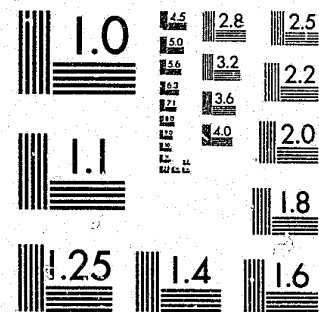


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**PUNISHMENT AND DISADVANTAGE:
Are There Differentials in Treatment Among Federal Offenders?**

U.S. Department of Justice
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Samuel L. Myers Jr.
Graduate School of Public and International Affairs
3E27 Forbes Quad
University of Pittsburgh
Pittsburgh, PA 15260
(412) 624-3650
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Abstract

PUNISHMENT AND DISADVANTAGE:
Are There Differentials in Treatment Among Federal Offenders?

This paper develops an econometric procedure for establishing the existence and efficiency of racial differences in punishment of federal prisoners.

A test of "residual discrimination" in prison commitment rates and time served is performed. A large and statistically significant racially discriminatory gap in time served is found. Black federal offenders would serve 5.6 fewer months in prison if treated the same as white offenders. The residual racial gap in punishment, however, would be deemed "efficient" if its elimination would increase black crime rates. A conceptual test is performed that shows in fact the residual gap is inefficient.

There are similarly found to be racial differentials in pre-prison employment disadvantage, although elimination of these differentials would have but a small narrowing effect on the racial gap in post-prison recidivism.

PUNISHMENT AND DISADVANTAGE:
Are There Differentials in Treatment Among Federal Offenders?

Economists have begun to argue that punishment via incarceration may not be socially efficient. The thrust of the argument is that punishment does not always reduce crime (Myers, 1983). While efforts to deter would-be criminals may be effective, attempts to rehabilitate convicted criminals by making punishment more severe may be futile. For example, Myers (1980) demonstrates in a two-period rational choice model that criminal-human-capital accumulation or labor-market discrimination against ex-offenders may result from long periods of incarceration. Rather than reduce the relative return to crime, punishment might increase it. This, in turn, may lead to increased participation in illegitimate activities. Here lies the possible inefficiency of punishment.

The economic efficiency argument can be extended to examine differential treatment of offenders. For example, the certainty and severity of punishment have long been known to differ between blacks and whites. Although the evidence is at times conflicting (e.g. see Elison and Megargee, 1979, Hindelang, 1979, 1969, Gibson, 1978, Myers, 1979, and Wolfgang and Cohen, 1970) it does point to the likelihood that blacks experience harsher punishment than whites.

If punishment cannot be expected to rehabilitate, why then would it be dispensed in relatively more abundant quantities to blacks? Is the answer, perhaps, that black and white offenders differ in backgrounds or personal characteristics? Such differences, of course, could statistically explain any apparent inequalities in punishment. Or, is the answer simply that blacks are more criminal than whites and thus must be punished more soundly? With either answer the problem of

of economic efficiency re-emerges.

The efficiency issue here is whether racial disparities in punishment (if they exist) are necessary in light of the perceived higher criminality among blacks. Or equivalently, this question is posed: If blacks and whites were punished identically, would crime among black criminals increase? The answer clearly depends on how punishment affects criminality. It also depends on how well one controls for other mitigating factors associated with race, such as employment disadvantage, that may determine criminality. Racial differences in punishment would be economically efficient if when eliminated they resulted in higher crime rates.

Is there racial discrimination in the treatment of offenders? Does punishment reduce crime? Is lengthy or more certain imprisonment socially efficient? These questions are hardly novel. In the interest of brevity we will not detail the long and often polemic list of answers offered to date. (Recent examples of answers to the first question, however, include Blumstein, 1982 and National Minority Advisory Council on Criminal Justice, 1980. Answers to the last two are the source of a lively debate between Myers, 1983 and Witte, 1983.) Our purpose in raising these issues once again, nonetheless, is to suggest a rigorous econometric approach that may prove useful in continuing research on punishment, disadvantage and crime. In particular this paper develops an econometric test of the existence and efficiency of racial differences in the certainty and the severity of punishment. It also examines the existence of and tests for the effects of preprison employment disadvantage.

At the outset it is essential to establish the central limitations of this exercise. The data are drawn from the federal prison system.

This represents a small and specialized portion of the U.S. criminal justice system. Not necessarily representative of the population in our nation's prisons, the sample cannot be truly reflective of the actual criminal population. However, unlike other national samples such as the Uniform Parole Reports that are restricted to parolees it does include offenders released on parole, sentence expiration and mandatory release.

Previous research on repeat offenders is beset by a number of nagging measurement problems. Crime is often measured by recidivism-- basically rearrest or parole violation. This neither captures actual involvement in illicit activities by exoffenders nor does it come close to representing participation in illegal activities among non-prisoners. Admittedly a common problem with use of individual data, it can be partially remedied by use of self-report data (e.g. Marquis and Ebener, 1981) or victimization data (e.g. Hindelang 1979). Similarly, the recidivism rate fails to provide the range and diversity revealed in other widely used "criminal severity" indices (Chaiken and Chaiken, 1982).

Another problem is that employment disadvantage and other indicia of economic opportunities have less than perfect measures in most criminal justice data sets (Witte, 1983). One would like to know exprisoner's wages, occupations, hours worked, sources and amount of illicit income, and nonearned income. This information is not as detailed as we would like in the sample used.

It is impossible to know whether the conclusions reached would differ if the perfect data set suddenly emerged. This concern is an obvious price that all honest scholars must be willing to pay. What one receives for the price in the present instance is the illustration

of a novel econometric methodology applied to a recurring problem in law and society research: disentangling the effects of race and disadvantage in criminal justice system outcomes.

The results of the illustration are not entirely surprising. It is found in the analysis of federal offenders that the effects of punishment on black and white ex-offenders differ. Blacks and whites do in fact lower their postprison participation in crime--measured by rearrest rates--after longer prison sentences but their participation in crime tends to increase when punishment is more certain, measured by ratio of prison commitments to convictions. Indeed, the crime-increasing effect of punishment certainty is almost twice as large for blacks as it is for whites. However, treating blacks and whites equally in punishment, so that their race-neutral rates of time-served and commitment to prison are equated, does not cause an increase in black recidivism. This counterintuitive result is in spite of the fact that blacks serve an average of five more months in federal prisons than they would in a racially neutral sentencing scenario.

The findings also suggest that the incidence of preprison employment disadvantage among black and white federal offenders is approximately the same. In contrast, the effects on employment of various factors such as marital status, mental health problems and drug and alcohol use differ substantially between blacks and whites. When these differing effects are equalized between the groups, blacks experience better employment. Our findings point, therefore, to possible labor market discrimination against black offenders.

The employment along with other forms of disadvantage may help explain why offenders have high crime rates in the first place.

But whether the subsequent punishment reduces recidivism--i.e. is efficient--remains to be shown in the paper. The methodology sketched in the next section is designed both to measure any racial disparities in punishment and to establish its efficiency.

I. METHODOLOGY

The problem of this paper is to determine whether racially discriminatory differences in punishment exist in the federal prisons and if so whether these differences are justified on efficiency grounds. The method to be used to establish the existence of discrimination is an econometric procedure familiar in the labor market literature and known as a "residual discrimination" test. The procedure essentially measures the inequality of outcomes that cannot be attributed to such objective factors as background or personal characteristics.

