent white; 3.6 percent American Indian; and 0.9 percent Asian American.137 (Unfortunately, our data do not include any indication of Hispanic/Latinx ethnicity.) Finally, the average age at offense of the prisoners in our sample was about 30.5 years.138

<table>
<thead>
<tr>
<th>Variable</th>
<th>N</th>
<th>Range</th>
<th>Mean (SD)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Life With</td>
<td>450</td>
<td>0–1</td>
<td>0.54 (0.50)</td>
</tr>
<tr>
<td><strong>Case-Severity and Case-Processing Factors</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Another Class A or B felony</td>
<td>450</td>
<td>0–1</td>
<td>0.25 (0.43)</td>
</tr>
<tr>
<td>Party to a crime</td>
<td>450</td>
<td>0–1</td>
<td>0.29 (0.46)</td>
</tr>
<tr>
<td>Dangerous weapon</td>
<td>450</td>
<td>0–1</td>
<td>0.38 (0.49)</td>
</tr>
<tr>
<td>Habitual criminality</td>
<td>450</td>
<td>0–1</td>
<td>0.10 (0.30)</td>
</tr>
<tr>
<td>Domestic violence</td>
<td>450</td>
<td>0–1</td>
<td>0.04 (0.21)</td>
</tr>
<tr>
<td>Jury trial</td>
<td>450</td>
<td>0–1</td>
<td>0.72 (0.45)</td>
</tr>
<tr>
<td>Guilty or no-contest plea</td>
<td>450</td>
<td>0–1</td>
<td>0.27 (0.44)</td>
</tr>
<tr>
<td>Number of victims</td>
<td>450</td>
<td>1–4</td>
<td>1.12 (0.48)</td>
</tr>
<tr>
<td>Number of prior-conviction cases</td>
<td>450</td>
<td>0–15</td>
<td>1.9 (2.40)</td>
</tr>
<tr>
<td><strong>Court Actors</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Judge gender (woman = 1)</td>
<td>450</td>
<td>0–1</td>
<td>0.17 (0.38)</td>
</tr>
<tr>
<td>Judge law school (Marquette = 1)</td>
<td>450</td>
<td>0–1</td>
<td>0.26 (0.44)</td>
</tr>
<tr>
<td>Years as judge</td>
<td>450</td>
<td>0–32</td>
<td>12.87 (7.68)</td>
</tr>
<tr>
<td>Judge up for reelection (yes = 1)</td>
<td>450</td>
<td>0–1</td>
<td>0.18 (0.38)</td>
</tr>
<tr>
<td>Judge ran in contested election (yes = 1)</td>
<td>450</td>
<td>0–1</td>
<td>0.14 (0.35)</td>
</tr>
<tr>
<td>Was judge previously prosecutor (yes = 1)</td>
<td>450</td>
<td>0–1</td>
<td>0.42 (0.49)</td>
</tr>
<tr>
<td>Was judge previously public defender (yes = 1)</td>
<td>450</td>
<td>0–1</td>
<td>0.05 (0.22)</td>
</tr>
<tr>
<td>Defense lawyer gender (woman = 1)</td>
<td>449</td>
<td>0–1</td>
<td>0.17 (0.38)</td>
</tr>
<tr>
<td>Defense lawyer law school (Marquette = 1)</td>
<td>449</td>
<td>0–1</td>
<td>0.34 (0.47)</td>
</tr>
<tr>
<td>Court-appointed defense lawyer (yes = 1)</td>
<td>449</td>
<td>0–1</td>
<td>0.72 (0.45)</td>
</tr>
<tr>
<td>Years out of law school of defense lawyer</td>
<td>449</td>
<td>3–53</td>
<td>23.86 (9.50)</td>
</tr>
<tr>
<td>Prosecutor gender (woman = 1)</td>
<td>450</td>
<td>0–1</td>
<td>0.15 (0.35)</td>
</tr>
<tr>
<td>Prosecutor law school (Marquette = 1)</td>
<td>450</td>
<td>0–1</td>
<td>0.40 (0.49)</td>
</tr>
<tr>
<td>Years out of law school of prosecutor</td>
<td>450</td>
<td>1–41</td>
<td>21.68 (8.35)</td>
</tr>
<tr>
<td>DA up for reelection (yes = 1)</td>
<td>450</td>
<td>0–1</td>
<td>0.20 (0.40)</td>
</tr>
<tr>
<td><strong>Defendant/Victim Characteristics</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Defendant race (black = 1)</td>
<td>450</td>
<td>0–1</td>
<td>0.46 (0.50)</td>
</tr>
<tr>
<td>Defendant gender (woman = 1)</td>
<td>450</td>
<td>0–1</td>
<td>0.04 (0.20)</td>
</tr>
<tr>
<td>Defendant age at offense</td>
<td>450</td>
<td>14–76</td>
<td>30.51 (10.73)</td>
</tr>
<tr>
<td>Victim race (white =1)</td>
<td>404</td>
<td>0–1</td>
<td>0.58 (0.49)</td>
</tr>
<tr>
<td>Victim gender (woman = 1)</td>
<td>450</td>
<td>0–1</td>
<td>0.41 (0.49)</td>
</tr>
<tr>
<td>Victim age</td>
<td>450</td>
<td>0–1</td>
<td>27.88 (0.36)</td>
</tr>
<tr>
<td>------------</td>
<td>-----</td>
<td>---------</td>
<td>--------------</td>
</tr>
<tr>
<td>Victim intimate partner (yes = 1)</td>
<td>450</td>
<td>0–1</td>
<td>0.18 (0.38)</td>
</tr>
</tbody>
</table>

**Aggregate (County-Level) Measures**

<table>
<thead>
<tr>
<th>Measure</th>
<th>N</th>
<th>Lower</th>
<th>Upper</th>
<th>Mean (Std)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Unemployment rate</td>
<td>450</td>
<td>0.022–0.13</td>
<td>0.06 (0.02)</td>
<td></td>
</tr>
<tr>
<td>Violent crime rate</td>
<td>450</td>
<td>18.5–1077.6</td>
<td>439.8 (343.6)</td>
<td></td>
</tr>
<tr>
<td>Percent black/Latinx</td>
<td>450</td>
<td>0.005–0.44</td>
<td>0.21 (0.16)</td>
<td></td>
</tr>
<tr>
<td>Percent of the population under 18</td>
<td>450</td>
<td>0.14–0.28</td>
<td>0.237 (0.018)</td>
<td></td>
</tr>
<tr>
<td>Change in unemployment rate</td>
<td>450</td>
<td>−0.02–0.06</td>
<td>0.0001 (0.001)</td>
<td></td>
</tr>
<tr>
<td>Change in violent crime rate</td>
<td>450</td>
<td>−184.6–195.5</td>
<td>10.0 (66.56)</td>
<td></td>
</tr>
<tr>
<td>Change in percent black/Latinx</td>
<td>450</td>
<td>−0.003–0.009</td>
<td>0.004 (0.003)</td>
<td></td>
</tr>
<tr>
<td>Mean Republican</td>
<td>450</td>
<td>0.28–0.68</td>
<td>0.42 (0.10)</td>
<td></td>
</tr>
</tbody>
</table>

When examining victim characteristics, we observe some patterns consistent with broader homicide trends, but we also observe at least one key departure. At the outset, we note that the victim race variable is missing in about 10 percent of our cases; we thus cautiously report estimates of this variable. When victim race was available, 58 percent involved a victim who was White. The average victim age was just under 28, and 18 percent of the cases involved an intimate partner.

More strikingly, 41 percent of the victims in our sample are female. This stands in marked contrast to the overall state homicide data, in which 21% of victims are female. Similarly, in national data, women comprise about 22 percent of homicide victims. There seem two possible explanations for the prevalence of female victims in our cases. First, women may disproportionately be the victim of the most culpable intentional homicides, in comparison to homicides that are accidental or otherwise mitigated. Second, regardless of the underlying facts, legal decisionmakers may tend to view homicides with female victims as more culpable, leading, for instance, to a higher likelihood that female-victim offenses would be charged as first-degree intentional homicides, as opposed to a less severe homicide offense. However, our data do not permit us to test these hypotheses.

With regard to the case-severity and case-processing factors, 25 percent of the defendants had at least one additional conviction for a Class A or B felony beyond the first-degree intentional homicide of interest, 29 percent were charged as a party to a crime, 38 percent were charged with a dangerous weapon enhancement, 10 percent were charged with a habitual criminality enhancement, and 4 percent included a domestic violence enhancement. We also observe that most of the cases included in our analysis were decided by a jury trial (72 percent). The mean number of homicide victims in these cases was 1.19, while the defendants had on average 1.9 prior-conviction cases reflected in the DOC records.

The court-actor variables also lead to some noteworthy findings. To begin, most of the judges (83 percent), defense attorneys (83 percent), and prosecutors (85 percent) are men. Next, very few of the cases (14 percent) involved a judge who had faced an opponent in his or her most recent election or reelection bid. The proportion of court actors who received their law degree from Marquette Law School varied from 26 percent for judges, to 34 percent for defense attorneys, to 40 percent for prosecutors. The data also indicate that 72 percent of the defense attorneys were court appointed.

The mean county-level unemployment rate during the period of observation was 6 percent. The mean black/Latinx proportion by county was 21 percent. We also draw attention to partisan voting patterns in presidential elections; the mean county-level GOP vote for our cases was 42 percent —reflecting, in part, the prevalence in our data of cases from heavily Democratic Milwaukee.
Bivariate Analysis

Our bivariate analysis uncovered several notable patterns. First, we found a connection between sentencing outcomes and victim gender, with female-victim cases more likely to result in LWOP. More specifically, in Table 2, with victim gender as columns and the Life With variable across rows, we find a difference of about 16 percentage points in the likelihood that a defendant with a female victim will receive leniency in comparison with a defendant with a male victim, a percentage difference that is statistically significant.

<table>
<thead>
<tr>
<th></th>
<th>Male Victim</th>
<th>Female Victim</th>
<th>Totals</th>
</tr>
</thead>
<tbody>
<tr>
<td>LWOP</td>
<td>105 (39.5%)</td>
<td>102 (55.4%)</td>
<td>207 (46%)</td>
</tr>
<tr>
<td>Life With</td>
<td>161 (60.5%)</td>
<td>82 (44.5%)</td>
<td>243 (54%)</td>
</tr>
<tr>
<td>Totals</td>
<td>266 (100%)</td>
<td>184 (100%)</td>
<td>450 (100%)</td>
</tr>
</tbody>
</table>

Notes: Chi-square = 11.16** (p < 0.01). We use ** to indicate a probability of less than 1 percent (i.e., p < 0.01) that an apparent relationship between variables (here, sentence and victim gender) is due to random variability or chance.

