

A LONGITUDINAL STUDY OF THE EFFECT OF RACE ON SENTENCING

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Data covering a ten-year period in Milwaukee, Wisconsin, are used to estimate the effect of race and other variables on sentences given to persons charged with armed robbery and burglary. Longitudinal data, coupled with interviews of participants, are used to overcome problems of sample selection bias usually associated with cross-sectional studies of the determinants of sentencing. The research shows that race had a clear effect on both the decision to imprison and the length of prison terms in the earliest period (1967-1968) but not in the two later periods (1971-1972 and 1976-1977). The racial neutrality of sentencing in the later periods appears to be the result of changes in the composition of the judiciary, a greater bureaucratization of the prosecutorial and defense bar, and the rise of decision rules that reduce the effect of judicial ideology on outcomes.

The empirical evidence regarding the effect of race on the sentencing of convicted offenders is, at best, contradictory. Early studies, notably those dealing with capital punishment, seemed to show that blacks were much more likely to be treated severely than whites. But more recent studies, especially those analyzing the disposition of non-capital cases, suggest that race has an independent effect on sentences in some instances but not others and that its effect, where present, is much less than the effects of such legal factors as the nature of the offense and the prior record of the offender (Hagan, 1974; Hagan and Bumiller, 1983; Spohn *et al.*, 1981-1982). There is at least as much research indicating no racial effect on sentencing as there is research indicating such an effect. And even where an effect is found, it is too small by itself to account for the existing overrepresentation of blacks in prison (Blumstein, 1982).

I. METHODOLOGICAL ISSUES

These contradictory findings may be explained by the existence of behavioral differences (some judges in some jurisdictions impose racially discriminatory sentences and

LAW & SOCIETY REVIEW, Volume 17, Number 4 (1983)

others do not), or by methodological problems that render the results of different studies non-comparable, or both. The research reported here is an effort to deal simultaneously with the possibility of behavioral differences and the reality of methodological defects. To examine the hypothesis that race may affect sentences by certain judges at certain times and not others, we analyze the sentences issued over a ten-year period in a single jurisdiction to see if the passage of time and changes in the composition of the bar and bench were associated with changes in the effect race had on sentences, independent of other factors. To avoid some of the methodological problems that have plagued many previous studies, we attempt to deal with three of the most common: measurement errors, specification errors, and sample selection bias.

Measurement Errors

If variables included in an equation designed to model some aspect of the criminal justice process are inaccurately measured, the results of the equation will be biased. The direction of the bias will vary depending on whether the error in measurement is random or is positively or negatively correlated with the observed value of the variable (Klepper *et al.*, 1983). Some variables, such as sex and race, are likely to be measured with little error. Other variables—and for our purposes, some of the most important ones—are likely to be measured with substantial error. Almost all research suggests that the seriousness of the offense and the prior record of the offender are the most important determinants of sentences. Neither variable can be observed directly, and available data (the written reports of police officers, prosecutors, probation officers, and correctional officials) are typically in the form of summary documents that often omit or at best give incomplete accounts of matters that affect the probability of arrest, prosecution, conviction, and imprisonment. There is no solution to this problem (what is not recorded in the original data cannot later be observed by the analyst of that data); at best, we can only approximate the true seriousness of the offense and the record of the offender by including as many elements from the available data as possible. From our data it was possible to glean the following elements of the offense: legal definition (armed robbery or burglary), whether a weapon was used, whether injury occurred, whether there was a prior relationship between the offender and the victim, and whether the crime involved a commercial establishment, a residence, or

a person. As will become evident when we consider the results of our estimation, several of these variables are highly significant and failure to consider them (as has often been the case in prior studies) would bias the estimate of the effect of race (Pope, 1975; Burke and Turk, 1975).

We were able to obtain the following measures of the prior record of the offender: number of prior arrests as a juvenile and an adult, number of prior convictions, and number of prior incarcerations. These data were summarized in the form of a four-point scale, based on one developed by Greenwood *et al.* (1973).

An important but usually neglected form of measurement error attaches to the dependent variable. Some studies use the decision to incarcerate as the relevant outcome (Pope, 1975), others consider the length (or the minimum length) of sentences given to those who are incarcerated (Chiricos and Waldo, 1975; Levin, 1977; Lizotte, 1978), and still others attempt to combine various non-jail dispositions and prison terms of varying length into a single scale (Bernstein *et al.*, 1977; Feeley, 1979). Many have suggested that the decision whether or not to incarcerate is, for judge and offender alike, a very different decision (in its implications and the factors that lead to it) from the decision as to length of imprisonment for those incarcerated (Sutton, 1978; Spohn *et al.*, 1981-1982). In this study, we use two measures of the dependent variable—a sentence scale (in years) and a dichotomous prison-no prison variable. We compare the effect of race using each measure separately and the two in combination.

