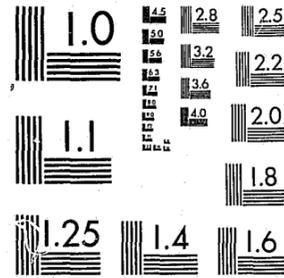


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~~EMPLOYMENT OPPORTUNITIES AND CRIME~~

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EXECUTIVE SUMMARY

A. OBJECTIVES

This research project was designed to explore to what extent improved employment opportunities would reduce crime and to determine the effects of criminal history and disadvantaged backgrounds on the employment prospects of exoffenders. An additional objective was to examine the role racism or racial discrimination plays in the interaction of the criminal justice system and labor markets.

B. METHODS

Employment opportunities were measured by preprison work experience and postprison unemployment, hours worked, and wages. Participation in crime was captured by convictions, arrests, and time served before release from prison and by rearrest and parole violations after release from prison. Multiple-regression analysis, nonlinear least squares, and maximum-likelihood methods were employed to obtain estimates of both the effects of employment on crime and the effects of other variables on employment in linear, log-linear, and logistic model specifications.

C. DATA

A random sample of 2,500 exfelons released from the U.S. Federal Prison System in 1972 was obtained from the U.S. Board of Parole.

Information about these exfelons includes background characteristics, preprison employment, criminal history, offense characteristics, prison outcomes, and rearrest and parole violation records (obtained from FBI "rap" sheets one year after release).

A sample of 432 high-risk male repeat offenders participating in a cash-subsidy program in Baltimore was obtained from researchers involved in the evaluation of the program. Information included background characteristics, preprison employment, criminal history, offense characteristics, prison outcomes, and monthly information on rearrest, postprison wages, hours worked, and unemployment. There was a one-year follow-up.

D. THE RESULTS

- Criminal history has a weak and insignificant effect on postprison employment.
- Although preprison employment experiences do have a slight impact on postprison employment outcomes, success in the labor market after imprisonment hinges largely on whether the exoffenders (a) had a job arranged for him, (b) became employed during the first few months after release from prison, and (c) on the hours worked in the first six months out of prison.

- Black exoffenders have less favorable postprison employment experiences than white exoffenders.
- Family background explains much of the variation in postprison employment among white exoffenders, but not among blacks.
- Preprison employment experience translates into fewer hours worked for blacks than for whites.
- White exoffenders are molded by their backgrounds; virtually nothing after prison release seriously alters their previously established pattern of employment experience.
- Better wages reduce crime.
- Preprison employment experiences have a weak and almost insignificant effect on postprison rearrest.
- There are significant racial differences in recidivism rates.
- There are substantial differences in how blacks and whites are treated in the federal criminal justice system.
- However, these differences cannot account for their differences in recidivism.
- When controlling for differences in criminal history and background characteristics, much of the racial gap in recidivism narrows.
- No further narrowing comes about by conceptually ridding the criminal justice system of residual discrimination.
- If black-white preprison employment discrimination were eliminated, the predicted recidivism rate for blacks would converge to the actual rate for whites.

E. POLICY SIMPLICATIONS

Our findings suggest that policies designed to improve the employment performance of exoffenders should be implemented

immediately upon release from prison. They also suggest that the most disadvantaged of exoffenders--those from minority groups and those with the lowest wages and highest unemployment--are most likely to be responsive to employment intervention strategies. In addition, we argue that training programs, public-work jobs, sheltered or other public-service employment may be ineffective in reducing recidivism. Instead, wage subsidies seem to represent a more promising strategy for reducing recidivism.

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CHAPTER I. EMPLOYMENT AND CRIME: AN OVERVIEW

A. INTRODUCTION

During the past decade and a half, various economic and social policies and programs have evolved in response to, among other things, a concern about crime and about the efficiency and equity of the criminal justice system. Concomitantly, manpower training and skill development programs have emerged to assist low-income, minority, and disadvantaged workers.

As a consequence in part of the social labelling and selective enforcement aspects of criminal sanctions in America, many exoffenders are among the most disadvantaged of disadvantaged workers. Yet, the cornerstone of most early federal manpower programs, the Manpower Development and Training Act (MDTA) of 1962, initially prohibited the training of prison inmates (U.S. Department of Labor, 1973). Although over the years--by way of amendments to the MDTA of 1962 and the evolution of the Comprehensive Employment Training Act (CETA)--numerous employment programs for exoffenders, parolees, juvenile offenders, and prison inmates have been implemented, a clear understanding of how employment opportunities and crime interact has not emerged.

The absence of an unequivocal indication of the degree and direction in which crime and employment opportunities interact with

one another does not reflect a lack of scholarly interest in such an interaction. In particular, economists and labor market specialists have had a lot to say about legitimate opportunities and crime. However, none of that research helped much in averting the failure and demise of numerous prototype employment programs for criminals and exoffenders.

Undeterred by early program failures, and unguided by relevant or realistic economic models of how crime and labor markets interact, manpower specialists in the 1970s devised and implemented numerous labor market aids designed to reduce crime. Their innovations have had only a limited success. Yet they have resulted in rich data sources on offenders and workers that permit for the first time a careful assessment of how the labor market and the criminal justice system interact. So although we may not have better manpower programs that work to reduce crime, we have learned much about employment and crime. Imparting that information is a central objective of this monograph.

At the outset, however, it is useful to convey an essential concern motivating this analysis. Frequently researchers have blamed distortions for the failure of labor markets to keep people employed or the failure of the criminal justice system to keep people out of crime. Distortions, or more specifically market distortions are

often unrelated or external interferences that impede the ability of the system to operate efficiently. Racism or racial discrimination is one such distortion. Racism in the labor market results in workers through time being denied access to certain job opportunities--a denial that forecloses even the possibility of subsequent economic mobility. It can be a subtle form of enslavement. Racism in criminal justice systems results in some alleged offenders being incarcerated and psychologically brutalized, virtually guaranteeing their continued entrapment in the criminal world and the criminal justice system. It can be a stark form of enslavement.

The distortions of racial discrimination and racism not only cause inefficiency, but they also cause gross inequity. Some writers have argued that this is not so unusual at all. They contend that the system achieves a balance, given a certain level of desired inequity, through gains or pay-offs elsewhere. For example, some black people will always make it "big" in crime by progressing out of street crime into white-collar crime. Some black people will make it big through legitimate employment, perhaps into the ranks of corporate management. The balance comes because of dualism in the crime and legitimate worlds. White-collar crimes are defined, litigated, and punished differently than street crimes. But in fact they are all crimes. Corporate managerial jobs are more differentiated from the lowest occupational categories by the people who hold them than by the tasks that are performed. But there is

room for a few token blacks who ordinarily would be relegated to the bottom. Such is the balancing act.

This description suggests two possible lines of inquiry. On the one hand, it could be argued that the apparent equilibrium of racially unequal outcomes in both the labor market and the criminal justice system is purposeful. It is rooted in a conscious, systematic design. It has its original legacy in slavery and other racist institutions that have changed over the years only in their outward appearance.

On the other hand, it could be argued that the distortion of racism or racial discrimination, however evolved, creates the central link between labor markets and crime. Racial discrimination in labor markets results in lower incomes to blacks, so they turn to crime. Because of discriminatory treatment by police, blacks are more likely to be accused of crimes of theft or crimes against property; so they are arrested, go to jail, serve long sentences, and withdraw from the labor market. With criminal records and little work experience, offenders, who are disproportionately black, cannot find employment.

These two perspectives are neither mutually exclusive nor collectively exhaustive. But the latter is persuasive enough alone to propel the discussion that follows. First, we summarize the theoretical and empirical evidence on the relationship between

employment and crime. Dualism plays an important role in this literature. Then we outline in detail the way racial discrimination links together outcomes in the labor market and in the criminal justice system and we suggest why numerous manpower programs for exoffenders have failed. In a penultimate section some new empirical evidence is offered in support of the perspective advanced.

B. VIEWING CRIME IN A MANPOWER PERSPECTIVE.

There are two distinct models of crime. One is called the choice-theoretic model. The other is regarded as a dual or segmented labor market model. Both adopt a manpower perspective because either crime is regarded as a job as any other income earning activity is, or it can better be understood within the context of the labor market structure from which it often springs. The models are different in that the relative importance of rational decisionmaking differs between them. But in practice it is difficult to distinguish between the models because most empirical analyses yield results consistent with them both.

1. CHOICE-THEORETICAL APPROACH TO CRIME

Although some of the earliest modern writings on crime by economists concerned the interaction of labor markets and criminal behavior, much of the literature on economics and crime published since the contribution by Becker (1968) on crime and punishment has

concerned the deterrent effectiveness of the certainty and severity of punishment.

The concern over the certainty and severity of punishment stems in part from the desire to analyze how the consequential opportunity costs of punishment affect the choices of rational, self-interested decisionmakers.

Ehrlich (1973) expands on the Becker theory by investigating the potential criminal's optimal allocation of time to crime and work. Making choices in the face of uncertainty, the individual chooses either to enter or not to enter criminal activity in the process of maximizing his expected utility--an index of personal well-being and preferences--calculated for contingent states of the world. Since expected utility declines for increasing certainty or severity of punishment, optimal participation in crime declines for increasing punishment. The central results of Becker and Ehrlich have not gone unchallenged.

Flock and Heineke (1975) argued that the Becker-Ehrlich results are based on restrictive assumptions about the probability distributions for success or failure in criminal activity. In general, it is discovered that the effects of the certainty and severity of punishment on optimal participation in crime are not determinate for arbitrary success or failure distributions.

Moreover, recent National Academy of Sciences studies have concluded that the empirical tests of the Becker model, while appearing to support the theoretical results of the deterrent effectiveness of the certainty and severity of punishment, should be viewed with extreme caution due to the insufficient attention paid to the statistical problem of correctly identifying the direction of causation in the model.

Furthermore, Frier and Fienberg (1980) in a careful review of virtually all of the econometric tests of the Becker-Ehrlich model cite many problems of data reliability and inappropriate statistical techniques as rendering the majority of the favorable tests useless.

However, in theory and often in application, the choice-theoretic approach predicts that employment affects participation in crime. In extremely simplified versions of the model, higher unemployment leads to lower expected returns to work and thereby increases the propensity to engage in crime. Better wages, higher income, and lower unemployment will have ambiguous effects on crime, however, in more general choice-theoretic models.

2. SEGMENTED LABOR MARKET APPROACH

Another economic approach to crime similarly emphasizes opportunities as well as environmental conditions. Some "opportunity-

environmentalists" suggest that crimes are directly or indirectly determined by such economic factors as poverty or inequality or by the oppression of laws (a function of the political structure that is dependent on the economic superstructure). Closely related is the set of hypotheses advanced by theorists of segmented labor markets who argue that though actors appear to make rational self-interested choices, their opportunities and preferences are actually determined by institutional arrangements.

The first systematic description of criminal behavior within the context of segmented labor markets was provided by Piore (1968). In reflecting on the characteristics of jobs and workers in two distinct employment sectors, Piore argues that the behavioral patterns fostered by low-paying, menial, and unpleasant "secondary labor market" jobs are reinforced by a lower-class life style that is "more compatible with welfare and illicit activity than with legitimate employment." However, these same behavior patterns, for example, lateness and absenteeism, tend to shape both the opportunities of disadvantaged workers and the characteristics of the jobs they face. In a sense, then, secondary labor market workers' actions are both determined and determining.

Piore defines the primary labor market as that employment sector reflecting good pay, good working conditions and generally steady hours, and fair and equitable policies. The secondary labor market

is defined as that employment sector with particularly less attractive features than the primary market with principally low wages, poor working conditions, and less regularity of work. Disadvantaged urban ghetto workers are almost exclusively confined to the secondary labor market. Not only do lack of skills, training, and education prevent those workers from participation in the primary market, but the traits generated and cultivated in the secondary market, such as high rates of absenteeism, lateness, insubordination, and other forms of "deviant conduct" are not tolerated in the employment sector paying good steady wages.

The confinement of the poor urban ghetto dweller to the secondary labor market can be thought of as a direct result of socially imposed obstacles as educational shortcomings, police records, spotty prior work experiences with frequent job changes or extended unemployment, or as an indirect result of these same obstacles as manifested in the feeling of hopelessness and uncertainty for the future.

Piore and other segmented labor market theorists point out that-- despite attempts by public and private programs to eliminate the more visible barriers to good and adequate jobs for ghetto workers through training and education--poverty, misery, and discontent appear to have increased rather than diminished. Nevertheless, an economic alternative can and does persist for the frustrated of urban ghettos: illegal activity. This alternative is not viewed as a rational

response in the sense of the free-willed, individualistic potential criminal described by writers as Becker and Ehrlich. Opportunities do play a role in rational calculus, but institutionally determined decisions do not. To the extent that police records and the like are often inequitably distributed social sanctions, some individuals are labelled criminal independently of the rational choices they make.

It is possible to both believe in segmented labor markets and to model crime using conventional choice theory. The result would be an equilibrium model of criminal and legitimate labor markets, appropriately stratified, that exhibit distinct imperfections. But this means they are not temporary imperfections, so another theory would be required to explain how they are maintained through time.

3. EVIDENCE ON CRIME AND EMPLOYMENT

Many studies have examined the relationship between crime and employment. The glaring deficiency of time series and cross-section analyses of unemployment rates and crime rates by Brenner (1976), Fleischer (1966), and Glaser and Rice (1959) lies in the aggregate nature of the studies. However, strong, although contradictory, evidence is found in these reports linking crime to aggregate levels of unemployment.

Gillispie (1975) provides a thorough review of virtually all of the early studies on crime and income. He also concludes that the

aggregate data are at best suggestive of a link between economic variables and crime but without revealing how that link might be formed. More recently Witte (1979), examining micro data sets in addition to the volumes of studies using aggregate data, shows greater scepticism. She thinks that there probably is no direct connection between unemployment and crime. She suggests that extreme caution should be exercised in drawing conclusions from evidence showing a significant relationship between employment and crime.

If there is some doubt about a general relationship between employment and crime, there is little doubt that there are specific interactions that involve both labor markets and the criminal justice system. Miller (1978) has estimated that nearly one quarter of the labor force have criminal records. The existence of a criminal record has been shown to restrict the type of occupation one can enter (Portney, 1970), to increase the chances of dismissal from a job one already holds (Leonard, 1967), and generally increase the likelihood that one will be unemployed (Leiberg, 1978). The employment prospects of exoffenders are bleak. Pownall (1971) reveals that released offenders have higher turnover rates, higher unemployment rates, and lower wages than the general population.

There also seems to be specific discernible effects of poor employment opportunities on participation in crime. Phillip Cook

1975), in an analysis of a sample of Massachusetts parolees, concludes that improved job opportunities reduce the probability that an exoffender will recidivate. Robert Taggart (1972) and others have cited findings that suggest that participation in illegal activity is linked to failure in the job market.

The difficulty with these findings, it is conceded, is that the populations being examined are disproportionately black and disadvantaged. Are their unfavorable outcomes due to their specific disadvantage of being exoffenders or to their general disadvantage of being poor and black? Is race merely an intervening variable here, or is it the central factor linking performance in the labor market with outcomes in the criminal justice system?

C. RACE, CRIME, AND UNEMPLOYMENT

There is substantial statistical support for the contention that blacks are overrepresented in the criminal justice system. In 1975, for example, of the 1.8 million arrests for serious crimes reported in the United States, nearly one-third were arrests of black men and women. More than half of those under eighteen years old who were arrested for violent crimes were black youths. (National Criminal Justice Information and Statistics Service, 1978.)

Because blacks are more likely to be convicted and then receive longer prison terms, their overrepresentation is even greater in the

prisons. While they account for 11 percent of the total U.S. population, blacks represent 47 percent of the total prison population. (National Prison Statistics Special Reports, 1979). Some contend that these facts are due to the greater propensity of blacks to engage in crime. Others assert that there is a greater propensity of blacks to be labeled criminal, although in fact they are not. These facts have fueled an ongoing debate about the crime-rate differentials for blacks and whites. More important, though, these facts correlate almost amazingly with racial disparities in employment.

Repeatedly, labor market studies reveal that relative to white workers blacks receive lower wages, are disproportionately represented in menial occupations, have higher turnover rates, and consistently have higher unemployment rates.

The following simple model, in the tradition of Becker and Ehrlich, illuminates how race, crime, and employment may interact. Assume that there are exactly two income-earning prospects facing potential criminals--work and crime--and that total income is the sum of legal and illegal earnings. Illegal and legal earnings, of course, depend upon the rates of return, or wages, to crime and to legitimate activities.

Now suppose that a person chooses the amount of time to spend in crime and work so as to maximize expected income. Then it can be shown that the optimal allocation of time to crime depends upon the relative attractiveness of crime and work. This simple, abstract model yields an intuitive result that could have been obtained through a more realistic vehicle. Implicit is the assumption that we are all rational, self-interested, individualistic, calculating beings. Despite this obvious lack of realism, the model goes further.

Suppose that blacks and whites are identical in every respect except that blacks are less likely to be hired, receive lower wages, and, therefore, expect lower wages than whites because they are discriminated against in the labor market. Given these assumptions, relative returns to crime for blacks are greater than those for whites. Hence, rational, self-interested, individualistic, calculating blacks should spend more time in crime, because it pays. It is easy to see how a color-blind criminal justice system interacting with a racially imbalanced labor market can lead to very unequal outcomes for blacks and whites. Within the context of this simple model, however, it is possible to visualize unequal outcomes for blacks and whites when the labor market is perfectly balanced and the criminal justice system is fair and unbiased.

Suppose, once incarcerated, blacks are less likely to be released, not because they are black, but because they have in

abundance characteristics that the statisticians have discovered are intimately linked to crime. Thus blacks will serve longer prison sentences, and all other things being equal, prisons will be disproportionately black. When they are released, blacks and whites with the same job experiences and employment histories will be paid the same wages and be offered equivalent jobs. This is fair and just; it derives from the assumption that the labor market is perfect -- that there are no distortions, imbalances, or ugly imperfections such as "racial discrimination." Yet for the very reason that blacks serve longer sentences than whites, they will receive lower average wages upon release from prison than whites; they have accumulated less work experience and are less valuable to rational employers. Since the returns to work will be lower for blacks, their relative returns to crime will be higher; thus they should rationally allocate more time to crime, because crime pays.

We see in the model an apparent absence of racism or racial discrimination of any sort, but the outcomes are very unequal. The apparent absence of racism in the model of course does not wish it away in reality. Why are blacks overly endowed with characteristics that are statistically related to criminality? Because of a historical legacy that denied access to political and economic mobility for their ancestors? Because the laws have been defined-- explicitly during Jim Crow Years, implicitly thereafter--to make what they do punishable by imprisonment while what the Kennedys and

...offenders do is rewarded by elected office? The answer one provides is independent of the consequential result: Race is the major factor that relates labor-market success to criminal justice system outcomes.

The assertion that crime and employment are linked together by the ubiquitous phenomenon of racism astonishes some, puzzles too many. But upon reflection the relation is not so obscure at all. If one examines the thousands of jobs that are beyond the reach of ex-offenders because of state and federal licensing restrictions, one is amazed that a significant public outcry for changing these laws was not heard prior to the early seventies. Yet, as these restrictions topple, the numbers of unemployed ex-offenders remains virtually unchanged. Why? Few ex-offenders, who we have previously argued are disproportionately black, qualify for these licensed jobs, criminal record or not. Moreover, white-collar criminals, even disbarred attorneys and expelled congressmen, manage to live in a style supported by the rules of the game they themselves wrote. Would prostitution be illegal if streetwalkers wrote the laws?

But more concretely, it can be established that the duality of the criminal justice system evolved in tandem with the duality of the labor market. Historians of penal reform, notably Thorsten Sellin, provide ample documentation. It might be recalled that the constitutional amendment that eliminated slavery also assured the

continued use of involuntary servitude in the criminal justice system. This left the door open for the continued slavery of blacks. After abolition of slavery there was a strong need for a pool of cheap labor. Some freed slaves refused to work for their former masters. Some did not work. Most, converging on the cities, could find no paid work. "Vagrancy" often resulted. And this, you need not be reminded, was a crime.

Imprisonment rates for blacks more than doubled after the Civil War. Incarcerated blacks particularly in the South were often placed in convict camps, hired out to their former slave masters, and then inhumanly beaten and brutalized if they did not settle back into the slavish routine. This sort of forced labor became so well institutionalized that many states required that the prison systems become self-sufficient. Even well into the twentieth century, when convict-lease systems were replaced by service to state-run "chain gangs," blacks' employment in prison mirrored their pre-Civil War conditions. They had the lowest-paying, dirtiest, most menial jobs available. In prison and out of prison the heritage of slavery had been preserved.