The method to be used to establish the inefficiency of racial discrimination is straightforward. A recidivism model is estimated, taking into account punishment and other factors. The model is then re-estimated with the actual punishment values replaced with (racially neutral) predicted values obtained by conceptually ridding the system of discrimination in punishment. If recidivism increases, then the racial discrimination rests on an efficiency justification. If recidivism remains unchanged or is reduced, the discrimination is inefficient and therefore unjustified. Since recidivism is unobserved among incarcerated offenders several modifications of the procedure are suggested.

These procedures are detailed below.

A. Residual Discrimination

In early labor market literature empirical estimates of the magnitude of wage or earnings discrimination against a given group was simple. An earnings equation of the form

$$Y = \alpha_0 + \sum_{i=1}^k \alpha_i X_i + \alpha_{k+1} X_{k+1} + \epsilon \quad (1)$$

is estimated. Earnings, Y, is a function of a vector of k productivity-related characteristics. The first k X_i 's are endowments of productivity related characteristics and the α_i 's are the effects on earnings of these characteristics. A residual category, X_{k+1} , is a dummy variable equal to one for members of the possibly discriminated against group. A stochastic disturbance term is usually assumed to be normally distributed, with zero mean, a constant variance across observations, a zero covariance between pairs of observations and a zero covariance with the X_i 's and X_{k+1} .

Measuring discrimination by the coefficient α_{k+1} , unfortunately, amounts to constraining the effects of all characteristics on earnings to be the same between groups. Blinder (1973) proposes an extension that relaxes this constraint. The most well-known application is found in Garfinkle and Haveman (1979). First the sample is partitioned between the dominant group, W, and the possibly discriminated against group B. Separate Y equations are estimated for B and W:

$$\hat{Y}^B = \hat{\alpha}_0^B + \sum_{i=1}^k \hat{\alpha}_i^B X_i^B \quad (2)$$

$$\hat{Y}^W = \hat{\alpha}_0^W + \sum_{i=1}^k \hat{\alpha}_i^W X_i^W \quad (3)$$

Any difference in Y between group B and group W can be divided between differences in "treatment", i.e. effects of endowments on outcome Y, and differences in endowments of characteristics.

The former is discrimination, the later is not. The gross difference in Y between B and W is

$$\hat{Y}^B - \hat{Y}^W = (\hat{\alpha}_0^B - \hat{\alpha}_0^W) + (\sum \hat{\alpha}_i^B X_i^B - \sum \hat{\alpha}_i^W X_i^W) \quad (4)$$

The discriminatory differences are obtained first by computing the value Y^B , the outcome for B's who are treated like W's but who still possess endowments X^B :

$$Y^B = \hat{\alpha}_0^W + \sum_{i=1}^k \hat{\alpha}_i^W X_i^B \quad (5)$$

The difference between the "equal treatment" value Y^B and the regression estimate \hat{Y}^B is the discriminatory gap or residual discrimination:

$$\hat{Y}^B - Y^B = (\hat{\alpha}_0^B - \hat{\alpha}_0^W) + \sum (\hat{\alpha}_i^B - \hat{\alpha}_i^W) X_i^B \quad (6)$$

A sufficient condition for no discrimination is:

$$(\hat{\alpha}_0^B - \hat{\alpha}_0^W) = 0 \text{ and } \sum (\hat{\alpha}_i^B - \hat{\alpha}_i^W) = 0 \quad (7)$$

A simple test for these sufficient conditions can be performed using the covariance matrix of the estimates from the following single regression:

$$\hat{Y} = \hat{\alpha}_0 + \sum_{i=1}^k \hat{\alpha}_i X_i + \hat{\alpha}_{k+1} X_{k+1} + \sum_{i=1}^k \hat{\beta}_i (X_{k+1}) X_i \quad (8)$$

It can be shown that $(\hat{\alpha}_0^B - \hat{\alpha}_0^W) = \sum (\hat{\alpha}_i^B - \hat{\alpha}_i^W) = 0$ implies and is implied by $\hat{\alpha}_{k+1} = \hat{\beta}_i = 0$, for all i . This interaction-effect version of the model is infrequently employed, however, because it ignores the possibility of off-setting favorable and unfavorable discriminatory effects.

The best critique and summary of the limitations of the residual discrimination model is to be found in Francine Blau's (1983) recent essay. The major problems are found to be (1) inability to measure all of the X's, i.e. certain endowments of characteristics are omitted for lack of data or inherent immeasurability; (2) errors in measurement of the X's or errors in variables due to use of proxies; (3) group differences in endowments of X's reflecting prior or indirect discrimination; (4) differences in X's that arise because of intergroup differences in tastes or preferences and not differences in "treatment"; and (5) sample selection bias.

Obviously the researcher should try to measure and measure well all of the relevant correlates of the outcome variable, one should take account of differences in tastes and preferences, one should avoid sample selection and one should acknowledge the possibilities of indirect discrimination that may cause differences in endowments. These attempts are often imperfect, but to date no better methodology for empirically measuring discrimination appears to have been offered, at least not in the econometrics literature.

The "residual discrimination" technique appears to be quite well suited for asking whether there are racially discriminatory differences in the treatment of repeat offenders. There are modifications, however, needed to accommodate alternative functional forms chosen for estimation of the certainty and severity of punishment.

It is shown below that these modifications have no material effect on the utility of the residual discrimination model.

The certainty of punishment--discussed in greater detail in the data section of this proposal--is a probability measure. It is well known (e.g. Theil, 1971:628) that a linear specification of a dichotomous dependent variable model generally violates the assumptions underlying the classical regression model. One arbitrary but attractive specification is a logistic function.

Denote the dichotomous event of being punished by C. Then the probability of punishment, conditioned on a vector of characteristics X and of effects β is given by:

$$P(C = 1 | X\beta) = \left[1 + \exp(-\beta_0 - \sum_{i=1}^k \beta_i X_i) \right]^{-1} \quad (9)$$

Maximum likelihood techniques can be used to estimate the separate logistic equations to obtain $\hat{\beta}^B$ and $\hat{\beta}^W$. The estimated probability of punishment for blacks is then calculated. It equals \hat{P}^B . Similarly, a \hat{P}^W can be calculated. It is:

$$\hat{P}^B = \left[1 + \exp(-\hat{\beta}_0^W - \sum_{i=1}^k \hat{\beta}_i^W X_i^B) \right]^{-1} \quad (10)$$

This calculation is regarded as the "equal treatment" probability of punishment. Does it make sense to regard the difference, $\hat{P}^B - \hat{P}^W$, a measure of discrimination as was done with the linear specification? The answer is yes. Note that for $0 < \hat{P}^B < 1$ and $0 < \hat{P}^W < 1$ the following transformations can be made:

$$\ln\left(\frac{\hat{P}^B}{1 - \hat{P}^B}\right) = \hat{\beta}_0^B + \sum \hat{\beta}_i^B X_i^B \quad (11)$$

and

$$\ln\left(\frac{\hat{P}^W}{1 - \hat{P}^W}\right) = \hat{\beta}_0^W + \sum \hat{\beta}_i^W X_i^W \quad (12)$$

Where the expressions in brackets are odds ratios, or the ratio of the probability of being punished to the probability of not being punished. The difference between the log-odds ratios is

$$\ln\left(\frac{\hat{P}^B}{1 - \hat{P}^B}\right) - \ln\left(\frac{\hat{P}^W}{1 - \hat{P}^W}\right) = \ln\left(\frac{\hat{P}^B / \hat{P}^W}{(1 - \hat{P}^B) / (1 - \hat{P}^W)}\right) \quad (13)$$

$$= (\hat{\beta}_0^B - \hat{\beta}_0^W) + \sum (\hat{\beta}_i^B - \hat{\beta}_i^W) X_i^B$$

It is easy to see that equality of coefficients between the two groups (equal treatment) implies in $\ln(\cdot) = 0$, which is satisfied for $\hat{P}^B / \hat{P}^W = 1$ or $\hat{P}^B - \hat{P}^W = 0$. In other words, residual discrimination is absent when the predicted probability of punishment equals the equal treatment punishment probability.