Specification Errors

Efforts to estimate the effect of individual and legal variables on sentencing outcomes will be biased if variables that affect those outcomes are omitted. It is generally recognized that variables describing the defendant and the charges against him must be included. It is less often recognized that variables describing how the case against the defendant is processed should also be included, since the nature of these processes—whether the matter was settled by plea or trial, whether bail was granted or not, whether public or private counsel was available—may influence the outcome independently of the race of the accused. For example, some of the effect of the availability of counsel may be attributed to the race of the defendant if the former variable is omitted. We were able to include the following process variables in our

model: whether there were multiple charges against the accused; whether some of the additional charges were dropped and whether the gravity of the principal charge was reduced; whether the accused was defended by privately retained counsel, assigned private counsel paid by the state, or a public defender; whether the accused was free on bail and the amount of bail; how the accused was convicted (plea, bench trial, jury trial); and how much time elapsed between indictment and disposition.

Sample Selection Bias

It is obvious that those convicted of crimes will differ from those arrested for crimes but not convicted. What is less obvious is that there may be important unobserved differences among persons convicted of the same charged crime, and that these differences may not be random but may instead be correlated with other attributes of the offender, including race. One way to deal with this problem, of course, is to observe as many of the possible attributes of the cases as possible, so that we are not misled into thinking that everybody convicted of armed robbery has engaged in identical (or at least legally equivalent) behaviors. We try to do this when we reduce the possibility of measurement error. But measurement error cannot be completely eliminated. As a result, two persons, each convicted of armed robbery against a stranger in which a knife was used and an injury resulted, may, in fact, have acted very differently. One robber may have injured his victim without provocation, laughing all the while, whereas the other robber may have injured his victim inadvertently in the process of being pursued by the victim and may have expressed great remorse at the harm done. Or the first robber may have stolen from an elderly woman in a wheelchair while the second stole from a prostitute who was herself known to be a thief. These behavioral differences cannot ordinarily be detected from the court records. We can afford to overlook them—*if* we believe they are randomly distributed among the convicted offenders.

But suppose that the criminal justice system is biased. Suppose that it favors whites and is hostile to blacks. In that case the unobserved differences may not be randomly distributed. That may happen because the pro-white police and prosecutors will only arrest, indict, and prosecute those whites who have committed such heinous versions of armed robbery (the laughing, wanton assaults on elderly women) that their crimes cannot be overlooked, whereas these officials will

cheerfully arrest and prosecute even minor transgressors of the robbery statute, provided they are black. If judges share the biases of the police and prosecutor, they may give similar sentences to the white and black robbers even though the moral gravity of their offenses is quite different. For a biased judge, in other words, an ordinary black robber deserves the same sentence as a white who laughingly assaults an elderly woman. The analyst who later comes upon these data will conclude, even after using the most sophisticated estimation procedures, that blacks and whites receive the same sentences for the "same" offenses. The recent report of the Panel on Sentencing Research of the National Research Council has an excellent discussion of this issue based in large part on the paper prepared for it by Klepper *et al.* (1983).

We propose in this paper to deal with the problem of sample selection bias by using longitudinal data. We ask if there is a positive association, independent of other variables, between race and sentence length at three different points in time in the criminal courts of a single jurisdiction. If such an association exists at all three points (t_1 , t_2 , and t_3), then judicial discrimination exists, provided we do not believe that the agencies that screen cases before they get to court (i.e., the police and prosecutors) have in each time period treated blacks more leniently than whites. If they have, then what appears at the sentencing stage as more harsh penalties for blacks is not anti-black discrimination at all but merely judicial correction of pro-black discrimination. If we find no positive association between race and sentence length at t_1 , some at t_2 , and more at t_3 , we could only dismiss this as an artifact of sample selection bias, rather than accept it as a measure of discrimination, if we believe that the police and prosecutors were becoming markedly *less* discriminatory in their decisions. That is to say, a growing positive association between race and severity of sentences could only be attributed to sample bias if we believe that the police and prosecutors are becoming fairer so that the prior biases of judges are now being unmasked. Social scientists are probably inclined to assume that the police (and possibly prosecutors) will, if anything, treat blacks more harshly than whites, and so they will be inclined to treat these cases as instances of judicial discrimination. To us, however, whether police under-arrest or over-arrest blacks is very much an open question.

The third possibility is the one of interest to us because it conforms to our empirical findings. If there is an association

between race and outcome (such that blacks are disadvantaged) at t_1 but less so at t_2 and not at all at t_3 , then in order to reject the conclusion that racial discrimination in the sentencing behavior of judges is disappearing, we would have to believe that the behavior of the police and prosecutors is becoming *more* discriminatory. If police and prosecutors become more likely to over-select blacks for court appearances, then the apparent racial neutrality in sentencing outcomes at t_3 would be a sign of bias rather than fairness on the part of judges. Later in this article we will consider the plausibility of this assumption and such data as we have that bear upon it. To anticipate our conclusions, we find no grounds for accepting this assumption and thus no grounds for rejecting our finding that the courts we studied came to give less attention to race in their sentencing decisions.