As difficult as it is to believe, a scenario of the prison system and, indeed, the entire criminal justice system as a powerful political and economic force emerges. Both the dual system of justice and the dual economic system are contemporary versions of

... pre-Civil War predecessors. The racial underpinnings of these dual systems are still there. The outward manifestations of racism, of course, have been eliminated.

PROGRAM FAILURES

Most programmatic efforts based on even the most loosely constructed conceptualization of an interaction between employment and crime have focused on the exoffender. There are exceptions, of course. Crime prevention strategies like those based on the community anticrime model admit a peripheral relationship between local job opportunities and crimes of theft and vandalism. Summer job programs for youth, for example, reduce the idleness of a large crime-prone group and thereby reduce crime. Yet evaluation of the effectiveness of such measures is elusive. How many crimes would have been committed in the absence of the program? Can we adequately control for all of the other intervening variables--for example, the effect of hot weather on crime or the effect of being out of the hot weather and in an air-conditioned work place--and still isolate the independent influence that work has on crime?

Both because the population is targeted and easily identified through official sources and because "treatment" effects can be more rigorously examined, exoffenders and in particular exprisoners have often been the subjects of programs and experiments designed to

reduce crime through improved employment. Four types of programs have been explored thoroughly: employment and vocational training, job-search assistance, cash subsidies, and supported work. All have similar failings.

The logic behind training for exoffenders and for current or recently released prisoners is straightforward. These individuals have little education, spotty previous employment experiences, and few marketable job skills. In other words they have what economists call low endowments of human capital. Enhancing the human capital of offenders is the task of training programs. If these potential workers can be given a skill, it is thought, they will find better jobs and thereby will find work more attractive as a livelihood than crime. Thus they will not return to crime. As reasonable as this perspective may sound, however, reviews of the many offender employment and training programs implemented during the 1960s reveal that few of them work. Correctly, one Abt Associates' (1971) report concludes that it is not so much that the programs did not work. It is just that on the basis of poor evaluation, poor design, or poor implementation it is often impossible to ascertain what effect if any training has on reducing recidivism. A case in point is an evaluation of federal community treatment centers (CTC) conducted by the U.S. Bureau of Prisons (1978). One group of federal prisoners was released on parole and another group was released to community treatment centers. At the CTCs, exoffenders received a wide variety

of employment and vocational training and counseling. The paroled group received no treatment. Although the groups were "matched" in the sense that both would have been eligible for release to community treatment centers, there remained significant differences in the groups. Nonetheless, it was found that the CTC group had lower rearrest rates than the comparison group.

Closer examination will reveal that the findings are based upon small fractions of the original samples. There is an extraordinary amount of attrition and that attrition seems to differ systematically between the "treatment" group and the comparison group. Until the statistical results can be adjusted for this apparent selection bias, any conclusions drawn should be viewed with extreme caution.

The rationale for job-search assistance is that recently released exoffenders do not know where the available jobs are. Although many states have employment referral services, the consensus is that these sources represent only a minor fraction of all matches between jobs and applicants in the labor market. Counselors who are attuned to the needs and special problems of offenders, it is argued, can be more effective in securing jobs for their clients than the impersonal bureaucratic mechanisms that have evolved in local CETA programs. The results of all of the job experiments have not been succinctly evaluated. But if the experience of the Baltimore LIFE (Living

insurance for Exprisoners) is in any way illustrative, then they fare no better than training programs.

In a sample of 432 hardcore, repeat property offenders, one quarter received job-search assistance, one quarter received a cash subsidy equivalent to unemployment compensation, and one quarter received neither cash nor assistance. A last group received a combination of cash and job assistance. The postprison rearrest rate for the job assistance group was insignificantly different from the control group that received nothing, so Mallar and Thorton (1978) find.

Cash subsidies flowing from a distinctly different notion of the real problems faced by exoffenders have had a more promising record. Kenneth Lenihan (1974) has carefully studied the problems faced by recently released prisoners. He believes that along with the social-pathological problems of readjustment to old friends, neighborhoods, and family, there is the acute problem of low financial resources. Exprisoners just do not have savings or a cash cushion upon release from prison in order to adequately bridge the gap between the two worlds of prison and work. The Baltimore LIFE project mentioned above was designed in part to bridge the gap. By providing a cash subsidy for a number of weeks after release from prison, the exoffender would have sufficient time to readjust to the outside world, time to look on his own for the kind of job that he can feel comfortable in, time to think, and time to settle down. Indeed,

Nellar and Thorton (1978) find that this sort of unemployment insurance has a strong and significant impact on reducing postprison recidivism. However, Rossi and his colleagues (1980) in an effort to replicate the Baltimore project in Texas and Georgia do not observe this encouraging finding. They cite problems of administrative differences in disbursing the subsidies and differences in the composition of the samples. But their most important finding is one that is the key to a reasoned criticism of all of these programs for exoffenders and a clue to why those programs "fail."

Rossi's work argues that there is a strong work disincentive effect operative. Unemployment insurance discourages work. Work is inversely related to crime. Hence, unemployment insurance increases crime rather than reduces it. But the objective of this and many other well intentioned programs was to reduce recidivism, not to increase employability. Often the outcomes, particularly in the vast majority of federally funded criminal justice programs, are measured in terms of "success" or "failure" where these concepts are typically unrelated to the notions of adjustments, personality change, improved sense of self-worth, or other sociopsychological correlates that have been well documented as accurate indicators of satisfactory transition from the world of prison to the world of work.

A more recent experiment builds on past mistakes. The supported-work model assumes that the adjustment from the world of prison to

work requires a strategically phased reentry program. There is a need for encouragement and positive response. There is a need for support from peers who have been through it all themselves. Piliavin and Gartner (1980) report on some of their preliminary findings on the postprison outcomes of individuals participating in this sort of work environment with peer support. The evidence is disappointing. Despite severe problems of attrition and sample-selection bias, these programs do not appear to work. Recidivism rates are not lower for program participants. Employment rates appear not dramatically affected.

It should be concluded that these or other programs could work. For example, cash subsidies to employers for hiring exoffenders or subsidies to supplement the wages of exoffenders could possibly bypass the work-disincentive effects cited in the review of that program failure. But it should be remembered that none of the programs of labor market intervention was designed or implemented with a careful conceptualization of the role that racism and racial discrimination play in distorting both the labor market and the criminal justice system. Without a better theoretical concept of how race, crime, and employment are intertwined, future programs and experiments may suffer similar fates as those discussed here.

c. NEW EVIDENCE

The discovery of the role that racism plays in forging a link between labor markets and the criminal justice system justifies a closer reexamination of previous studies. Many writers and researchers have observed that there are significant racial differences in both recidivism and in postprison employment. Undoubtedly, these differences could be accounted for by factors correlated with race. Or these differences could be accounted for by diverging demographic profiles between blacks and whites. In either case, one must be extremely cautious in blaming racism for the unequal outcomes.

For example, young people have lower earnings and higher unemployment regardless of their race. They are also more likely to be arrested because in their early years of adulthood they may be more active in visible street crimes. If the age distribution of the black population is skewed toward the younger ages relative to the white population, the higher crime rates and lower employment rates among blacks could be accounted for by their youthfulness alone. Numerous statistical techniques are available for controlling for such possibilities. These techniques are well known and are widely used in a large body of literature on racial discrimination. In the course of engaging in such orthodox exercises, I uncovered some interesting new evidence. The evidence illuminates the complex interaction between institutionalized forms of racism and the institutions of the labor market and the criminal justice system.

Peter Hoffman and Barbara Meierhoefer (1979) have reported on an excellent data set of the postrelease arrest experiences of federal prisoners. For six years after their release from prison in 1970, nearly 2,000 exfelons were traced using the FBI's "rap sheet" records. Hoffman and Meierhoefer have graciously lent a later version of this data set to me.

A number of factors have been found to be significant in determining postprison rearrest probabilities. These include a measure of preprison employment experiences, a measure of the certainty of punishment, and a measure of treatment by the criminal justice system. It is found that blacks are more likely to be rearrested than whites. But if rearrest depends on employment opportunities, the higher crime rates among blacks could be accounted for by their differing preprison employment experiences. If rearrest depends on the certainty of punishment--that is, on the risks of engaging in crime--then rearrest disparities between blacks and whites could be attributable to their differing perceived risks or responses to these risks. Finally, if rearrest depends on how one is treated within the system or similarly how one gets out of the system, the gap between black and white recidivism could be explained by unequal treatment.

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In table 1.1, preprison employment experience is captured by the probability that one was employed for more than four years prior to

the most recent incarceration. The average for blacks is 11 percent. Black exfelons are less likely to have had any significant stretch of stable employment than whites. But these differences could arise because of differences in background characteristics like age or education. Unlikely as it may seem, if blacks in the sample were highly educated, at very early ages blacks would display low values of the employment experience variable because they would not have been in the labor force long enough to have accumulated this experience after leaving school. Of course, what one would like to do is to control for these influencing factors to capture the ceteris paribus rates of preprison employment for blacks and whites. Indeed when a logit regression technique--discussed by Henry Theil (1971)--is employed to control for age, IQ, sex, education, marital status, drug or alcohol usage, and previous confinement in a mental hospital, the average preprison employment probability converges for blacks and whites. The regression equation predicts that 7 percent of the blacks would have worked on one job for more than four years before prison; it predicts that 7 percent of the whites would have too.

TABLE 1.1
Actual and Predicted Probabilities of
Preprison Employment, Imprisonment, and Parole

| | Actual Probability | | Predicted Probability | |
|------------------------------------|--------------------|-------|-----------------------|-------|
| | Black | White | Black | White |
| Pre-Prison Employment ^a | .11 | .13 | .07 | .07 |
| Imprisonment ^b | .05 | .04 | .00 | .01 |
| Parole ^c | .36 | .50 | .32 | .50 |

SOURCE: U.S. Board of Parole

^a Dependent variable in logit model is probability of having been employed prior to incarceration more than four years. Independent variables are age, IQ, sex, education, marital status, drug or alcohol usage, previous confinement in mental hospital.

^b Dependent variable in logit model is ratio of previous commitments to prison to previous convictions. Independent variables are age, IQ, sex, education, marital status, drug or alcohol usage, previous confinement in mental hospital.

^c Dependent variable in logit model is probability of release on parole. Independent variables are age, IQ, sex, education, marital status, drug or alcohol usage, previous confinement in mental hospital, number of parole hearings, prison punishment, theft conviction, "white-collar" conviction.

Again in table 1.1, risk of punishment is measured by the ratio of the number of previous prison commitments to the number of previous convictions. It is the probability that one goes to prison given a conviction. It is not exactly the "probability of getting caught" variable that many researchers have in mind when they think of the risk of engaging in crime. In fact, this measure is positively related to recidivism: Past failures are more likely to become failures again. Nonetheless, we observe that 5 percent of black convictions result in incarceration while 4 percent of whites who are convicted go to jail or prison. This low ratio for both races is due principally to the fact that an offender can be convicted for any number of crimes but typically only receives one prison commitment in a given trial. Once in prison, however, the offender may serve many concurrent or consecutive sentences. So an alternative interpretation of these diverging ratios is that blacks get sent to prison on the basis of fewer convictions than whites.

A ceteris paribus calculation is valid here also. Controlling for the same background characteristics as in our previous example, we compute the risk of punishment for blacks to be zero percent and for whites just one percent. Because of rounding this finding obscures the fact that the predicted probabilities in fact converge to the same low value of less than one commitment out of one hundred convictions for both blacks and whites.

A third variable of interest in table 1.1 is release on parole. It measures the treatment within the system and the method out of the system. Being paroled often means being adjudged "rehabilitated" by correctional personnel. It means you've paid your dues to society. It means that less of your sentence will be served than the law can demand. It means freedom. Only 36 percent of the blacks found their way out of prison in this manner; half of the whites did so.

But does the average figure tell the whole story? Are not offenders convicted of less serious crimes more likely to be paroled? Are not offenders who avoid mischief in prison more likely to be paroled? Calculating as we did before the predicted probability and controlling for both background characteristic and characteristics of the offense and prison adjustment, we find that blacks are still significantly less likely to be paroled. Even after controlling for the number of parole hearings, prison punishment, and type of offense for which the individual was convicted, whites are one and one half times more likely to be released on parole than blacks. Differences in age, education, IQ, sex, marital status, drug or alcohol usage, and history of mental-hospital confinement cannot account for this disparity. Since we have controlled for any number of background variables and other factors related to the seriousness of the crime and to the adjustment of the prisoner while incarcerated, then federal parole boards must be looking at some other variable in making parole decisions for blacks and whites.

further analysis has shown that controlling for more observable characteristics of the offender and the offense does not diminish the gap much more. Maybe parole boards are using some unobservable index to make their decisions. This unobservable variable is not racially neutral. Or parole boards are using race itself as a screening device. Whichever hypothesis you subscribe to, it is difficult to conclude that parole-board decisions result in racially unbiased outcomes.

What this exercise reveals is that while there are disparities between blacks and whites in the labor market and within the criminal justice system, all of these inequalities cannot be explained away by differences in innate or background characteristics of blacks and whites. Blacks have higher recidivism rates than whites. People released on parole have lower recidivism rates. Blacks are less likely to be released on parole. It seems like a self-fulfilling prophecy. But why are blacks more likely to be in prison in the first place? Surely we have found that, controlling for background characteristics, the chances of having had a bad previous employment history are just the same as those of white inmates.

Here lies the key to the argument being made. Although they are very similar to one another in their preprison employment experiences and their risk of punishment, black and white prisoners compare to the general population in strikingly different ways. The white

prisoner, while similar to the black prisoner in having little education, a spotty work history, and a long involvement in crime, is very unlike the typical white worker. Black prisoners, however, are surprisingly similar to the general black labor force. This finding is true for state prisons and county jails as well as the federal correctional institutions. It is almost as if one could pick people at random from the black labor force and obtain a matching profile of the black prison population, while in order to get a good match for the white prison population the most downtrodden, depressed, and decrepit portions of the white labor force must be scanned.

In addition to comparing to the general population in very different ways, black and white prisoners--who are very similar to one another--are treated very differently within the system. The way out is decidedly different. Freedom from imprisonment, like freedom from involuntary servitude, comes about by way of diverging routes for blacks and whites. What the federal prisoner data reveals is that racism in the criminal justice system seems to deny both the timely release of black workers from bondage and their ready return to the labor market. Future research will need to document the parallels of dualism in the job market and in the prisons. But for now, evidence exists that suggests that even after prison the gaps and disparities continue.

Kenneth Lenihan and Louis Geneive who collected the Baltimore LIFE data provided me with a copy of the computer tape used by Mallar and Thorton. The average hours worked per week for the first twelve months after release from prison are computed and displayed in table 1.2. Blacks worked less than twenty-five hours on average, while whites worked nearly twenty-seven hours. At the then-prevailing minimum wage this differential implies a premium of about \$250 per year. Whites earned more per hour than blacks, so the annual premium is even larger.

TABLE 1.2

Postprison Hours Worked by Blacks and Whites

| | Blacks (N=379) | Whites (N=53) |
|--|-------------------|------------------|
| Actual average per week | 24.39 | 26.86 |
| Predicted using linear regression ^a | 24.93 | 26.78 |
| Predicted using white "criminal record" regression ^b | 26.67 | - - |
| Predicted using white "employment history" regression ^c | 26.66 | - - |

SOURCE: Baltimore LIFE Sample

^a Independent variables include treatment group, age, education, previous employment experience, job arranged upon release from prison, parole or discharge, last job was white-collar job, age when first arrested, family member ever in prison, time served for last offense, total times arrested, last arrest for property crime.

^b Independent variables include treatment group, total times arrested, last arrest property crime, age, age when first arrested, family member ever in prison.

^c Independent variables include treatment group, age, last job was white-collar job, education, previous employment experience.

Controlling for age, education, previous employment experience, age when first arrested, number of previous arrests, and a host of other variables, we predict virtually the same racial gap in post-prison employment. The differing hours worked by black and white exoffenders cannot be accounted for by differing preprison employment experiences, background characteristics, criminal history, or even parole history.

Sceptics will argue that the racial gap in hours worked merely mirrors the gap in crime rates. Blacks "choose" to engage in crime rather than to look for legitimate employment. Hence, on average they work fewer hours. Certainly criminal histories affect this decision. The more time we have invested previously in criminal pursuits the more profitable it will be to continue to engage in crime. But there is another effect of criminal history: A criminal record is a barrier to obtaining employment. Blacks may want to work more hours, but because of their extensive records they are denied job opportunities. To test this hypothesis further, we estimated for whites an equation for hours worked that depended on criminal record variables. Then we inserted the black values of the independent variables into the estimated white equation. In other words, we posed this conjecture: Suppose the effect of criminal record on post-prison employment were the same for both black exoffenders and white exoffenders, would the gap between black and white employment

narrow? The answer is yes. While the average hours worked for blacks is actually 24.9, the predicted value, using the white criminal-history equation, yields 26.7 hours per week.

The sceptics would not be satisfied. "Blacks may prefer crime because with their spotty previous work histories, legitimate opportunities just do not appear as attractive," they might argue. There is some sense to this argument, because indeed the work histories of black exoffenders are very spotty. But do they really prefer to work less in favor of crime? Just as criminal records pose a barrier to blacks, so too may their previous employment history. We perform a similar test to see how many hours blacks would work if the effects of their previous employment histories on postprison employment were the same as the observed effects for whites. First an hours-worked equation, which depends on preprison employment history, is estimated for whites. Then hours worked are predicted for blacks using this equation but inserting the actual values of the independent variables for blacks. Thus we recognize that blacks and whites have different preprison employment histories; we are just interested in knowing if the impact of these histories on postprison employment were the same, would blacks work more hours. The answer is yes. Whereas blacks worked not quite twenty-five hours on average, if treated like whites, even with their more spotty previous work records, they would have worked almost twenty-seven hours.

These results demonstrate that the barriers of a criminal record and of scant preprison employment experiences affect black and white exoffenders differently. These results suggest that racial discrimination, racism, or some racially nonneutral phenomenon is intervening in the interaction between crime and employment.

The examples we have presented here are just illustrative of the entirely different perspective that emerges when the issue of race is introduced into a reasoned discussion of labor markets and crime. By ignoring this perspective, we still observe intimate interactions between crime and employment. But the true sources of these interactions are obscured. Programmatic efforts based on an obscured view yield puzzling and often contradictory results as we have seen. It appears that the programs do not work. The programs are scrapped. Labor-market aids are abandoned. This is a hefty price to pay, because the alternatives are all the traditional tried and true methods of crime control that can reinforce failure in employment and crime, further obscuring the role that racism plays in it all.

F. SUMMARY

The role of racial discrimination or institutional racism in learned discussions on crime and employment has heretofore been ignored. We can only be suggestive in outlining possible theoretical

and empirical relations between race on the one hand and employment and crime on the other.

In a purposefully cavalier manner we introduced a Becker-Ehrlich choice-theoretic model of race, crime, and employment. In that model blacks allocated more time to crime because crime pays. What one gets out of a choice-theoretic model is roughly what we put into it. The image of blacks choosing to be criminals is at once absurd and provocative. It is absurd because all of the barriers, impediments, constraints, and other distortions in the real world can transform matters of rational choice to matters of necessity or even habit. No mention had been made of drugs. The absurdity is apparent when you think of a mellowed-out 500-gram-a day heroin addict maximizing his expected utility, calculating the relative costs and benefits of a life in crime or a rewarding career in legitimate employ, and then preparing to pull up his sleeve to perfect these worldly calculations.

It is provocative, though, because it provides an empirical foundation for examining the role played by racism. If black exoffenders do not work as many hours as white exoffenders then it is either because they do not choose to do so, or because they are not given the chance to do so. Without a theory of choice, however unrealistic, we would be forever grasping in a vacuum at a theory of chance.

We also summarized the well-known failings of manpower and subsidy programs to reduce crime. It would be foolish to continue to advocate expenditures of large sums for labor market intervention strategies to aid exoffenders or to reduce crime if in fact these strategies do not or can not be expected to work. However, none of these programs were based on even a foggy notion of the interface among racism and labor markets and the criminal justice system. A more careful look at previous employment and crime programs may reveal when such an interface is acknowledged and incorporated into the overall strategy that such programs prove to be more effective.