There is no loss of generality in using the residual discrimination methodology when other monotonic transformations are considered. Time served is an example. The severity of punishment, to be discussed later and measured by time served, is for released prisoners a doubly truncated variable. It is bounded from below by a small positive number. It is also bounded from above. The latter bound can be ignored for the case where there is no clustering at the upper limit. If there is similarly no significant clustering at zero, a convenient functional specification is log-linear, or

$$\ln(TS) = \gamma_0 + \sum_{i=1}^m \gamma_i X_i + \mu \quad (14)$$

Partitioning the sample between blacks and whites, estimating the two time served equations, and then obtaining the estimates $\hat{\gamma}^B$ and $\hat{\gamma}^W$ permit the calculation of

$$\hat{TS}^B = \exp(\hat{\gamma}_0^B + \sum \hat{\gamma}_i^B X_i^B) \quad (15)$$

and

$$\hat{TS}^W = \exp(\hat{\gamma}_0^W + \sum \hat{\gamma}_i^W X_i^W) \quad (16)$$

or the predicted severity of punishment and the "equal treatment" severity of punishment. The differences between these two, $\hat{TS}^B - \hat{TS}^W$ is again a straightforward measure of residual discrimination. It is easy to see that $\hat{\gamma}_0^W = \hat{\gamma}_0^B = \hat{\gamma}_1^W = \hat{\gamma}_1^B$ implies $\hat{TS}^B = \hat{TS}^W$. That is, equal treatment means no discrimination.

Actually, the log-linear test is subject to retransformation bias (Neyman and Scott, 1960). The problem--not encountered with the probability of punishment because it is possible to perform tests on the untransformed dependent variable--is that the exponential of the natural logarithm of predicted time served is not an unbiased estimate of time served. This is so because the time served estimate is obtained from the equation (1.14) where μ is a stochastic error term. Given knowledge of the true parameters γ it is obvious that

$$E(TS) = E \left[\exp (\gamma_0 + \sum \gamma_i X_i + \mu) \right] \quad (17)$$

$$\neq \exp (\gamma_0 + \sum \gamma_i X_i)$$

If however, μ is normally distributed then:

$$E(TS) = \exp (\gamma_0 + \sum \gamma_i X_i + \frac{\text{Var}(\mu)}{2}) \quad (18)$$

Consistent estimates can be obtained of the variance of the disturbance using least squares regression and these estimates can

be used to obtain asymptotically unbiased estimates of time served. It can be shown, moreover, that if the error variance in the W regression is identical to that in the B regression then the residual discrimination gap ($\hat{TS}^B - \hat{TS}^W$) is biased downwards in absolute value. The size of the bias is $\exp(\hat{\sigma}^2/2) - 1$, where $\hat{\sigma}^2 = \text{Var}(\mu)$. (Note that as $\hat{\sigma}^2$ approaches zero, this bias approaches zero.) What this means in essence is that residual discrimination computed from a re-transformed log-linear measure of time served is likely to underestimate the size of the gap between predicted and equal treatment values of punishment.

B. Analysis of Efficiency

In section A methods were described for determining whether racial differences in punishment exist. These differences might be called discrimination. Because many individuals differ in their background characteristics, prior criminal involvement and experiences, an entire distribution of punishment is observed for both whites and blacks. The distribution of punishment faced by blacks may be very different from the distribution that they might face if they were treated like whites. The following question emerges: Can the "discriminatory" gap be due to the perceived higher levels of criminality among blacks? We propose to answer this question in the following way. Actual involvement in crime after imprisonment is introduced as a dependent variable. It depends on punishment received. It also depends on preprison disadvantage.

The probability of recidivism (p)--measured by rearrest or parole violation during the year after release from prison--is assumed to depend on employment characteristics, personal back-

ground characteristics, criminal history and offense characteristics, variables relating to the criminal justice system, and punishment. This is one empirical counterpart to the economists' theoretical supply-of-crime model. Employment characteristics capture the returns to work; offense, criminal history, and CJS variables denote the returns to crime; and personal background characteristics corelate with tastes. Let $X_1, X_2, X_3, \dots, X_m$ be the list of these m explanatory variables. Then

$$p = f(X_1, X_2, \dots, X_m; \beta_1, \beta_2, \dots, \beta_m),$$

where the β 's are unknown parameters. They measure the effects of the m independent variables on p . For computational simplicity, we assume that the functional form of $f(\cdot)$ is logistic. Specifically, we assume that

$$p = 1 / (1 + (\exp - (\sum \beta_i X_i))). \quad (19)$$

Suppose now that X_k is a measure of punishment. If we replace X_k with \tilde{X}_k , a discrimination-free measure of punishment obtained from a "residual discrimination" analysis, then what happens to p ? The answer depends on both $\hat{X}_k - \tilde{X}_k$ -- the discriminatory gap in punishment -- and on $\hat{\beta}_k - \tilde{\beta}_k$ -- the differences in the effects that actual and discrimination-free punishment have on recidivism. Obviously, if $\hat{X}_k - \tilde{X}_k = 0$, then $\hat{\beta}_k - \tilde{\beta}_k = 0$: There is no discrimination and so trying to eliminate it will not affect recidivism. More likely, if $\hat{X}_k - \tilde{X}_k > 0$ then whether elimination of discrimination is efficient or not depends critically on whether $\hat{\beta}_k - \tilde{\beta}_k = 0$, and whether $\hat{\beta}_k < 0$. To be concrete, if punishment deters crime and discrimination increases punishment, then elimination of discrimination might increase crime! In such a case we consider discrimination efficient. It is possible, however for there to be discrimination,

$\hat{X}_k - \tilde{X}_k > 0$, for punishment to deter crime, $\hat{\beta}_k < 0$, but for elimination of discrimination not to increase crime. This might occur when $\hat{\beta}_k < \tilde{\beta}_k < 0$, that is when the effect of punishment on recidivism is smaller (in absolute value) in a discrimination-free world than in a racially biased world. Punishment falls but the effect of punishment on crime also falls; the total effect of eliminating discrimination, then, is ambiguous. It becomes, then an empirical exercise to determine whether the racially biased punishment is inefficient.