II. THE STUDY

Our data are drawn from a sample of 1,512 criminal defendants charged with armed robbery or burglary in the courts of Milwaukee, Wisconsin, between 1967 and 1977. Approximately one-third of the entire sample was selected at random from those charged during each of three periods: 1967-1968 ($n=502$), 1971-1972 ($n=524$), and 1976-1977 ($n=486$). The sample consisted of individual defendants, not cases or charges, and the sampling frame was composed of all persons charged at arraignment with either armed robbery or burglary. (Many were convicted of lesser charges; the effect of this charge reduction on sentencing outcomes is considered in the estimated model.) Some of the characteristics of these offenders broken down by race are given in Table 1.

Prosecutorial and court records were scrutinized to obtain information on the case, the defendant, and court procedures, and judges, prosecutors, and defense lawyers were interviewed to learn something about how the Milwaukee courts operate and how that operation changed over the ten-year period. Judges were also asked to respond to a closed-end questionnaire designed to measure their judicial ideology on a scale from "most liberal" to "most conservative." In Appendix II we define the variables used to tap judicial attitudes and the manner in which the ideology scale was constructed.

The dependent variable was defined in two ways: a dichotomous prison-no prison variable and a sentencing scale (described in Appendix I). To estimate the effect of race and

Table 1. Characteristics of Milwaukee Crime and Criminals
(Controlling for Race)
Armed Robbery and Burglary

	1967-1968		1971-1972		1976-1977	
	Whites	Blacks	Whites	Blacks	Whites	Blacks
Age						
mean	23.7	22.8	24.0	22.4	22.3	22.1
median	21.7	21.2	21.8	22.0	21.5	21.1
Prior Record						
none/minor	56.9%	61.8%	54.8%	58.0%	52.9%	52.3%
major/prison	43.1%	38.2%	45.2%	42.0%	47.1%	47.7%
Weapon*						
none	0	0	1.2%	2.3%	1.0%	2.2%
knife/other	13.6%	25.6%	22.2%	25.4%	38.8%	39.2%
firearm	86.4%	74.4%	76.6%	72.2%	60.4%	59.6%
Violence*						
yes	24.8%	22.4%	22.0%	19.6%	30.6%	32.5%
no	75.2%	77.6%	78.0%	80.4%	69.4%	67.5%
Location of Crime						
commercial	67.2%	66.4%	51.8%	52.4%	53.1%	54.9%
personal/home	32.8%	33.6%	49.2%	47.6%	46.9%	45.1%
Stranger-to-Stranger						
yes	73.7%	75.6%	70.8%	70.3%	85.1%	85.3%
no	26.3%	24.4%	29.2%	29.7%	14.9%	14.7%
Indigence						
yes	61.4%	78.2%	67.8%	86.2%	83.1%	87.3%
no	38.6%	21.8%	32.2%	13.8%	16.9%	13.7%
Ability to Post Bail						
yes	39.9%	43.6%	55.7%	46.6%	36.9%	33.8%
no	60.1%	56.4%	44.3%	53.4%	63.1%	66.2%

*Armed robbery only.

other variables on length of sentence, ordinary least squares (OLS) regression methods were employed; to estimate their effect on the in-out decision, a probit equation was used. The three equations—a probit equation on the in-out decision for all armed robbers and burglars, and two OLS equations on sentence length (one for armed robbery and one for burglary)—are given in Tables 2, 3, and 4.

III. FINDINGS

Whichever form the dependent variable takes, whether using probit or OLS estimation procedures, and for both armed robbery and burglary, the effect of race was the same: other things being equal, race had an independent effect on sentencing outcomes at t_1 (1967-1968) and no statistically significant effect at t_2 (1971-1972) or t_3 (1976-1977). In the earliest period, black defendants were more likely to go to

Table 2. Factors Influencing the Decision to Imprison, Armed Robbery and Burglary Combined^a

Dependent Variable = Prison/No Prison

Independent Variable	Coefficient Values		
	1967-1968	1971-1972	1976-1977
Defendant Characteristics			
Age	.194* (.071)	.167* (.067)	.144* (.063)
Race	.383* (.177)	-.159 (.144)	-.128 (.104)
Prior Record	.402* (.200)	.440* (.206)	.495* (.213)
Offense Characteristics			
Offense	1.817* (.204)	1.250* (.328)	1.740* (.327)
0 = burglary			
1 = armed robbery			
Stranger-to-Stranger Offense	.379* (.190)	.526* (.200)	.498* (.180)
Case-Processing Variables			
Read-In	.468* (.202)	.269 (.141)	.538* (.211)
Charge Reduction	-.179 (.155)	-.138 (.177)	-.322 (.185)
Delay	.104 (.095)	.066 (.049)	.139* (.066)
Method of Conviction	.226 (.142)	.588* (.195)	.375* (.169)
Type of Counsel			
Assigned Counsel (Private)	.784* (.299)	-.363* (.136)	-.333* (.160)
Public Defender	—	-.614* (.138)	-.178 (.120)
Judicial Ideology			
	-.280* (.104)	-.275* (.095)	-.053 (.166)
Constant	-5.769	-3.798	-4.724

Method: Probit

* Statistically significant at the .05 level.