In the next three chapters an attempt is made to explore, whenever possible, the role that racism plays in the link between employment and crime. Chapter two explores the determinants of postprison employment among exoffenders. Do exoffenders perform poorly in the labor market because they are exoffenders or because they are disproportionately young, unskilled, and black? Are there differences between black and white exoffenders in their postprison employment experiences? And can these differences be accounted for by differences in their backgrounds, previous experiences, or how they are treated? Chapter three examines the determinants of recidivism among two distinct criminal populations: federal offenders and prison releases from one Maryland correctional institution. Our interest is in discerning whether preprison employment experience or other labor market factors are related to

postprison rearrest or parole violation. If there is a connection between employment and recidivism, can labor market aids like unemployment insurance be effective in reducing crime? Does the effectiveness of labor market intervention strategies depend upon the relative deprivation--or race--of the criminal? In fact, would the traditional law-and-order approach of increasing the certainty and severity of punishment work just as well? Chapter four goes more deeply into an analysis by racial differences in recidivism by dissecting potential sources of the racial gap in crime. Is there racial discrimination in the criminal justice system? Are there significant differences between blacks and whites in their preprison employment experiences or in their criminal histories? And could these factors or even differences in backgrounds alone account for the differing rates of crime between blacks and whites? If racism or racial discrimination in labor markets or the criminal justice system could be eliminated tomorrow, would there be any appreciable effect on racial differences in crime, rearrest rates, or recidivism? We return, in a concluding commentary, to the implications for policy of the evidence presented. The evidence suggests strongly that employment opportunities do have an important effect on crime and that racial factors indeed contribute to the employment experiences of exoffenders. But neither the evidence nor the historical record suggests readily how hundreds of years of inequities both in the administration of justice and in the provision of employment can be rectified by legislative initiative, judicial order, or executive foresight.

CHAPTER II. EMPLOYMENT PROBLEMS OF EXOFFENDERS

A. BACKGROUND

Researchers have repeatedly found that the labor market performance of exoffenders is dismal (Cook, 1975; Pownall, 1971; Taggart, 1972; Witte, 1976). The products of our nation's prisons experience high levels of unemployment, receive low wages when they are employed, and face high turnover due to dismissals, quits, and layoffs. Some researchers have argued that these conditions exist because the characteristics of exoffenders are predominantly those of low-skilled, disadvantaged workers. But is the dismal performance of exoffenders in the labor market primarily due to their criminal record specifically or their disadvantaged status generally?

Phillip Cook (1975) has argued that the poor labor market performance of exoffenders is due to their heavy endowment with characteristics associated with disadvantaged workers. They are young and nonwhite and hold unstable, low-paying jobs even before entering crime. While this situation may have pushed them into crime, having once been criminals intensifies the disadvantaged worker effect. If Cook is correct, then among exoffenders with varying previous employment experiences, the least disadvantaged should perform better. Disadvantage can be measured by not having held a job for any appreciable period, having worked the longest stretch in a poorly paid, low-status, high-turnover job, or achieving only low educational status. It is reasonable to expect

that, after prison, these measures would be highly correlated with failure in the labor market.

On the other hand, if Cook is incorrect, varying postprison unemployment experiences among exoffenders should not be explained simply by differences in these measures of employment disadvantage but by measures of (1) varying criminal records, if employers discriminate against exoffenders as exoffenders or by (2) unmeasured characteristics that may reflect the degree to which the offender has been rehabilitated, such as high degree of motivation, sincerity, desire for the job, and so forth.

It is extremely useful to make a distinction between poor labor market performance due to general disadvantage as opposed to individual specific phenomena. The policy implied by the former is a broad provision of traditional manpower training and employment assistance. The policy implied by the latter requires specific remedies designed to address the particular categorical needs associated with such specific conditions as having a criminal record.

The substantive methodological problem arising from attempts to make such a distinction between general and specific disadvantage is that some of the hypothetical unmeasured characteristics may well be correlated with outcome variables, with the measures of disadvantage, or both. This problem is one familiar to labor economists studying state dependence and heterogeneity.

Researchers investigating the labor-market experiences of individuals have observed that previous unemployment appears to affect the probability of becoming or remaining unemployed. Is this because being out of work causes potential employees to lose valuable work experience, making them less productive and thereby less likely to be hired? Or is this because some unobserved characteristics, such as attitudes or motivation, affect the propensity to remain unemployed and by remaining constant through time, lead to a spurious correlation between current and future unemployment? The former case has been called "State Dependence" and the latter "Heterogeneity." Chamberlain (1979) and Heckman (forthcoming) have pointed out that generally it is difficult to empirically differentiate between these competing hypotheses regarding the underlying cause of the observed correlation between past and current outcomes. A rough test of the hypothesis of no state dependence is, for example, a test that variables that do not change across spells of unemployment have statistically insignificant regression coefficients. This test is restricted to a limited definition of state dependence and appears less useful in analysis when the past is discontinuous, that is, when there is a period of employment experiences prior to imprisonment followed by another period after release.

Lacking a rigorous statistical procedure for solving the Cook Problem, we pose instead three interrelated questions:

- What effect does previous employment experience have on the postprison performance of exoffenders?

- Does it matter whether previous experience is before or after imprisonment?
- Are exoffenders with more extensive criminal histories less successful in the labor market?

It can be hypothesized that if the poor labor market performance of exoffenders does not come about because of general disadvantage but as a consequence of either in-prison or criminal experiences, then postprison outcomes should have no relation (or a weak relation) to preprison employment. In addressing the above questions, we find very mixed evidence in support of the specific-disadvantage hypothesis.

B. THE BALTIMORE LIFE DATA

The Department of Labor sponsored an experiment in Baltimore between 1971 and 1974 wherein 432 high-risk male offenders were divided into groups that received weekly stipends of up to \$60 a week for thirteen weeks, got assistance in finding a job, or got neither or both. To minimize work disincentives, stipends were continued (but reduced) when employment was found until a sum of \$780 had been received. The sample is drawn from the Baltimore Life Insurance for the Ex-Prisoners experiment (LIFE). (See Maller and Thornton 1979.)

The sample consists of males released from Maryland's state prisons to the Baltimore metropolitan area who had low financial

resources, were repeat offenders, had no known history of alcohol or narcotic abuses, and had not been on work release for more than three months. While the average age was twenty-four, 37 percent of the exoffenders were under twenty-one years and only 10 percent were over thirty-five. On the average, 4.4 years were served in prison for the current offense. Eighty-one percent had served five years or less. The range of time served was two to twenty-one years. About 87 percent of the sample was black, most had been raised in families with male heads ($\bar{x}_i = 67.8\%$), and most had jobs arranged when they were released from prison ($\bar{x}_i = 57.9\%$). However, a significant fraction had been previously arrested for disorderly conduct or were subsequently rearrested for this crime ($\bar{x}_i = 17.6\%$). Most had held principally secondary labor market jobs or were previously unemployed ($\bar{x}_i = 52.5\%$), and all had extensive criminal records. The average number of previous arrests was eight with 30 percent having ten or more. The total number of arrests ranged to forty. Similarly, on the average the exoffenders had been convicted four times with a range to twenty-five previous convictions.

Experience, denoted by the longest job held discounted by time since longest job held, averaged 17.5 months. It was calculated on the basis of the following formula:

Y = experience in months

X = length of time on longest job in months

Z = months since longest job

Y = X · exp(-.004167(Z))

The discount rate is approximately 5 percent per year.

Ten percent had had less than two months discounted experience, 30 percent less than six months, and about 50 percent less than a year. A group of 10 percent had had from forty-three to fifty-nine months of discounted experience. The average school grade completed was the ninth, and 60 percent had completed less than eight years of school.

At the end of the year following release from prison, 61 percent had been unemployed an entire month for at least one month. Of these, 25 percent had only one month of unemployment, 23 percent experienced two months, 16 percent three months, 11 percent four months, and 6 percent five months and nearly 20 percent with one half of a year or more of unemployment. Moreover, almost 100 of the exoffenders experienced more than one nonadjacent month of continuous of unemployment.

One year following release from prison, younger workers were more likely to have been unemployed the entire month, either in jail or sick or both than employed full- or part-time. There was no difference in the preprison arrest records of those who were unemployed the full month and those who worked full-time, although those who worked twenty-one to thirty-five hours per week had slightly fewer arrests while those who worked less than twenty-four hours per week had slightly more arrests than those who remained

unemployed the entire month. More of those who were working twenty-one to thirty-five hours and those who were sick or in jail had some previous work experience than those who were either full-time workers or unemployed individuals. These results are displayed in table 2.1 along with other descriptors of the sample.

Although the average monthly full-time employment rate remained steady at two separate plateaus (at about 6 percent in the first six months and at almost 3 percent in the last six months) the month-to-month unemployment averages declined consistently with a few late-year exceptions. It should be pointed out that these figures are not adjusted for business cycle variations although the t^{th} month employment experience occurred for different individuals at different times during the year (since the reference point is date of release from prison).

On the basis of a variety of measures of disadvantage (work experience, education, race, and arrest history), it is not surprising that we observe such extensive unemployment among those in the sample. But just as the degree of disadvantage varies widely in the sample, so too does the severity of the unemployment experience. It is legitimate, then, to ask of a sample like this to what extent the variance in unemployment experiences is explained by varying degrees of preprison disadvantage.

TABLE 2.1

Description of Post-Prison Employment
Experience One Year After Release

| | Employed (hrs./week) | | | Unempl. Whole Month | Not Employed | |
|--|----------------------|-------|-------|---------------------------|-----------------|------------------------------|
| | 35 | 21-35 | <24 | | Jail or Sick | Jail, Sick and/or Unempl. |
| Age | 24.85 | 25.72 | 29.66 | 23.49 | 22 | 22.89 |
| Previous Arrests | 8 | 6 | 9 | 8 | 16 | 7 |
| Race | .87 | .89 | .67 | .93 | 1.00 | 1.00 |
| Married | .13 | .17 | .17 | .05 | .30 | .11 |
| Months with Previous Work Experience | .56 | .67 | .33 | .51 | .67 | .33 |
| Supported by Persons Other than Welfare | .35 | .39 | .17 | .24 | .53 | .56 |
| Number of Family Members Other than in Prison | .39 | .39 | .17 | .34 | 1.00 | .78 |
| Number of Times Drank Liquor in First Week After Release | 2.06 | 3.50 | 1.50 | 1.88 | 1.00 | 2.89 |
| Number of Arguments in First Week After Release | | | | | | |
| Number of Arguments with Father, Mother; | .04 | .00 | .00 | .05 | .00 | .11 |
| Number of Arguments with Mother, Sister; | .04 | .00 | .00 | .05 | .00 | .11 |
| Number of Arguments with Wife, Girlfriend | .13 | .17 | .00 | .05 | .33 | .11 |

C. POSTPRISON UNEMPLOYMENT

Table 2.2 presents estimates of coefficients in a logistic model of the probability of being unemployed the entire month that marked the first year after being released from prison. Positive and significant are the effects of the number of previous postrelease months of unemployment and the probability of having fought in the current month. Fighting was found in previous analysis to be strongly affected by living arrangements. It is seen in table 2.2, though, that living with one's family tends to lead to lower probabilities of being unemployed. Because of the collinearity between fighting and living arrangements, it is difficult to discern the independent effects of these two important adjustment factors. The evidence is clear that unemployment is correlated with the occurrence of previous employment. While preprison work experience, age, race, arrest occurrence, or financial aid do not appear to have strong effects on unemployment one year after release, the number of months of either adjacent or separate spells of unemployment strongly influences the unemployment rate measured in this way.

Another way of measuring unemployment one year after release from prison is to compute the probability that in the first twelve months of freedom there is at least one entire month of unemployment. Of course, it is no longer legitimate to include post-prison unemployment occurrence as a separate determining factor. However, one would expect that in the absence of the dependence of the probability of unemployment on the occurrence of previous unemployment, time-invariant factors would exhibit no independent, significant effects upon unemployment.

TABLE 2.2

Nonlinear Least Squares Estimates of Coefficients in
Logistic Model of Unemployment During Month
One Year After Release from Prison

| | Coefficient | Elasticity |
|--|----------------------|------------|
| Constant | -3.5269 (-3.4112) | --- |
| Previous Months Unemployed | .4347 (7.3632) | .8192 |
| Fought this Month | 2.0520 (5.2729) | .1179 |
| Age | .0017 (.0519) | .0418 |
| Experience | .0078 (.6171) | .1304 |
| Living with Family | -1.1539 (-3.2620) | -.8819 |
| Race | .0716 (.0895) | .0519 |
| Treatment Group | .1144 (.3713) | .0546 |
| Previous Months Arrested | .2869 (.9355) | .1319 |
| \hat{P} (predicted probability of unemployment) | .0457 | |

In table 2.3, results of estimating a logistic model of the probability of being unemployed the entire month for at least one month after release are displayed. Older, more experienced workers are less likely to be unemployed after prison, while blacks, those with more postprison arrests, and those receiving financial assistance are more likely to be unemployed. By calculating the derivative of the odds against not being unemployed the entire month, it is found that the odds are that blacks are one and a fifth times more likely to be unemployed, and those in the treatment group receiving financial assistance are almost one half times more likely to be unemployed. (Each additional postprison arrest increases the odds of being unemployed by about seven tenths.) Each additional month of pre-prison discounted work experience subtracts 3/100 of a point from the unemployment odds while each year of older age at the time of release from prison subtracts 6/100 of a point.

It is seen in table 2.3 that while there is a significant effect of time-invariant variables upon unemployment outcomes, preprison employment has a relatively inelastic effect. Moreover, postprison criminality exhibits a strong influence on the unemployment odds ratio. Thus we cannot rule out the possibility of state dependence (a hypothesis we would reject if exoffender, postprison unemployment were caused by general disadvantage), nor can we rule out the possibility that there is an independent influence of criminal or

TABLE 2.3

Nonlinear Least Squares Estimates of Coefficients in Logistic Model of Unemployment (t-statistics in parentheses)

| Independent Variable | Coefficient | Elasticity | Odds Change ^a |
|------------------------|---------------------|------------|--------------------------|
| Constant | 1.1709 (1.5444) | .4443 | 1.9195 |
| Months Fought | .0489 (.8149) | .0305 | .0802 |
| Age | -.0373 (2.8514) | -.3497 | -.0611 |
| Experience | -.0209 (-2.8514) | -.1399 | -.0343 |
| Race | .7256 (2.3799) | .2415 | 1.1895 |
| Treatment Group | .2930 (1.3826) | .0556 | .4803 |
| Months Arrested | .4265 (2.0659) | .0858 | .6992 |
| Education | -.0506 (.9762) | -.1739 | -.0830 |
| Secondary Labor Market | -.0372 (.1681) | -.0074 | -.0610 |

^aThe derivative of the unemployment odds ratio with respect to each independent variable.

prison experiences on postprison performance (a hypothesis we would accept if there were specific disadvantage). In other words, general disadvantage may be less an impediment to employment of exoffenders than specific disadvantage.

Another perspective on postprison employment experience is gained by examining the probability of full-time work at least one full month in the twelve months following release. In table 2.4, it is found that younger, more experienced workers are more likely to be working full-time as are those who received financial aid. Blacks, in addition, have lower probabilities of full-time employment. Postprison arrest history, however, is insignificantly (although negatively) related to full-time employment. While the results of table 2.4 do not provide strong evidence for the existence of specific disadvantage, the case for general disadvantage remains unclear when performance is measured by full-time employment.

For each month a logistic equation was estimated for the probability of being unemployed the entire month. In tables 2.5 and 2.6, the following results are displayed:

- (1) Except in the first month, the effect of previous month's unemployment is strongly positive on current unemployment.
- (2) Previous work experience has a negative and significant effect in only five months.
- (3) The effect of criminal history is positive and significant at the 5 percent level in only three months.

TABLE 2.4

Nonlinear Least Squares Estimates of Logistic
Model of Full-Time Work During Year Following
Release from Prison^a (t-statistics in parentheses)

| Independent Variable | Mean | Coefficient | Elasticity |
|------------------------|-----------|--------------------------------------|------------|
| Constant | -- | -.827755 (-.919539) | -- |
| Months Arrested | .530093 | -.028187 (-.124207) | -.012339 |
| Age | 24.708333 | -.034157 ^b (-1.390684) | -.696951 |
| Experience | 17.581019 | .011737 ^b (1.387987) | .170404 |
| Race | .877315 | -.428449 ^b (-1.366127) | -.310409 |
| Education | 9.041667 | .034845 (.578090) | .026018 |
| Treatment Group | .5000 | .332294 ^b (1.398226) | .137208 |
| Months Fought | 1.643519 | .089345 ^b (1.409740) | .121259 |
| Secondary Labor Market | .525463 | -.084682 (-.341684) | -.036746 |
| \bar{P}^c | .224537 | | |
| \hat{P}^d | .174192 | | |
| RMS ^e | .174 | | |

^aThe dependent variable is defined as follows: P = 1 if subject worked 35 hours per week at least one month in the year. P = 0 otherwise.

^bSignificant at 10% level.

^cActual mean unemployment probability.

^dPredicted mean unemployment probability.

^eRoot mean square.

- (4) Being in the secondary labor market has an insignificantly positive effect for the first three months; positive and significant in the fourth, sixth, and eighth months; negative the intervening months; and ultimately negative and significant for the last four months.

With the exception of wrong signs for secondary labor market, these results can be regarded as weak evidence in favor of the general-disadvantage view, especially if preprison work experience determines where one ends up in the first month's labor pool. For example, because of state dependence, after the first month subsequent unemployment may be determined by previous unemployment. This would imply that the covariance of preprison work experience and monthly unemployment is nonzero. Analysis of the monthly variance-covariance matrices suggests this is indeed the case.

However, the same results could suggest a heterogeneity argument. Exoffenders may be partitioned within the labor market on the basis of some unmeasured set of characteristics. These characteristics are correlated with the propensity to remain unemployed for the entire month. Since these characteristics do not change from month to month, then current unemployment appears to be the cause of subsequent unemployment. In either case, the general-disadvantage view is supported.

The wrong signs for secondary labor market deserve special comment. It is reasonable to assume that the insignificance of having been relegated to the secondary labor market before prison

could be due to the experiment itself. Because of the provision of job assistance and financial aid, the differences between secondary and primary labor market workers may temporarily have been obscured. Indeed, at the fourth month, when most individuals had received their entire stipend, the coefficient on secondary labor market jumps to a large positive value. There is, then, some instability of the signs, whereupon the strong negative effects are noticed in the last four months. It is unlikely that these negative effects are due to the experiment. They are more likely due to the high turnover nature of jobs in the secondary labor market. Such jobs as cook, sanitation worker, or parking-lot attendant are not necessarily those for which unemployment during the entire month is to be expected. Instead, we would expect to find casual employment in these low-paying occupations. By using as the dependent variable unemployment the entire month, this aspect of labor market structure is not captured. Nevertheless, to the extent that being confined to the secondary labor market is a measure of disadvantage, one firmly committed to the general-disadvantage view should expect to observe a consistent negative effect of previous dismal employment on postprison employment.