II. THE DATA

A random sample was drawn on all persons released from federal prisons by parole, mandatory release, or expiration of sentence during 1972. The sample, consisting of 2,495 observations, was restricted to federal prisoners with maximum sentences of more than one year and one day who were released to the community as opposed to other legal authorities. For each sample case, information on personal characteristics, previous employment, criminal-justice-system characteristics, criminal history, and offense characteristics was compiled by researchers at the U.S. Board of Parole. Follow-up information was obtained for one year after release from prison concerning whether the individual had been rearrested or whether a warrant for parole or mandatory release violation had been issued. Nearly one-third of the subjects failed in the first year to remain free of arrest or of parole or mandatory release violations. This percentage corresponds roughly to the first year's performance of a similar data set reported by Hoffman and Meierhoefer (1979). Although in subsequent years additional subjects fail, the at-risk population for computing the first-failure (i.e., first time to fail) rate is declining. Hence, so Hoffman and Meierhoefer have found, the recidivism rate declines asymptotically when calculated for at-risk populations. After six years, however, the rates for different risk groups tend to converge. What this means, of course, is that any significant differences in recidivism observed for differing groups of exoffenders one year after release may appear less significant in later years.

It is important to note that the measure of recidivism used here does not distinguish between felony and misdemeanor arrests. Similarly

the measure does not take account of the fact that some warrants for parole violation do not result in revocation. To the extent that parole boards are uninterested in minor postprison infractions but instead are concerned with serious crimes, our measure may be biased. More refined measures of recidivism, unfortunately, are unavailable in this data set.

One can conjecture in any event that parole officers seek warrants for minor infractions when major criminal activities are strongly suspected except for which there is no proverbial "smoking gun." In such a case, the bias from using our recidivism measure is likely to be small.

In Table 1, characteristics of the black and white exprisoners from the federal prison system are summarized. These federal exoffenders are somewhat older than many recently released prisoners from state and local prisons. Both whites and blacks are about 30 years old. The one-quarter representation of blacks in the sample is decidedly lower than it is in the disproportionately black prison population in the United States. Educational attainment at almost 10 years is slightly higher than that for inmates generally, but still lower than the national average. Blacks, though, had a mean school completion rate closer to the average for all inmates in state correctional institutions. "Employed more than 4 years" is a dummy variable equal to zero if the longest job held was of a duration of less than 4 years. Only a minority of the releases had ever worked for more than 4 years at a stretch. While 12.7 percent of white exprisoners had extensive preprison work experience, only 10.4 percent of black exprisoners did.

The average number of parole hearings was nearly one and three-quarters, although the average for blacks was lower than that figure. That a little more than a third of blacks were released on parole while half of the white

Table 1
 Characteristics of the Black and White Exprisoners
 of the Federal Prison System

Variables	Both Races ^a (N=2127)	Blacks ^a (N=546)	Whites ^a (N=1581)
Age (in years)	30.541	30.915	30.412
Black	.257	--	--
Female	.051	.086	.039
Grade Claimed	9.452	9.036	9.595
Married	.264	.214	.281
Previously in Mental Hospital	.091	.036	.110
No Drug or Alcohol Abuse	.828	.855	.819
Employed More than 4 Years ^b	.121	.104	.127
Parole Hearings (number)	1.762	1.529	1.843
Release on Parole	.464	.358	.501
Age at First Commitment (in years)	22.136	21.751	22.269
Time Served (in months)	23.992	24.696	23.748
Previous Convictions (number)	5.971	6.624	5.745
Prison Punishment	.297	.285	.301
Commitment/Convictions	.130	.141	.126
First Offender	.102	.075	.111
Robbery, Theft, Burglary	.544	.483	.505
Over \$5,000	.061	.027	.073
White Collar (forgery, counterfeiting, or fraud)	.228	.258	.218

Source: Data from U.S. Board of Parole Research Unit.

Note: Unless otherwise specified, figures are proportions within sample.

^aExcludes selective service and Immigration and Naturalization Service violators and races other than black or white.

^bA dummy variable equal to 0 if longest job held was less than four years.

sample was released would be understandable if blacks had served shorter sentences. Yet, time served--a measure of the severity of punishment--was on average a month longer for blacks than for whites. In addition, blacks are somewhat younger at their first imprisonment, are less likely to be first offenders, and are less likely to have received punishment while incarcerated than are whites.

The average number of previous convictions is nearly six. This mean is slightly larger for blacks, as is the ratio of prison commitments to convictions, a measure of the certainty of punishment. The type of offense committed differs for whites and blacks also. In the entire sample, about half of the cases relate to robbery, burglary, larceny, and auto theft. When selective service of immigration violations are eliminated, this fraction rises. Yet blacks are less likely to have been committed for these "serious" forms of theft than whites. Indeed, the proportion of blacks whose offenses were the white collar crimes of forgery, counterfeiting, and fraud (which includes income tax evasion) is higher than that for whites. Nonetheless, the haul was usually smaller: blacks were less likely to have netted over \$5,000 from the crime than whites.

III. RESULTS

Maximum likelihood methods can be employed to yield unbiased and consistent estimates of the parameters, in equation (19). Since p is nonlinear in the X 's, it is useful to calculate the derivatives of the predicted recidivism rate, p , and to evaluate them at the means of the independent variables. This procedure permits examination of the marginal effects of increases in given independent variables on recidivism. The results are displayed in Table 2.

The general findings can be conveniently summarized. Older ex-offenders, females, and married persons are less likely to recidivate. Blacks, those with fewer years of schooling, and those who have been confined to mental hospitals are more likely to be rearrested or to violate parole. A more stable preprison employment history is generally associated with a lower postprison failure rate, while alcohol or drug use is associated with high failure rates. More extensive criminal records and less time between incarcerations are positively related to recidivism. There is little variation in the effects of type of crime on recidivism. All categories have higher recidivism rates relative to the omitted category "other offenses." However, ex-offenders who net over \$5,000 are less likely to fail: either they are adept in avoiding rearrest or they turn to more legitimate activities. On the other hand, those who were punished while in prison or who appeared more frequently before the parole boards were more likely to fail. Finally, despite claims that paroled offenders represent a biased sample of prison releases, when controlling for other factors, release on parole has no significant effect on recidivism.

Table 2 also provides a comparison of the effects of (a) the cer-

Table 2.

MAXIMUM LIKELIHOOD ESTIMATES OF PROBABILITY OF POST-PRISON REARREST
(t-STATISTICS IN PARENTHESES)