To continue unpacking the role of gender in case adjudication, we split the cross-classification table by black male and other defendants. (The number of female defendants is too small to permit further breakdown of the data along those lines.) As indicated in Table 2, when statistically controlling for the race and gender of the defendant, we found that for nonblack male defendants, the gap in outcomes between female- and male-victim cases decreased from 16 to 9 percentage points—and was no longer statistically significant. By contrast, with black male defendants, the difference in receiving LWOP between cases with a female versus a male victim increased to 24.5 percentage points and retained statistical significance. Black men convicted of killing women were significantly more likely to be given LWOP (by nearly 25 percentage points) than were black men who were convicted of killing other men. Otherwise stated, the connection between victim gender and sentencing may be conditioned by defendant variables, with victim gender seemingly more salient in cases involving black defendants than white ones. In particular, we note that among the four defendant-victim categories explored in Table 3, the group most likely to receive Life With was black male defendants who killed other men. This pattern could possibly reflect, for instance, negative perceptions regarding the behavior or character of the victims in these cases.

<table>
<thead>
<tr>
<th></th>
<th>Male Victim</th>
<th>Female Victim</th>
<th>Totals</th>
</tr>
</thead>
<tbody>
<tr>
<td>Nonblack Male Defendant</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>LWOP</td>
<td>55 (44%)</td>
<td>63 (52.9%)</td>
<td>118 (48.4%)</td>
</tr>
<tr>
<td>Life With</td>
<td>70 (56%)</td>
<td>56 (47.1%)</td>
<td>126 (51.6%)</td>
</tr>
<tr>
<td>Totals</td>
<td>125 (100%)</td>
<td>119 (100%)</td>
<td>244 (100%)</td>
</tr>
<tr>
<td>Chi-square = 1.95</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black Male Defendant</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>LWOP</td>
<td>50 (35.5%)</td>
<td>39 (60%)</td>
<td>89 (43.2%)</td>
</tr>
<tr>
<td>Life With</td>
<td>91 (64.5%)</td>
<td>26 (40%)</td>
<td>117 (56.8%)</td>
</tr>
<tr>
<td>Totals</td>
<td>141 (100%)</td>
<td>65 (100%)</td>
<td>206 (100%)</td>
</tr>
<tr>
<td>Chi-square = 10.92**</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: We use ** to indicate a probability of less than 1 percent (i.e., p < 0.01) that an apparent relationship between variables (here, sentence and victim gender) is due to random variability or chance.
To test whether the age of defendants is linked to sentencing leniency, we conducted t tests, which show that the mean age of defendants who were given Life Without was 33.63 years, while the mean age of defendants who were given Life With was 27.86 years. This difference is statistically significant \( p < 0.001 \). The finding indicates that younger defendants are more likely to receive sentencing leniency than are older ones. We also conducted a test of whether the mean age for black defendants is different than the mean age for nonblack defendants, which did prove to be the case. The mean age for black defendants was 27.64 and the mean age for nonblack defendants was 33.52 \( p < 0.001 \), demonstrating that black defendants tend to be younger than nonblack ones. It seems that younger defendants are treated with more leniency, and that black defendants are on average younger. We reexamine the connection between age, race, and sentencing in greater depth when we discuss the results of the regression models.

We find several additional variables that share a bivariate relationship with sentencing leniency. The data indicate that defendants with more prior convictions are significantly less likely to receive leniency, as are defendants convicted of an additional Class A or B felony. Defendants who were sentenced by judges who were up for reelection within a year of the case were significantly less likely to receive sentencing leniency, as were defendants who were prosecuted by graduates of Marquette Law School. As the number of victims increased, so, too, did the defendant’s chances of receiving LWOP. As the county in which defendants were sentenced became increasingly Republican during the period of observation, the likelihood that they would receive sentencing leniency declined. Curiously, counties with higher rates of violent crime or increasing violent crime rates corresponded with increased likelihood of receiving Life With.

C Multivariate Regression Models
The final step of our analysis was to observe statistically controlled relationships for all the variables simultaneously. As previously noted, we trimmed the variables included in the binary logit regression models. To be included, covariates had to either share a statistically significant relationship with Life With at the bivariate level or be identified as a significant correlate of sentencing outcomes in prior studies (capital or noncapital). We employed a step-wise model approach, as indicated in Table 4. Model 1 includes only the case-severity and case-processing factors, while Model 2 also includes the court-actor characteristics, and the like.

<table>
<thead>
<tr>
<th>Variable</th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
<th>Model 4</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Case-Severity and Case-Processing Factors</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Class A or B felony</td>
<td>-1.04 (0.28)</td>
<td>-1.11 (0.30)</td>
<td>-1.24 (0.31)</td>
<td>-1.39 (0.32)</td>
</tr>
<tr>
<td>Weapon</td>
<td>0.59 (0.23)</td>
<td>0.79 (0.24)</td>
<td>0.50 (0.26)</td>
<td>0.24 (0.27)</td>
</tr>
<tr>
<td>Habitual criminality</td>
<td>-0.68 (0.36)</td>
<td>-0.87 (0.38)</td>
<td>-0.99 (0.39)</td>
<td>-0.88 (0.41)</td>
</tr>
<tr>
<td>Jury trial</td>
<td>-0.53 (0.89)</td>
<td>-0.60 (0.90)</td>
<td>-1.26 (0.97)</td>
<td>-1.45 (1.01)</td>
</tr>
<tr>
<td>Guilty or NC plea</td>
<td>-0.12 (0.90)</td>
<td>-0.13 (0.91)</td>
<td>-0.61 (0.98)</td>
<td>-0.78 (1.02)</td>
</tr>
<tr>
<td>Number of victims</td>
<td>-0.49 (0.36)</td>
<td>-0.53 (0.38)</td>
<td>-0.47 (0.39)</td>
<td>-0.57 (0.40)</td>
</tr>
<tr>
<td>Number of prior-conviction cases</td>
<td>-0.14 (0.045)</td>
<td>-0.11 (0.05)</td>
<td>-0.09 (0.05)</td>
<td>-0.10 (0.05)</td>
</tr>
<tr>
<td><strong>Court Actors</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Judge gender (woman = 1)</td>
<td>0.23 (0.28)</td>
<td>0.065 (0.30)</td>
<td>0.04 (0.31)</td>
<td></td>
</tr>
<tr>
<td>Judge MU law</td>
<td>-0.44 (0.24)</td>
<td>-0.39 (0.25)</td>
<td>-0.26 (0.26)</td>
<td></td>
</tr>
<tr>
<td>Judge reelection</td>
<td>-0.53 (0.28)</td>
<td>-0.40 (0.29)</td>
<td>-0.34 (0.30)</td>
<td></td>
</tr>
<tr>
<td>Judge was defense</td>
<td>-0.46 (0.48)</td>
<td>-0.36 (0.51)</td>
<td>-0.60 (0.55)</td>
<td></td>
</tr>
<tr>
<td>Judge was prosecutor</td>
<td>0.58 (0.22)</td>
<td>0.54 (0.23)</td>
<td>0.59 (0.24)</td>
<td></td>
</tr>
<tr>
<td>Court appointed defense</td>
<td>0.08 (0.27)</td>
<td>0.02 (0.29)</td>
<td>0.07 (0.30)</td>
<td></td>
</tr>
<tr>
<td>Prosecutor gender (woman = 1)</td>
<td>-0.05 (0.30)</td>
<td>-0.01 (0.32)</td>
<td>-0.17 (0.34)</td>
<td></td>
</tr>
<tr>
<td>Prosecutor MU law</td>
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<td>-0.58 (0.23)</td>
<td>-0.54 (0.25)</td>
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The results of Model 1 in Table 4 show three statistically significant covariates. Defendants were less likely to receive Life With when they had a prior or contemporaneous conviction for another Class A or B felony ($\beta = -1.04; p < 0.001$), which echoes frequent findings in both the capital and noncapital sentencing literature that criminal history seems to play a significant role in sentencing decisions. Similarly, Model 1 also indicates that the likelihood of leniency in our cases declined as the number of prior-conviction cases increased ($\beta = -0.14; p < 0.01$). Surprisingly, though, we found that the chances of leniency actually increased when prosecutors sought a dangerous weapon enhancement. The pseudo $R^2$ of Model 1 suggests that approximately 8.4 percent of the variance across the dependent variable is captured by these factors.

The results of Model 2 in Table 4 are similar to those of Model 1 with regard to the case-severity and case-processing factors, except now we observe that the habitual criminality enhancement variable is statistically significant. This finding again highlights the seeming importance of criminal history to the sentencing decision.

Model 2 of Table 4 also indicates two statistically significant court-actor measures. Cases with judges who were former prosecutors were significantly more likely to result in a Life With sentence ($\beta = 0.58; p < 0.01$). While seemingly counterintuitive, it could be the case that judges who had been prosecuting attorneys feel less compelled as a political matter to demonstrate a "tough-on-crime" approach to sentencing. We also found that cases with prosecutors who graduated from Marquette Law School were less likely to result in Life With ($\beta = -0.61; p < 0.05$). We are unable to determine an underlying cause of this "law school effect" with the available data. For instance, there may be self-selection dynamics, for example, future prosecutors who attend Marquette Law School may tend to bring more punitive attitudes with them to law school, which may translate later in their careers to a greater tendency to push for more severe sentences. Alternatively, there may be something in the law school experience itself, such as a more formalist approach by the faculty to teaching law, that in some enduring way affects the attitudes or practices of former students who become prosecutors. Perhaps the most obvious difference between Marquette and most of the other law schools represented in our data lies in Marquette's religious (Catholic) affiliation, which might plausibly contribute to either self-selection or institutional-experience effects. In any event, by whatever underlying mechanism, our results do suggest that the background or personal characteristics of court actors shape the litigation of homicide cases in ways that may, in the aggregate, affect sentencing outcomes. Our findings in Model 2 thus echo a finding common in the extant noncapital-sentencing research that sentences are affected by the specific judges and prosecutors working on a given case. However, we are not aware of any earlier studies that specifically examined the

<table>
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</table>

* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$. 
judge and prosecutor background variables that we found to be significant. With the addition of these variables, the pseudo $R^2$ jumps to 12.3 percent in Model 2.

We observe some key changes in Model 3 in Table 4. Here, only two factors from Model 1 are significant net of defendant and victim characteristics. As in Model 2, defendants who have another prior or contemporaneous Class A or B felony or who are subject to a habitual criminality enhancement were less likely to receive Life With. Also, the two court-actor variables that were significant in Model 2 remain significant in Model 3. The defendant and victim characteristics added in this model were defendant race, defendant gender, defendant age, and victim gender, but only one of these variables proved to be significant: age of defendant ($\beta = -0.06; p < 0.001$). Consistent with the capital sentencing research, older defendants in our data were less likely to receive sentencing leniency than were younger ones.152 We also observe the pseudo $R^2$ estimate increase from 12.3 percent to 18.1 percent.

As noted in Section I, the death-penalty research consistently finds that victim race is a significant predictor of sentencing outcomes in murder cases. Because we lacked victim-race data in about 10 percent of our cases, we were not able to include this notable covariate in our analysis of the full sample. However, in light of the potential importance of racial effects in this area, we completed a separate set of parallel analyses using just the cases for which victim race was known. Following the same step-wise model-building approach that we used with the full sample, victim race proved non-significant in all the models.153

Model 4 of Table 4, or the full model, contains all the previous covariates, but now also includes aggregate county measures. All the covariates that were found to be significant in Model 3 are also significant in Model 4. Additionally, cases that were sentenced in counties with increasing rates of violent crime were more likely to result in lenience. While this finding is unexpected and somewhat unusual,154 it seems plausible that law-enforcement and court resources in counties with increasing violent crime are under particular strain, which may impede the ability of police and prosecutors to put together the strongest possible cases against defendants and may otherwise incentivize prosecutors to take more generous plea-bargaining positions so as to resolve cases more quickly. There may also be a measure of jadedness that sets in during a period of increasing violent crime.