When monthly full-time employment is tallied, as in table 2.7, similar results emerge. Preprison employment experience is weakly related to the probability of working full-time during the month: In only one month out of twelve is the expected positive effect observed. The effect of the secondary labor market is generally

TABLE 2:5

Nonlinear Least Squares Estimates of Coefficients in Logistic Model of Monthly Unemployment
(asymptotic standard errors in parentheses)

| | U_1 | U_2 | U_3 | U_4 | U_5 | U_6 | U_7 | U_8 | U_9 | U_{10} | U_{11} | U_{12} |
|------------------------|------------------------|-------------------|-------------------|-------------------|--------------------|-------------------|--------------------|---------------------|--------------------|--------------------|--------------------|--------------------|
| Constant | .0456 (.6609) | -.1544 (.7105) | -1.813 (.7770) | -5.108 (.9580) | 1.3460 (9.572) | -5.567 (1.005) | -6.3900 (.7366) | -.7782 (2.1060) | -4.4412 (.9850) | -2.157 (.8542) | -2.954 (.8648) | -18.883 (1.464) |
| Experience | -.0153 (.0084) | .0101 (.0090) | .0010 (.0098) | -.0264 (.0107) | -.0367 (.0133) | -.0030 (.0110) | -.0244 (.0125) | .1113 (.0298) | -.0189 (.0110) | -.01118 (.0121) | -.0024 (.023) | .0134 (.0170) |
| Race | .8911 (.4326) | -.0206 (.4137) | -.0001 (.3947) | 1.1110 (.5405) | .7230 (.6455) | 1.1760 (.6752) | 4.5780 (NC) | -4.0100 (4.6790) | .3688 (.5313) | -.2135 (.5213) | 1.2540 (.4999) | -.3852 (.8453) |
| Treatment Group | .1660 (.2134) | .4786 (.2348) | .2467 (.3528) | -.3104 (.2661) | -.3479 (.3025) | -.1065 (.2655) | -.0825 (.2741) | .2614 (.3747) | .6436 (.2988) | .6145 (.3009) | .6507 (.3131) | -.8558 (.4865) |
| Paroled | -.5968 (.2443) | -.2188 (.2614) | .0391 (.2832) | .7288 (.3258) | -.0649 (.3361) | .19180 (.3134) | -.0808 (.2961) | -.2074 (.3920) | .2410 (.3673) | -.2749 (.3227) | .4960 (.3281) | .7741 (.5471) |
| Secondary Labor Market | .0248 (.2194) | .0597 (.2420) | .1602 (.2529) | .6253 (.2806) | -1.0190 (.3453) | .9683 (.3015) | -.2238 (.3048) | 1.4977 (.4405) | -.5236 (.3120) | -.4001 (.2909) | -.9841 (.3299) | -.7857 (.4464) |
| Job Arranged | -.5648 (.2217) | -.0959 (.2426) | -.7549 (.2658) | -.2861 (.2751) | -.2347 (.3043) | -.4366 (.2857) | -.9914 (.2994) | -1.3614 (.4390) | .9352 (.3824) | -.5454 (.2961) | -1.4980 (.3562) | -1.015 (.4408) |
| Age | -.0151 (.0207) | -.0373 (.0226) | -.0149 (.0267) | .0519 (.0246) | -.0480 (.0289) | .0527 (.0251) | -1.0130 (.0290) | -.3058 (.0967) | .0392 (.0267) | .0186 (.0293) | -.0540 (.0325) | -.0992 (.0633) |
| Previous Arrests | -.0163 (.0177) | -.0028 (.0185) | -.0077 (.0184) | .0228 (.0213) | .0396 (.0213) | -.0275 (.0226) | .0124 (.0290) | .0930 (.0365) | -.0189 (.0232) | -.0385 (.0274) | .0090 (.0261) | .2097 (.0398) |
| Income _t | -.0007 (.0006) | .0003 (.0010) | .0004 (.0005) | -.0001 (.0006) | -.0007 (.0010) | -.0001 (.0005) | -.0011 (.0007) | -.0036 (.0016) | .0003 (.0006) | -.0003 (.0007) | .0011 (.0006) | .0135 (.0040) |
| Unempl _{t-1} | 10.6534 (7102.2000) | 2.3406 (.3023) | 2.290 (.2813) | 2.645 (.3094) | 3.5920 (.4024) | 3.3000 (.3762) | 2.927 (.3023) | 9.7115 (4.579) | 3.710 (.4281) | 3.011 (.3305) | 4.3401 (.4483) | 19.916 (.00001) |
| Root Mean Squared | .213 | .146 | .123 | .111 | .090 | .084 | .090 | .071 | .086 | .090 | .056 | .062 |

TABLE 2.7

Work History and Full-Time Employment

| | Pre-Prison Experience | Secondary L.M. | Previous Months' Experience |
|----------|--------------------------|--------------------|--------------------------------|
| Release | .0006 | -.0155 | -- |
| Month 1 | -.0006 | -.0125 | -- |
| Month 2 | -.0007 | -.0119 | .5546 ^a |
| Month 3 | .0007 | .0024 | .4977 ^a |
| Month 4 | .0004 | -.0325 | .5623 ^a |
| Month 5 | .0005 | .0113 | .5766 ^a |
| Month 6 | .0000 | -.0338 | .5443 ^a |
| Month 7 | .0003 | -.0172 | .5479 ^a |
| Month 8 | .0000 | -.0172 | .4749 ^a |
| Month 9 | .0006 | .0294 ^a | .5554 ^a |
| Month 10 | .0004 | .0069 | .8393 ^a |
| Month 11 | .0011 ^a | .0288 ^a | .6921 ^a |
| Month 12 | -.0002 | -.0105 | .6591 ^a |

^aSignificant at 10% level.

Coefficients denote the derivatives of the probability of full-time employment in month t with respect to months pre-prison work experience, secondary labor market job status, and status of full-time employment the previous month. Estimates were obtained from a linear regression model wherein other independent variables were: job arranged, race, other income, experimental group membership, living with family, age, and parole status.

insignificant with unstable signs, although in the fourth month (when the financial assistance was exhausted) the effect is negative and significant. Only previous month's experience (being employed full-time the month before) had consistent and significant effects. The probability of being employed full-time in month t is positively and significantly affected by the probability of having been employed full-time in month $t-1$. Although these results should be qualified in light of the linear regression estimation techniques employed, even the most cautious conclusion would appear to be that preprison effects are less significant than postprison effects.

An important observation should be made about arrest history. Although this has been found to be highly important in determining annual unemployment, criminal arrest has only a minor impact on monthly unemployment. The measure of arrest history in the annual case, however, is the frequency of arrests subsequent to release from prison, while in the monthly case, it is the frequency of arrests prior to prison release. We do not report results of monthly unemployment using frequency of arrests subsequent to release from prison as a separate independent variable. Instead, in Table 2.8, estimates are provided of the effect of having been arrested in the previous month on the current month's unemployment. Similar, inconsistent, and insignificant effects are found. It may well be that different lag structures or nonlinear estimation would alter those conclusions in support of the general disadvantage view. But the monthly unemployment results, at odds with the annual

TABLE 2.8

Ordinary Least Squares Estimates of Coefficients in Linear Model of Monthly Unemployment

(F-statistics in parentheses)

| Independent Variable | U ₀ | U ₁ | U ₂ | U ₃ | U ₄ | U ₅ | U ₆ | U ₇ | U ₈ | U ₉ | U ₁₀ | U ₁₁ | U ₁₂ |
|------------------------|--------------------|--------------------|---------------------|--------------------|--------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|---------------------|--------------------|
| Experience | .0004 (2.1979) | -.0030 (3.2543) | .0008 (.3408) | -.0005 (.1470) | -.0025 (4.5285) | -.0015 (1.7567) | .0005 (.2461) | -.0009 (.7035) | .0012 (1.4918) | -.0025 (5.7407) | -.0003 (.0563) | -.0011 (1.4833) | -.0002 (.0339) |
| Race | -.0354 (8.6300) | .1616 (5.5123) | -.0192 (.1145) | .0004 (.0000) | .0950 (3.6585) | .0307 (.4417) | .0699 (2.5945) | .0955 (4.4185) | -.0232 (.3102) | .0724 (2.6193) | -.0214 (.2274) | .0842 (5.5165) | .0053 (.0196) |
| Fight _t | .0009 (.0114) | .0269 (.2377) | -.0189 (.1664) | -.0175 (.1314) | .0961 (3.9258) | .0039 (.0070) | .0058 (.0152) | .0244 (.2268) | .0082 (.0344) | .0647 (1.4270) | .0639 (1.1466) | -.0614 (1.4976) | .1904 (12.9140) |
| Treatment Group | .0083 (1.0805) | .0440 (.9601) | .0691 (3.4187) | .0376 (1.2009) | -.0599 (3.3396) | .0138 (.2087) | .0105 (.1340) | .0186 (.1319) | -.0067 (.0620) | .0333 (1.3635) | .0349 (1.3913) | -.0312 (1.7304) | -.0145 (.3338) |
| Released on Parole | -.0268 (7.1622) | -.1268 (4.9658) | -.0288 (.3730) | -.0000 (.0000) | .0515 (1.5517) | .0071 (.0340) | .0158 (.1938) | .0138 (.1368) | .0256 (.5599) | -.0277 (.5932) | -.0190 (.2614) | .0493 (2.7603) | -.0151 (.2293) |
| Secondary Labor Market | -.0007 (.0077) | .0081 (.0296) | .0057 (.0210) | .0615 (.2110) | .4166 (1.4784) | -.0397 (1.5600) | .0488 (2.6671) | -.0027 (.0072) | .0463 (2.6539) | -.0747 (6.1783) | -.0313 (1.0239) | -.0627 (6.2983) | -.0138 (.2689) |
| Arrest _{t-1} | -- | -- | .0659 (.2583) | -.1330 (3.0488) | .0360 (.2245) | .0327 (.1935) | -.0675 (.9042) | -.0835 (1.9735) | .0569 (.5982) | .0979 (1.4753) | -.0953 (2.0681) | .0541 (.8251) | .1027 (2.3779) |
| Unempl _{t-1} | -- | -- | .4183 (108.5522) | .3812 (89.8456) | .4187 (98.5603) | .5333 (174.4623) | .4996 (167.2716) | .4965 (133.1059) | .4846 (157.0995) | .5656 (166.1925) | .4828 (129.3634) | .5986 (309.7886) | .3882 (97.1432) |
| Job Arranged | -.0114 (1.6728) | -.1195 (5.7785) | -.0159 (.1479) | -.0800 (4.4607) | -.0275 (.5857) | .0728 (4.7333) | -.0559 (3.1209) | -.1059 (10.0776) | -.0489 (2.5589) | .0843 (6.2879) | -.0378 (1.3294) | -.0826 (10.1266) | -.0379 (1.8471) |
| Age | -.0003 (.1790) | -.0034 (.6937) | -.0052 (2.3128) | -.0026 (.6956) | .0051 (2.8888) | -.0053 (3.7414) | .0012 (.2209) | -.0013 (.2422) | -.0054 (4.7876) | .0019 (.5388) | -.0021 (.6175) | -.0011 (.2540) | -.0000 |
| Constant | .0612 (7.7880) | .4794 (14.6638) | .2210 (4.4214) | .1977 (4.3052) | -.1005 (1.2357) | .2582 (9.7599) | -.05128 (.4188) | .0989 (1.4537) | .1695 (5.1156) | -.0065 (.0068) | .1825 (5.0099) | .0373 (.3282) | .0798 (1.3614) |
| R ² | .0522 | .0903 | .2403 | .2247 | .2346 | .3358 | .3188 | .3046 | .3189 | .3078 | .2544 | .4603 | .2529 |
| F(8/423) | 2.9121 | 4.6546 | 13.3176 | 12.2031 | 12.9067 | 21.2835 | 19.7040 | 18.4437 | 19.7173 | 18.7238 | 35.9059 | 14.2507 | 14.2507 |
| U _t | .0069 | .3543 | .2500 | .1921 | .17361 | .1713 | .1435 | .1551 | .1273 | .1412 | .1389 | .1204 | .0949 |

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results, do not suggest that frequency of arrests, either before prison or after prison, significantly affect post-prison performance. At the same time, a finding of no effect of arrest record on unemployment among exoffenders is not a finding of an absence of discrimination against exoffenders as exoffenders.

To summarize, previous employment experience does affect postprison employment but pre-prison experience exhibits a weaker effect than the experiences had after prison. Similarly, criminal history has a weak effect on postprison performance, particularly if one concentrates on the criminal history prior to release for the current offense. In table 2.9, the relative magnitudes of the partial changes in the monthly unemployment odds ratios due to previous month's unemployment, preprison employment and preprison arrest history are displayed. And it is clear that more recent unemployment experience consistently leads to higher joblessness after prison.

D. POSTPRISON HOURS WORKED

A more straightforward manner of investigating the effects of employment experience and criminal history on postprison performance is to examine closely the determinants of postprison hours worked. We can test directly the explanatory power of each of the hypothesized determinants by constructing an F-statistic from pairs of R-squares obtained from regressions based on the following models.

TABLE 2.9

Work Experience, Criminal History, and Post-Prison Unemployment^a

| Dependent Variable | Partial Change in Odds due to Previous Months' Unemployment | Partial Change in Odds due to Each Additional Month's Pre-Prison Experience | Partial Change in Odds due to Each Additional Previous Arrest |
|------------------------|---|---|---|
| Unemployed in Month 1 | 0 | -.012 | 0 |
| Unemployed in Month 2 | 3.86 | 0 | 0 |
| Unemployed in Month 3 | 3.21 | 0 | 0 |
| Unemployed in Month 4 | 3.65 | -.017 | 0 |
| Unemployed in Month 5 | 5.35 | -.019 | .054 |
| Unemployed in Month 6 | 4.46 | 0 | 0 |
| Unemployed in Month 7 | 3.75 | -.016 | 0 |
| Unemployed in Month 8 | 23.31 | .787 | .196 |
| Unemployed in Month 9 | 4.82 | -.014 | 0 |
| Unemployed in Month 10 | 3.91 | 0 | -.028 |
| Unemployed in Month 11 | 6.43 | 0 | 0 |
| Unemployed in Month 12 | 60.74 | 0 | 1.120 |

The partial change in the odds ratio is found by^b

$$\frac{\partial \left(\frac{P}{1-P} \right)}{\partial x_i} \Bigg|_{\beta_j = 0} = \frac{\beta_i}{e} \beta_i x_i$$

^aInsignificant coefficients set equal to zero (10% level)

^bComputations based on results of table 2.5

$$(1) h_t = f(\tilde{X})$$

$$(2) h_t = f(\tilde{X}, \tilde{Y})$$

$$(3) h_t = f(\tilde{X}, \tilde{Z}_1, \tilde{Z}_2)$$

$$(4) h_t = f(\tilde{X}, \tilde{Z}_2)$$

$$(5) h_t = f(\tilde{X}, \tilde{Y}, \tilde{Z}_1)$$

$$(6) h_t = f(\tilde{X}, \tilde{Y}, \tilde{Z}_1, \tilde{Z}_2)$$

We denote hours worked--specifically average weekly hours worked for the twelve months after release from prison--by h_t . General background characteristics can be described by the vector \tilde{X} . It includes age, race, whether a family member ever was in prison, and whether the subject received unemployment insurance (treatment). The preprison experience vector is denoted by \tilde{Y} . It includes preprison employment experience (longest job held, discounted for time since that job), whether the last job held was a white-collar job, and the highest school grade completed. There are two crime vectors. Criminal history--which includes total times arrested, age at first arrest, and type of offense--is denoted by \tilde{Z}_1 . Prison outcome denoted by \tilde{Z}_2 , includes whether a job was arranged while imprisoned, whether release was by parole, and time actually served upon release.

Ordinary least-squares estimates for both the log of hours worked and hours worked specifications of equations (1)-(6) are displayed in tables 2.11-2.16. In addition to presenting the results obtained using the entire sample, we include estimates for blacks and whites.

TABLE 2.11
 ORDINARY LEAST SQUARES ESTIMATES OF POST-PRISON EMPLOYMENT; MODEL 1
 (t-statistics in parentheses)

| Independent Variables | HOURS WORKED | | | LN(HOURS WORKED) | | |
|------------------------------|--------------------|--------------------|--------------------|-------------------|-------------------|-------------------|
| | Total | Blacks | Whites | Total | Blacks | Whites |
| General background | | | | | | |
| Treatment | -1.597 (-1.846) | -1.019 (-1.084) | -6.390 (-3.286) | -.081 (-1.379) | -.053 (-.815) | -.319 (-3.359) |
| Race | -1.727 (-1.379) | -- | -- | -.139 (-.55) | -- | -- |
| Age | .243 (3.423) | .260 (3.337) | .066 (.432) | .009 (1.961) | .010 (1.904) | .000 (.046) |
| Family member ever in prison | -.763 (-.862) | -.415 (-.431) | -4.665 (-2.243) | -.032 (-.542) | -.012 (-.183) | -.252 (-2.480) |
| Constant | 21.777 (9.740) | 19.189 (9.135) | 29.710 (6.796) | 3.045 (19.865) | 2.862 (19.468) | 3.459 (19.167) |
| F-statistic | 4.565 | 4.189 | 4.841 | 2.204 | 1.427 | 5.072 |
| Significance level | (.001) | (.006) | (.005) | (.068) | (.234) | (.004) |
| Multiple R | .202 | .180 | .478 | .142 | .106 | .436 |
| R ² | .041 | .032 | .228 | .020 | .011 | .236 |
| Adjusted R ² | .032 | .024 | .181 | .021 | .003 | .190 |

SOURCE: Baltimore Life Insurance for the Ex-Prisoner Experiment

TABLE 2.12
ORDINARY LEAST SQUARES ESTIMATES OF POST-PRISON EMPLOYMENT: MODEL 2
(t-statistics in parentheses)

| Independent Variable | HOURS WORKED | | | LN (HOURS WORKED) | | |
|------------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
| | Total Sample | Blacks | Whites | Total | Blacks | Whites |
| General Background | | | | | | |
| Age at arrest | -1.598 (-1.883) | -1.016 (-1.098) | -5.875 (3.0041) | -.082 (-1.399) | -.053 (-.813) | -.297 (-3.066) |
| Sex | -1.672 (-1.597) | -- | -- | -.136 (-1.532) | -- | -- |
| Marital status | .120 (1.539) | .130 (1.522) | -.116 (-.656) | .002 (.474) | .002 (.482) | -.007 (-.859) |
| Family member ever in prison | -.873 (-.998) | -.638 (-.670) | -4.043 (-1.950) | -.038 (-.637) | -.023 (-.354) | -.225 (-2.192) |
| Prison Experience | | | | | | |
| Prison experience | .097 (3.207) | .100 (3.037) | .140 (1.818) | .005 (2.705) | .006 (2.572) | .005 (1.561) |
| White collar | -2.208 (-1.762) | -2.696 (2.006) | 3.699 (1.044) | -.102 (-1.185) | -.125 (-1.319) | .150 (.857) |
| Education | .398 (1.765) | .318 (1.289) | .417 (.779) | .015 (.993) | .010 (.625) | .019 (.716) |
| Constant | 19.845 (6.656) | 18.258 (5.989) | 27.300 (4.098) | 2.993 (14.525) | 2.866 (13.322) | 3.345 (10.125) |
| F statistic | 5.153 (.000) | 4.690 (.000) | 3.368 (.008) | 2.726 (.009) | 2.201 (.042) | 3.201 (.010) |
| Adjusted R ² | .280 .078 .063 | .265 .070 .055 | .552 .305 .214 | .207 .043 .027 | .185 .034 .018 | .542 .294 .202 |

SOURCE: Baltimore Life Insurance for the Ex-Prisoner Experiment

TABLE 2.13
ORDINARY LEAST SQUARES ESTIMATES OF POST-PRISON EMPLOYMENT: MODEL 3
(t-statistic in parentheses)

| Independent Variable | HOURS WORKED | | | LN (HOURS WORKED) | | |
|--|--------------------|-------------------|--------------------|-------------------|-------------------|-------------------|
| | Total | Blacks | Whites | Total | Blacks | Whites |
| General background | | | | | | |
| Treatment | -1.558 (-1.797) | -.963 (-1.01) | -6.615 (-3.294) | -.079 (-1.343) | -.050 (-.765) | -.333 (-3.400) |
| Race | -2.169 (-1.612) | -- | -- | -.167 (-1.811) | -- | -- |
| Age | -.211 (.076) | .218 (2.571) | .051 (.326) | .008 (1.612) | .008 (1.494) | -.000 (-.052) |
| Family member ever in prison | -.749 (-.844) | -.349 (-.362) | -5.395 (-2.393) | -.052 (-.541) | -.008 (-.125) | -.294 (-2.676) |
| Criminal history | | | | | | |
| Total times arrested | -.028 (.071) | -.028 (-.355) | -.127 (-.921) | -.003 (-.780) | -.004 (-.762) | -.067 (-1.061) |
| Age at first arrest | .159 (1.307) | .179 (1.355) | -.026 (-.072) | .006 (.820) | -.007 (-.881) | .000 (.015) |
| Robbery, burglary, larceny, auto theft | -.000 (.000) | .009 (.009) | -1.224 (-.562) | .010 (.165) | .011 (.164) | .064 (.602) |
| Constant | 20.625 (7.093) | 17.497 (6.059) | 33.112 (4.574) | 3.011 (15.092) | 2.800 (13.844) | 3.622 (10.257) |
| F statistic | 2.969 (.005) | 2.542 (.020) | 2.506 (.035) | 1.540 (.152) | 1.044 (.396) | 2.685 (.026) |
| Multiple R | .216 | .198 | .496 | .157 | .128 | .509 |
| Adjusted R ² | .046 .030 | .039 .023 | .246 .148 | .024 .008 | .016 .0007 | .259 .162 |

SOURCE: Baltimore Life Insurance for the Ex-Prisoner Experiment

TABLE 2.14
ORDINARY LEAST SQUARES ESTIMATES OF POST-PRISON EMPLOYMENT: MODEL 4
(t-statistic in parentheses)

| Independent Variable | HOURS WORKED | | | LN(HOURS WORKED) | | |
|--------------------------------|--------------------|--------------------|--------------------|-------------------|-------------------|-------------------|
| | Total | Blacks | Whites | Total | Blacks | Whites |
| General Background | | | | | | |
| Treatment | -1.878 (-2.22) | -1.307 (-1.433) | -6.354 (-3.045) | -.098 (-1.703) | -.070 (-1.099) | -.319 (-3.131) |
| Race | -1.635 (-1.279) | -- | -- | -.132 (-1.506) | -- | -- |
| Age | .203 (2.737) | .229 (2.846) | .097 (.569) | .007 (1.453) | .008 (1.539) | .002 (.348) |
| Family member ever in prison | -.375 (-.431) | -.131 (-.140) | -4.957 (-2.208) | -.007 (-.123) | .006 (.0984) | -.275 (-2.514) |
| Prison Outcome | | | | | | |
| Unarranged | 4.273 (4.618) | 4.788 (4.873) | -.445 (-.160) | .269 (4.229) | .383 (4.378) | -.063 (-.466) |
| Arrested | .231 (.215) | .712 (.617) | -1.762 (-.625) | .008 (.118) | .031 (.390) | -.049 (-.360) |
| Time served | -.149 (-.755) | -.191 (-.920) | -.000 (.000) | -.012 (-.913) | -.014 (-.980) | -.005 (-.171) |
| Constant | | 17.510 (8.042) | 30.623 (6.413) | 2.981 (18.966) | 2.767 (18.023) | 5.502 (15.087) |
| Statistical significance level | 6.310 (.000) | 7.061 (.000) | 2.445 (.039) | 4.286 (.000) | 4.557 (.000) | 2.590 (.030) |
| Multiple R | .307 | .319 | .491 | .257 | .261 | .502 |
| F | .094 | .102 | .241 | .066 | .068 | .252 |
| Adjusted R ² | .079 | .087 | .142 | .0506 | .053 | .155 |