Independent Variables	Total			Whites			Blacks			Without Employment			Without Certainty and Severity of Punishment				
	β	$\partial\beta/\partial x_1$		β	$\partial\beta/\partial x_1$		β	$\partial\beta/\partial x_1$		β	$\partial\beta/\partial x_1$		β	$\partial\beta/\partial x_1$			
Age	-.033 (-3.822)	-.007	-.027 (-2.624)	-.003	-.029 (-3.209)	-.013	-.036 (-4.300)	-.007	-.039 (-2.925)	-.006	-.063 (-3.376)	-.014	.0003 (.052)	.0001 (1.212)	.001 (2.121)	-.029 (-2.184)	-.006
Female	-.389 (-1.552)	-.001	-.360 (-1.044)	-.074	-.500 (-1.361)	-.111	-.373 (-1.506)	-.001	-.332 (-1.020)	-.073	-.485 (-1.304)	-.106	-.449 (-1.030)	-.092 (-1.400)	-.100 (-1.336)	-.007 (-1.663)	-.134
Grade Claimed	-.026 (-1.130)	-.004	-.021 (-.976)	-.004	-.038 (-1.952)	-.001	-.01 (-1.107)	-.004	.01 (.932)	-.004	-.039 (-1.976)	-.001	-.016 (-1.350)	-.003 (-1.236)	-.027 (-1.336)	-.002 (-1.011)	-.007
Married	-.350 (-2.923)	-.074	-.314 (-2.752)	-.079	-.345 (-1.001)	-.053	-.363 (-3.040)	-.077	-.196 (-2.047)	-.032	-.249 (-1.022)	-.024	-.375 (-3.192)	-.079 (-3.039)	-.414 (-3.039)	-.006 (-1.080)	-.027
No Drug or Drink	-.316 (-2.648)	-.071	-.375 (-2.568)	-.077	-.424 (-1.553)	-.092	-.340 (-2.680)	-.072	-.376 (-2.575)	-.078	-.439 (-1.602)	-.096	-.339 (-2.707)	-.072 (2.087)	-.413 (2.087)	-.006 (1.009)	-.029
Mental Hospitals	.493 (3.062)	.104	.480 (2.772)	.099	1.162 (2.371)	.253	.477 (3.104)	.106	.484 (2.792)	.100	1.170 (2.374)	.256	.443 (2.742)	.092 (2.337)	.396 (2.337)	.002 (1.070)	.236
Parole Hearings	.109 (2.204)	.023	.106 (1.850)	.022	.127 (1.185)	.027	.109 (2.213)	.023	.106 (1.844)	.022	.120 (1.190)	.028	.062 (1.353)	.013 (1.129)	.020 (1.129)	.086 (.830)	.019
Prison Punishment	.398 (3.359)	.084	.437 (3.313)	.090	.293 (1.324)	.064	.396 (3.346)	.084	.436 (3.309)	.090	.291 (1.309)	.063	.364 (3.358)	.077 (3.319)	.426 (3.319)	.007 (.046)	.039
Release on Parole	.010 (.090)	.002	-.016 (-.118)	-.003	.170 (.716)	.037	.007 (.066)	.001	-.016 (-.118)	-.003	.160 (.675)	.035	-.092 (-.818)	-.019 (-.901)	-.128 (-.901)	.068 (.300)	.015
Robbery, Theft, Burglary	.148 (1.120)	.031	.105 (.673)	.021	.082 (.309)	.018	.149 (1.132)	.031	.103 (.676)	.022	.090 (.339)	.019	.268 (2.101)	.057 (1.513)	.227 (1.513)	.202 (.808)	.044
White Collar	.018 (.117)	.004	-.095 (-.491)	-.020	.049 (.163)	.010	.016 (.103)	.003	-.090 (-.501)	-.020	.051 (.170)	.011	.179 (1.160)	.038 (.377)	.071 (.377)	.014 (.650)	.041
Greater Than \$5000	-.615 (-2.141)	-.130	-.688 (-2.071)	-.142	-.307 (-1.592)	-.084	-.615 (-2.135)	-.130	-.692 (-2.085)	-.143	-.350 (-1.532)	-.076	-.674 (-2.360)	-.143 (-2.401)	-.781 (-2.401)	-.163 (-.369)	-.051
First Offender	-.312 (-1.260)	-.066	-.317 (-1.260)	-.065	-.170 (-.386)	-.037	-.330 (-1.552)	-.070	-.331 (-1.310)	-.068	-.193 (-.445)	-.042	-.354 (-2.663)	-.118 (-2.532)	-.609 (-2.532)	-.127 (.825)	-.078
Age at First Conviction	.001 (.133)	.0003	.011 (.962)	.002	-.043 (-1.910)	-.009	-.001 (-.116)	-.0002	.009 (.066)	.001	-.046 (-2.077)	-.010	-.026 (-3.061)	-.005 (-1.906)	-.014 (-1.906)	-.004 (-.3310)	-.014
Employed More than 4 Years	-.356 (-1.720)	-.075	-.364 (-1.105)	-.054	-.544 (-1.217)	-.107	---	---	---	---	---	---	-.515 (-2.354)	-.110 (-1.914)	-.439 (-1.914)	-.091 (-1.013)	-.173
Time Served	-.007 (-1.014)	-.001	-.004 (-1.279)	-.0009	-.001 (-1.423)	-.001	-.005 (-1.762)	-.001	-.004 (-1.234)	-.0009	-.001 (-1.400)	-.001	---	---	---	---	---
Commitment/Convictions	1.044 (5.607)	.390	1.446 (3.949)	.320	2.615 (4.129)	.570	1.077 (5.793)	.402	1.501 (4.025)	.328	2.708 (4.271)	.593	---	---	---	---	---
Convictions	.027 (4.700)	---	.016 (5.242)	.018	.015 (.633)	.003	.064 (4.909)	.013	.019 (5.412)	.018	.017 (.690)	.003	---	---	---	---	---
Constant	-.045	---	.535	---	2.219	---	.015 (.135)	---	.475 (-1.255)	---	2.363 (2.944)	---	.182 (.570)	---	-.136 (-.381)	---	1.928 (2.586)
Weighted Mean of Dependent Variable	.320	---	.318	---	.357	---	.320	---	.310	---	.357	---	.328	---	.318	---	.356
Predicted Probability at Weighted Means of Independent Variables	.304	---	.293	---	.321	---	.305	---	.294	---	.324	---	.307	---	.296	---	.328
Chi-Square	218.061	---	171.010	---	86.285	---	215.007	---	169.750	---	84.762	---	174.630	---	134.061	---	60.579

SOURCE: U.S. Board of Pardon.

tainty and severity of punishment and (b) employment on crime. Note that those who were employed for more than 4 years before imprisonment have lower crime rates. Although the marginal effect of previous employment is larger for blacks than for whites, for neither group is the effect significantly different from zero. If we wished to assert that improved employment before prison is perfectly correlated with better employment prospects after prison, then we could argue that employment opportunities are only weakly related to post-prison illegal activities. In fact, to test the hypothesis that employment opportunities have additional explanatory power in our recidivism equation, a likelihood-ratio test can be performed. The chi-square statistics for this test for total, for whites, and for blacks are 3.05, 1.26, and 1.52 respectively. For these low values, a significance level of 1 percent, and one degree of freedom, we reject the hypothesis that previous employment improves the explanatory power of the model. This does not mean that better jobs or higher wages will not deter crime. It only means that pre-prison employment appears to have a small impact on recidivism. Indeed, preprison employment experience may be only weakly correlated with post-prison employment. Thus it may be a poor proxy for the returns to post-prison legitimate activities and therefore may provide an imperfect test of the view that employment opportunities are related to recidivism.