^a There were too few armed robbers who did not go to prison to permit a probit estimation for each offense separately.

prison (Table 2) and, if in prison, to serve longer sentences (Tables 3, 4). In the later two periods, being black had no significant effect on the chances of going to prison or on the length of prison terms. Indeed, for the last two periods with respect to armed robbery and for the middle period with respect to burglary, the sign of the race variable, although not statistically significant, is negative, indicating that in the sample studied being black was, other things being equal, associated somewhat with leniency in sentencing. We cannot, however, discard the hypothesis that this association is due to chance.

The effect of age is strong and consistent across all three time periods. Older defendants, other things being equal, were more likely to be sentenced to prison and to be sentenced for longer terms. Since this analysis has already held constant the nature of the offense and the prior record of the defendant, age is not serving as a proxy for these variables. This implies that the courts were less inclined to give older offenders a break, a finding consistent with other studies which seem to show that the chances of going to prison increase with age without regard to record (Boland and Wilson, 1978; Petersilia *et al.*, 1977; Peterson *et al.*, 1980).¹ In another study, this fact, along with others, was used to help explain the growth in prison populations during the time period being investigated (Pruitt, 1982).

In any study where there is a substantial risk of measurement error and omitted variables, it is important to test alternative specifications of the model. This is especially the case when the significance attached to one variable—race—is likely to be controversial. We examined, in tables not reproduced here, the effect of estimating the value of the independent variables separately for burglary and armed

¹ Our data suggest that this finding is not the result of a small group of older defendants (30 and over) receiving significantly more severe sentences than those in younger age groups. A review of average sentences across age groups reveals a steady escalation in sentence severity as one moves into older classes of defendants. For example, using the sentencing scale described in Appendix I, the median sentences awarded to armed robbers in four age groups are these:

Age Group	Median Sentences Armed Robbery		
	1967-68	1971-72	1976-77
18-20	30	14	20
21-24	30	20	30
25-29	40	20	40
30+	65	40	40

Table 3. Factors Influencing the Length of Prison Sentences for Imprisoned Armed Robbers

Dependent Variable = Length of Prison Sentences^a

Independent Variable	Standardized Coefficient Values		
	1967-1968	1971-1972	1976-1977
Defendant Characteristics			
Age	.414* (.166)	.181* (.089)	.202* (.070)
Race	2.579* (.950)	-2.312 (1.241)	-.821 (.701)
Prior Record	4.422** (.980)	3.766** (.764)	3.729** (.811)
Offense Characteristics			
Weapon	1.899* (.862)	4.111** (.993)	3.622** (.869)
Violence	3.502* (1.482)	5.622** (1.284)	5.943** (1.262)
Stranger-to-Stranger Offense	1.500 (.852)	3.811** (1.066)	2.114* (.929)
Case-Processing Variables			
Read-In	8.004** (2.094)	3.164* (1.066)	13.862** (2.111)
Charge Reduction	-9.405** (2.291)	-4.522** (1.053)	-8.363** (.846)
Delay	1.382** (.362)	.804* (.311)	2.914** (.246)
Method of Conviction	.770 (.511)	3.682* (1.307)	3.604** (1.064)
Type of Counsel			
Assigned Counsel (Private)	5.944** (1.812)	-2.660** (.482)	-3.942** (1.226)
Public Defender	—	-.803** (.279)	-2.241* (.922)
Judicial Ideology	-3.633** (.240)	-2.011** (.182)	-.332 (.173)
R ² Value	.817	.838	.844
Constant	5.986	7.642	7.888
Standard Error	5.131	4.522	7.911

Method: Ordinary least squares

* Statistically significant at the .05 level.

** Statistically significant at the .01 level.

^a Sentencing scale appears in Appendix I.

robbery, of including such variables as bail status and whether or not the victim of the offense was a commercial establishment, and of using dummy variables instead of scales to measure the effect of prior record, method of conviction, and weapon type. None of these alternative specifications materially affected the parameter estimates of the independent variables as given in Tables 2, 3, and 4.

We also considered the possibility that to include the case-processing variables (delay, charge reduction, method of conviction, type of counsel, and whether additional charges were read in at the sentencing stage) was to include by proxy other factors, including some linked to race. For example, race may affect one's ability to acquire private as opposed to public counsel or to delay the final case disposition. If race is so associated with case processing, its effects may be masked by including both race and case-processing variables in the equations. To check this, the length-of-sentence equations were estimated omitting the case-processing variables. There were no significant changes in the values of the coefficients for race, age, or prior record. When the equations were estimated omitting case-processing variables except for type of counsel, the coefficients for offender attributes did not change, but the coefficient for type of counsel increased somewhat. Omitting case-processing variables did reduce the total amount of explained variance (from an R^2 of about .8 to an R^2 of .6 to .7), suggesting that case-processing factors have a substantial effect independent of race. As a further check on the possibility that some racial effects were proxied into our equations via the case-processing variables, we estimated equations in which each processing variable was the dependent variable. There was no significant evidence that race was sufficiently collinear with case processing as to call into question the observed absence of a race effect on sentencing at t_2 and t_3 . In sum, the results for race reported in Tables 3 and 4 seem reasonably robust to alternative specifications, though omitting the case-processing variables will reduce the explained variance. If one wishes to find the best explanation for sentencing decisions, case-processing variables must be included in the equation, but if one wishes only to estimate the effect, if any, of race on sentences, then, at least for this sample, such variables are of little importance.