SOURCE: Baltimore Life Insurance for the Ex-Prisoner Experiment

TABLE 2.16
ORDINARY LEAST SQUARES ESTIMATES OF POST-PRISON EMPLOYMENT: MODEL 6
(t-statistics in parentheses)

| Independent Variables | HOURS WORKED | | | LN(HOURS WORKED) | | |
|--|--------------------|--------------------|--------------------|-------------------|-------------------|-------------------|
| | Total | Blacks | Whites | Total | Blacks | Whites |
| General Background | | | | | | |
| Treatment | -1.880 (-2.238) | -1.344 (-1.474) | -4.715 (-2.180) | -.099 (-1.709) | -.727 (-1.122) | -.259 (-2.327) |
| Race | -1.813 (-1.389) | -- | -- | .144 (-1.596) | -- | -- |
| Age | 0.657 (.738) | .084 (.861) | -.247 (-1.169) | .001 (.175) | .002 (.300) | -.012 (-1.139) |
| Family member ever in prison | -.586 (-.675) | -.407 (-.434) | -5.524 (-2.373) | -.018 (-.512) | -.007 (.106) | -.308 (-2.655) |
| Pre-Prison Experience | | | | | | |
| Experience | .076 (2.309) | .074 (2.087) | .249 (2.439) | .004 (1.807) | .004 (1.688) | .010 (2.186) |
| White collar | -1.909 (-1.543) | -2.248 (-1.702) | 5.164 (1.331) | -.084 (-.986) | -.098 (-1.044) | .209 (1.081) |
| Education | .450 (1.985) | .393 (1.583) | .475 (.802) | .017 (1.108) | .014 (.797) | .021 (.717) |
| Criminal History | | | | | | |
| Total times arrested | .003 (.498) | .016 (-.212) | -.091 (-.715) | -.002 (-.438) | -.001 (-.327) | -.005 (-.834) |
| Age at first arrest | .039 (-1.861) | .058 (.425) | -.063 (-.180) | .000 (.0468) | .000 (.0790) | -.001 (-.0733) |
| Incarcerated for: | | | | | | |
| Robbery, burglary, larceny, auto theft | .085 (.092) | -.022 (-.022) | -.579 (-.242) | .017 (.274) | .015 (.187) | -.037 (-.321) |
| Constant | 19.800 (5.726) | 18.051 (5.067) | 30.711 (3.273) | 3.023 (12.655) | 2.892 (11.511) | 3.524 (7.595) |
| F-statistic significance level | 3.617 (.000) | 3.157 (.001) | 2.190 (.042) | 1.973 (.055) | 1.546 (.130) | 2.125 (.048) |
| Multiple R | .281 | .267 | .560 | .211 | .190 | .554 |
| F | .079 | .071 | .314 | .044 | .036 | .307 |
| Adjusted R ² | .057 | .048 | .170 | .022 | .012 | .163 |

SOURCE: Baltimore Life Insurance for the Ex-Prisoner Experiment

The results can be conveniently summarized. Background characteristics tend to be more strongly related to hours worked than preprison-experience, criminal-history, or prison-outcome variables. In the total sample, the unemployment insurance lowers hours worked, being black reduces hours worked, while the effects of age and having family member even in prison are mixed (although older workers generally work more hours). With the exception of employment experience and job arranged while imprisoned, there is no statistically significant pattern seen in the other vectors of variables.

Although there are some differences between the log-linear and linear models, the most striking differences in results arise when the black and white samples are considered. Unemployment insurance works as a systematic work disincentive for whites. Even in the total sample, it is found that being in the treatment group and receiving a cash subsidy upon release from prison tends to reduce the weekly average of hours worked for the year. This is the typical employment disincentive effect discovered in numerous other studies. However, for blacks in the sample the work disincentive appears inoperative. The estimated coefficient for receipt of unemployment insurance (treatment) is significant at the weak 10 percent statistical level in only two of twelve separate equations.

Another revealing difference between the black and white samples is evidenced. Whereas having a family member who was ever in prison

has no appreciable effect on the hours worked by blacks, this general background characteristic consistently lowers hours worked by whites. In addition, having a job arranged increases the hours worked by blacks, but has no effect on white workers. And, to further highlight apparent black-white differences in hours worked, the preprison experience variable, which increases employment for both whites and blacks, generally has a smaller coefficient for blacks than for whites. What this means is that an extra month of preprison employment assures more work for whites than for blacks.

It is clear from inspection that general background characteristics, as we have measured them, are more consistently related to postprison performance than are preprison experiences, criminal history, or prison outcomes. But in light of the significance of the employment experience variable and the job-arranged variable, we might inquire how much do factors other than general background characteristics contribute to explain the variation in hours worked? Collectively, do criminal history variables, or preprison experience variables, or prison outcome variables significantly improve the equations' explanatory power beyond that provided by background characteristics alone?

One technique for addressing these questions has been described by both Goldberger (1964) and Kmenta (1971). Model 1, for example, states that hours worked depends only on general background characteristics. Model 2, on the other hand, asserts that hours

worked depends on both general background characteristics and previous experience. We can rewrite those competing models as

$$(1)' h_t = \beta' X + \epsilon_i$$

$$(2)' h_t = \beta' X + \gamma' Y + \epsilon_i$$

Where β is a $K \times 1$ vector of parameters to be estimated and γ is a $(Q-K) \times 1$ vector and ϵ_i is the error term. A test of model 2-- whether the $Q-K$ additional explanatory variables are significant-- suggests that the null hypothesis,

$$H_0: \gamma = 0$$

be tested against the alternative hypothesis

$$H_1: \gamma \neq 0.$$

The appropriate test statistic is

$$F = \left[\frac{R_Q^2 - R_K^2}{1 - R_Q^2} \right] \left[\frac{n - Q}{Q - K} \right]$$

where the R^2 's are unadjusted and the Q subscript denotes model 2, in which there are Q independent variables, and K corresponds to model 1, in which there are K independent variables. From table 2.11, we see that for background characteristics alone the R -squared value is .041. This value rises to .078 when preprison experience variables are added, as seen in table 2.13. The F -statistic is 5.685, which is significant at the 1 percent level. We reject, therefore, the hypothesis that the coefficients of the additional variables capturing preprison experiences are zero. In other words, model 2 is correct: Hours worked depends not only on general characteristics but also previous experiences.

The identical F -test can be performed for different combinations by models 1 through 6. The F -statistics are computed and displayed in table 2.23. The results can be conveniently summarized. The addition of criminal-history variables adds little to the explanatory power of the hours-worked equation. Even when combined with employment experience the added contribution of information on criminal history is minor. On the other hand, prison outcomes-- capturing information on method of release from prison, time served, and whether a job was arranged--significantly add to the explanatory power of the hours worked equation. This is true whether the equation includes background characteristics alone or background

characteristics combined with preprison experience and criminal history. However, this finding is only true for blacks and the total sample. There is no statistically significant change in the R-squared value for white hours worked regardless of which new explanatory variables are added.

These results suggest further that the dominating factor explaining post-prison hours worked, for blacks at least, is an outcome related more to recent employment--specifically whether a job was arranged. The other elements of the prison-outcome vector--whether paroled and time served--have very low t-statistics associated with their estimated coefficients. Factors related to preprison employment certainly are not as strong explanatory variables as is the job-arranged variable. The previous experience factors are extremely sensitive to choice of the functional form for the hours worked equation. The prison-outcome vector, which includes the job-arranged variable, is robust with respect to functional form in our F-tests.

This conclusion suggests that more recent, perhaps even postprison experiences are more relevant in explaining postprison employment of exoffenders than previous experiences. In tables 2.17-2.21, we explicitly test the hypothesis that postprison experiences explain postprison employment. First, we consider the following model:

$$(7) h_t = f(X_t, \bar{h}_{t-1}).$$

TABLE 2.17
 ORDINARY LEAST SQUARES ESTIMATES OF POSTPRISON EMPLOYMENT: MODEL 7
 (t-STATISTICS IN PARENTHESES)

| Independent Variable | Hours Worked Second Six Months | | | LN(Hours Worked Second Six Months) | | |
|--|--------------------------------|-------------------|--------------------|------------------------------------|-------------------|-------------------|
| | Total | Blacks | Whites | Total | Blacks | Whites |
| General Background Treatment | -.529 (-.587) | -.480 (-.501) | -3.318 (-1.173) | -.005 (-.080) | -.007 (-.108) | -.187 (-.929) |
| Race | -.781 (-.572) | -- | -- | -.056 (-.555) | -- | -- |
| Age | .046 (.625) | .075 (.941) | -.165 (-.801) | .001 (.195) | .002 (.427) | -.010 (-.700) |
| Family Member Ever in Prison | -.404 (-.437) | -.102 (-.104) | -4.620 (-1.643) | .011 (.173) | .035 (.488) | -.303 (-1.518) |
| Postprison Outcome Hours Worked First Six Months Out | .447 (10.489) | .468 (10.451) | .178 (1.211) | .029 (9.326) | .031 (9.403) | .006 (.647) |
| Constant | 16.661 (6.776) | 14.539 (6.435) | 31.514 (4.671) | 2.465 (13.455) | 2.323 (13.719) | 3.521 (7.329) |
| F-statistic Significance Level | 24.184 (.000) | 29.477 (.000) | 1.862 (.132) | 18.502 (.000) | 23.208 (.000) | 1.072 (.380) |
| Multiple R | .470 | .481 | .366 | .422 | .445 | .286 |
| R ² | .221 | .239 | .134 | .178 | .198 | .082 |
| Adjusted R ² | .211 | .231 | .062 | .168 | .190 | .005 |

SOURCE: Baltimore Life Insurance for the Ex-Prisoner Experiment.

TABLE 2.18
 ORDINARY LEAST SQUARES ESTIMATES OF POSTPRISON EMPLOYMENT: MODEL 8
 (t-STATISTICS IN PARENTHESES)

| Independent Variable | Hours Worked Second Six Months | | | LN(Hours Worked Second Six Months) | | |
|--|--------------------------------|-------------------|--------------------|------------------------------------|-------------------|-------------------|
| | Total | Blacks | Whites | Total | Blacks | Whites |
| General Background Treatment | -1.264 (-1.258) | -.830 (-.764) | -4.687 (-1.605) | -.055 (-.757) | -.034 (-.428) | -.249 (-1.335) |
| Race | (-1.762) (-1.154) | -- | -- | -.118 (-1.066) | -- | -- |
| Age | .185 (2.246) | .217 (2.423) | -.121 (-.582) | .010 (1.696) | .012 (1.840) | -.008 (-.547) |
| Family Member Ever in Prison | -.790 (-.766) | -.354 (-.319) | -5.308 (-1.868) | -.013 (-1.173) | .019 (.236) | -.337 (-1.700) |
| Postprison Outcome Arrested First Six Months Out | 1.827 (1.634) | 2.060 (1.722) | .001 (.0465) | .090 (1.107) | .100 (1.140) | .053 (.233) |
| Constant | 24.506 (9.358) | 21.484 (8.762) | 35.664 (5.898) | 2.993 (15.653) | 2.808 (15.562) | 3.663 (8.708) |
| F-statistic Significance Level | 2.276 (.046) | 2.434 (.047) | 1.459 (.229) | 1.165 (.326) | 1.219 (.302) | .974 (.430) |
| Multiple R | .161 | .159 | .329 | .116 | .113 | .274 |
| R ² | .026 | .025 | .103 | .0134 | .012 | .075 |
| Adjusted R ² | .014 | .014 | .034 | .00191 | .002 | -.001 |

SOURCE: Baltimore Life Insurance for the Ex-Prisoner Experiment.

TABLE 2.19
 ORDINARY LEAST SQUARES ESTIMATES OF POSTPRISON EMPLOYMENT: MODEL 9
 (t-STATISTICS IN PARENTHESES)

| Independent Variable | Hours Worked Second Six Months | | | LN(Hours Worked Second Six Months) | | |
|--|--------------------------------|--------------------|--------------------|------------------------------------|-------------------|-------------------|
| | Total | Blacks | Whites | Total | Blacks | Whites |
| General Background Treatment | -1.167 (-1.199) | -1.126 (-1.078) | -4.629 (-1.822) | -.045 (-.660) | -.051 (-.675) | -.229 (-1.117) |
| Race | -1.699 (-1.150) | -- | -- | -.117 (-1.089) | -- | -- |
| Age | .073 (.888) | .094 (1.053) | -.114 (-.549) | .002 (.395) | .003 (.516) | -.008 (-.604) |
| Family Member Ever in Prison | -.671 (-.671) | -.323 (-.302) | -5.396 (-1.917) | -.005 (-.075) | .020 (.266) | -.326 (-1.630) |
| Postprison Outcome Average Salary First Six Months | .058 (5.512) | .064 (5.607) | .556 (.172) | .004 (5.313) | .004 (5.407) | .000 (.128) |
| Constant | 24.149 (9.557) | 21.395 (9.147) | 35.536 (5.950) | 2.958 (16.021) | 2.779 (16.184) | 3.669 (8.613) |
| F-statistic Significance Level | 7.932 (.000) | 9.681 (.000) | 1.451 (.232) | 6.623 (.000) | 8.270 (.000) | .963 (.436) |
| Multiple R | .291 | .306 | .328 | .268 | .285 | .272 |
| R ² | .085 | .093 | .107 | .0721 | .081 | .074 |
| Adjusted R ² | .074 | .084 | .033 | .0612 | .071 | -.002 |

SOURCE: Baltimore Life Insurance for the Ex-Prisoner Experiment.

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TABLE 2.20
ORDINARY LEAST SQUARES ESTIMATES OF POSTPRISON EMPLOYMENT: MODEL 10
(t-STATISTICS IN PARENTHESES)

| Independent Variable | Hours Worked Second Six Months | | | LN(Hours Worked Second Six Months) | | |
|---|--------------------------------|--------------------|--------------------|------------------------------------|-------------------|-------------------|
| | Total | Blacks | Whites | Total | Blacks | Whites |
| General Background Treatment | .607 (-.559) | -.618 (-.641) | -1.191 (-1.408) | -.015 (-.234) | -.019 (-.266) | -.054 (-.254) |
| Race | -.732 (-.521) | -- | -- | -.050 (-.484) | -- | -- |
| Age | .070 (-.731) | -.039 (-.378) | -.525 (-2.204) | -.004 (-.561) | -.001 (-.229) | -.042 (-2.149) |
| Family Member Ever in Prison | -.589 (-.631) | -.364 (-.367) | -5.731 (-2.277) | .007 (.109) | .025 (.354) | -.354 (-1.502) |
| Preprison Experience | .021 (.594) | .009 (.243) | .365 (2.815) | -.000 (-.246) | -.001 (-.657) | .024 (2.554) |
| White Collar | -3.029 (-2.281) | -3.345 (-2.397) | 7.419 (1.516) | -.190 (-1.929) | -.207 (1.655) | .510 (1.421) |
| Education | .227 (.932) | .195 (.744) | .515 (.691) | .004 (.242) | .000 (.005) | .054 (.521) |
| Criminal History | | | | | | |
| Total Time Arrested | .0599 (.500) | .086 (1.034) | -.105 (-.512) | .004 (.797) | .005 (.905) | -.004 (.372) |
| Age at First Arrest | .060 (.603) | .083 (.836) | .204 (.437) | .006 (.549) | .009 (.919) | .010 (.262) |
| Incarcerated for: Robbery, Burglary, Larceny Auto Theft | -.379 (-.387) | -.373 (-.357) | -.505 (-1.170) | .002 (.057) | .014 (.179) | -.058 (-1.270) |
| Prison Outcomes | | | | | | |
| Job Arranged | 1.225 (1.292) | 1.776 (1.651) | -7.099 (-1.815) | .159 (2.067) | .173 (2.154) | -.300 (-1.045) |
| Paroled | -1.890 (-1.631) | -1.417 (-1.154) | -5.295 (-1.465) | -.152 (-1.761) | -.115 (-1.264) | -.425 (-1.552) |
| Time Served | .345 (1.572) | .314 (1.326) | 2.033 (2.021) | .013 (.777) | .009 (.548) | .139 (1.885) |
| Postprison Outcome | | | | | | |
| Hours Worked First Six Months Out | .436 (9.773) | .453 (9.60) | .115 (.615) | .025 (5.557) | .030 (2.655) | .002 (.216) |
| Constant | 15.561 (4.091) | 13.414 (3.476) | 20.301 (2.351) | 2.410 (5.512) | 2.261 (7.552) | 3.513 (3.711) |
| F-statistic | 9.523 (.000) | 10.074 (.000) | 1.505 (.007) | 7.459 (.000) | 5.057 (.000) | 1.250 (.277) |
| Significance Level | | | | | | |
| Multiple R | .492 | .513 | .612 | .447 | .473 | .545 |
| R ² | .242 | .264 | .375 | .200 | .223 | .295 |
| Adjusted R ² | .216 | .237 | .157 | .173 | .194 | .061 |

SOURCE: Baltimore Life Insurance for the Ex-Prisoner Experiment.

TABLE 2.21
ORDINARY LEAST SQUARES ESTIMATES OF POSTPRISON EMPLOYMENT: MODEL 11
(t-STATISTICS IN PARENTHESES)

| Independent Variable | Hours Worked Second Six Months | | | LN(Hours Worked Second Six Months) | | |
|--|--------------------------------|--------------------|--------------------|------------------------------------|-------------------|-------------------|
| | Total | Blacks | Whites | Total | Blacks | Whites |
| General Background Treatment | -.476 (-.528) | -.375 (-.390) | -1.338 (-1.450) | -.003 (-.152) | -.006 (-.057) | -.057 (.310) |
| Race | -.926 (-.663) | -- | -- | -.060 (-.582) | -- | -- |
| Age | -.066 (-.694) | -.039 (-.385) | -.573 (-2.111) | -.003 (-.553) | -.001 (-.232) | -.040 (-2.047) |
| Family Member Ever in Prison | -.436 (-.587) | -.381 (-.389) | -6.928 (3.036) | .005 (.072) | .025 (.343) | -.407 (-1.828) |
| Preprison Experience | .029 (.352) | .021 (.571) | .368 (2.502) | -.000 (-.079) | -.001 (-.425) | .024 (2.553) |
| White Collar | -2.990 (-2.265) | -3.370 (-2.438) | 7.471 (1.509) | -.188 (-1.912) | -.203 (-2.002) | .515 (1.419) |
| Education | .194 (.509) | .124 (.475) | .576 (.746) | .002 (.146) | -.002 (-.155) | .039 (.697) |
| Criminal History | | | | | | |
| Total Time Arrested | .041 (.552) | .056 (.576) | -.103 (-.597) | .003 (.521) | .004 (.645) | -.004 (-.357) |
| Age at First Arrest | .050 (.382) | .047 (.333) | .189 (.597) | .006 (.692) | .007 (.739) | .008 (.247) |
| Robbery, Burglary, Larceny Auto Theft | -.337 (-.346) | -.355 (-.353) | 1.202 (.375) | .004 (.058) | .014 (.184) | -.043 (-1.195) |
| In Prison | | | | | | |
| Job Arranged | 1.292 (1.268) | 1.720 (1.613) | -7.244 (-1.822) | .157 (2.070) | .170 (2.125) | -.313 (-1.075) |
| Paroled | -1.751 (-1.528) | -1.259 (-1.034) | -5.290 (-1.444) | -.145 (-1.655) | -.110 (-1.205) | -.437 (-1.650) |
| Time Served | .317 (1.395) | .271 (1.154) | 2.061 (2.021) | .0117 (.690) | .007 (.422) | .141 (1.895) |
| Postprison Outcomes | | | | | | |
| Hours Worked First Six Months Out | 2.549 (2.528) | 3.024 (2.523) | 1.202 (.575) | .133 (1.774) | .160 (1.957) | .108 (.461) |
| Hours Worked First Six Months Out | .443 (9.976) | .453 (9.374) | .105 (.720) | .029 (5.774) | .031 (3.815) | .001 (.123) |
| Constant | 15.488 (4.098) | 13.537 (3.540) | 29.537 (2.239) | 2.406 (5.511) | 2.267 (7.877) | 3.444 (3.553) |
| F-statistic | 9.430 (.000) | 10.103 (.000) | 1.549 (.110) | 7.207 (.000) | 7.552 (.000) | 1.161 (.342) |
| Significance Level | | | | | | |
| Multiple R | .503 | .529 | .614 | .454 | .481 | .547 |
| R ² | .253 | .279 | .379 | .206 | .232 | .299 |
| Adjusted R ² | .226 | .252 | .148 | .177 | .202 | .041 |

SOURCE: Baltimore Life Insurance for the Ex-Prisoner Experiment.