The effects of the certainty and severity of punishment are stronger. Longer prison sentences reduce recidivism. More certain punishment, measured by the probability of going to prison (given conviction), is expected to be a deterrent to crime too. But the denominator in this measure--number of convictions--also measures criminal record and labeling effects. Thus, to fully capture the certainty of punishment effects, convictions must be controlled for. When this is done, rather than obtaining a conventional

deterrent effect, we observe just the opposite. Those more likely to have been imprisoned after conviction and those with longer conviction records are more likely to be rearrested. This finding could be consistent with the Block-Heineke (1975) version of the economic model of crime if we argue that the risk preference of offenders leads them to be undeterred by more certain punishment. But the finding appears more consistent with a labeling or discrimination theory of postprison behavior. Ex-offenders do not choose to get rearrested. Although their participation in crime may or may not have diminished for greater perceived risks of punishment, they nonetheless end up being caught again because of their extensive criminal records. Other researchers' findings that the certainty of punishment does indeed deter crime may be accounted for by their omission of relevant criminal-history variables. Witte's (1980) findings appear to be subject to this bias.

Whether one regards our measures of punishment as proxies for the certainty and severity of punishment or as indicators of previous criminal history, which serves as a negative signal to potential employers and dutiful law-enforcement personnel, it is legitimate to inquire how much punishment adds to the explanatory power of the recidivism equation. The chi-square values are 43.43, 36.14, and 17.71 for the total, white, and black samples respectively in the likelihood-ratio test for the exclusion of the punishment variables. We cannot reject the hypothesis that punishment significantly increases the explained variance in recidivism rates.

In summary, then, we have found in a sample of Federal prison releases that a wide variety of personal background characteristics--"taste" variables--are significant determinants of recidivism. Holding these and other variables constant, we find that favorable preprison employment ex-

periences add little to the explanatory power of the model, although generally, better employment opportunities reduce crime. In addition, we find that punishment plays a strong and significant role in affecting recidivism, but the effects are not consistent with other research findings: the increased severity of punishment is a deterrent to crime, but increased certainty of punishment is positively related to rearrest rates.

Table 2 reveals that blacks are more prone to recidivism than whites: 35.7 percent of blacks become recidivists after release from Federal prison, while only 31.8 percent of whites do so. The gap in the probability of recidivism is 3.9 percentage points. With (1581 + 546 - 2) degrees of freedom and a t-statistic of 1.65, we reject the hypothesis that the black/white recidivism difference is equal to zero. When one controls for any number of seemingly exogenous factors, the percentages become 32.1 and 29.3 for blacks and whites respectively (Table 2, second row from bottom). This, of course, virtually eliminates the gap in recidivism. The new differential is now found to be 2.8 percentage points. The t-statistic for this difference is only 1.28; this implies that while there is a perceptible racial gap in recidivism, it is barely statistically significant. At this point other researches would stop. But for us the heart of the analysis lies in the examination of punishment and disadvantage. We want to know first whether there is any racial discrimination and second whether its elimination will affect recidivism.

Tables 3 to 5 present the results of the first-stage estimations needed to obtain the racially unbiased measures used to predict recidivism.

Separate black and white logistic equations are estimated for the probability of having been employed for more than 4 years prior to in-

Table 3.
Maximum likelihood estimates of the probability that preprison employment was greater than 4 years
(t-statistics in parentheses)

Independent Variables	Blacks		Whites	
	$\hat{\beta}$	$\frac{\partial \hat{p}}{\partial x_i}$	$\hat{\beta}$	$\frac{\partial \hat{p}}{\partial x_i}$
Age	.107 (6.511)	.006	.100 (13.015)	.006
IQ	-.006 (-.512)	-.000	-.007 (-.959)	-.000
Female	-.336 (-.509)	-.021	.254 (.644)	.017
Grade claimed	.124 (1.812)	.008	.122 (3.602)	.008
Married	.771 (2.472)	.049	1.003 (6.074)	.068
No use of drug or drink	.917 (1.668)	.058	.353 (1.324)	.024
Previously in mental hospital	-.810 (-.759)	.052	-.675 (-2.048)	-.086
Constant	-7.326 (-5.455)	--	.127 (-7.803)	--
Weighted mean of dependent variable	.106	--	.127	--
Predicted probability at weighted means of independent variables	.068	--	.074	--
Chi-square	64.046	--	291.047	--

Source: Data from U.S. Board of Parole Research Unit.

carceration. As can be seen in Table 3, the effects of age, IQ, and education are about the same for whites and blacks. Being female has an insignificant impact on preprison employment for both races. Being married and not having drinking or drug problems raises preprison employment for both blacks and whites, although at different rates. Finally, prior mental hospital confinement has no significant effect for blacks but markedly lowers preprison employment for whites.

It is easy to see that blacks are less likely to have had long, stable employment before imprisonment than whites. While 12.7 percent of whites were employed more than 4 years, only 10.6 percent of blacks were. Yet, when controlling for differences in age, education, sex, and other background characteristics, little of the gap remains: the predicted fraction of blacks with preprison employment of that length is 6.8 percent, while for whites it is 7.4 percent.

When blacks are "treated" just the same as whites, however, the results change dramatically. If the preprison employment probability for blacks were determined by the white predictive equation but appropriately evaluated at the average values of the black characteristics, then we predict that 11.6 percent of blacks would have been employed more than 4 years. This figure not only approaches the actual mean for whites, but it also exceeds the value predicted for white ex-offenders using the very same equation. What this means is that while much of the employment disparity between black and white ex-offenders can be explained by differences in background characteristics, the low employment predicted for blacks is due largely to racial discrimination.

This is an important finding regarding the employment experiences of black and white ex-offenders. Both black and white criminals come to the labor market with disadvantaged background characteristics. In this analysis,

we do not observe the types of jobs held, the occupations or the wages paid. But we do observe a measure of previous stable employment; and the effects of background characteristics on this measure easily can be ascertained. What we find is that many of these effects are similar if not identical between blacks and whites.

A key difference is that of drug use. The marginal effect of drug use (or drinking problems) on employment of blacks is twice as large as that on the employment of whites. Having a drug or drinking problem clearly is related to less favorable preprison employment experiences among both whites and blacks. The deleterious impact of drug use, however, is simply greater among blacks. Put differently, among black exoffenders with no record of drug or alcohol problems, the expectation of a preprison history of stable employment is larger than it is among similarly qualified whites.

We have previously observed that marital status and mental health problems also differently affect employment among black and white exoffenders. Further, the actual characteristics of these offenders diverge. Blacks, as compared to whites in our federal sample, are less likely to be married, less likely to have drug problems, and are more likely to be females. Thus, what is observed is a sample of blacks and whites who differ both with respect to background characteristics and also with respect to the effects that these characteristics have on employment. On first blush, it is not surprising that much of the racial gap in employment is narrowed when control is made for backgrounds of blacks and whites. The real question, though, is what happens when control is made for the effects of background on employment? What happens when blacks are "treated" the same as whites? Our findings clearly suggest that black federal exoffenders could have expected more stable employment had they been treated like white federal exoffenders in

the labor market. The employment probability would rise from a predicted 6.3 percent to a "discrimination-free" rate of 11.6 percent, a statistically significant rise by a wide margin.

In Tables 4 and 5, estimates are provided for black and white measures of the certainty and severity of punishment. The certainty of punishment is computed as the ratio of previous prison commitments to previous convictions. It is essentially the subjective probability of being punished by imprisonment if convicted. This ratio is .049 for blacks and .039 for whites. Although being a white female means experiencing significantly lower probabilities of punishment than being a white male, the marginal effects of all other characteristics are virtually zero. Hence, when these characteristics are accounted for, the punishment probabilities for blacks and whites tend to converge toward zero. Further, when the black punishment probability is predicted using the white equation, the estimated value, .032, moves closer to the actual value for whites. In sum, blacks experience only slightly more certain punishment than whites.