Though this analysis is primarily concerned with the effect of race on sentencing, a few remarks may be made in passing about the apparent effect of other factors. The more serious

Table 4. Factors Influencing the Length of Prison Sentences for Imprisoned Burglars

Dependent Variable = Length of Prison Sentences^a

Independent Variable	Coefficient Values		
	1967-1968	1971-1972	1976-1977
Defendant Characteristics			
Age	.438** (.126)	.383** (.105)	.355** (.112)
Race	1.566* (.722)	-1.294 (.822)	.588 (.594)
Prior Record	2.565** (.511)	.788** (.210)	1.742** (.208)
Offense Characteristics			
Multiple Charges	1.680* (.596)	9.988** (1.114)	8.966** (1.077)
Location of Crime	-.988* (.346)	-2.001** (.369)	-2.005** (.461)
Stranger-to-Stranger Offense	.358 (.445)	1.098 (.605)	1.733* (.821)
Case-Processing Variables			
Read-In	4.686** (.939)	1.102 (.621)	5.885** (.512)
Charge Reduction	-2.066 (1.151)	-2.912** (.752)	-5.882** (.666)
Delay	.854** (.122)	.272* (.093)	1.202** (.179)
Method of Conviction	2.611** (.597)	3.602** (.612)	2.223** (.638)
Type of Counsel			
Assigned Counsel (Private)	4.989** (.788)	-3.332** (.601)	-4.987** (.932)
Public Defender	—	-5.017** (.698)	-2.432** (.463)
Judicial Ideology	-3.387** (.182)	-2.139** (.246)	-.516* (.239)
R ² Value	.776	.814	.773
Constant	3.976	6.829	2.141
Standard Error	3.366	4.017	2.833

Method: Ordinary least squares

* Significant at the .05 level.

** Significant at the .01 level.

^a Sentencing scale appears in Appendix I.

the prior records of defendants, the more likely they were to go to prison and to go for longer terms. Armed robbers who used a gun or who injured their victims were likely to serve longer sentences than those armed with a knife or causing no injury, though the kind of weapon and the existence of an injury did not affect the in-out decision. Burglars who entered commercial establishments received longer sentences than those who entered residences, perhaps because in the former case they stole more, or more valuable, things (we have no information on the amount of burglary losses). If the crime was committed against a stranger, rather than a relative or acquaintance, the chances of going to prison were greater and the length of the sentence (at least for armed robbery at t_2 and t_3 and for burglary at t_3) was likely to be longer. In general these findings are what one would have expected. What was not expected was that the influence of offender attributes and the nature of the offense was pretty much the same on both the prison-no prison decision and the length of sentence. In this study, at least, little information about the effect of these variables would have been lost by combining the two kinds of decisions into a single scale. Although we do not report our results, the situation is no different if we break the in-out decision between probation and jail or prison.

A great deal of information would have been lost, however, if we had used a single dependent variable to estimate the effect of case-processing variables. One form of plea bargaining—reducing the initial charge—had no effect on the in-out decision but a strongly negative effect on the length of sentence during two of the three time periods. Defendants who persuaded the prosecutor to reduce the original charge (say, from armed robbery to simple theft) were not more likely to escape prison, but they served shorter terms than those whose charges were not reduced. Similarly, delay in disposition (measured in the number of months elapsing between indictment and conviction) had no effect (except at t_3) on the chances of going to prison but had a strong effect on the length of the prison term—the longer the delay, the longer the term.² We are not quite certain why this should be, especially since we have already taken into account one possible explanation: the form of the final disposition. As we expected, defendants convicted after a trial were more likely to go to prison and to go for longer terms (at least at t_2 and t_3) than those who pleaded

² Controlling for bail status does not affect this relationship.

guilty. Perhaps the defendants who experienced the longest delays were those who faced charges that were graver in ways not measured by our data and sought these delays in the (evidently vain) hope of seeing the evidence against them decay or the prosecutor's determination waver. Or perhaps the prosecutors postponed the dispositions so that they could build especially strong cases against those persons whom they most wished to punish.

The kind of counsel available to the defendant affected the outcome, independently of other factors, but the effect was complex and not entirely consistent over time. The public defender's office did not exist at t_1 . By t_2 , after the office had been created, using a public defender rather than a private attorney made a defendant *less* likely to go to prison and more likely to serve a shorter term for burglary (the effect on robbery terms at t_2 was insignificant); by t_3 , using the public defender seemed to make no difference in the in-out decision but made for shorter sentences for both robbers and burglars. At a minimum, these findings do not support the commonly expressed view that defendants using a public defender are at a clear disadvantage in the criminal justice system.

III. DISCUSSION

The apparent decline in the significance of race as a factor associated with sentence severity in Milwaukee might be a spurious finding if, as we have already explained, the police and prosecutors during this period were discriminating at an increasing rate against blacks. We have no reason to believe that there was such an increase and good reason to believe there was not.