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Here hours worked in a given period depends both on background characteristics and hours worked in the previous period. Let us relate h_t to the second six months out to prison; the \bar{h}_{t-1} is the hours worked the first six months out of prison. For purposes of discussion, we concentrate on the log-linear form of the black hours worked equation. From estimates of model 1 based on the second six months' hours worked, we obtained an R-squared value of .01. This jumps to .20 in table 2.17 where \bar{h}_{t-1} has been added as an explanatory variable. The F-statistic for this change in R-squares is 101.41 and is significant at the 1 percent level.

There are other postprison outcomes that could potentially affect employment. Although previous criminal record was found not to matter much in determining postprison employment, what about postprison arrest? Denote the event of having been arrested in the first six months out of prison by \bar{A}_{t-1} . Then an alternative to model 1 is

$$(8) h_t = f(X, \bar{A}_{t-1})$$

Recall that for blacks the R-squared value for model 1 (log-linear form) is .01. From table 2.18, we compute an R-squared of .012;

this, it is easily seen, is not a statistically significant change. Another alternative is the following model:

$$(9) h_t = f(X, \bar{W}_{t-1})$$

where \bar{W}_{t-1} is the average weekly salary in the first six months. The R-squared value rises to .08, not quite as much as the rise when including \bar{h}_{t-1} .

Further experimentation yields similar results. The following models capture the added influence of postprison outcomes on hours worked the second six months.

$$(10) h_t = f(X, Y, Z_1, Z_2; \bar{h}_{t-1})$$

$$(11) h_t = f(X, Y, Z_1, Z_2; \bar{A}_{t-1}, \bar{h}_{t-1})$$

$$(12) h_t = f(X, Y, Z_1, Z_2; \bar{A}_{t-1}, \bar{h}_{t-1}, \bar{W}_{t-1})$$

These models are fully loaded in the sense that they include general background, preprison experience, criminal history, and prison-outcome characteristics. The results are displayed in tables 2.20-2.22. To fully appreciate what the highly significant coefficients on postprison outcomes really mean, it is useful to consult table 2.24. Here, the F-test results for the change in R-squares are displayed. In every case in which the postprison outcome variables

TABLE 2.22
ORDINARY LEAST SQUARES ESTIMATES OF POSTPRISON EMPLOYMENT: MODEL 12
(t-STATISTICS IN PARENTHESES)

| Independent Variable | Hours Worked Second Six Months | | | LN(Hours Worked Second Six Months) | | |
|---|--------------------------------|--------------------|--------------------|------------------------------------|-------------------|-------------------|
| | Total | Blacks | Whites | Total | Blacks | Whites |
| General Background Treatment | .477 (-.528) | -.357 (-.372) | -2.147 (-.694) | -.000 (-.132) | -.006 (-.564) | -.121 (-.534) |
| Race | -.891 (-.628) | | | -.061 (-.564) | | |
| Age | -.092 (-.657) | -.034 (-.336) | -.583 (-2.145) | -.003 (-.555) | -.001 (-.226) | -.041 (-2.074) |
| Family Member Ever in Prison | -.627 (-.676) | -.372 (-.379) | -7.123 (-2.338) | .004 (.714) | .025 (.342) | -.420 (-1.876) |
| Preprison Experience | .030 (.864) | .023 (.605) | .332 (2.857) | -.000 (-.034) | -.001 (-.419) | .025 (2.624) |
| White Collar | -3.000 (-2.270) | -3.394 (-2.450) | 8.439 (1.659) | -.188 (-1.909) | -.208 (-2.002) | .550 (1.559) |
| Education | .203 (.831) | .130 (.498) | .683 (.875) | .002 (.141) | -.003 (-.153) | .046 (.812) |
| Criminal History Total Time Arrested | .041 (.551) | .055 (.667) | -.093 (-.539) | .003 (.620) | .004 (.544) | -.003 (-.304) |
| Age at First Arrest | .048 (.364) | .045 (.323) | .054 (.111) | .005 (.693) | -.001 (-.226) | -.000 (-.007) |
| Incarcerated for: Robbery, Burglary, Larceny Auto Theft | -.362 (-.371) | -.385 (-.371) | -.791 (-2.259) | .005 (.071) | .014 (.162) | -.073 (-1.325) |
| In Prison Job Arranged | 1.338 (1.303) | 1.771 (1.648) | -6.961 (-1.745) | .157 (2.049) | .170 (2.113) | -.294 (-1.005) |
| Paroled | -1.731 (-1.498) | -1.218 (-.996) | -5.558 (-1.515) | -.145 (-1.685) | -.110 (-1.195) | -.455 (-1.666) |
| Time Served | .321 (1.409) | .272 (1.155) | 2.238 (2.156) | .011 (.687) | .007 (.421) | .153 (2.014) |
| Postprison Outcome Arrested First Six Months Out | 2.424 (2.295) | 2.868 (2.525) | 1.525 (.472) | .135 (1.709) | .159 (1.850) | .129 (.545) |
| Hours Worked First Six Months Out | .457 (8.150) | .479 (7.692) | .149 (.979) | .029 (6.929) | .031 (5.332) | .004 (.352) |
| Average Salary First Six Months Out | -.005 (-.399) | -.006 (-.415) | -.031 (-.953) | .000 (.025) | .000 (-.038) | -.002 (-.571) |
| Constant | 15.298 (4.012) | 13.359 (3.468) | 31.024 (2.334) | 2.408 (8.441) | 2.255 (7.513) | 3.543 (3.624) |
| F-statistic | 8.832 (.000) | 9.420 (.000) | 1.598 (.122) | 5.741 (.000) | 7.317 (.000) | 1.125 (.357) |
| Multiple R | .502 | .529 | .627 | .454 | .481 | .560 |
| R ² | .254 | .280 | .393 | .205 | .232 | .313 |
| Adjusted R ² | .225 | .250 | .147 | .175 | .200 | .035 |

SOURCE: Baltimore Life Insurance for the Ex-Prisoner Experiment.

TABLE 2.23
F-TESTS OF COMPETING MODELS OF HOURS WORKED

| Added Variable Set | Hours Worked | | | LN(Hours Worked) | | |
|--|--------------------------------|--------------------------------|------------------|--------------------------------|--------------------------------|------------------|
| | Total | Blacks | Whites | Total | Blacks | Whites |
| Pre-Prison Experience | 5.685 ^c F(3,415) | 5.347 ^c F(3,323) | 1.961 F(3,47) | 3.708 ^b F(3,425) | 2.762 ^b F(3,323) | 1.287 F(3,47) |
| Criminal History | .742 F(3,425) | 1.164 F(3,323) | .373 F(3,47) | .508 F(3,425) | .631 F(3,323) | .486 F(3,47) |
| Prison Outcome | 8.287 ^c F(3,425) | 9.968 ^c F(3,323) | .268 F(3,47) | 6.977 ^c F(3,425) | 7.604 ^c F(3,323) | .335 F(3,47) |
| Pre-Prison Experience, Prison Outcome and Criminal History | 4.294 ^c F(9,410) | 4.634 ^c F(9,370) | 1.015 F(9,44) | 3.090 ^c F(9,410) | 3.153 ^c F(9,370) | .933 F(9,44) |
| Pre-Prison Experience and Criminal History | 2.901 ^c F(6,423) | 2.721 ^b F(6,373) | .919 F(6,47) | 1.765 F(6,423) | 1.599 F(6,373) | 1.599 F(6,47) |

SOURCE: Baltimore Life Insurance for the Ex-Prisoner Experiment.

a... significant at 10 percent level
b... significant at 5 percent level
c... significant at 1 percent level.

TABLE 2.24
F-TESTS FOR COMPETING MODELS OF HOURS WORKED: POST-PRISON OUTCOMES

| Added Variables | Hours Worked Second Six Months | | | LN(Hours Worked Second Six Months) | | |
|--|-----------------------------------|---------------------------------|-----------------|---------------------------------------|---------------------------------|------------------|
| | Total | Blacks | Whites | Total | Blacks | Whites |
| Hours Worked First Six Months | 95.952 ^c F(1,417) | 68,076 ^c F(1,365) | .640 F(1,39) | 75.240 ^c F(1,417) | 74,895 ^c F(1,365) | 4.539 F(1,38) |
| Hours Worked First Six Months and Arrested First Six Months | 51.636 ^c F(2,416) | 50,877 ^c F(2,364) | .407 F(2,38) | 39,389 ^c F(2,416) | 39,222 ^c F(2,364) | .111 F(2,38) |
| Hours Worked First Six Months, Arrest First Six Months, Average Salary First Six Months | 35.404 ^c F(3,415) | 34,040 ^c F(3,363) | .584 F(3,37) | 26,826 ^c F(3,415) | 26,542 ^c F(3,363) | .645 F(3,37) |
| Arrested First Six Months | 3.074 ^c F(1,427) | 3,075 ^c F(1,375) | 0 F(1,49) | 1,281 F(1,427) | 1,162 F(1,375) | 0 F(1,49) |
| Average Salary First Six Months | 31,256 ^c F(1,427) | 35,550 ^c F(1,375) | .049 F(1,49) | 28,523 ^c F(1,427) | 29,362 ^c F(1,375) | 0 F(1,49) |

SOURCE: Baltimore Life Insurance for the Ex-Prisoner Experiment.

- a. significant at 10 percent level
- b. significant at 5 percent level
- c. significant at 1 percent level

are added to the fully loaded model, there is a statistically significant change in the R-square value. The implication is straightforward. There is a substantial contribution to the explanation of differing hours worked among exoffenders by their varying experiences immediately following release from prison. This contribution is above and beyond that found by differences in background characteristics, criminal histories, preprison experiences, or recent prison-outcomes. Indeed, for blacks at least, these other factors explain very little of their postprison employment prospects. This is not true for whites, however. A fully loaded model like model 6 does well in explaining differences in white hours worked. It does so well, in fact, that when it is reestimated for the second six months, postprison outcomes, including the hours worked the first six months, contribute virtually nothing to the explanatory power of the model. This suggests that while postrelease intervention strategies can be very powerful in assisting minorities in improving their employment prospects, for whites the die is cast. Their past has cast their employment profiles in a mold that is difficult to alter.

E. SUMMARY

We have investigated the determinants of postprison employment. When looking at both hours worked and unemployment, the following conclusions emerge. Criminal history has a minor and weak effect on postprison employment. Preprison employment experience, while decidedly associated with both hours worked and unemployment,

explains less of the variance in employment performance than more recent outcomes. One variable stands out as a particularly strong predictor of employment performance. This is whether a job was arranged when released from prison.

When looking at the probability of being unemployed, we adopted a state dependence--heterogeneity perspective. Because of the difficulty of distinguishing between heterogeneity and state dependence in the data set, it is difficult to know whether the dismal postprison employment experiences of exoffenders in the Baltimore LIFE experiment are due to being disadvantaged workers generally or exoffenders specifically.

This inconclusive result could have emerged under a variety of scenarios. First, and highly likely, is the possibility that both specific disadvantage and general disadvantage are so intimately intertwined that attempts to isolate one or the other weaken tests of the independent effects of either. To examine whether being in the secondary labor market, having poor skills, having low education, or being black, generally, leads to a career in crime, specifically, requires a data set that includes both offenders and nonoffenders. One could look at the effect of measures of general disadvantage on the rearrest rate of exoffenders, and we do this in the next chapter. However, in all of the results, there is a significant amount of covariance between measures of specific and general disadvantage making the isolation of the separate effects particularly formidable.

Second, and no less likely, is the possibility that there is not a sufficient amount of variation of disadvantage in the sample to adequately distinguish between specific and general disadvantage, let alone to detail how one affects the other. Although some of the exoffenders had extremely long criminal records, while others had only a few previous convictions, there are no first offenders in the group. In addition, there is virtually no representation of highly educated, well-trained, successful criminals.

Third, and questionable, is the possibility that disadvantage, whether specific or general, is not directly a cause of the failure in the labor market by exoffenders at all. The sometimes weak and often inconsistent effects of preprison work experience, secondary labor market status, and criminal history all may come about because none of these factors is really a determinant of postprison labor market performance. The puzzle, then, is why are the effects of previous month's performance so strong, consistent, and robust? Perhaps because the lagged variable is capturing unmeasured aspects of disadvantage or unobserved correlates of the measured disadvantage? This is the central unresolved issue.

More conclusive are the answers to the three subsidiary questions posed. What effect does previous employment experience have on the postprison performance of exoffenders? When performance is measured

by the probability of being unemployed one entire month for at least one month during the year after release from prison, experience is found to be inversely related to postprison employment failure. Yet, this effect is inelastic and results in only a small marginal change in the unemployment-odds ratio. When performance is measured by full-time employment the effect is positive yet again inelastic. When monthly unemployment is chosen as the performance measure, the inverse relationship between preprison employment experience and postprison unemployment is found to be statistically significant in only five months and even then the marginal effects are small.

Does it matter whether "previous" experience is before or after prison? Yes. However performance is measured, when both the effects of measures of preprison employment and postprison employment are viewed together, the relative magnitude of the postprison employment effects on performance is larger.

Are exoffenders with more extensive criminal histories less successful in the labor market? Those with more postprison arrests are more likely to be unemployed at least one month during the year following release and are less likely to be employed full-time (although insignificantly so). This supports the view that it is post-prison experiences that matter. Monthly unemployment performance, in contrast, is only weakly related to criminal history. The number of preprison arrests is positively and significantly related to unemployment in just three months out of

twelve. Thus, while criminal history may matter, the more recent history is probably the more damaging for employability.

To summarize, previous employment experience does affect post-prison employment but preprison experience exhibits a weaker effect than the experiences had after prison. Similarly, criminal history has a weak effect on postprison performance particularly if one concentrates on the criminal history prior to release for the current offense. An important qualification to these findings, however, is that when employment, measured by hours worked, is examined separately for blacks and whites, it is understood that the postprison outcomes have minimal effects on white workers. For blacks, on the other hand, there is substantial explanatory power in the events occurring immediately after release from prison.

CHAPTER III. EMPLOYMENT AND RECIDIVISM

A. INTRODUCTION

Economic models of crime explore the effects of incentives, like improved employment opportunities, and disincentives, like increased threat of punishment, on the propensity to engage in illicit activities. In even the simplest of models, the effects of these incentives and disincentives are often ambiguous.

Following the publication of Gary Becker's (1968) seminal work on crime and punishment, more than a decade was spent by researchers in attempts to verify or refute his--and classical criminologists'--central proposition: Increasing the certainty and severity of punishment will tend to reduce crime. The avalanche of empirical support for this proposition has been carefully scrutinized and the conclusion is that the case in favor of deterrence is quite weak.

Early evidence in support of the hypothesis that the certainty and severity of punishment deter crime was seriously challenged by a recent panel established by the National Academy of Sciences. In their summary of the panel's report, Blumstein and his colleagues (1978, p. 6) reported that although most available evidence reveals a negative association between aggregate crime rates and punishment, "Any conclusion that these negative associations reflect a deterrent effect . . . is limited principally by the inability to eliminate other factors that could account for the observed relationships, even

the absence of a deterrent effect." In a careful evaluation of virtually all of the published econometric tests of the deterrence hypothesis, statisticians Brier and Fienberg (1980, p. 151) concluded that the aggregate crime and imprisonment data used empirically to examine the conventional model of crime are "so untrustworthy as to render any serious analysis meaningless."

This is not surprising because careful extensions of the theoretical model of crime developed by Becker and extended by Isaac Ehrlich (1973) have shown that the effects of punishment are generally ambiguous. Block and Heineke (1975) introduced into a choice-theoretic model of crime (1) arbitrary arrest distributions--Becker and Ehrlich had assumed these distributions to be binomial--and (2) the disutility of work. They found that no straightforward generalizations based solely on attitudes toward risk (i.e., signing of the second derivations) could be made concerning the effects of punishment on crime. In a similar exercise Myers (1976) derived the familiar case of a backward-bending supply curve for illegal activity. Here, it was shown that increasing certainty or severity of punishment can increase participation in crime even for risk-averse individuals.

Until now, the case in favor of alternatives to traditional crime-control methods has been even weaker. For example, in his review of rehabilitation programs designed to reduce criminal recidivism, Robert Martinson (1974) concluded that "nothing works."

Summarizing the results of many years of research on the effectiveness of numerous correctional programs, Martinson could find little support for the belief that training, educational, or vocational programs for inmates would significantly reduce postprison recidivism.

Many of these programs were conceived at least partly within the context of a model where better legitimate income opportunities tend to reduce crime. Historians of thought will debate whether credit for this model should go to Bonger (1969); Cloward and Ohlin (1960); Fleisher (1960); Merton (1967); Phillips, Maxwell, and Votey (1972), or any other of the scores of sociologists and economists who have attributed crime to poverty and low incomes. This model motivated virtually hundreds of tests of hypotheses concerning the effects of (1) unemployment, (2) income, (3) labor-force participation, and (4) income dispersion on crime. Gillipsie (1978) reviewed most of these tests and could not confirm the case for this apparently "alternative" economic model: Unemployment, low income, labor-force participation, and income dispersion do not exhibit consistent and unambiguous effects on crime across the many econometric studies.

Gillipsie and more recently Anne Witte (1979) recognized that failure to confirm the alternative economic model in previous studies could be due to the aggregate nature of the data. Higher incomes in urban areas could mean both better legitimate opportunities for potential criminals and higher illegal gains due to

wealthier targets. Witte, in particular, has called for careful tests of the economic model using disaggregated data. Whether policies to increase wages result in lower crime is an open empirical question. But first, let us explore the theoretical question.

The Becker-Ehrlich model of crime and the alternative economic model of crime are really the same. There is indeed an alternative alternative economic model--it's actually a sociological, institutional model--described by Myers (1978) as the segmented labor market model. And, then, there is the "malevolent interdependence" economic model of crime formulated by Sheldon Danziger and David Wheeler (1975). But most economic writers on crime have in mind extensions and elaborations upon the following simple model.

Suppose that there are exactly two income-earning activities, work and crime. Initially, we might assume that work is a riskless activity and crime is rewarded at a rate G if one is successful and at a rate of $-L$ if not. The probability of success is given by $(1-\alpha)$. Denoting r as the random rate of return to crime, it is easy to see that the expected rate of return to crime is

$$E(r) = (1-\alpha) G - \alpha L.$$

Income Y is given by the sum of illegal and legal earnings. Let t be the fraction of time allocated to crime and $(1-t)$ the fraction of time allocated to work. Then expected income is found to be:

$$E(Y) = t[(1-\alpha) G - \alpha L] + (1-t)w$$

where w is the wage rate. If the rational, self-interested, potential criminal acted as if he maximized his expected income, then the optimal allocation of time to crime, t^* , would satisfy the following rules:

$$\text{if } \frac{E(r)}{w} > 1 \implies t^* = 1.$$

$$\text{if } \frac{E(r)}{w} < 1 \implies t^* = 0.$$

and

$$\text{if } \frac{E(r)}{w} = 1 \implies 0 \leq t^* \leq 1.$$

In other words, all time would be allocated to that activity with the highest rate of return. Increases in the expected return to crime increase the propensity to engage in crime. Increases in the return to work reduce the participation in crime. If the loss, L , depends positively upon the severity of punishment, and given that the expected return to crime falls for more certain punishment (increases in α), it is trivial to show that increases in the certainty and severity of punishment tend to reduce participation in crime.

Let us assume now that work is risky, that is, the rate of return, w , is not a constant but instead is stochastic. Then we might suppose that it takes on the value \bar{w} if one is employed with

probability $(1-u)$, where u is the probability of unemployment, and it is equal to 0 otherwise. In this case, the optimality conditions are essentially the same:

$$\frac{E(r)}{(1-u)\bar{w}} > 1 \implies t^* = 1$$

$$\frac{E(r)}{(1-u)\bar{w}} < 1 \implies t^* = 0$$

and

$$\frac{E(r)}{(1-u)\bar{w}} = 1 \implies 0 \leq t^* \leq 1.$$

This is the simplest economic representation of the often repeated claim "unemployment causes crime." As u rises, the expected return to work (the denominator in the above expressions) falls. So the relative attractiveness of crime to work increases, and the allocation of time to crime will rise if initially one were indifferent toward engaging in crime or work. Of course, it is true that as the wage received if one does work increases, so too does the expected wage, and, therefore, the relative attractiveness of participating in crime diminishes: Better wages reduce crime rates.