At first glance, blacks do not appear to experience more severe punishment than whites. The average time served by blacks is 24.7 months, while for whites it is one month less at 23.7 months. Taking account of personal background characteristics and factors related to the crime, however, the average time served for blacks is predicted to be 19.06 months when evaluated at the white parameters. This is a highly significant gap of almost six months. It is easy to see how the "discriminatory" punishment arises.

There are perceptible differences in the effects of background characteristics on black and white time served. Blacks who are married, have no drug or alcohol problems, who are convicted of white-collar crimes and who have more frequent parole hearings, serve shorter terms. Whites, on

Table 4.
Maximum likelihood estimates of the probability of commitment,
given conviction
(t-statistics in parentheses)

Independent Variables	Blacks		Whites	
	$\hat{\beta}$	$\frac{\partial p}{\partial x_i}$	$\hat{\beta}$	$\frac{\partial p}{\partial x_i}$
Age	.062 (3.169)	.000	.0792 (7.382)	.000
IQ	.010 (.599)	-.000	.024 (1.828)	.000
Female	-104.242 (-.062)	-.000	-14.387 (-5.533)	.211
Grade claimed	-.062 (-.715)	-.000	.043 (-.824)	-.000
Married	-.615 (-1.091)	-.000	.296 (-2.720)	.004
No use of drug or drink	-.061 (-.106)	-.000	.296 (.687)	.004
Previously in mental hospital	-222.771 (-.577)	-.000	.240 (-2.048)	.003
Constant	-5.230 (-3.084)	--	-8.198 (-5.872)	--
Weighted mean of dependent variable	.049	--	.039	--
Predicted probability at weighted means of independent variable	.000	--	.014	--
Chi-square	23.281	--	70.738	--

Source: Data from U.S. Board of Parole Research Unit.

Table 5.

Ordinary-least-squares estimation of ln (time served)

Independent Variables	ln (Time Served)	
	Whites $\hat{\beta}$	Blacks $\hat{\beta}$
Age	.008 (8.00)	.009 (3.00)
Sex	-.184 (-2.52)	-- --
Married	.011 (.34)	-.114 (-1.84)
No use of drug or drink	.027 (.73)	-.149 (-2.04)
Grade claimed	-.010 (-1.67)	-.030 (-2.73)
IQ	.002 (2.00)	.004 (2.00)
Robbery, theft, burglary	-.185 (-5.00)	-.510 (-7.61)
Offense value greater than \$5000	-.243 (-5.40)	.098 (.62)
White-collar offense	.015 (.26)	-.552 (-7.56)
Prison punishment	.370 (11.21)	.353 (5.98)
Paroled	-.331 (10.68)	-.246 (-4.17)
Number of parole Hearings	.221 (17.00)	-.186 (-6.64)
Constant	2.356	2.720
Multiple R	.534	.521
R ²	.285	.271
Adjusted R ²	.280	.256

Source: Data from U.S. Board of Parole Research Unit.

the other hand, whose times served appear unaffected by marital status, drug or alcohol problems, or commission of a white-collar crime, tend to serve longer sentences for more frequent parole hearings. Moreover, among whites, being female and having committed a crime netting over \$5,000 tends to lower time served, although among blacks these factors have no effect on punishment. Given these many differences in the determinants of punishment for black and white felons, it is not surprising that there is a dramatic reduction of nearly six months in the time served by blacks when they are treated like whites.

To extend the conceptual experiment a step further, it becomes useful to replace for blacks the actual values for employment and punishment with the predicted "discrimination-free" values. Table 6 displays reestimates of the black recidivism functions. The odd-numbered columns list the estimated coefficients and associated statistics. In the even-numbered columns are the partial derivatives of the predicted probability of recidivism. First, in column 1 the black recidivism function from Table 2 is reproduced. Note that the actual failure rate is 35.7 percent and the predicted rate is 32.1 percent. In column 3, we replace the actual time served with the discrimination-free predicted value. Now the marginal effect of an extra month in prison is larger, but since blacks serve shorter sentences in this racially neutral scenario, the recidivism rate remains the same. In column 5 we insert the predicted certainty-of-punishment value. More certain punishment lowers recidivism, but racially neutral certainty of punishment means that blacks now have lower probabilities of being punished by imprisonment; hence, they are more likely to be recidivists. A racially neutral punishment policy does not appear to reduce black recidivism.

A different result emerges regarding a racially neutral employment policy.

TABLE 6

Maximum Likelihood Estimates of Black Recidivism and Residual Discrimination
(t-statistics in parentheses)

Independent Variables	Recidivism ^a		Recidivism With Predicted Time Served ^b		Recidivism With Predicted Ratio of Commitments/Convictions ^c		Recidivism With Predicted Employment ^d	
	(1) β	(2) $\partial\hat{\rho}/\partial x_i$	(3) β	(4) $\partial\hat{\rho}/\partial x_i$	(5) β	(6) $\partial\hat{\rho}/\partial x_i$	(7) β	(8) $\partial\hat{\rho}/\partial x_i$
Age	-.059 (-3.289)	-.013	-.041 (-1.987)	-.009	-.010 (-.414)	-.002	-.029 (-.841)	-.006
Time Served	-.008 (-1.423)	-.001	--	+	-.004 (-.885)	-.001	-.009 (-1.556)	-.002
Predicted Time Served	--	--	-.110 (-1.871)	-.024	--	--	--	--
Female	-.508 (-1.361)	-.111	-.786 (-1.894)	-.171	-.737 (-1.890)	-.162	-.444 (-1.188)	-.096
Grade Claimed	-.038 (-.952)	-.008	-.043 (-1.090)	-.009	-.038 (-.963)	-.008	-.018 (-.419)	-.004
Married	-.245 (-1.003)	-.053	-.220 (-.900)	-.048	-.353 (-1.322)	-.077	-.046 (-1.129)	-.010
No Use of Drug or Drink	-.424 (-1.553)	-.092	-.319 (-1.160)	-.069	-.266 (-.987)	-.058	-.325 (-1.114)	-.070
Previously in Mental Hospital	1.162 (2.371)	.253	1.177 (2.395)	.256	1.142 (2.315)	.251	1.020 (2.012)	.221
No. of Parole Hearings	.127 (1.185)	.027	.656 (2.068)	.143	.110 (1.033)	.024	.128 (1.197)	.028
Prison Punishment	.293 (1.324)	.064	1.039 (2.119)	.226	.223 (1.021)	.049	.308 (1.384)	.066
Robbery, Theft, Burglary	.062 (.309)	.018	-.237 (-.687)	-.051	.150 (.572)	.033	.095 (.356)	.020
Release on Parole	.170 (.716)	.037	-.572 (-1.200)	-.124	.043 (.189)	.009	.180 (.756)	.039
White Collar Offense	.049 (.163)	.010	-.387 (-.917)	-.084	.137 (.457)	.030	.059 (.196)	.012
Offense Value Greater than \$5000	-.387 (-1.592)	-.084	-.435 (-1.664)	-.095	-.240 (-.979)	-.052	-.359 (-1.556)	-.078
First Offender	-.170 (-.386)	-.037	-.116 (-.264)	-.025	-.380 (-1.579)	-.083	-.160 (-.367)	-.034
Age at First Commitment	-.043 (-1.918)	-.009	-.044 (-1.969)	-.009	-.068 (-3.348)	-.015	-.049 (-2.164)	-.010

TABLE CONTINUED . . .