Blacks made up about 74 percent of those charged with armed robbery and 61 percent of those charged with burglary in 1976-1977. We can never know whether this corresponds to the true rate of black participation in these crimes because the great majority of both crimes go unsolved, and almost all of the burglaries go unobserved. We can, however, compare the characteristics of persons charged with armed robbery with the characteristics of those observed by their victims committing such robberies. In 1974, the Census Bureau conducted a victimization survey in Milwaukee by interviewing 23,495 residents selected at random. Those who had been victimized by robbers, either singly or in groups, were asked about the racial identity of the offender(s). Table 5 shows the proportion

of black robbers as reported by victims of different kinds of robberies.

Table 5.
Race of Robbers as Reported by Victims, Milwaukee, 1974

Offense	Percent Black	
	Single Offenders	Multiple Offenders
All robberies	66%	68%
Robbery with injury	72%	66%
Robbery without injury	64%	69%

Source: *Criminal Victimization Surveys in Milwaukee*, 1977, Tables 9, 11.

Roughly two-thirds of all robbers were reported by their victims to be black, and this proportion did not differ significantly whether the robbery involved one offender or multiple offenders, injuries or no injuries. This proportion is close to the proportion of persons arrested and charged for armed robbery in Milwaukee in 1976-1977. In short, there is little evidence that, in the aggregate, the police and prosecutors produced before the courts persons charged with robbery whose racial characteristics differed substantially from the way those characteristics were described by robbery victims (cf. Hindelang, 1978).

Unfortunately, the victim survey was not carried out in Milwaukee in the earliest period covered by our study, so nothing can be said directly about changes, if any, in the racial characteristics of victim-observed robbers. However, the close correspondence in our last time period between victim reports and prosecutor reports is clearly inconsistent with the hypothesis that officials were becoming more discriminatory. Were that the case, we should expect there to be a sharp disproportion between the racial characteristics of observed and arrested robbers in 1976-1977.

If we rule out the possibility that our findings can be explained away by increasingly discriminatory police practices, we still must confront the possibility that the sudden decline in the effect of race on sentence outcomes between t_1 and t_2 is spurious—perhaps an artifact of our statistical procedures. Any change as sharp as the one we observed naturally arouses the suspicions of social scientists accustomed to finding evidence of, at best, evolutionary change.

But our statistical results are amply confirmed by our interview data. Judges between t_1 and t_3 became more liberal

by all measures we could devise. *All* judges but one who served on the Milwaukee bench between t_1 and t_3 were interviewed, and both their verbatim self-descriptions and their responses on the fifteen point scale of judicial ideology (Appendix II) were recorded. The interviews captured changes retrospectively—that is, all the judges were interviewed in the late 1970s; so the attitudes of those who served in the earlier period were assessed some years after those attitudes could have affected their behavior. This creates the possibility of a bias resulting from faulty memory or imaginative reconstruction, but two things lead us to suspect that this bias, if it exists at all, is not large. First, it seems unlikely that judges would claim to university interviewers that fifteen years earlier they had judged blacks especially harshly if in fact they had not; the natural reporting bias would be in the opposite direction. Second, and more important, other judges as well as prosecutors and defense attorneys confirmed that the older judges had in fact displayed conservative views quite different from those of the judges appointed in the late 1960s and early 1970s.

Every bit of evidence we could find points consistently to the fact that at t_1 , the felony court judges of Milwaukee were conservatives with strong personalities who believed in tailoring sentences to the characteristics of individual offenders in order to protect society. By t_3 , the felony bench had come to consist of much more liberal judges who, though they might have wished to individualize sentences, saw their job as processing a growing caseload produced by an ever more professionalized prosecutorial and defense bar, and doing so on the basis of implicit rules governing how *offenses*, rather than offenders, ought to be treated.

On the judicial ideology scale (with zero representing the most conservative set of views), the judges on the bench in 1967-1968 had a mean score of 2.06; the judges in 1971-1972 had a mean score of 5.57, and the judges in 1976-1977 had a mean score of 7.12. This change occurred chiefly by replacement rather than conversion. That is, new judges were appointed (mostly by a governor who was a liberal Democrat) to a bench that was growing in size, and in time the views of these new judges dominated, statistically, the views of the small number of older judges who remained.

The governor who appointed the new judges sought persons with backgrounds and attitudes quite different from those of the older judges, and by and large he succeeded. They

were, for example, about three times as likely as their predecessors to agree with such statements as, "Crime will only be stopped or controlled when the social conditions which cause it are eliminated," "Poverty is a major cause of crime," and "Most criminal acts are the result of forces largely beyond the control of the offender."

Not only did the views of the judges change; the extent to which those views affected sentence outcomes (other things being equal) changed as well. At t_1 our measure of judicial ideology contributed substantially and significantly to the explained variance; by t_3 , it contributed little. At t_3 judicial ideology had no effect on the in-out decision or on the length of robbery sentences and only a small effect on the length of burglary sentences.