These straightforward results can be destroyed in innumerable ways. We could drop the assumption of risk neutrality (i.e., expected income maximization) and assume a more general utility index. Then, the results depend upon the attitudes toward risk. We could assume that there are more than two income activities. Then

the results will depend on the various substitutabilities and complementarities among legal and illegal activities. We could assume that work and crime are irksome. Then the results generally will be ambiguous. But in many extensions or elaborations upon this model the specification for empirical purposes could be given as

supply of crime = f (expected returns to crime,
expected returns to work,
other exogenous variables
reflecting, in part, tastes).

The expected returns to crime should depend upon gains and losses of participation in crime as well as the certainty and severity of punishment. The expected returns to work should depend upon wages and unemployment. Because a general theory leaves the effects of these variables on crime in doubt, it is the task of empirical analysis to establish any relationships that may dominate.

Generally, one does not know the amount of crime in which individuals engage. There are some samples, however, like those of prison releases, where one measure of criminal activity--rearrest--is nationally compiled. From the FBI's computerized criminal history system, researchers for law-enforcement agencies can obtain records of arrest ('rap sheets') for each prison releasee, given the offender's FBI number. Thus it is useful to measure the supply of crime by the rearrest rate when examining ex-prisoner samples. However, in ex-prisoner samples, there is only meager information on employment opportunities. To the extent that pre-prison employment,

training, or education are related to employment opportunities, these measures might capture features of the desired variables.

In the analysis that follows, we study two samples having better than average attributes for examining employment and recidivism. The federal prison data, to be detailed below, is superior in that it refers to a large national sample. The Baltimore LIFE data, previously described, stands out because of the wealth of information on employment outcomes.

E. THE CASE OF FEDERAL RELEASED PRISONERS

A random sample of all persons released from federal prisons by parole, mandatory release, or expiration of sentence during 1972 was drawn. 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. 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 on 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 parole violation. 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 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.

In table 3.1 characteristics of the U.S. prison sample are summarized. The federal exoffenders are somewhat older than many recently released prisoners. Both whites and blacks are about thirty years old. The one quarter representation of blacks in the sample is decidedly lower than the even more disproportionately black prison population in the U.S. Educational attainment at almost ten years is slightly higher than inmates generally, but still lower than the national average. Blacks, though, had a mean school completion rate closer to the average for inmates in state correctional institutions.

Employment characteristics are measured in a number of ways. "Employed greater than four years" is a dummy variable equal to zero if the longest job held was of a duration of less than four years. "Longest job" equals the length, in years, of the longest job held if and only if the longest job lasted less than four years. "Last civilian experience" denotes whether the subject was employed more than 25 percent of the time in the last two years of civilian life.

TABLE 3.1
DESCRIPTION OF THE FEDERAL PRISON SAMPLE

| Variables | All Races ^a | All Races ^b (N=2127) | Blacks ^b (N=546) | Whites (N=1581) |
|--|------------------------|------------------------------------|--------------------------------|--------------------|
| Personal Characteristics (N=2224) | | | | |
| Age in Months | 361,850 | -- | -- | -- |
| Age in Years | -- | 30,541 | 30,915 | 30,412 |
| Black | .254 | -- | -- | -- |
| Female | .049 | .051 | .086 | .039 |
| Grade Claimed | 9,533 | 9,452 | 9,036 | 9,592 |
| Married | .267 | .264 | .214 | .281 |
| Alcoholic | .367 | -- | -- | -- |
| No Drug Use | .000 | -- | -- | -- |
| Mental Hospital | .087 | .091 | .036 | .111 |
| IQ | 103,010 | -- | -- | -- |
| No Drug or Drink | -- | .828 | .855 | .815 |
| Employment (N=1557) | | | | |
| Employed More Than 4 Years | -- | .121 | .104 | .127 |
| Longest Job | 1,316 | -- | -- | -- |
| Last Civilian Experience | .789 | -- | -- | -- |
| On-the-Job Training | .316 | -- | -- | -- |
| Criminal Justice System (N=2495) | | | | |
| New Commitment | .822 | -- | -- | -- |
| Parole Violator | .127 | -- | -- | -- |
| Regular Adult | .524 | -- | -- | -- |
| Immediate Parole | .232 | -- | -- | -- |
| VCA Indeterminates | .161 | -- | -- | -- |
| FDA Minority | .040 | -- | -- | -- |
| Maximum Custody | .001 | -- | -- | -- |
| Close Custody | .105 | -- | -- | -- |
| Medium Custody | .174 | -- | -- | -- |
| Minimum Custody | .323 | -- | -- | -- |
| Work Release | .195 | -- | -- | -- |
| Parole Hearings | 1,733 | 1,762 | 1,529 | 1,84 |
| Release on Parole | -- | .464 | .358 | .50 |
| Criminal History (N=2488) | | | | |
| Free Less Than 6 Months | .352 | -- | -- | -- |
| Free More Than 6 Months, Less Than 36 Months | .355 | -- | -- | -- |
| Prior Commitment | .902 | -- | -- | -- |
| Prior Incarceration | 2,550 | -- | -- | -- |
| Parole Revoked | .407 | -- | -- | -- |
| Incarcerations/Convictions | .368 | -- | -- | -- |
| Age of First Commitment | 22,330 | 22,136 | 21,751 | 22,26 |
| Time Served | -- | 23,992 | 24,696 | 23,74 |
| Convictions | 5,836 | 5,971 | 6,624 | 5,74 |
| Escaped | .200 | -- | -- | -- |
| Prison Punishment | .286 | .297 | .285 | .30 |
| Commitment/Convictions | -- | .130 | .141 | .12 |
| First Offender | -- | .102 | .075 | .11 |
| Offense (N=2497) | | | | |
| Robbery, Theft, Burglary | .503 | .544 | .483 | .51 |
| Sex Offenses | .008 | -- | -- | -- |
| Other Violent | .019 | -- | -- | -- |
| Alcohol and Drug | .217 | -- | -- | -- |
| Less Than \$500 | .237 | -- | -- | -- |
| \$500 to \$5000 | .101 | -- | -- | -- |
| Over \$5000 | .059 | .061 | .027 | .0 |
| White Collar | -- | .228 | .258 | .2 |

SOURCE: U.S. Board of Parole Research Unit.

- a. Listwise deletion of missing values.
- b. Excludes selective service and immigration and naturalization service violators. Also excludes races other than black or white. Listwise deletion missing values.

As can be seen from the table, only a minority of the releases had ever worked for more than four years at a stretch. The average employment for the rest has only about sixteen months. Almost a quarter of the sample had not worked more than 25 percent of the time in the two years preceding imprisonment. These employment measures are all extremely correlated. We concentrate on the "employment greater than four years" variable in our analysis.

The criminal justice system, offense characteristics, and criminal history variables displayed in the first column of Table 3.1 refer to the entire sample of nearly 2,500 cases. In much of the analysis that follows the sample is restricted to about 2,100 cases of blacks and whites who were not violators of either the selective-service or the Immigration and Naturalization Service (INS) laws. Moreover, few of the many criminal-justice variables had strong independent influences on recidivism. We highlight here, therefore, only those variables included in subsequent analysis.

The average number of parole hearings was nearly one and three quarters, although it was lower than that for blacks. While half of the white sample was released on parole, only a little more than a third of blacks were. Receiving fewer parole hearings and being less likely to be released on parole would be understandable for blacks if they 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 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 armed or unarmed robbery, burglary, larceny, and auto theft. By eliminating selective-service or immigration violations, 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 accused of 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 in the alleged crime than whites.

In summary, then the federal prison-release sample differs markedly, by inspection, from the typical state-prison population. Moreover, there are distinct differences between the black and the white exoffenders both in background characteristics and in treatment within the criminal justice system. The task at hand is to identify the economic and noneconomic determinants of recidivism. In a later chapter, we explore more carefully the racial differences in recidivism.

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 background characteristics, criminal history and offense characteristics, and variables relating to the criminal justice system. This is one empirical counterpart to the 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 correlate 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

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

The method of maximum likelihood can be employed to yield unbiased and consistent estimates of the parameters, β . Since p is nonlinear in the β 's, it is useful to calculate the derivatives of the predicted recidivism rate, \hat{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. Tables 3.2, 3.3, and 3.4 provide parameter estimates and derivatives for sets of explanatory variables entered into the equation separately. In table 3.5 combinations of explanatory variables are explored, while in table 3.6 a complete model with highly collinear variables deleted is displayed.

The general findings can be conveniently summarized. Older exoffenders, 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, exoffenders 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 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 3.2
DETERMINANTS OF THE PROBABILITY OF REARREST: EMPLOYMENT AND PERSONAL CHARACTERISTICS
(t-STATISTICS IN PARENTHESES)

| Independent Variables | (1) $\hat{\beta}$ | (2) $\partial\hat{\beta}/\partial x_1$ | (3) $\hat{\beta}$ | (4) $\partial\hat{\beta}/\partial x_1$ | (5) $\hat{\beta}$ | (6) $\partial\hat{\beta}/\partial x_1$ | (7) $\hat{\beta}$ | (8) $\partial\hat{\beta}/\partial x_1$ | (9) $\hat{\beta}$ | (10) $\partial\hat{\beta}/\partial x_1$ |
|--|----------------------|---|----------------------|---|----------------------|---|----------------------|---|----------------------|--|
| Age | -.001 (-4.379) | -.000 | -.002 (-4.823) | -.000 | --- | --- | --- | --- | -.001 (-1.715) | -.002 |
| Black | .316 (2.687) | .067 | .263 (2.162) | .057 | --- | --- | --- | --- | .396 (2.519) | .075 |
| Female | -.551 (-2.276) | -.117 | -.662 (-1.994) | -.143 | --- | --- | --- | --- | -.653 (-2.043) | -.124 |
| Grade Claimed | -.058 (-2.796) | -.012 | -.040 (-1.921) | -.008 | --- | --- | --- | --- | -.078 (-2.648) | .014 |
| Married | -.531 (-4.640) | -.112 | -.493 (-4.270) | -.107 | --- | --- | --- | --- | -.561 (-3.022) | -.107 |
| Alcoholic | .435 (4.327) | .092 | .373 (3.707) | .081 | --- | --- | --- | --- | .496 (3.663) | .094 |
| No Drug Use | -13.260 (-13.260) | -2.814 | -13.260 (-13.260) | -2.878 | --- | --- | --- | --- | 13.260 (13.047) | -2.528 |
| Mental Hospital | .697 (4.469) | .147 | .639 (4.101) | .138 | --- | --- | --- | --- | .564 (2.681) | .107 |
| Race and Sex Interaction | --- | --- | .089 (.019) | --- | --- | --- | --- | --- | --- | --- |
| IQ | .000 (.054) | .001 | -.001 (-.451) | .000 | --- | --- | --- | --- | -.002 (-.420) | -.000 |
| Employed less than 4 Years | --- | --- | --- | --- | 1.497 (6.284) | .290 | 1.632 (6.785) | .328 | .984 (3.243) | .187 |
| Longest Job | --- | --- | --- | --- | -.204 (-2.847) | -.039 | -.211 (-2.898) | -.042 | -.115 (-1.319) | -.022 |
| Last Civilian Experience | --- | --- | --- | --- | -.410 (-2.978) | -.079 | -.286 (-2.064) | -.057 | -.394 (-2.581) | -.075 |
| On the Job Training | --- | --- | --- | --- | .231 (1.866) | .044 | .139 (1.096) | .028 | .109 (.795) | .020 |
| Work Release | --- | --- | --- | --- | --- | --- | -.127 (-.954) | -.025 | --- | --- |
| Constant | .239 (.576) | --- | .456 (1.092) | --- | -1.744 (-7.528) | --- | -1.903 (-7.747) | --- | -.076 (-.111) | --- |
| Weighted Mean of Dependent Variable | .316 | --- | .328 | --- | .277 | --- | .294 | --- | .283 | --- |
| Predicted Probability of Weighted Means of Independent Variables | .305 | --- | .318 | --- | .262 | --- | .279 | --- | .256 | --- |
| Chi-Square | 109.903 | --- | 99.521 | --- | 81.396 | --- | 80.262 | --- | 129.674 | --- |

TABLE 3.3
DETERMINANTS OF THE PROBABILITY OF REARREST: CRIMINAL HISTORY AND OFFENSE CHARACTERISTICS
(t-STATISTICS IN PARENTHESES)

| Independent Variables | (1) $\hat{\beta}$ | (2) $\partial\hat{\beta}/\partial x_1$ | (3) $\hat{\beta}$ | (4) $\partial\hat{\beta}/\partial x_1$ | (5) $\hat{\beta}$ | (6) $\partial\hat{\beta}/\partial x_1$ |
|--|----------------------|---|----------------------|---|----------------------|---|
| Free Less than 6 Months | .344 (2.302) | .071 | --- | --- | .343 (2.267) | .070 |
| Free More than 6 Months, Less than 36 Months | .696 (6.106) | .143 | --- | --- | .655 (5.655) | .133 |
| Prior Commitments | .071 (1.821) | .014 | --- | --- | .084 (2.138) | .017 |
| Prior Incarcerations | .017 (.424) | .003 | --- | --- | .027 (.645) | .005 |
| Parole Revoked | .363 (3.583) | .074 | --- | --- | .328 (3.210) | .066 |
| Incarcerations/Convictions | .173 (.820) | .035 | --- | --- | .117 (.557) | .024 |
| Age at First Commitment | -.035 (-4.753) | -.007 | --- | --- | -.027 (-3.628) | -.005 |
| Convictions | .021 (1.317) | .004 | --- | --- | .012 (.785) | .002 |
| Escaped | .104 (.686) | .021 | --- | --- | .095 (.791) | .019 |
| Prison Punishment | .409 | .064 | --- | --- | .358 (3.926) | .081 |
| Robbery, Theft, Burglary | --- | --- | .811 (6.710) | .170 | .554 (4.221) | .112 |
| Sex Offenses | --- | --- | .144 (.276) | .030 | .296 (.557) | .060 |
| Other Violent | --- | --- | .735 (2.287) | .154 | .655 (1.933) | .133 |
| Alcohol and Drug | --- | --- | .007 (.050) | .001 | .076 (.508) | .015 |
| Less than \$500 | --- | --- | .575 (5.013) | .120 | .539 (4.421) | .109 |
| \$500 to \$5000 | --- | --- | .073 (.450) | .015 | .179 (1.056) | .036 |
| Over \$5000 | --- | --- | -.855 (-3.267) | -.179 | -.438 (-1.569) | -.089 |
| Constant | -1.066 (-4.710) | --- | -1.368 (-11.772) | --- | -1.619 (-6.182) | --- |
| Weighted Means of Dependent Variable | .308 | --- | .309 | --- | .308 | --- |
| Predicted Probability at Weighted Means of Independent Variables | .290 | --- | .299 | --- | .294 | --- |
| Chi-Square | 235.900 | --- | 107.130 | --- | 275.523 | --- |

SOURCE: U.S. Board of Parole.

TABLE 3.4
DETERMINANTS OF THE PROBABILITY OF REARREST:
CRIMINAL JUSTICE SYSTEM VARIABLES
(t-STATISTICS IN PARENTHESES)

| Independent Variables | $\hat{\beta}$ | $\frac{\partial \hat{p}}{\partial x_i}$ |
|--|-------------------|---|
| New Commitment | .945 (-4.921) | -.199 |
| Parole Violator | -.650 (-2.898) | -.136 |
| Regular Adult | -.723 (-3.352) | -.152 |
| Immediate Parole | -.632 (-2.837) | -.133 |
| YCA Indeter | -.310 (-1.350) | -.065 |
| FJDA Minority | -.491 (-1.678) | -.103 |
| Maximum Custody | -.004 (-.003) | -.001 |
| Close Custody | .466 (2.848) | .098 |
| Medium Custody | .191 (1.317) | .040 |
| Minimum Custody | -.060 (-.466) | -.012 |
| Work Release | .070 (.489) | .014 |
| Parole Hearings | .074 (1.692) | .015 |
| Prison Punishment | .517 (5.184) | .109 |
| Constant | .263 (.887) | -- |
| Chi-Square | 116.707 | -- |
| Weighted Mean of Dependent Variable | .309 | -- |
| Predicted Probability at Weighted Means of Independent Variables | .301 | -- |

SOURCE: U.S. Board of Parole.

TABLE 3.5
MAXIMUM LIKELIHOOD ESTIMATES OF THE PROBABILITY OF POSTPRISON REARREST FROM:
ALTERNATIVE MODELS OF RECIDIVISM
(t-STATISTICS IN PARENTHESES)

| Independent Variable | Model 1 | | Model 2 | | Model 3 | |
|--|-------------------|---|-------------------|---|-------------------|---|
| | $\hat{\beta}$ | $\frac{\partial \hat{p}}{\partial x_i}$ | $\hat{\beta}$ | $\frac{\partial \hat{p}}{\partial x_i}$ | $\hat{\beta}$ | $\frac{\partial \hat{p}}{\partial x_i}$ |
| Age | -.007 (-1.273) | -.001 | -.034 (-3.696) | -.007 | -.015 (-1.876) | .003 |
| Black | .217 (1.959) | .046 | .213 (1.884) | .045 | .232 (2.676) | .049 |
| Female | -.605 (-2.442) | -.129 | -.418 (-1.675) | -.088 | -.451 (-1.814) | -.096 |
| Grade Claimed | -.040 (-2.130) | -.008 | -.023 (-1.180) | -.004 | -.027 (-1.420) | -.605 |
| Married | -.359 (-3.054) | -.077 | -.346 (-2.901) | -.073 | .364 (-3.078) | -.077 |
| No Drug or Drink | -.388 (-3.108) | -.083 | -.373 (-2.949) | -.079 | -.380 (-3.018) | -.081 |
| Mental Hospital | .417 (2.627) | .089 | .517 (3.194) | .109 | .512 (3.200) | .109 |
| Parole Hearings | .075 (1.640) | .016 | .107 (2.157) | .022 | .107 (2.182) | .022 |
| Prison Punishment | .392 (3.626) | .084 | .391 (3.490) | .082 | .395 (3.561) | .084 |
| Release on Parole | -.132 (-1.174) | -.028 | .015 (.127) | .003 | -.070 (-.612) | -.015 |
| Robbery, Theft, Burglary | .429 (3.463) | .092 | .271 (2.104) | .057 | .208 (1.586) | .044 |
| White Collar | .410 (2.584) | .088 | .257 (1.630) | .054 | .093 (.527) | .017 |
| Greater than \$5000 | -.840 (-2.964) | -.180 | -.636 (-2.190) | -.135 | -.641 (-2.236) | -.136 |
| Employed More than 4 Years | -.765 (-3.715) | -.164 | --- | --- | --- | --- |
| Longest Job | -.068 (-1.332) | -.014 | --- | --- | --- | --- |
| Last Civilian Job | -.183 (-1.366) | -.034 | --- | --- | --- | --- |
| On the Job Training | -.174 (-1.174) | -.028 | --- | --- | --- | --- |
| Free Less than 36 Months | --- | --- | .016 (.138) | .003 | .021 (.185) | .004 |
| Time Served | --- | --- | -.004 (-1.628) | -.001 | -.005 (-1.767) | -.001 |
| Age of First Commitments | --- | --- | -.003 (-.305) | -.007 | -.020 (-2.101) | -.004 |
| Commitments/Convictions | --- | --- | (5.297) | | (4.452) | |
| Convictions | --- | --- | .060 (4.612) | .012 | --- | --- |
| First Offender | --- | --- | -.371 (-1.716) | -.078 | -.580 (-2.751) | -.123 |
| Constant | .087 (.258) | --- | -.079 (-.230) | --- | .320 (.951) | --- |
| Weighted Mean of Dependent Variable | .328 | --- | .328 | --- | .328 | --- |
| Predicted Probability at Weighted Means of Independent Variables | .311 | --- | .305 | --- | .307 | --- |
| Chi-Square | 168.112 | --- | 218.101 | --- | 192.644 | --- |

SOURCE: U.S. Board of Parole.