We also found that counties with higher average rates of Republican voters during presidential elections were associated with lower likelihoods of leniency ($\beta = -4.92; p < 0.01$). This replicates a finding in some, but not all, prior studies that have attempted to determine whether there is a link between county political orientation and sentencing outcomes.155 Additionally, we note that the pseudo $R^2$ in Model 4 of Table 4 increases to 20.0 percent.

The final step in our analysis was to focus on the way in which age is tied to sentencing outcomes. To do so, we estimated conditional probabilities for Life With while setting all other covariates at their global mean. We then calculated the conditional probabilities at each defendant age included in the sample, which ranged from 14 to 76. We illustrate these conditional probabilities, and their 95 percent confidence intervals, at each age in Figure 1.

1 Conditional probabilities of receiving leniency for the full sample with 95 percent CIs.
At the earliest ages, defendants have nearly an 80 percent chance of receiving Life With holding all other covariates at their global means. For the oldest defendants, there is less than a 10 percent chance of leniency. At 34 years of age, the chance of receiving leniency drops below 50 percent, indicating the threshold where the odds are 50–50.

We also calculated the same probabilities separately for black and nonblack defendants. Figure 2 indicates the conditional probabilities of receiving Life With over the 14–76 age range for each group. Figure 2 shows a downward trend similar to the conditional probabilities for the entire sample, but black defendants are slightly less likely to receive leniency at each age examined. The difference between the two groups is fairly minor ranging from approximately one to three percentage points.

2 Conditional probabilities of receiving leniency for black and nonblack defendants by age.

To arrive at a clearer sense of the gap between the conditional probability for black and nonblack defendants, we also illustrate this difference in Figure 3. The figure shows an interesting pattern in which the gap between black and nonblack defendants increases to nearly 3 percentage points until approximately a defendant’s mid-30s and then falls throughout the remaining ages and approaches convergence when defendants face sentencing during their 60s. These findings support the notion that a defendant’s race plays a complicated and possibly even changing role in sentencing as defendants age. Ultimately, however, our data indicate that a defendant’s race is not the most central or influential variable in explaining why some intentional homicide cases result in LWOP and others do not.

3 Black–nonblack difference in conditional probabilities of leniency by age.

VI. Policy Implications

Some readers may have a good news/bad news reaction to our analysis. First, the good news. Consistent with widely—albeit not universally—accepted normative views of sentencing, we find that intentional-homicide defendants who have more serious criminal histories are more likely to receive LWOP, even holding constant a diverse set of covariates. This suggests that the judges in these cases are, at least to some extent, attending to legally relevant case-specific sentencing considerations.
Similarly, defendant age, another of our significant covariates, should probably also be regarded as an appropriate consideration. As the Supreme Court has emphasized in cases like *Roper v. Simmons* and *Miller v. Alabama*, younger defendants tend to have lower culpability and better rehabilitative prospects than older defendants. Additionally, it may be appropriate to regard LWOP as a harsher sentence for younger than older defendants because a whole-life sentence will likely translate into more years in prison for the younger defendant.

Moreover, in contrast to numerous studies of murder sentencing in death-penalty states, our multivariate regression models show no indication of a statistically significant relationship between sentencing outcomes and either victim race, victim gender, defendant race, or defendant gender. To be sure, though, any reassurance offered by this analysis must be tempered by an appreciation that a finding of non-significance is not the same thing as concluding that race or gender have no importance or role in sentencing outcomes; we cannot rule out the possibility that an analysis with a larger number of cases or a different array of covariates would reveal subtle race or gender effects that are not captured by our current models. Indeed, our bivariate cross-classification analysis suggests that victim gender, defendant gender, and defendant race may interact with one another in significant ways, although our sample size is not large enough to determine whether these interaction effects would hold up if other variables were held constant. Similarly, our age-race conditional probabilities analysis may also point to subtle race effects.

Some may also find it reassuring that we found no evidence of a "trial penalty," that is, a significant relationship between sentence and mode of conviction.

On the "bad news" side of the ledger might be indications that sentencing outcomes are influenced by the identity of the judge and prosecutor. There is no apparent normative rationale for punishment to turn on the professional or educational background of the key courtroom actors, whose assignment to the case may be an arbitrary matter. Of course, as noted in Section I, it is a common finding in empirical sentencing research that the identities of the judge and prosecutor matter, but this aspect of sentencing reality may be regarded as particularly troubling when the stakes are as high as they are in first-degree intentional homicide cases, when the extreme sentence of LWOP is on the table.

It may also be regarded as troubling that outcomes are seemingly influenced by county-specific social and political factors. On the one hand, it may to some extent be desirable for sentences to reflect the needs and values of the community in which the crime occurred. This is, after all, a locale that is likely to experience the harm of the offense far more acutely than other communities and thus have a uniquely compelling stake in the penal response. Moreover, the place of prosecution is not arbitrary in the same sense as the identity of the judge and prosecutor, but normally flows from the perpetrator's decision about where to commit the crime. Additionally, a policy preference for some localization in the administration of justice seems implicit in the state's system of electing trial court judges and district attorneys at the county level. Yet, on the other hand, it remains the case that the locally accountable district attorneys prosecute in the name of the state, not the county. It is, moreover, a state agency, the Department of Corrections, that administers sentences of imprisonment, and hence state taxpayers who collectively assume financial responsibility for those sentences. Indeed, as other criminologists have observed, local control over sentencing creates a sort of free-rider problem—a "correctional free lunch," so to speak. Through their imprisonment-related decisions, local officials are able to rid their communities of troublesome individuals and appeal to voters' tough-on-crime instincts without having to fully bear the resulting fiscal burdens. Resources like imprisonment that can be used at no or relatively low cost to the "consumer" may tend to be over-utilized relative to their full costs and benefits.
On balance, we think the evidence of inappropriate and questionable influences over LWOP sentencing warrants attention by policymakers. We think consideration should be given to reforms that would focus attention at sentencing more squarely on core considerations of defendant culpability and criminal history, and that would promote greater case-to-case, county-to-county uniformity in the assessment and weighing of these considerations.

We recognize that a half-century of efforts to bring greater uniformity to U.S. sentencing have likely produced more failures than successes.162 In particular, the history of the federal sentencing guidelines offers a sobering reminder that centralized command-and-control approaches to sentencing are apt to produce considerable unfairness and may be routinely circumvented by judges and prosecutors in ways that are difficult to monitor and correct.163 At the same time, we believe that the difficulties with such guideline systems derive in large part from the profound conceptual challenges raised by attempts to fit highly diverse sets of offenses into a single, coherent penal scheme, and by the equally vexing practical impediments to achieving widespread guidelines compliance in a justice system that is dominated by plea bargaining.164 The prospects for fair, effective centralized guidance may be stronger when the guidance focuses only on a single offense—especially when that offense is of such extreme gravity that guilty pleas are the exception, not the rule. Even at that, uniformity goals should be modest, with some room allowed for discretion and localization. Inflexible rules—for example, automatic LWOP in all multiple-victim cases—are apt to produce injustice, circumvention, or both.

The criteria and procedures used for capital sentencing may, in a very general sense, serve as a model for LWOP sentencing. Under the shadow of the Supreme Court’s Eighth Amendment jurisprudence, death-penalty states have adopted narrowing statutes that are designed, at least nominally, to limit capital sentences to the worst of the worst.165 These statutes include lists of aggravating circumstances and require that at least one aggravating circumstance be found before the defendant can be considered for death.166 Even then, a capital sentence is not automatic, but must be based on a case-specific weighing of both aggravating and mitigating circumstances.167

In practice, the weakness of such narrowing statutes has been that they include too many aggravating circumstances, some of which tend to be rather vague, such as whether the murder was especially "heinous, cruel or depraved."168 Such statutes are poorly designed to ensure that capital punishment truly is reserved for the worst of the worst.169

If the concept of statutory narrowing is transposed to the LWOP context, as we think merits consideration, the list of aggravating circumstances ought to be relatively short and objective in character. As a starting point for discussion, we propose the following:

- Whether there were multiple homicide victims in the case;
- Whether the defendant intentionally or knowingly caused the victim to suffer severe, protracted pain or psychological distress;
- Whether the victim was below the age of 18;
- Whether the killing was planned in advance; and
- Whether the defendant had a prior conviction and prison sentence for a felony-level crime of violence.170

Further mirroring the constitutional safeguards in place for capital sentencing, consideration should also be given to categorical restrictions on who is subject to LWOP, such as juveniles,171 the intellectually disabled,172 and the severely mentally ill.173

One arguable downside of our proposal is that defendants would have a constitutional right to a jury trial on the aggravating factors.174 This would not only make LWOP sentencing somewhat more procedurally cumbersome,
but would also introduce the potential for the disparities and biases that are associated with jury decision making, as in the capital-sentencing arena. On the other hand, if the aggravating circumstances are such that their presence or absence can be readily determined, as we think would be the case with those that we have suggested, the necessary fact-finding could be performed quickly and conveniently post-conviction by the trial jury, with less room for improper considerations to influence the outcomes. The ultimate weighing of aggravating and mitigating circumstances could still potentially be performed by a judge.\textsuperscript{175}

A substantial argument remains that LWOP should simply be eliminated altogether. Perfectly consistent, coherent decision-making based exclusively on legally and morally defensible considerations cannot likely be achieved in any sentencing system designed and administered by human beings. Some real-world messiness may be tolerable when judges are choosing between relatively modest sentences, say, a three-year prison term versus a five-year prison term. However, when the stakes are as high as they are with LWOP sentencing, it may be appropriate to demand a higher level of decisional quality than is actually feasible in practice.

Frustrated by the nation’s lack of progress in improving the consistency and parsimony of capital sentencing in the 1990s, Justice Harry Blackmun famously declared that he would no longer "tinker with the machinery of death" and adopted a position of categorical opposition to capital punishment.\textsuperscript{176} It may be similarly futile to "tinker with the machinery of life," as we have proposed. We take no position on this difficult and increasingly pressing question. We simply urge, if research continues to indicate that LWOP sentencing is influenced by inappropriate or questionable factors, that a due regard for the profound severity of the LWOP sentence warrants—at a minimum—the adoption of new safeguards regarding its use.