TABLE 6 (Continued)

Independent Variables	Recidivism ^a		Recidivism With Predicted Time Served ^b		Recidivism With Predicted Ratio of Commitments/Convictions ^c		Recidivism With Predicted Employment ^d	
	(1) β	(2) $\partial\hat{p}/\partial x_i$	(3) β	(4) $\partial\hat{p}/\partial x_i$	(5) β	(6) $\partial\hat{p}/\partial x_i$	(7) β	(8) $\partial\hat{p}/\partial x_i$
Commitments/Convictions	2.615 (4.129)	.570	2.534 (4.098)	.552	--	--	2.586 (4.035)	.561
Predicted Commitments/Convictions	--	--	--	--	-4.193 (-.775)	-.922	--	--
Convictions	.015 (.633)	.003	.012 (-.496)	.002	-.013 (-.567)	-.003	.013 (.551)	.002
Employed More than 4 Years	-.544 (-1.217)	-.187	-.569 (-1.274)	-.124	-.826 (-1.866)	-.181	--	--
Predicted Employment Greater than 4 Years	--	--	--	--	--	--	-3.586 (-1.115)	-.778
Constant	2.219 (2.786)	--	3.118 (3.139)	--	1.851 (2.175)	--	1.449 (1.278)	--
Weighted Mean of Dependent Variable	.357	--	.357	--	.356	--	.357	--
Predicted Probability of Weighted Means of Independent Variables	.321	--	.321	--	.326	--	.318	--
Chi-Square	86.285	--	87.709	--	70.092	--	86.061	--

Source: Data from U.S. Board of Parole Research Unit.

^aFrom Table 2.

^bPredicted values computed from Table 5.

^cPredicted values computed from Table 4.

^dPredicted values computed from Table 3.

Eliminating the racial disparity in preprison employment has a decidedly direct effect on blacks' postprison failure rates. The predicted recidivism probability falls from .321 to .318, as seen in column 7. Although this reduction appears miniscule on first glance, it is seen as one of the few obvious means of reducing the racial gap in recidivism.

IV. CONCLUSION

Punishment is dispensed in relatively more abundant quantities to blacks than to whites. Blacks have longer prison terms and face more certain punishment. Simple theory suggests that if punishment reduces participation in crime, meting out less of it to blacks would increase black crime rates and, ceteris paribus, increase the racial gap in crime.

Even so, our empirical findings suggest that the racial gap in recidivism remains unaffected when punishment is dispensed in a racially neutral manner. What this means is that the longer prison sentence lengths typically given to black relative to white felons are not necessary in order to assure that black ex-felons do not disproportionately become recidivists. They will become recidivists, to be sure. But that is because of their significant employment disadvantages, perhaps attributable to racial discrimination in the labor market. The latter factor may be related systematically to blacks being disproportionately in prison in the first place but cannot be counterbalanced by unequal punishment. In a word, then, the unequal quantity of punishment is socially inefficient.

Other writers have alluded to a legacy of racism as the cause of inequalities in the criminal justice system. They suggest that slavery and its aftermath are at the root of the continuing injustice of longer sentences served by blacks, of their high probabilities of being sent to prison and generally of the harsher punishment they face in the criminal justice system. Indeed, one writer has argued that this state of affairs is intimately linked to labor markets: after the Civil War, a loss of a whole class of workers in Southern agriculture mandated that the prison system--already evolving as a labor market mechanism--supply public labor

when private involuntary servitude had been eliminated (Sellin, 1976).

Sellin's story goes something like this. In the early years of the nation, penitentiaries were designed to house criminals from the master class. Slaves were punished through beatings or execution. Free black criminals were sold as slaves or deported. There was, however, a significant push to make the penitentiaries occupied by the master-class criminals self-supporting, since the costs of imprisonment represented a heavy burden on taxpayers. Why not make the prison turn a profit? In Kentucky this was tried during the early nineteenth century, and the convict-lease system was born. In this system, a profit was made by hiring out the convicts. Attempting to fight the high prices of Northern manufacturers and to train machine operators, other Southern states, including Louisiana, invited private firms to set up shop in the prisons. Following the Civil War, however, both prison industries and convict-lease systems faced a major challenge in the South. Would these systems apply to thenewly emancipated blacks? Would the master class and the former slaves be forced to workside-by-side? The answer was simple. Since the economy was shattered and there was a rapid outflow of labor from the agricultural sector--where blacks allegedly held a comparative advantage--prisons could be used effectively as a means of continuing slavery. With a system of penal servitude, private slavery would be replaced with public slavery. In part, the Thirteenth Amendment to the U.S. Constitution explicitly authorized "involuntary servitude" as punishment for illegal activities. Southern legislatures rushed to enact legislation and to revise their penal codes, with an almost unbelievably rapid result: Within a decade after the Civil War, prison populations in the South shifted from being virtually all white to being disproportionately black. And, so the story goes, this is how prisons have become what they are today in America.

The federal prison system, however, serves a somewhat different constituency than do state penitentiaries. Imprisonment is a sanction in numerous sections of U.S. codes, including those relating to income tax evasion, selective-service violations, and interference with federally protected activities (e.g. civil rights violations). With the exception of punishment of residents of the District of Columbia, Indian reservations, and U.S. territories, the arm of the federal criminal law rarely extends to common street crimes. Most forms of robbery, burglary, larceny, auto theft, assault, rape, and homicide are prosecuted at the state or local level, even though they are also prosecuted at the federal level.

In addition, the origins of the federal prison system lay principally in the North, the capitalist mecca that the Southern states were competing with when they devised the convict-lease system and prison industries. In some respects, then, it is less obvious as to how the racial disparities in the federal criminal justice system are rooted in the same legacy of slavery and racism detailed by Sellin. Still, we find significant racial disparities in punishment even in the federal prisons. And eliminating these criminal justice disparities has not impact at all on black crime.

Our evidence is consistent with the view that the nature of "racism" allegedly pervading the criminal justice system might well be understood by a careful examination of labor market inequities. Our evidence is also consistent with the view that eliminating racism in the criminal justice system probably won't eliminate the racial gap in crime. Blumstein's (1982) hostily received research on racial differences in incarceration rates perhaps goes too far in placing the blame outside of the criminal justice system: we find that indeed there is a significant and measurable gap--of five months--between the punishment blacks receive and that which they could expect in a racially

neutral scenario. Racial discrimination in punishment does exist, if our methodology and model specification are to be believed. The key to our findings is that eliminating the discrimination in punishment will not make blacks more criminal but it will mean fewer blacks serving long sentences (and possibly proportionately fewer blacks in prison). Since a major social cost is incurred in housing blacks in our nation's federal prisons for longer periods than equally situated whites, the racially discriminatory punishment is seen to be both unfair and inefficient.

If these conclusions are overdrawn it is not for want of a superior methodology. Indeed, the methodology cries out for other researchers using alternative data sets to confirm or refute these findings. We invite skeptics to meet this challenge head-on.

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