Of the eight judges appointed beginning in the 1970s, all but one had a working-class background, five went to law school outside of Milwaukee, and only two were former prosecutors. A prosecutor described the changes in these terms:

At the beginning of the 1970s you had an influx of new judges who thought very differently [from older ones]. . . . An example of the difference was in their attitudes toward minorities. The new judges had a much better understanding. [The older judges] came from an era in which there were no blacks in their schools and neighborhoods. The new judges, by contrast, had personal friends in the black community.

But the best evidence that something had changed came from the words of the judges themselves. Among the remarks of the older judges were these:

There was a total loss of authority in the schools and in the homes. We found an excuse for everything and refused to deal firmly with these young people. . . . When these kids came into the community, they didn't fit in. So they ended up in front of me as burglars, robbers, and arsonists. It was up to me to give them a firm hand.

I remember being in a parade in South Milwaukee [a predominately white, ethnic, blue-collar area], and hundreds of people cheered me and shouted, "Give 'em hell, judge." There were scores of elected officials begging for attention but it was *my* parade. South Milwaukee thought I was representing them.

I felt the people in the ghetto deserved as much protection as I receive or as those in the suburbs get. The only way they'll get this protection is if the

criminal knows that if he breaks the law he is going to do the time.

In contrast to the emphasis among the older judges on protecting society by dealing firmly with offenders, the newer, more liberal judges expressed a desire to help disadvantaged defendants:

I saw my job as trying to determine if there were savable qualities in the individual who was before me. I believed, unlike certain other judges, that you had to consider more than punishment. I had a case in which a drug addict was convicted of robbery. He had ten or eleven felonies on his record, but the guy had never been in a drug program. I knew he wasn't going to be helped in prison, and if he didn't get help, he was going to get out and go right back to a life of crime. So I put him on probation with a strict requirement that he attend a local drug program. I thought the DA was going to lynch me.

The differences in ideology between the newer and older judges were clear and sharp, but little in these attitudes, taken alone, explains why the older judges should have decided cases in ways that allowed race to become a factor or why the newer ones should have decided cases in what appears to be a race-blind manner. The stern emphasis on protecting society among the older judges could have resulted in "letting the punishment fit the crime" in ways that were insensitive to individual differences, whereas the lenient attitude of the newer judges might have led them to tailor sentences to individual differences in ways that allowed race or class to have an effect (even to the extent of making the punishment of blacks less severe than that accorded whites). To explain why differences in ideology made a difference in sentencing outcomes, two additional factors must be noted.

First, it is possible that the social protection views of the older judges either were code words for anti-black sentiments or expressed a concern for unmeasured features of the cases which happened to be associated with the race of the defendant. This might have been the case if the older judges had thought it was important to crack down on offenders who were troublemakers in school, unable to hold a job, lacked strong family ties, or failed to show remorse. Since we have no measures of these traits, their influence on sentences could have been proxied into our equations through the race variable *if* they were correlated with race. We cannot choose conclusively between racism and omitted variables as explanations for the decisions of the older judges. We can

recall, however, that although the data in Table 1 suggest that white defendants in 1967-1968 were somewhat more likely than black ones to have had prior prison records and to have used firearms, white defendants received more lenient sentences than blacks. We are, therefore, somewhat skeptical of the suggestion that unmeasured but legally or morally defensible traits of black defendants account for their more severe sentences at t_1 , but we cannot reject this possibility.

By the same token, the rehabilitative instincts of the newer judges may have inclined them toward more individualized (and thus, possibly, more race-sensitive) decisions, but by t_3 black and white defendants had become, insofar as we can tell from Table 1, more similar. There were no significant differences in 1976-1977 between blacks and whites in age, prior record, use of weapons or violence, indigence, or ability to post bail. This may reflect actual changes in the traits of black and white defendants or changes in police arrest practices. But if police practices had changed, the change seems to have been in the direction of greater fairness. It is possible, that is, that at t_1 a white had to have committed a graver act than a black in order to be arrested and charged, whereas by t_3 charged persons had no race-related differences. This further strengthens our confidence that the decline of race as a factor influencing sentencing was not an artifact of an increase in discriminatory arrest and charging policies, and it helps explain why the preference of the newer judges for individualized sentences did not lead to racial differences in sentences.

Second, many things changed in the Milwaukee felony courts besides judicial attitudes. The prosecutor's office became more professionalized, a public defender's office was created, and court caseloads grew rapidly. The effect of these changes was to make more formal the rules governing case dispositions, to regularize and subject to common understanding plea bargaining negotiations, and to reduce the amount of time judges could devote to any single case. These effects, in turn, reduced the opportunity for judges to sentence according to personal preferences that were inconsistent with well-understood legal criteria. Though the judges had become more liberal in their outlook, the court as an organization had become more bureaucratized. Cases were more carefully screened before being carried to pleading or trial; and this screening, in which a newly created public defender's office was an important participant, increased the emphasis given to

the characteristics of the case (the weight of evidence, the willingness of the defendant to plead guilty) as opposed to the attributes of the offender. Heavier workloads and more bureaucratic procedures made it harder for the newer judges to act on the basis of their rehabilitative norms than it had been for the older judges, with lighter workloads and a more informal organizational environment, to act on the basis of their social protection (or possibly anti-black) views.³

These changes in the organizational structure of the criminal justice system of Milwaukee should increase our confidence in our finding that the influence of race on sentencing declined sharply from t_1 to t_3 . It is almost inconceivable that the reduced significance of the race variable could be the result of increased discrimination against blacks practiced by prosecutors and defense attorneys. The prosecutor's office had become more professionalized and the public defender's office, largely created in the 1970s, had attracted liberal, activist attorneys.