Footnotes
3 See Section II.B.
6 There is, to be sure, substantial overlap in some states between capital sentencing and LWOP sentencing inasmuch as LWOP is often an alternative when a capital sentencing jury rejects the death penalty. See, e.g., Tenn. Code Ann. § 39-13-204 (West 2020) ("the jury shall fix the punishment in a separate sentencing hearing to determine whether the defendant shall be sentenced to death, to imprisonment for life without possibility of parole, or to imprisonment for life."). However, our chief interest in this paper is with noncapital sentencing proceedings in which a judge must choose a life-without-parole sentence or some less severe punishment.
7 See, e.g., Kennedy v. Louisiana, 554 U.S. 407, 438 (2008) ("Consistent with evolving standards of decency and the teachings of our precedents we conclude that, in determining whether the death penalty is excessive, there is a distinction between intentional first-degree murder on the one hand and nonhomicide crimes against individual persons, even including child rape, on the other.").
8 Ashley Nellis, The Sentencing Project, Still Life: America's Increasing Use of Life and Long-Term Sentences 12 (2017) (finding that 70 percent of LWOP sentences are for first- or second-degree murder).
9 See Ring v. Arizona, 536 U.S. 584, 609 (2002) ("Accordingly, we overrule Walton to the extent that it allows a sentencing judge, sitting without a jury, to find an aggravating circumstance necessary for imposition of the death penalty.").
10 In 32 states, some or all trial-court judges are elected. See Methods of Judicial Selection, Nat’l Ctr. for State Courts, available at http://www.judicialselection.us/judicial%5fselection/methods/selection%5fof%5fjudges.cfm?state=.


13 O’Hear, supra note 15, at 160.

18 Nellis, supra note 8, at 26.

19 The closest antecedent to this work may be a forthcoming article that analyzes the use of LWOP sentences in North Carolina. See Brandon L. Garrett et al., Life Without Parole Sentencing in North Carolina, N.C. L. Rev. (forthcoming) (available at https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3552636). In North Carolina, however, LWOP is a mandatory minimum alternative to death in cases of first-degree homicide. Id. at *12. Therefore, the North Carolina research does not particularly cover LWOP as a discretionary judicial sentencing option, as ours does. Additionally, the North Carolina study uses as its dependent variable the frequency of LWOP sentences in each county. Id. at *20. By contrast, our focus is on the LWOP-or-less sentencing decisions made by judges at the level of the individual case. In this sense, our study more closely replicates in the LWOP context the many capital-sentencing studies that seek to determine the case-specific offense and offender variables that are correlated with the imposition of the death penalty.

20 See Wis. Stat. Ann. § 973.014(1g)(a) (West 2020) (“[W]hen a court sentences a person to life imprisonment ..., the court shall make an extended supervision eligibility date determination regarding the person and choose one of the following options: 1. The person is eligible for release to extended supervision after serving 20 years. 2. The person is eligible for release to extended supervision on a date set by the court .... 3. The person is not eligible for release to extended supervision.”).

21 See Section IV.A.

22 See Section IV.C.

23 See Section V.C.

24 These factors relate to the overarching penal purposes of retribution (i.e., giving the defendant his or her just deserts) and crime-prevention, which are explicitly authorized purposes in most jurisdictions. See, e.g., 18 U.S.C. § 3553(a) (“The court, in determining the particular sentence to be imposed, shall consider ... (2) the need for the sentence imposed—(A) to reflect the seriousness of the offense, to promote respect for the law, and to provide just punishment for the offense; (B) to afford adequate deterrence to criminal conduct; (C) to protect the public from further crimes of the defendant; and (D) to provide the defendant with needed educational or vocational training, medical care, or other correctional treatment in the most effective manner ....”). See also Michele Cotton, Back with a Vengeance: The Resilience of Retribution as an Articulated Purpose of Criminal Punishment, 37 Am. Crim. L. Rev. 1313 (2000) (discussing adoption by many states in the mid-20th century of explicit statements of sentencing purposes that emphasized utilitarian crime-prevention, and subsequent legislative and judicial embrace in many states of retribution as an additional authorized purpose). Although there has been a lively theoretical debate over the relevance of harm caused, as opposed to harm intended, non-theorists generally seem to accept that harm caused is an important aspect of a defendant’s blameworthiness. See, e.g., Larry Alexander & Kimberly Kessler Ferzan, Results Don’t Matter, in Criminal Law Conversations 147, 149 (Paul H. Robinson, Stephen P. Garvey & Kimberly Kessler Ferzan, eds., 2009) (discussing views of laypeople on harm). Likewise, the use of criminal history as a sentencing factor has been the subject of much theoretical debate, see Richard S. Frase, Just Sentencing: Principles and Procedures for a Workable System 180–89 (2013) (summarizing debate), but remains a well-established aspect of sentencing law and practice, see, e.g., id. at 121 (noting that all state sentencing guidelines systems “are based primarily on elements of current and prior conviction offenses”
25 See, e.g., U.S. Sent’g Comm’n, U.S. Sentencing Guidelines Manual § 5H1.10 (2018) (stating that race, sex, national origin, creed, religion, and socio-economic status “are not relevant in the determination of a sentence”).


28 Id. at 16–17.

29 Studies of the federal sentencing system have analogous limitations, that is, permitting analysis of variables that potentially distinguish one federal district from another, but not of uniform, systemwide characteristics.


32 Glenn L. Pierce, Michael L. Radelet & Susan Sharp, Race and Death Sentencing for Oklahoma Homicides Committed Between 1990 and 2012, 107 J. Crim. L. & Criminology 733, 739–40 (citing Catherine M. Grosso et al., Race Discrimination and the Death Penalty: An Empirical and Legal Overview, in America’s Experiment with Capital Punishment 525, 538–39 (J.R. Acker, R.M. Bohm & C.S. Lanier eds., 3d ed. 2014)). To be sure, much research suggests that there may be important interaction effects involving defendant race and victim race. More specifically, cross-racial, black-on-white killings seem more likely to result in the death penalty than do other racial combinations. See, e.g., id. at 739–40 (noting that, out of 36 studies completed in the late 1990s and early 2000s, nine “found that black defendants with white victims were more harshly treated than other homicide defendants”; citing Catherine M. Grosso et al., Race Discrimination and the Death Penalty: An Empirical and Legal Overview, in America’s Experiment with Capital Punishment 525, 538–39 (J.R. Acker, R.M. Bohm & C.S. Lanier eds., 3d ed. 2014)).

33 Christopher J. Mariela et al., Victim Age and Capital Sentencing Outcomes in North Carolina (1977–2009), 31 Crim. Just. Stud. 62, 69 tbl. 2 (2018). A different study of North Carolina also found a statistically significant relationship between death sentences and defendant age, specifically, if the defendant was under the age of 20, then the odds of a death sentence were reduced by about 75 percent. Barbara O’Brien et al., Untangling the Role of Race in Capital Charging and Sentencing in North Carolina, 1990–2009, 94 N.C. L. Rev. 1997, 2026 tbl. 4, 2045 App. A (2016) (showing significance of variable “YoungDef” in regression models; defining “YoungDef” as “Defendant is less than 20 years old”). Similarly, a study of Ohio found there was less likelihood the death penalty would be used if the defendant was under the age of 25. Williams & Holcomb, supra note 31, at 365 tbl. 3. But see Scott Phillips, Status Disparities in the Capital of Capital Punishment, 43 L. & Soc’y Rev. 807, 829 tbl. 5 (2009) (in study of Harris County, Texas, finding no significant relationship between death sentences and defendant age).

34 It is these considerations that led that Supreme Court to ban the death penalty for individuals who were under the age of 18 at the time of their offense. Roper v. Simmons, 543 U.S. 551, 569–71 (2005).

35 See Katherine Beckett & Heather Evans, Race, Death, and Justice: Capital Sentencing in Washington State, 1981–2014, 4 Colum. J. Race & L. 77, 85 (2016) (“most studies find that defendants convicted of killing Whites are significantly more likely to receive a death sentence than others, even after controlling for a wide range of legal and extra-legal factors that may also influence outcomes in capital cases.”). For instance, in a study of 1,514 death-eligible homicide cases in North Carolina between 1990 and 2009, Barbara O’Brien and colleagues found that the odds of a death sentence being imposed were more than twice as high when there was a white than a black victim. Barbara O’Brien et al., supra note 33, at 2026
Such findings are common, but not quite ubiquitous in the research literature. Out of 36 studies completed in the late 1990s and early 2000s, 24 found a race-of-victim effect. Pierce, Radelet & Sharp, supra note 32, at 739–40 (citing Catherine M. Grosso et al., Race Discrimination and the Death Penalty: An Empirical and Legal Overview, in America’s Experiment with Capital Punishment 525, 538–39 (J.R. Acker, R.M. Bohm & C.S. Lanier eds. 3d ed. 2014)). Marian Williams and Jefferson Holcomb have suggested two reasons why victim race may affect outcomes:

First, homicides with black victims may be perceived as less harmful to society relative to crimes against whites. Given the historical marginalization and oppression of blacks in American society, black victim crimes may be considered unworthy of the most severe criminal justice response. A second and perhaps related explanation relates to stereotypes about black conduct. If decision makers perceive victim conduct as contributing to their victimization, then they typically assign less blame to the defendant. Stereotypes about the behavior of blacks that suggest they are more likely to engage in illegal or morally questionable behavior may affect the perceived blameworthiness of the defendant, the amount of harm done, and the credibility of the victim as a “victim.”

Williams & Holcomb, supra note 31, at 353 (citations omitted).

For instance, the study of North Carolina by O’Brien and colleagues found that defendants who killed a female victim were about 1.8 times as likely to receive a death sentence as were killers of male victims. O’Brien et al., supra note 33, at 2026 tbl. 4. Similarly, a study of 446 death-eligible cases in Harris County (Houston), Texas, in the 1990s found that prosecutors were 2.73 times more likely to seek, and juries 2.16 times more likely to impose, death sentences when the victim was female. Phillips, supra note 33, at 823 tbl. 3. For a succinct summary of several other studies in other jurisdictions finding gender-of-victim effects, see Schatz & Dalton, supra note 31, at 1251–52. Note, though, that the research is not entirely unanimous on this point. See, e.g., Beckett & Evans, supra note 35, at 98 (finding no victim-gender effect in administration of death penalty in Washington State). Marian Williams and Jefferson Holcomb have suggested reasons why victim gender may have such effects:

[One] perspective suggests that crime and, in particular, violence against females is viewed as more harmful than crimes against male victims. This may be mitigated or aggravated by decision makers’ perceptions of the victim’s familial role and responsibilities. Furthermore, females are thought to engage in fewer behaviors that contribute to their victimization .... As a consequence, female victims may be perceived as less blameworthy for their own victimization ....

Williams & Holcomb, supra note 31, at 353 (citations omitted). Other research suggests that race and gender effects may also interact in significant ways. For instance, a study of 4,668 homicide cases from Oklahoma between 1990 and 2012 found that all female-victim cases presented an elevated likelihood of the death penalty, but that the likelihood was even higher with a white female victim than a minority female victim. Pierce, Radelet & Sharp, supra note 32, at 756 tbl. 11. Similarly, an Ohio study found that the odds of a death sentence were only about one-third as great when the victim was a black female, a black male, or a white male, in comparison with a white female. Williams & Holcomb, supra note 31, at 366 tbl. 4.

Christopher Mariera and co-authors have provided the most methodologically sophisticated recent study that focuses specifically on age. Mariera et al., supra note 33. Examining 1,189 capital cases that advanced to a penalty phase in North Carolina from 1977 to 2009, the researchers found a significant relationship between victim age and sentencing outcomes. Id. At 69. On average, the odds of a death sentence decreased with each additional year of the victim’s age by about 1 percent. Id. This finding suggests that the number of years of life lost by the victim may be a factor used by sentencing juries to assess the seriousness of a homicide—the more years of life lost, the more deserving the perpetrator is of the ultimate sanction. Id. By contrast, the findings are not consistent with the competing hypothesis that sentencing juries use age as a proxy for vulnerability, and are more punitive with defendants who prey on the very young or the very old. Id.
38 See, e.g., id. at 65 (reviewing prior studies; “In sum, the extant empirical literature on the influence of victim age on criminal case processing, especially with regard to capital charging and sentencing decisions, is very limited and quite mixed.”).

39 Donohue, supra note 30, at 673.

40 Id.

40 Schatz & Dalton, supra note 31, at 1255. For a summary of some of the relevant research, see id. at 1253–54.

41 See, e.g., O'Brien et al., supra note 33, at 2026 tbl. 4, 2045 App. A (in North Carolina study, showing significant relationship between likelihood of death sentence and “VoteSplit1”; defining “VoteSplit1” as indicating percent of county voting Republican in presidential elections from 1988 through 2008); Beckett & Evans, supra note 35, at 103 (in Washington study, showing a statistically significant relationship between likelihood of death sentence and percent of county voting Republican, albeit at a higher alpha level (p < 0.1) than typically employed in social science research (p < 0.05)); Glenn L. Pierce & Michael L. Radelet, The Impact of Legally Inappropriate Factors on Death Sentencing for California Homicides, 1990–1999, 46 Santa Clara L. Rev. 1, 38 (2005) (finding, in California study, that “[w]hen the effects of all variables are considered simultaneously, death sentencing rates are lowest in counties with the highest non-white population”); Glenn L. Pierce & Michael L. Radelet, Race, Religion, and Death Sentencing in Illinois, 1988–1997, 81 Or. L. Rev. 39, 65 (2002) (in Illinois study, finding death sentence significantly more likely in rural counties than in Cook County (Chicago)).

42 See, e.g., O'Brien et al., supra note 33, at 2026 tbl. 4 (in North Carolina study, showing significant relationship between likelihood of death sentence and percent black in county; that is, race effect is opposite of what is found in some other studies); Pierce & Radelet, supra note 41, at 35 (in California study, finding no significant relationship between likelihood of a death sentence and a county population density measure); Williams & Holcomb, supra note 31, at 365 (in Ohio study, finding no significance in relationship between likelihood of death sentence and whether crime was committed in urban area); David C. Baldus et al., Arbitrariness and Discrimination in the Administration of the Death Penalty: A Legal and Empirical Analysis of the Nebraska Experience (1973–1999), 81 Neb. L. Rev. 486, 626 (2002) (in Nebraska study, finding no significance in relationship between likelihood of death sentence and whether crime was committed in urban area).

43 Baldus et al., supra note 42, at 639.


45 Id.

46 Baldus et al., supra note 42, at 636–37.

47 Id. at 637.

48 Phillips, supra note 33, at 823 tbl. 3. Indeed, the odds of a death sentence nearly tripled with an appointed lawyer. A similar result was found in a study of Pennsylvania. Jeffrey T. Ulmer, Criminal Courts as Inhabited Institutions: Making Sense of Difference and Similarity in Sentencing, 48 Crime & Just. 483, 500 (2019).

49 Examples include whether the offense involved rape, see, e.g., Mariera et al., supra note 33, at 69 (in North Carolina study, finding association between rape and death penalty); but see Phillips, supra note 33, at 823 (in study of Harris County, Texas, finding no such association); whether there were multiple victims, see, e.g., Pierce, Radelet & Sharp, supra note 32, at 749 (in Oklahoma study, finding association between multiple victims and death penalty); but see Beckett & Evans, supra note 35, at 100 tbl. 6 (in study of Washington State, finding no such association); whether the defendant was a stranger, see, e.g., O'Brien et al., supra note 33, at 2026 tbl. 4 (in North Carolina study, finding association between stranger crime and death penalty); but see Mariera et al., supra note 33, at 69 tbl. 2 (in different study of North Carolina, finding no significance to victim-defendant relationship); and whether the victim was criminally involved, see, e.g., Mariera et al., supra note 33, at 68 (in North Carolina study, finding inverse association between criminal involvement and death penalty, i.e., death penalty less likely when victim
criminally involved); but see Phillips, supra note 33, at 823 (in study of Harris County, Texas, finding no association between victim’s criminal record and death penalty).

50 See, e.g., O’Brien et al., supra note 33, at 2026 tbl. 4, 2045 App. A (in North Carolina, finding significance to “AggE3”; defining “AggE3” as “Defendant previously convicted of a violent felony”); Phillips, supra note 33, at 823 (in Harris County, finding significance to prior violent conviction, but no significance to prior nonviolent).

51 On the one hand, geographic disparities in sentencing might be defended, for instance, on the ground that punishment appropriately reflects the specific local needs and values of the community in which the crime occurred and that was presumably most directly affected by the offense. On the other hand, place of prosecution does not have an entirely clear, direct relationship to core purposes of sentencing (just deserts and crime prevention). Moreover, it might be argued that a death-penalty system that is established by state-level legislators, administered by state-level corrections officials, and paid for by state taxpayers should operate in a uniform manner across the state. To assess the propriety of geographic disparities, it might help to have a clearer sense of the underlying causes of county-to-county variation in sentencing patterns than is available through the existing research.


54 King & Light, supra note 52, at 366.

55 Id. at 370–71.

56 Id. at 373.

57 Id. at 378. Related to evidence of racial disparities in sentencing, a small literature explores the impact of darker skin tone and “afrocentric” facial features. The research finds that these race-connected personal characteristics are associated with longer sentences, even when race itself is controlled. See Ryan D. King & Brian D. Johnson, A Punishing Look: Skin Tone and Afrocentric Features in the Halls of Justice, 122 Am. J. Socio. 90, 106 (2016); William T. Pizzi, Irene V. Blair & Charles M. Judd, Discrimination in Sentencing on the Basis of Afrocentric Features, 10 Mich. J. Race & L. 327, 332 (2005). Thus, for instance, lighter-skinned blacks tend to fare better at sentencing than darker-skinned blacks, while lighter-skinned whites tend to fare better than darker-skinned whites. King & Johnson, supra, at 108, 110. The research on Hispanic ethnicity points to much the same patterns as with race. King & Light, supra note 52, at 371. Indeed, as with race, the ethnicity effects interact in important ways with gender and age. More specifically, the disparities are greatest with respect to young Hispanic males. Jeffery T. Ulmer & Kaitlyn Konefal, Sentencing the “Other”: Punishment of Latinx Defendants, 66 UCLA L. Rev. 1716, 1720 (2019). Notably, though, the ethnicity disparities are also intertwined with disparities in the area of citizenship, that is, the tendency for non-U.S. citizens to receive harsher sentences than otherwise similar citizen defendants. Indeed, efforts to disentangle the two effects have found that “citizenship explains the majority of the punishment gap between Hispanic and white offenders.” King & Light, supra note 52, at 424. For instance, one study of federal sentences in 2006 found that Hispanic U.S. citizens actually received shorter sentences on average than non-Hispanic whites, but that undocumented immigrants received sentences that were on average 13 percent longer. Ulmer & Konefal, supra, at 1722. Some research suggests that judges view noncitizens “as more morally blameworthy and offensive in that they [a]re seen as having violated the hospitality of the United States by committing crimes.” Id.


59 Doerner, supra note 58, at 314. Several studies find that gender effects vary by defendant race and ethnicity. See Jeffrey S. Nowacki, An Intersectional Approach to Race/Ethnicity, Sex, and Age Disparity in Federal Sentencing Outcomes: An Examination of Policy Across Time Periods, 17 Criminology & Crim. Just. 97, 101 (2017) (citing studies). Indeed, some research indicates that “black women are sentenced about as harshly as white men.” Id.


61 Jeffery T. Ulmer, Recent Developments and New Directions in Sentencing Research, 29 Just. Q. 1, 18 (2012). Notably, the gender disparities interacted with race and ethnicity disparities. Those who victimized white females and Hispanic females received the longest sentences. Id.

62 Mariera et al., supra note 33, at 64.


64 See, e.g., Eric P. Baumer & Kimberly H. Martin, Social Organization, Collective Sentiment, and Legal Sanctions in Murder Cases, 119 Am. J. Socio. 131, 158 (2013) (in study of murder cases in large urban counties, finding no significant relationship between sentence severity and victim race, ethnicity, gender, or age).

65 Ulmer, supra note 61, at 14.

66 See, e.g., Rhys Hester & Eric L. Sevigny, Court Communities in Local Context: A Multilevel Analysis of Felony Sentencing in South Carolina, 39 J. Crime & Just. 55, 62 (2016) (in study of South Carolina felony sentencing finding that 2.5 percent of variation in decision to incarcerate was attributable to county-level factors).

67 For instance, some studies find significance to concentrated socioeconomic disadvantage. See, e.g., id. at 73 (in study of South Carolina juvenile cases, finding greater use of confinement in counties with more concentrated disadvantage); Kathleen Auerhahn et al., Are You Judged by the Residence You Keep? Homicide Sentencing, Attribution and Neighborhood Context, 18 Criminology, Crim. Just., L. & Soc'y 28, 41 tbl. 3 (2017) (finding significant association between sentence length in homicide cases and the level of disadvantage in the defendant’s home neighborhood within the city of Philadelphia). But see Hester & Sevigny, supra note 66, at 66 (in study of South Carolina sentencing, finding no significance to changes in concentrated disadvantage); Elsa Y. Chen, Is All Punishment Local? The Effects of Jurisdictional Context on Sentence Length, 94 Soc. Sci. Q. 1372, 1385 (2013) (in study of California sentencing, finding no significance to county unemployment rate); Jeffrey T. Ulmer & Brian Johnson, Sentencing in Context: A Multilevel Analysis, 42 Criminology 137, 161 tbl. 4 (2004) (in study of Pennsylvania sentencing, finding no significance to poverty rate). Likewise, some studies find measures of “urbanness” as significant covariates in their models. See, e.g., Patrick G. Lowery, John D. Burrow & Robert J. Kaminski, A Multilevel Test of the Racial Threat Hypothesis in One State’s Juvenile Court, 64 Crime & Delinquency 53, 73 (2018) (in study of South Carolina juvenile cases, finding greater use of confinement in counties with more urban population). But see Meghan Sacks & Alissa R. Ackerman, Bail and Sentencing: Does Pretrial Detention Lead to Harsher Punishment?, 25 Crim. Just. Pol'y Rev. 59, 70 (2014) (finding no statistically significant relationship between county urbanness and sentencing in New Jersey net of the other factors in the models). Other studies focus on violent crime rate. See, e.g., Chen, supra, at 1385 (in study of California sentencing, finding county violent crime rate to be significant predictor of sentence length, with higher violent crime rates associated with shorter sentences); Cooney & Burt, supra note
63, at 511 (in study of homicide cases in large urban counties in 1988, finding significantly shorter sentences in counties with lower homicide rates). Still others highlight the potential importance of political orientation. See, e.g., Ulmer & Johnson, supra, at 161 tbl. 4 (in study of Pennsylvania sentencing, finding that prevalence of Republicans in county was significant predictor of sentencing outcomes). But see Ulmer, supra note 61, at 15 (“broad political climate measures (such as percent Republican voters) have generally not been found to be strong predictors of sentencing patterns”); Michael Cassidy & Jason Rydberg, Analyzing Variation in Prior Record Penalties Across Conviction Offenses, 64 Crime & Delinquency 831, 843 tbl. 2 (2018) (in Pennsylvania study, finding no significant effects of county political orientation). Much of the work on the influence of place has focused on the so-called racial threat theory. The theory predicts that discrimination against a minority group will grow as the group comes to represent a larger percentage of the local population, and hence presents a greater threat to the economic and political domination of the majority group. King & Light, supra note 52, at 403. Such discrimination may be manifest in a number of different ways, including, some have hypothesized, in more punitive responses to minority crime. However, few sentencing studies have found statistically significant correlations between a growing black population and widening racial disparities in punishment. See id. at 406–07 (discussing studies). Similarly, the evidence fails to provide much support for the analogous “Hispanic threat theory” in the sentencing sphere. Id. at 407.