IV. CONCLUSIONS

Though we cannot say what happened in jurisdictions other than Milwaukee, our findings suggest that any generalizations about the effect of race on sentencing should be carefully qualified by attention to time periods and jurisdictions and should take into account both judicial ideology and organizational setting. The use of a longitudinal analysis, especially when supported by a detailed account of the context in which changes in sentencing occurred, is one method by which the problem of sample selection bias—a bias that has plagued most previous studies of racial effects on sentencing—can be overcome.

APPENDIX I DEFINITION OF VARIABLES

Independent Variables

Age	(in years)
Race	(0) = White; (1) = Black
Prior Record	(0) = None; (1) = Minor; (2) = Major; (3) = Prison

³ One ironic consequence of changes in workloads and a growth in bureaucratization was that the "lenient" newer judges were more likely to send offenders to prison than were the "tough" older judges.

Read-In ¹	(0) = No; (1) = Yes
Charge Reduction ²	(0) = No; (1) = Yes
Delay	(number of months between preliminary hearing and final disposition of case)
Method of Conviction	(0) = Guilty Plea; (1) = Bench Trial; (2) = Jury Trial
Weapon	(0) = None; (1) = Knife, Other; (2) = Firearm
Violence	(0) = No; (1) = Yes
Stranger-to-Stranger Offense	(0) = No; (1) = Yes
Multiple Charges	(0) = No; (1) = Yes
Location of Crime	(0) = Commercial; (1) = Home/Personal
Assigned Counsel (Private) ³	(0) = No; (1) = Yes
Public Defender ⁴	(0) = No; (1) = Yes

Dependent Variables

Prison/No Prison	(0) = probation and/or less than 1 year in jail; (1) = 1+ years in prison
Length of Prison Sentences ⁵	(8) = 1-2 years; (10) = 2-3 years; (12) = 3-4 years; (14) = 4-5 years; (20) = 5-6 years; (30) = 6-10 years; (40) = 10-15 years; (50) = 15-20 years; (65) = 20-45 years

¹ A “read-in” occurs when a person charged with two or more offenses pleads guilty in exchange for the prosecutor’s promise to dismiss (by “reading in” to the record) one or more charges.

² Charge reduction occurs when the original charge (e.g., armed robbery) is reduced by the prosecutor to a lesser included offense (e.g., larceny from a person), usually in return for a guilty plea.

³ An assigned counsel is a private attorney assigned to represent an indigent defendant at the expense of the state.

⁴ Public defenders are full-time defense attorneys hired and paid for by the state.

⁵ The sentencing scale used is the same as the one employed by the U.S. Department of Justice in their studies of sentencing in the federal courts.

**APPENDIX II
MEASUREMENT OF JUDICIAL IDEOLOGY**

The scale designed to measure a judge’s ideological orientation was developed from responses to a set of fifteen agree/disagree questions. The questions, with the “conservative” response in parentheses, are these:

Questions Relating to the Causes of Criminal Behavior

1. There is no question of moral guilt or blame in most types of crime; rather, criminal behavior should be treated as a physical and/or mental illness. (disagree)
2. Most criminals deliberately choose to prey upon society. (agree)
3. Crime will only be stopped or controlled when the social conditions which cause it can be eliminated. (disagree)
4. Poverty is a major cause of crime. (disagree)
5. Most criminal acts are the result of forces largely beyond the control of the offender. (disagree)
6. The legal system in the U.S. greatly favors the rich over the poor. (disagree)
7. A poor person doesn't have much chance of getting fair and equal treatment in Milwaukee's courts. (disagree)

Questions Relating to a Judge's Goals in Sentencing

8. Irrespective of how effective current efforts at rehabilitation of criminals are, we should keep in mind that rehabilitation is the only legitimate goal in sentencing. (disagree)
9. Rehabilitation is only one of many goals in sentencing convicted criminals; even if prisons don't rehabilitate, it is sometimes legitimate to send a person to prison. (agree)
10. One of the main objectives in sentencing offenders should be to deter potential offenders from committing crimes. (agree)
11. Removing offenders from the community is an important goal to be achieved in sentencing convicted criminals. (agree)
12. Punishment is an archaic approach to sentencing; the desire to punish an offender is almost never a legitimate goal in sentencing. (disagree)
13. Most of those who advocate rehabilitative treatment of criminals do not attach sufficient weight to the seriousness of the crime committed. (agree)
14. Most crimes are committed without rational thought; therefore deterrence isn't very effective. (disagree)
15. The death penalty is a valuable deterrent. (agree)

Each "conservative" response was scored 0, each "liberal" one was scored 1. The scale thus ranges from 0 to 15 (least to most liberal).

Source: Gibson, 1978.

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