68 Cassidy & Rydberg, supra note 67, at 834.

69 See, e.g., King & Johnson, supra note 57, at 105 (in study of Twin Cities area, finding no statistically significant relationship between sentences and type of defense lawyer); Erin A. Orrick & Alex R. Piquero, Assessing the Impact of Mexican Nativity on Sentence Length, 26 Crim. Just. Pol’y Rev. 643, 653 (2015) (in study of state and federal inmates, finding statistically significant inverse relationship between sentence length and having a public defender in drug and property crime cases; no statistically significant relationship in violent crime cases); John Wooldredge et al., Is the Impact of Cumulative Disadvantage on Sentencing Greater for Black Defendants? 14 Criminology & Pub. Pol’y 187, 209 (2015) (in study of sentencing in one city, finding no statistically significant relationship between sentences and type of defense lawyer); Kareem L. Jordan, Juvenile Status and Criminal Sentencing: Does it Matter in the Adult System? 12 Youth Violence & Juvenile Just. 315, 325 (2014) (in national study of sentencing in state courts, finding that having public defender was correlated with longer jail sentences, but shorter prison sentences); Sacks & Ackerman, supra note 67, at 70 (in study of sentencing in New Jersey, finding no statistically significant relationship between sentence length and type of defense lawyer); Ulmer, supra note 61, at 16–17 (discussing study in Chicago finding greater trial penalty for defendants represented by private attorneys); Auerhahn et al., supra note 67, at 41 tbl. 3 (in study of homicide cases in Philadelphia, finding no significant association between sentence length and attorney type).


71 Id. The researchers also found that the judge-prosecutor “dyad” was an even more powerful predictor of outcomes than just the identity of the judge or prosecutor standing alone. Id.


73 Kim, Spohn & Hedberg, supra note 70, at 601–02.

74 See King & Light, supra note 52, at 413 (summarizing prior studies).

75 See O’Hear, supra note 11, at 131; Ulmer, supra note 48, at 497.

76 See Nadel, Scaggs & Bales, supra note 72, at 849 (citing studies). At least one study focused on Wisconsin judges, albeit at the supreme court level and in relation to decisions in criminal cases generally, not just sentencing decisions. See Jason J. Czarnezki, Voting and Electoral Politics in the Wisconsin Supreme Court, 87 Marq. L. Rev. 323, 347 (2003) (“Justices Abrahamson, Wilcox, and Steinmetz all display statistically significant changes in voting patterns in the last two years of terms compared to the votes made in initial years of a term.”).
77 Carlos Berdejo, & Noam Yuchtman, Crime, Punishment, and Politics: An Analysis of Political Cycles in Criminal Sentencing, 95 Rev. Econ. & Statistics 741, 742 (2013). Notably, the researchers found no increased sentence lengths for less serious crimes, which presumably are of relatively little interest to voters, or with respect to retiring judges in their final term in office. Id.

78 Johnson, supra note 26, at 313–14.

79 Id. at 314. Although the preferential treatment given to defendants who plead guilty is sometimes justified on the view that these defendants are more remorseful and present better prospects for rehabilitation, the more widely accepted explanation is that judges and prosecutors want to encourage and reward pleas that save the state the expense, trouble, and uncertainty of a jury trial. Id. at 320–21. Most studies assess mode of conviction dichotomously—either a guilty plea or a trial conviction—but one recent study of Pennsylvania undertook a more nuanced comparison of a fuller range of options. Freiburger, Jordan & Hilinski-Rosick, supra note 60. In comparison with cases involving a negotiated guilty plea, the researchers found shorter prison sentences in the cases with a non-negotiated guilty plea, longer prison sentences in cases with conviction after a bench trial, and still longer sentences in cases with conviction after a jury trial. Id. at 1076. The researchers found no statistically significant difference in the prison terms following a no-contest plea versus a negotiated guilty plea. Id. Consistent with the hypothesis that defendants who go to trial are “punished” based on the burdens their decision creates for the state, other studies have also found a more severe trial tax for jury than for bench trials. Johnson, supra note 26, at 345. In addition to mode of conviction, there may be other case-processing variables that affect sentences. For instance, in a study of terrorism-related cases, researchers found a significant relationship between sentence length and the amount of time between indictment and conviction.

Mindy S. Bradley-Engen et al., The Time Penalty: Examining the Relationship Between Time to Conviction and Trial vs. Plea Disparities in Sentencing, 29 Just. Q. 829, 848 (2012). Controlling for mode of conviction and a variety of offense and offender variables, the researchers determined that each 1 percent increase in time to conviction was associated with a 3 percent increase in sentence length. Id. This finding, in conjunction with the trial-penalty research, suggests that punishment may depend in part on how burdensome the defendant makes the litigation process for the state.

80 See Graham V. Florida, 560 U.S. 48, 70 (2010) (“life without parole sentences share some characteristics with death sentences that are shared by no other sentences”).


90 See D.C Code Ann. § 22-2104(a) (West 2020) (“The punishment for murder in the first degree shall be not less than 30 years nor more than life imprisonment without release ....”); 730 Ill. Comp. Stat. Ann. 5/5-4.5-20(a) (West 2020) (“Imprisonment shall be for a determinate term ... of (1) not less than 20 years and not more than 60 years; ... or (3) natural life ... “); Me. Rev. Stat. Ann. tit. 17-A, § 1603 (West 2020) (“A person convicted of the crime of murder must be sentenced to imprisonment for life or for any term of years that is not less than 25.”); Md. Code Ann., Crim. Law § 2-201(b) (1) (West 2020) (“A person who commits a murder in the first degree is guilty of a felony and on conviction shall be sentenced to: (i) imprisonment for life without the possibility of parole; or (ii) imprisonment for life.”); N.Y. Penal Law §
60.06 (McKinney 2020) ("When a defendant is convicted of murder in the first degree ..., the court shall ... sentence the defendant to death, to life imprisonment without parole ... or to a term of imprisonment for a class A-I felony other than a sentence of life imprisonment without parole ....."); N.D. Cent. Code Ann. § 12.1–32-01 (West 2020) ("Offenses are divided into seven classes, which are denominated and subject to maximum penalties, as follows: 1. Class AA felony, for which a maximum penalty of life imprisonment without parole may be imposed. The court must designate whether the life imprisonment sentence imposed is with or without an opportunity for parole."); 11 R.I. Gen. Laws Ann. § 11–23-2 (West 2020) ("Every person guilty of murder in the first degree shall be imprisoned for life. Every person guilty of murder in the first degree [when one of seven aggravating circumstances is present] shall be imprisoned for life and if ordered by the court pursuant to chapter 19.2 of title 12 that person shall not be eligible for parole from imprisonment."); Vt. Stat. Ann. tit. 13, § 2303(a)(1) (West 2020) ("The punishment for murder in the first degree shall be imprisonment for: (A) a minimum term of not less than 35 years and a maximum term of life; or (B) life without the possibility of parole."). Note that although the New York statute provides for a death option, the courts have barred the death penalty in the state. See State by State, supra note 89. England and Wales also use a similar system for murder sentencing, with a mandatory life sentence and a judicial decision as to whether and when the defendant will be eligible to be considered for release. Barry Mitchell & Julian V. Roberts, Exploring the Mandatory Life Sentence for Murder 39 (2012).

91 See Alaska Stat. Ann. § 12.55.125 (West 2020) ("A defendant convicted of murder in the first degree ... shall be sentenced to a definite term of imprisonment of at least 30 years but not more than 99 years."); Conn. Gen. Stat. Ann. § 53a-35a (West 2020) ("For the class A felony of murder, a term not less than twenty-five years nor more than life."); § 53a-35b ("Life imprisonment means a definite sentence of sixty years, unless the murder is a capital offense, in which case the sentence shall be imprisonment for the rest of the defendant’s natural life.").

92 See Nellis, supra note 8, at 9 ("Given that the average age of admission to prison for those convicted of serious crime is in one’s mid-to-late thirties, we establish that a maximum sentence of at least 50 years before parole is equivalent to life in prison.").

93 These states are California, Cal. Penal Code § 190 (West 2020) ("Every person guilty of murder in the first degree shall be punished by death, imprisonment in the state prison for life without the possibility of parole, or imprisonment in the state prison for a term of 25 years to life."); Georgia, Ga. Code Ann. § 16–5-1(e)(1) (West 2020) ("A person convicted of the offense of murder shall be punished by death, by imprisonment for life without parole, or by imprisonment for life."); Indiana, Ind. Code Ann. § 35–50–2–3 (West 2020) ("A person who commits murder shall be imprisoned for a fixed term of between forty-five (45) and sixty-five (65) years .... Notwithstanding the above provision, a person who was: (1) at least eighteen (18) years of age at the time the murder was committed may be sentenced to: (A) death; or (B) life imprisonment without parole."); Kentucky, Ky. Rev. Stat. Ann. § 532.025 (West 2020) (noting sentencing options of “death, or imprisonment for life without benefit of probation or parole, or imprisonment for life without parole if the defendant has served a minimum of twenty-five (25) years”); Mississippi, Miss. Code Ann. §97–3-21(3) (West 2020) ("Every person who shall be convicted of capital murder shall be sentenced (a) to death; (b) to imprisonment in the State Penitentiary without parole; or (c) to imprisonment for life in the State Penitentiary with eligibility for parole ...."); Montana, Mont. Code Ann. § 45–5-102(2) (West 2020) ("A person convicted of the offense of deliberate homicide shall be punished by death ..., by life imprisonment, or by imprisonment in the state prison for a term of not less than 10 years or more than 100 years ...."); Oklahoma, Okla. St. Ann. tit. 21, § 701.9(A) (West 2020) ("A person who is convicted of or pleads guilty or nolo contendere to murder in the first degree shall be punished by death, by imprisonment for life without parole or by imprisonment for life."); Oregon, Or. Rev. Stat. Ann. § 163.115(5) (West 2020) ("(a) A person convicted of murder, who was at least 15 years of age at the time of committing the murder, shall be punished by imprisonment for life. (b) When a defendant is convicted of murder ... under this section, the court shall order that the defendant shall be confined for a minimum of 25 years without possibility of parole ....");
South Carolina, S.C. Code Ann. § 16-3-20 (B) (2020) (“If no statutory aggravating circumstance is found, the defendant must be sentenced to either life imprisonment or a mandatory minimum term of imprisonment for thirty years to life.”); Tennessee, Tenn. Code Ann. § 39-13-204 (West 2020) (“[T]he jury shall fix the punishment in a separate sentencing hearing to determine whether the defendant shall be sentenced to death, to imprisonment for life without possibility of parole, or to imprisonment for life.”); Texas, Tex. Code Crim. Proc. Ann. art. 37.071(1) (a) (West 2020) (“If a defendant is found guilty in a capital felony case in which the state does not seek the death penalty, the judge shall sentence the defendant to life imprisonment or to life imprisonment without parole ...”); Utah, Utah Code Ann. § 76-3-207.7(2)(a) (West 2020) (“The sentence under this section shall be: (i) life in prison without parole; or (ii) an indeterminate prison term of not less than 25 years and that may be for life.”); and Wyoming, Wyo. Stat. Ann. § 6–2–101(b) (West 2020) (“A person convicted of murder in the first degree shall be punished by death, life imprisonment without parole or life imprisonment according to law.”).

95 567 U.S. 460, 479 (2012).
96 See, e.g., Alaska Stat. Ann. § 13A-6-2(c) (West 2020) (establishing penalty for aggravated murder by juvenile as LWOP or life with a minimum of 30 years before parole); Iowa Code Ann. § 902.1(2)(a) (West 2020) (establishing penalty for murder by juvenile as life either with or without parole); Mich. Comp. Laws Ann. § 769.25(9) (West 2020) (“If the court decides not to sentence the [juvenile] individual to imprisonment for life without parole eligibility, the court shall sentence the individual to a term of imprisonment for which the maximum term shall be not less than 60 years and the minimum term shall be not less than 25 years or more than 40 years.”); 18 Pa. Stat. and Cons. Stat. Ann. § 1102.1 (West 2020) (establishing penalty for juveniles over age of 15 as LWOP or imprisonment not less than 35 years); Wash. Rev. Code Ann. § 10.95.030(3)(a)(ii) (West 2020) (“Any person convicted of the crime of aggravated first degree murder for an offense committed when the person is at least sixteen years old but less than eighteen years old shall be sentenced to a maximum term of life imprisonment and a minimum term of total confinement of no less than twenty-five years. A minimum term of life may be imposed, in which case the person will be ineligible for parole or early release.”).
98 The data were downloaded from the DOC’s offender locator website, Locator, Wis. Dep’t Corr., available at https://appsdoc.wi.gov/lop/.
99 We could not rely solely on prisoners facing life-sentences because it is possible for a Wisconsin defendant to receive a life sentence under the state’s version of “three strikes and you are out” without a Section 940.01 conviction. See Wis. Stat. Ann. § 939.62(2 m)(c) (West 2020) (requiring LWOP for “persistent repeaters”). Likewise, we could not solely rely on selecting cases based on a Section 940.01 conviction because DOC records use this statutory reference for people who were convicted of both completed and attempted first-degree intentional homicide. The attempt-only defendants are sentenced under a different law and do not recieve life sentences. See Wis. Stat. Ann. § 939.32(1)(a) (West 2020) (indicating that attempt to commit crime that carries life sentence results in conviction of Class B felony).
100 See Consolidated Court Automation Program, supra note 97 (discussing history of CCAP).
101 Missing defendants could potentially include any who died in prison during our time period, any who were serving a sentence on a different offense in another jurisdiction’s prison system, any who were convicted toward the start of our time period and received an extraordinary amount of time-served credit as a result of protracted jailing while their cases were pending, and any who managed to have their convictions overturned on appeal and who secured release on that basis before mid-2018. We would expect very few defendants to fall into these categories. To confirm the completeness of our data, we conducted a Westlaw search of all appeals in first-degree intentional homicide cases in 2001 or later. We found just four additional cases that were not included in our data. Google searches revealed...
that two of the four defendants were deceased. In another one of the four cases, the conviction was overturned on appeal. For the fourth, we were not able to determine a reason why the defendant was not in prison in August 2018. In any event, since we did not have DOC data on any of these four defendants, we did not add them to the 455. However, given the small number of these “missing cases” (0.09 percent of the overall sample), it is unlikely that excluding those cases from our analysis would have a substantive impact on our findings. We note that we cannot rule out the possibility that there may have been a few additional convicted first-degree intentional homicide defendants in our time period who did not appeal and were not in prison in August 2018, for example, due to death in custody. If an individual had more than one case in our time period with a first-degree intentional homicide conviction, we used only the most recent case. If the most recent case had more than one first-degree intentional homicide conviction, we focused our analysis on just the first-listed conviction count in CCAP. Although our study does cover many years of intentional homicide sentencing, there is little indication of large shifts in public attitudes toward crime and punishment in Wisconsin over the time period. For instance, there have been few, if any, durable changes to state sentencing law of widespread practical importance since Wisconsin’s enactment of “truth in sentencing” in 1998. (For an overview of the recent history of state sentencing law, see Michael O’Hear, Wisconsin Sentencing in the Walker Era: Mass Incarceration as the New Normal, 30 Fed. Sent’ing Rptr. 125 (2017.) Survey research also finds considerable continuity in public opinion. See, e.g., Michael O’Hear & Darren Wheelock, 103 Marq. L. Rev. 1035, 1048 n.52 (2020) (noting identical levels of support for early release option for prisoners in 2014 and 2018 polls of Wisconsin voters).

102 Wis. Stat. Ann. § 973.014(1) (West 2020) (indicating that judicial release decision is only made for offenses occurring on or after July 1, 1988).

103 The mechanism by which a life-sentenced inmate is considered for release varies depending on the date of the offense. For offenses committed on or after December 31, 1999, the inmate must petition the original sentencing court for release. Wis. Stat. Ann. § 973.014(1 g) (West 2020). For offenses committed on or after August 31, 1995, but before December 31, 1999, the inmate petitions the Wisconsin Parole Commission. Wis. Stat. Ann. § 973.014 (1) (West 2020). For offenses occurring between July 1, 1988, and August 31, 1995, the sentencing judge is required to set a parole eligibility date. Wis. Stat. Ann. § 973.014(1)(c) (West 2020). As a formal matter, this means that LWOP is not available for these offenses, but the judge could set the parole eligibility date so far in the future as to establish a de facto LWOP sentence.

104 See Graham v. Florida, 560 U.S. 48, 70 (2010) (“[LWOP] deprives the convict of the most basic liberties without giving hope of restoration, except perhaps by executive clemency—the remote possibility of which does not mitigate the harshness of the sentence”).

105 The only conceptual difficulty we encountered with this coding related to the small number of cases in our data arising from offenses that occurred between July 1, 1988, and August 31, 1995. In these four cases, the judge did not formally have the option of imposing an LWOP sentence, but could select a de facto LWOP sentence by deferring parole eligibility for so long that the defendant would not likely ever be able to seek release. See note 104. In the four cases, the judges ruled that the defendants could apply for parole in 31, 31, 35, and 77 years, respectively. The first three numbers are well within the norms we found for potential release in other cases in which the defendant was made eligible. However, the fourth is significantly longer than any other number we found in cases in which the judge chose Life With. We view the 77-year figure as de facto LWOP and coded it accordingly, that is, as 0. The other three sentences for pre-1995 offenses were coded as 1. A practical difficulty encountered in a small number of cases was that CCAP did not indicate what the sentencing judge decided about ES eligibility. In all these cases, we were able to find the information from other sources, primarily news media coverage and court documents in the Westlaw database.

We used DOC data to determine criminal history. Note that the DOC records would omit minor cases that did not result in a sentence to DOC custody or supervision, as well as criminal cases that were not prosecuted in Wisconsin state court.

These are the most serious classes of felonies in the Wisconsin felony classification system, which ranges from A (most serious) to I (least serious). Wis. Stat. Ann. § 939.50 (West 2020). Class A felonies result in a mandatory life term, while Class B felonies may result in imprisonment for up to 60 years. Wis. Stat. Ann. § 939.50(3)(a)–(b) (West 2020). We include here both completed Class A and B felonies and attempts, which are not distinguished in the DOC data.

The data are available from the authors upon request. In a minority of our cases, we were unable to confidently identify a matching SHR. In these cases, we were able nonetheless to determine the number of victims either by reviewing the factual summary of the case that was presented in a judicial opinion available on Westlaw or by searching media coverage of the case.

If the defendant had more than one defense counsel of record at the time of sentencing, we identified one lawyer as lead counsel based on indications in CCAP about which lawyer spoke on behalf of the defendant at sentencing. If CCAP did not include such information, then the lawyer with the earlier law school graduation date was assumed to be lead counsel on the basis of seniority.

If CCAP identified more than one prosecutor as having responsibility for the case, then the lead prosecutor for sentencing purposes was identified based on who spoke for the state at sentencing. If CCAP did not include such information, and one prosecutor who appeared at sentencing was the district attorney, then that prosecutor was assumed to be lead counsel. Otherwise, lead counsel was identified based on time out of law school. If CCAP did not indicate who appeared at sentencing for the state, but did indicate who appeared at trial or the guilty-plea hearing, then it was assumed that the lead prosecutor at either of those proceedings would most likely also have been lead prosecutor for sentencing purposes.

In a few cases, the prosecutor was identified on the formal judgment of conviction, which was available on Westlaw, or by reviewing the sentencing transcript, which was obtained from the relevant courthouse.

The case was coded as having appointed counsel if there was any indication in CCAP of the court appointing a lawyer any time before sentencing. In any case lacking any such explicit indication of court appointment in CCAP, it was assumed that the defendant had privately retained counsel. However, given the sporadic recording of case occurrences in the CCAP record of some of the older cases in our dataset, it is possible that there were a few cases in which the court did appoint counsel but the fact was not noted in CCAP.

Coding the gender variables for court actors requires explanation. Although the CCAP data include the names of judges and lawyers, they do not contain any demographic information on these individuals. To identify the gender of the sentencing judge for each case, the name of the judge was searched for on ballotpedia.org. This website provides biographical information on elected officials throughout the United States. Gender was not explicitly listed in each judge’s biography so we relied on pronouns in each description, such as “he” and “she” to determine the gender of the judges. For the small remainder of names that could not be found on the website, we assigned a gender to each person based on first names, which were nearly all recognizable as belonging exclusively to a male or female according to gender.checker.com. Identifying the gender of public defenders and prosecutors proved to be a bit more challenging since we could not utilize ballotpedia.org. For the majority of names, we could identify gender with gender.checker.com. We encountered some names, however, where the gender match was not definitive. In these cases, we tracked down media reporting about the case. Given the tendency of the news media to focus on high-profile violent crimes, we found news media reporting on the vast
In majority of these cases, which typically included information on the lead prosecutor and defense lawyer, including pictures and pronoun references to these individuals. We also conducted double-blind verification to validate the first two steps. This consisted of two researchers/research assistants reviewing randomized subsamples of cases and identifying the defense counsel or prosecutor’s gender by name. This led to a 99.77 percent match on gender identification through names.

121 Lawyer Search, State Bar of Wis., available at https://www.wisbar.org/Pages/BasicLawyerSearch.aspx.

122 For most judges, we were able to find this information on ballotpedia.org. For a few, we had to rely on news media sources or Westlaw.

123 For most judges, we were able to find prejudicial employment information on ballotpedia.org. For a few, we had to rely on news media sources or Westlaw.

124 In three cases, the DOC data did not include defendant race. In these cases, we used the racial information from the connected SHR. As to age at time of offense, we used birth year information from the DOC and offense date information from CCAP. In 17 cases, CCAP omitted the offense date, but we were able to find that information from media coverage, Westlaw documentation, or SHRs.

125 We also ran the analysis with white, black, and “other” as a third category, but the results did not substantively change.

126 See note 114 and accompanying text.

127 We switched the coding for race of victim compared to the race of the defendant to provide a clear test of the proposition that offenders who kill white victims are subject to greater criminal penalties.


130 UCR Offense Data, Wis. Dep’t Just., available at https://www.doj.state.wi.us/dles/bjia/ucr-offense-data. For the older years in our time period, we had to rely on paper copies of annual crime reports produced by the Wisconsin Department of Justice or the Wisconsin Office of Justice Assistance.


132 Binary logit regression models provide coefficient estimates for dependent variables that violate assumptions of linear continuous measures needed for OLS models. Binary logit models can model the normally distributed logit function of a binary variable.

133 Model overfitting can occur when researchers include too many parameter estimates relative to the number of cases in the analysis. Over-fit models can create concerns with model generalizability. Anastasiya Motrenko, Vadim Stijov & Gerhard-Wilhelm Weber, Sample Size Determination for Logistic Regression, 255 J. Computational & Applied Mathematics 743 (2014). We also conducted additional analysis on the potential impact of time, under the assumption that changing policies, public sentiment, or some other unmeasured time-bound factor could also impact the likelihood of a “Life With” sentence. We conducted a series of bivariate and multivariate tests for this possibility and did not find any evidence that time was relevant to explain variation across the dependent measure. Given the stability of sentencing policy and public opinion during the period of observation discussed in note 101, we did not find this surprising.


135 Since individuals are nested within counties in our data structure, the use of binary logistics regression in our full models is potentially problematic. On average, there were eight homicide cases during the period of observation per county (with a minimum value of one and a maximum value of 179, which was Milwaukee County). The potential for correlated errors is substantial with multilevel data so we corrected for the potential deflation of standard errors by estimating binary logistic random effects models in STATA 14. While typically utilized for longitudinal panel data, numerous researchers have also employed random effects and other HLM procedures for cross-sectional data where cases nest in
aggregate units. See, e.g., Ian Brunton-Smith, Patrick Sturgis & George Leckie, How Collective Is Collective Efficacy? The Importance of Consensus in Judgments About Community Cohesion and Willingness to Intervene, 56 Criminology 608, 617 (2018); Darren Wheelock, Meghan Stroshine & Michael O’Hear, Disentangling the Relationship Between Race and Attitudes Toward the Police: Police Contact, Perceptions of Safety, and Procedural Justice, 65 Crime & Delinquency 941, 950–51 (2018). A random effects model contains properties useful for the present data because it accounts for the error structure better than binary logistic regression. Random effects models include an error term with two components. One component represents the traditional error term unique to each observation and a second error term represents the difference between the cross-sectional units (counties in our data) and the intercept. Lois W. Sayrs, Pooled Time Series Analysis 32–51 (1989). The random effects model thus better accounts for within- and across-unit error relative to the basic binary logit models.


137 Nationally, at year-end 2018, black prisoners constituted 38 percent of the state prisoners who were serving time for murder; white prisoners, 23 percent; and Hispanic prisoners (any race), 23 percent. Carson, supra note 136, at 22. Note that these national data break out Hispanic ethnicity as a racial category distinct from black and white, whereas our data do not include any ethnicity classifications.

138 The mean age at sentencing is 32.41 years, suggesting that it takes, on average, nearly two years for defendants to be sentenced for their alleged offenses. The vast majority of cases were adjudicated within four years of the alleged offense (93.11 percent). A small proportion took longer than four years (6.89 percent). Some of these appear to be “cold case” scenarios. For instance, in six cases, the time between the offense and sentencing was in excess of 10 years, with a maximum gap of 16 years.

139 This figure was derived from the Wisconsin Supplemental Homicide Reports covering the years 2000–2019. The female proportion of the first-degree intentional homicide cases is greater than the overall state homicide norm in every single year of our time period. Although there is considerable year-to-year variation in our data (low of 24 percent female in 2002, high of 55 percent in 2010), there is no discernible pattern of consistent change over time.


141 A large majority of the individuals who are sentenced to prison for a homicide offense in Wisconsin have been convicted of an offense that is less severe than first-degree intentional homicide. Over the time period of 2001–2018, which closely corresponds to our observation period, 1,731 individuals were admitted to Wisconsin prisons for homicide offenses, which suggests that only about one in four homicide offenders is convicted of first-degree intentional. The cumulative data on prison admissions come from biennial information reports on adult corrections prepared by the Wisconsin Legislative Fiscal Bureau, which are available at https://docs.legis.wisconsin.gov/misc/lfb/informational_papers.

142 The latter finding may seem surprising insofar as we noted earlier that 18 percent of the cases were classified by police as involving a current or former intimate partner. The discrepancy may reflect differences in the assessment of the facts made by police investigators in comparison with prosecutors, or simply the view of many prosecutors that charging sentence enhancements is a gratuitous in cases that involve a mandatory life term.

143 This suggests an underrepresentation of women in our cases compared to national norms for the legal professional. Nationally, about one-third of judges in state courts of general jurisdiction are women. Nat’l Ass’n of Women Judges, 2019 State Court Women Judges, available at https://www.nawj.org/statistics/2019-us-state-court-women-judges. Meanwhile, about 36 percent of lawyers overall are women. Debra Cassens Weiss, Lawyer Count in US Increases 14.5% from Decade


145 In addition to the variables indicated in Table 1, the bivariate analysis also included: urban–rural character of county based on U.S. Department of Agriculture ranking, years from charging to sentencing, whether the killing was black-on-white, whether the killing was male-on-female, defendant age at sentencing, Republican percentage in the most recent presidential election year, whether the county DA had faced an opponent in his or her most recent election, year of sentencing, whether a firearm was used, whether the victim was under 17, and whether the victim was over 60.

146 See note 35 (hypothesizing reasons why victim race may affect capital-sentencing outcomes).

147 This bivariate relationship also holds when the number of victims measure is coded as a binary variable where (one victim = 0) and (more than one victim = 1). Defendants who had more than one victim were significantly less likely to receive Life With than defendants with only one victim. We credit a reviewer for pointing out that most homicide incidents involve one victim, so using a raw count measure may not capture the threshold distinction between one and more than one. The multivariate regression models were also estimated with a binary measure of number of victims, but we did not find a statistically significant relationship between that variable and Life With. This likely results from the fact that there are relatively few cases involving more than one victim (fewer than 15 percent). Hence, this measure is unable to account for a significant amount of variation in a multivariate context.

148 See Sections II.B and II.C.

149 It is not clear why this would be so, but we speculate that unarmed killings may tend to be extremely violent beatings that have an unusually gruesome character and/or to involve especially vulnerable victims, such as a child or elderly person. These circumstances may be viewed by sentencing judges as aggravating and thus warranting more severe punishment on average than armed killings.

150 This figure is typically interpreted as the proportion of the variance explained in linear models, but serves as a comparable overall model statistic for binary logit models.

151 See Section II.C.

152 See Section II.B.

153 Because the significance tests for the remaining variables replicate those performed for the full sample, we report our results in tabular form only for the full sample.

154 Cf. Chen, supra note 67, at 1385 (in study of California sentencing, finding county violent crime rate to be significant predictor of sentence length, with higher violent crime rates associated with shorter sentences).

155 See notes 41-42, 67.


158 To be sure, the rehabilitation point may be of limited practical significance in the context of a sentencing decision that involves a minimum of 20 years in prison, given that defendants in all age groups tend to diminish their criminal activity over time. See Michael O’Hear, Early Release for Prisoners Convicted of Violent Crimes: Can Anyone Escape the Incapacitation-Retribution Catch-22? 52 Conn. L. Rev. 653, 664–666, 668 (2020) (summarizing research).

159 With due regard to the need for caution in interpreting findings of non-significance, the non-significance of victim race in our study is especially striking in comparison with the frequent finding of victim-race effects in the capital-sentencing research. It is possible that this difference may reflect fundamental differences in the symbolic character of capital and noncapital sentencing, even when the noncapital proceeding involves the possibility of the extremely severe sentence of LWOP. We are grateful to an anonymous reviewer for suggesting this point.
160 See Wis. Stat. Ann. § 971.19(1) (West 2020) (“Criminal actions shall be tried in the county where the crime was committed, except as otherwise provided.”).


163 See, e.g., id. at 10 (arguing that the guidelines “were too severe, too complex, and too detailed,” and noting evidence that racial and other disparities increased under the guidelines); Stephen J. Schulhofer & Ilene H. Nagel, Plea Negotiations Under the Federal Sentencing Guidelines: Guideline Circumvention and its Dynamics in the Post-*Mistretta* Period, 91 Nw. U. L. Rev. 1284, 1289 (1997) (discussing phenomenon of guidelines circumvention in the federal system).


165 Cf. Gregg v. Georgia, 428 U.S. 153, 206 (1976) (plurality) (in affirming constitutionality of Georgia death penalty statute, observing, “The new Georgia sentencing procedures ... focus the jury’s attention on the particularized nature of the crime and the particularized characteristics of the individual defendant. While the jury is permitted to consider any aggravating or mitigating circumstances, it must find and identify at least one statutory aggravating factor before it may impose a penalty of death. In this way the jury’s discretion is channeled.”).


170 A similar, though not identical, list has been adopted in England and Wales to guide the use of LWOP as a sentence for murder. See Mitchell & Roberts, supra note 90, at 41. More specifically, the Criminal Justice Act of 2003 indicates that LWOP may be appropriate in the following circumstances: (1) two or more people murdered and either substantial planning or premeditation, abduction, or “sexual or sadistic conduct”; (2) child victim and child was either abducted or murder was sexually or sadistically motivated; (3) murder was motivated by advancement of political, religious, racial, or ideological cause; or (4) the defendant had a prior murder conviction. Id.


173 See Ford v. Wainwright, 477 U.S. 399, 410 (1986) (“The Eighth Amendment prohibits the State from inflicting the penalty of death upon a prisoner who is insane.”).

174 For instance, in the context of capital sentencing, the Supreme Court has held that there is a jury-trial right as to the aggravating factors that make a defendant eligible for the death penalty. Ring v. Arizona, 536 U.S. 584, 609 (2002).