Table 3.6 provides a comparison of the effects of the (a) certainty and severity of punishment and (b) employment on crime. We find that those who were employed for more than four 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 would argue that employment opportunities are only weakly related to postprison illegal activities. In fact, to test the hypothesis that employment opportunities have any additional explanatory power in our recidivism equation, a likelihood-ratio test can be performed. The chi-square statistics for this test for the total, whites, and blacks are 3.05, 1.26, and 1.52 respectively. For these low values, at 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 and higher wages will not deter crime. We discovered in our analysis of the Baltimore LIFE data that preprison employment experience is only weakly correlated with postprison employment. Thus it may be a poor proxy for the returns to postprison 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

TABLE 3.6
 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 | $\partial\beta/\partial x_i$ | | β | $\partial\beta/\partial x_i$ | | β | $\partial\beta/\partial x_i$ | | β | $\partial\beta/\partial x_i$ | | β | $\partial\beta/\partial x_i$ | | | | | | | | | | | | | |
| | | | | | | | | | | | | | | | | | | | | | | | | | | | |
| Age | -.033 (-3.860) | -.007 | | -.027 (-2.604) | -.005 | | -.059 (-3.709) | -.013 | | -.030 (-4.300) | -.007 | | -.009 (-2.955) | -.006 | | -.003 (-3.576) | -.014 | | .0003 (.052) | .0001 | | .009 (1.212) | .001 | | -.029 (-2.164) | -.006 | |
| Female | -.389 (-1.553) | -.001 | | -.360 (-1.044) | -.074 | | -.500 (-1.361) | -.111 | | -.373 (-1.506) | -.001 | | -.352 (-1.020) | -.073 | | -.485 (-1.304) | -.106 | | -.449 (-1.630) | -.066 | | -.480 (-1.400) | -.100 | | -.607 (-1.663) | -.134 | |
| Grade Claimed | -.026 (-1.130) | -.004 | | -.021 (-.976) | -.001 | | -.038 (-1.952) | -.000 | | -.01 (-1.107) | -.001 | | -.01 (-.932) | -.001 | | -.019 (-1.076) | -.000 | | -.016 (-1.150) | -.005 | | -.027 (-1.130) | -.005 | | -.011 (-.813) | -.007 | |
| Married | -.350 (-2.923) | -.074 | | -.314 (-2.752) | -.079 | | -.245 (-1.003) | -.053 | | -.363 (-3.040) | -.077 | | -.196 (-1.617) | -.082 | | -.249 (-1.022) | -.054 | | -.375 (-3.192) | -.079 | | -.414 (-3.059) | -.086 | | -.761 (-1.000) | -.057 | |
| No Drug or Drink | -.316 (-2.648) | -.071 | | -.375 (-2.568) | -.077 | | -.424 (-1.553) | -.092 | | -.340 (-2.600) | -.072 | | -.376 (-2.575) | -.078 | | -.439 (-1.602) | -.096 | | -.339 (-2.707) | -.072 | | -.413 (2.807) | -.086 | | .769 (-1.009) | -.059 | |
| 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 | | .433 (2.742) | .092 | | .396 (2.357) | .082 | | 1.070 (2.168) | .236 | |
| Parole Hearings | .109 (2.204) | .023 | | .106 (1.850) | .022 | | .127 (1.185) | .027 | | .109 (2.213) | .023 | | .106 (1.844) | .022 | | .128 (1.194) | .028 | | .062 (1.353) | .013 | | .068 (1.129) | .012 | | .086 (.830) | .019 | |
| Prison Punishment | .398 (3.559) | .084 | | .437 (3.313) | .090 | | .293 (1.324) | .064 | | .396 (3.546) | .084 | | .436 (3.309) | .090 | | .291 (1.309) | .063 | | .364 (3.358) | .077 | | .420 (3.319) | .087 | | .180 (.846) | .039 | |
| Release on Parole | .010 (.090) | .002 | | -.016 (-1.118) | -.003 | | .170 (.716) | .037 | | .007 (.006) | .001 | | -.016 (-1.118) | -.003 | | .160 (.675) | .035 | | -.092 (-.818) | -.019 | | -.128 (-1.901) | -.026 | | .060 (.300) | .015 | |
| Robbery, Theft, Burglary | .148 (1.120) | .031 | | .105 (.673) | .021 | | .082 (.309) | .018 | | .149 (1.132) | .031 | | .105 (.676) | .022 | | .090 (.339) | .019 | | .268 (2.104) | .057 | | .227 (1.513) | .047 | | .202 (.808) | .044 | |
| White Collar | .018 (.117) | .004 | | -.095 (-4.91) | -.020 | | .049 (.163) | .010 | | .016 (.103) | .003 | | -.098 (-5.011) | -.020 | | .051 (.170) | .011 | | .179 (1.160) | .038 | | .071 (.377) | .014 | | .187 (.658) | .041 | |
| Greater Than \$5000 | -.515 (-2.141) | -.130 | | -.588 (-2.071) | -.142 | | -.387 (-1.592) | -.084 | | -.615 (-2.135) | -.130 | | -.692 (-2.085) | -.143 | | -.350 (-1.532) | -.076 | | -.074 (-2.368) | -.143 | | -.781 (-2.401) | -.163 | | -.233 (-1.369) | -.051 | |
| First Offender | -.312 (-1.260) | -.066 | | -.317 (1.260) | -.065 | | -.176 (-.385) | -.037 | | -.330 (-1.552) | -.070 | | -.331 (-1.718) | -.060 | | -.193 (-1.445) | -.042 | | -.554 (-2.663) | -.118 | | -.609 (-2.532) | -.127 | | -.254 (.835) | -.078 | |
| Age at First Commitment | .001 (.133) | .0003 | | .011 (.965) | .002 | | -.043 (-1.918) | -.009 | | -.001 (-1.116) | -.0002 | | .009 (.806) | .001 | | -.046 (-2.077) | -.010 | | -.026 (-3.063) | -.005 | | -.014 (-1.906) | -.004 | | -.064 (-3.310) | -.014 | |
| Employed More than 4 Years | -.356 (-1.728) | -.075 | | -.264 (-1.105) | -.054 | | -.544 (-1.217) | -.187 | | --- | --- | | --- | --- | | --- | --- | | -.515 (-2.554) | -.110 | | -.439 (-1.914) | -.091 | | -.785 (-1.813) | -.173 | |
| Time Served | -.003 (-1.814) | -.001 | | -.004 (-1.279) | -.0009 | | -.008 (-1.423) | -.001 | | -.005 (-1.762) | -.001 | | -.004 (-1.234) | -.0009 | | -.008 (-1.408) | -.001 | | --- | --- | | --- | --- | | --- | --- | |
| Commitment/Convictions | 1.844 (5.667) | .390 | | 1.546 (3.949) | .320 | | 2.615 (4.129) | .570 | | 1.897 (5.793) | .402 | | 1.581 (4.055) | .328 | | 2.708 (4.271) | .593 | | --- | --- | | --- | --- | | --- | --- | |
| Convictions | .062 (4.700) | --- | | .085 (5.242) | .018 | | .015 (.633) | .003 | | .064 (4.909) | .013 | | .019 (5.412) | .018 | | .017 (.698) | .003 | | --- | --- | | --- | --- | | --- | --- | |
| Constant | -.045 | --- | | .535 | --- | | 2.219 | --- | | .015 (.135) | --- | | .475 (-1.255) | --- | | 2.363 (2.944) | --- | | .182 (.570) | --- | | -.136 (-1.381) | --- | | 1.928 (2.586) | --- | |
| Weighted Mean of Dependent Variable | .328 | --- | | .318 | --- | | .357 | --- | | .328 | --- | | .318 | --- | | .357 | --- | | .328 | --- | | .318 | --- | | .356 | --- | |
| Predicted Probability at Weighted Means of Independent Variables | .304 | --- | | .293 | --- | | .321 | --- | | .305 | --- | | .294 | --- | | .324 | --- | | .307 | --- | | .296 | --- | | .328 | --- | |
| R-Square | .218 | .61 | --- | .171 | .016 | --- | .86 | .285 | --- | .215 | .007 | --- | .169 | .750 | --- | .84 | .762 | --- | .174 | .70 | --- | .134 | .860 | --- | .68 | .579 | --- |

conviction, is expected to be a deterrent to crime too. But the denominator in this measure, number of convictions, also measures criminal record and labelling 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 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 labelling or discrimination theory of postprison behavior. Exoffenders 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 experiences 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 severity of punishment is a deterrent to crime, but increased certainty of punishment is positively related to rearrest rates.

Caution should be exercised in generalizing these results beyond federal exprisoners. The sample differs from other state and local samples of felons. In the analysis that follows, we concentrate on a predominately black, urban, male, repeat-offender sample in a limited geographical area.

C. THE BALTIMORE LIFE CASE

We have previously examined this sample. Because the data was obtained in conjunction with an income-supplement experiment, it is useful to examine that program explicitly. The logic of the program was straightforward. If a cash cushion were provided for released prisoners, their incentive to return to a life of crime immediately upon release from prison would be diminished. After some period of job search, individuals would find better, higher-paying jobs and in the long run would adjust better and be less likely to turn to crime than offenders lacking this special financial assistance.

A crucial point here is that the program was politically feasible. No major legislative labyrinth impeded the extension of normal unemployment benefit coverage to released prisoners. Nor could critics argue that exoffenders would be receiving special treatment when tens of thousands of other disadvantaged workers went without similar government subsidized support.

At first glance, the Baltimore LIFE experiment was a success. Evaluations of the carefully designed experiment revealed that the financial aid reduced recidivism. Twenty fewer arrests could be attributed to treatment effects. The program was expanded and tested in Texas and Georgia. But success was not forthcoming there. Unable to replicate the Baltimore results, researchers Rossi, Berk, and Lenihan (1980) have sought to explain the subsequent failure. They have four basic explanations. First, the Georgia and Texas

experiments were administered differently. Whereas in Baltimore the cash subsidies were paid out by the researchers, in the retests correctional personnel or state employment agency officials were responsible for making the unemployment-benefit outlays. Second, the tax rates varied from the earlier experiment. Lenihan suggests that the effective tax rate on the unemployment insurance benefit was approximately zero in the Baltimore case (Lenihan, 1976). The Texas and Georgia retests, on the other hand, had built-in explicit tax rates varying to 75 percent. Third, the sample's size and composition were enlarged in the later experiments. The Baltimore test included only male repeat offenders with no drug history, but the Georgia and Texas samples included females and first offenders. Finally, there existed strong work disincentives in both sets of experiments. It could be argued that the zero effective tax rate in the Baltimore test merely masked some of this work reduction effect, which, as the retests discovered, overshadows the reduction in recidivism.

Each of these explanations for the inability to replicate the Baltimore experiment is equally plausible. Yet, the last one is bothersome for analysts concerned with the reliability of the original program evaluation. If, indeed, there were work disincentive effects in the Baltimore experiment, what exclusion, omission, or oversight led the analysts to inadvertently overlook them? In the course of estimating the effect of employment opportunities on crime, we will discover that previous analysts

grossly overestimated the effectiveness of unemployment insurance benefits in reducing recidivism. A first step is to sketch out the rationale for using unemployment insurance to reduce recidivism.

The case for unemployment insurance can be seen clearly in the context of our earlier model. Rewrite the expected wage, $E(w)$, as

$$E(w) = (1-u)\bar{w} + u \cdot I.$$

The expected wage is equal to the wage if employed, plus the unemployment benefit, I , if unemployed. Clearly, the unemployment benefit raises expected wages and thereby lowers the relative attractiveness of crime. Also, when we introduce unemployment insurance, the effect of unemployment on crime is no longer unambiguous. At least in the context of this simple model, as benefits grow relative to the wage, if employed, crime may fall as people opt for unemployment rather than work or crime,

This model is highly simplified. It does not detail the dynamics of job search in the real world, or even the demand-side effects of employers' hiring criteria. But even in this highly simplified model, it is a trivial matter to contrive an explanation for the fact that unemployment insurance may not reduce recidivism.

Suppose that the probability of being unemployed is functionally dependent upon the level of unemployment benefits. This could be the single-period analog of the multi-period phenomenon by which the duration of unemployment is a function of the "cost" of further

search. Unemployment insurance, of course, reduces this cost and thus leads to longer job search. We could write the expected wage then as

$$E(w) = [(1-u(I))]\bar{w} + u(I)I.$$

A little computation reveals now that increased unemployment insurance does not unambiguously increase expected wages and thereby reduce the relative attractiveness of crime. Specifically, we differentiate $E(w)$ with respect to I to obtain

$$\frac{\partial E(w)}{\partial I} = -u' \bar{w} + u + u'I,$$

which is of ambiguous sign.

In fact, to the extent that increased unemployment benefits may increase unemployment, and increased unemployment may lower expected wages, it is possible for higher unemployment benefits to result in higher crime rates. It all depends on the extent to which unemployment rates are raised by the benefits and upon the wage rate and the probability of unemployment. Paradoxically, the work-distinctive effect would be smallest in this simple model when the wage rates are very low or the unemployment rate is very high. To see this note that

$$\frac{\partial E(w)}{\partial I} > 0 \text{ as } \frac{u + u'I}{u'} > \bar{w}.$$

Clearly, the larger \bar{w} or the smaller u , the less likely it will be that $\partial E(w)/\partial I > 0$, the necessary condition for unemployment insurance to reduce participation in crime. The more disadvantaged the

population, the better this intervention strategy can be in reducing recidivism.

In order to estimate an economic model of crime incorporating unemployment insurance, a number of proxies for desired variables were constructed. The certainty of punishment is measured by the ratio of previous convictions to previous arrests. This could be regarded as the individual's subjective probability of being punished again. The severity of punishment is measured by time served on the last offense; it is the difference between the year of arrest for the current conviction and the year of release--an admittedly crude proxy, but the best available measure given the limitation of the data set. Although there are no measures of the gains to crime, variables like age and race could be correlated with criminal returns.

Legitimate opportunities are captured in a variety of ways. First, education can be viewed as a form of investment in future earnings. Second, higher earnings may be associated with greater experience. A measure of experience is computed as the length of time on the longest job held prior to incarceration, appropriately discounted by the length of time since that job was held. Third, expected wages are computed as the average weekly wage for each month. Annually, this measure takes account of the weeks unemployed during the year. On a monthly basis, this measure incorporates the weeks unemployed during the entire month. Receipt of unemployment

insurance is entered as a separate variable rather than appended to the expected wage variable as is done in the expected wage equation in the previous section. This is done both because the actual amounts received are not available in this version of the Baltimore LIFE tape and because of a desire to estimate the separate effects of the unemployment benefit. Each of these measures of legitimate opportunities is expected to be inversely related to recidivism.

Because unemployment probabilities are significantly affected for exoffenders by the job arrangements prior to release from prison, the variable job arrangement was included. To ward off the possible bias associated with selective screening by correctional personnel, a last control for type of prison release was made.

The results of maximum likelihood estimates of logistic functions for the probability of being rearrested in the t^{th} month are presented in table 3.7. In the first column are the results of estimates of the probability of being rearrested during the year. Note that the dependent variable takes on the value of 0 if "successful," but only becomes 1, denoting rearrest, in at most one month. Thus the sum of the monthly probabilities equals the annual rearrest rate. This is somewhat of an anomaly. If the experiment works best to reduce crimes among those who would have committed only one crime during the year, then the estimated treatment effect using this dichotomous measure would seriously overstate the crime reduction benefits.

Nonetheless, the results are revealing. In the annual equation, increases in the average weekly wage have a strong negative effect on the rearrest rate. While receiving unemployment insurance reduces recidivism, the estimated coefficient is only significant at the 10 percent level. This one-tailed statistical test is notably weaker than the 1 percent level met by the wage variable. The only other variables significant even at the 10 percent level are race and age.

Turning to the monthly equations, the results are even more striking. Whereas in the first four months average weekly wages are strongly related to lower recidivism, the effects of the financial aid are mixed. In only the first, sixth, and ninth months are the estimated coefficients of the treatment effect significant at the 5 percent level. Then, in the ninth month, the effect is positive. Part of this arises because of the odd way of measuring monthly rearrest rates, a point that can easily be addressed by redefining success.

An alternative specification, detailed in table 3.8, is estimated to capture a more intuitive notion of postprison success. Here, the dependent variable is defined as the probability that the individual was not rearrested in month t , given that up until that point he was not rearrested. In essence, this conditional probability denotes the survival rate. The independent variables are the same and the results are no less surprising. In every month, save the first, the

TABLE 3.8
 Maximum Likelihood Estimates of Coefficients in Logistic Model of Monthly Survival Probabilities
 (t-statistics in parentheses)

| Independent Variable | Month 1 | Month 2 | Month 3 | Month 4 | Month 5 | Month 6 | Month 7 | Month 8 | Month 9 | Month 10 | Month 11 | Month 12 |
|--------------------------------------|-------------------|--------------------|--------------------|--------------------|--------------------|--------------------|--------------------|--------------------|--------------------|----------------------|---------------------|--------------------|
| Constant | 4.849 (2.012) | 1.826 (1.356) | 2.906 (2.640) | 2.619 (2.608) | 2.555 (1.796) | 1.480 (1.801) | 1.041 (1.289) | 0.431 (0.551) | -0.421 (-0.562) | -1.398 (-1.924) | -1.702 (2.259) | -2.541 (-3.350) |
| Treatment Group | 1.297 (1.572) | 0.576 (1.482) | 0.358 (1.163) | -0.009 (-0.032) | -0.071 (-0.280) | 0.256 (1.144) | 0.255 (1.151) | 0.250 (1.153) | 0.024 (0.116) | 0.184 (0.887) | 0.194 (0.933) | 0.250 (1.214) |
| Nonwhite | -.100 (-.092) | -0.085 (-0.150) | -0.353 (-0.688) | -0.361 (-0.784) | -0.561 (-1.325) | -0.508 (-1.370) | -0.509 (-1.431) | -0.680 (-1.954) | -0.350 (-1.077) | -0.210 (-0.662) | -0.444 (-1.3898) | -0.410 (-1.300) |
| Paroled | -.207 (-.262) | 0.010 (0.022) | 0.137 (0.368) | 0.752 (2.272) | 0.506 (-1.675) | 0.291 (1.040) | 0.388 (1.410) | 0.297 (1.101) | 0.044 (0.167) | 0.091 (0.351) | -0.049 (-0.187) | 0.024 (0.091) |
| Job Arranged | .684 (.855) | 0.533 (1.272) | -0.264 (-0.769) | -0.388 (-1.267) | -0.147 (-0.532) | -0.148 (-0.586) | -0.178 (-0.726) | -0.102 (-0.428) | 0.362 (1.579) | 0.301 (1.343) | 0.284 (1.242) | 0.332 (1.452) |
| Experience | .026 (.893) | 0.004 (0.026) | 0.008 (0.700) | 0.013 (1.119) | 0.022 (2.110) | 0.015 (1.695) | 0.011 (1.304) | 0.014 (1.608) | 0.009 (1.121) | 0.0000004 (0.998) | 0.009 (1.082) | 0.005 (0.626) |
| Convictions/Arrests | -.197 (-.155) | 0.234 (0.344) | 0.133 (0.237) | -0.028 (-0.055) | 0.204 (0.450) | 0.817 (1.983) | 0.449 (1.126) | 0.368 (0.943) | -0.077 (-0.204) | 0.060 (0.162) | -0.204 (-0.553) | -0.116 (-0.315) |
| Age | -.786 (-1.356) | -0.016 (-0.414) | -0.040 (-1.326) | -0.029 (-0.977) | -0.036 (-1.364) | -0.031 (-1.314) | -0.018 (-0.792) | 0.0004 (0.019) | 0.012 (0.569) | 0.033 (1.710) | 0.030 (1.382) | 0.043 (2.023) |
| Time Served | .146 (.663) | 0.002 (0.022) | 0.022 (0.304) | -0.046 (-0.700) | -0.025 (-0.421) | -0.032 (-0.577) | -0.023 (-0.429) | -0.032 (-0.596) | -0.006 (-0.106) | 0.010 (0.204) | 0.071 (1.288) | 0.060 (1.082) |
| Education | -.089 (-.518) | 0.004 (0.046) | -0.082 (-1.074) | -0.122 (-1.712) | -0.121 (-1.898) | -0.077 (-1.334) | -0.066 (-1.183) | -0.040 (-0.745) | -0.003 (-0.057) | 0.004 (0.085) | 0.042 (0.786) | 0.082 (1.582) |
| Average Weekly Wage in in Month t | .012 (1.282) | 0.010 (2.444) | 0.016 (4.519) | 0.019 (5.927) | 0.013 (5.170) | 0.010 (4.814) | 0.011 (5.584) | 0.011 (5.577) | 0.010 (5.707) | 0.010 (5.848) | 0.009 (5.337) | 0.007 (4.337) |
| [Mean Weekly Wage in Month t] | [\$49.75] | [\$57.09] | [\$60.19] | [\$65.70] | [\$63.71] | [\$63.34] | [\$62.24] | [\$61.24] | [\$59.00] | [\$60.08] | [\$58.56] | [\$51.26] |
| Mean Survival Rate | 97.92% | 92.59% | 87.73% | 83.33% | 78.94% | 72.22% | 68.75% | 65.71% | 60.19% | 55.32% | 51.62% | 46.76% |
| χ^2 | 9.515 | 13.388 | 29.848 | 61.946 | 49.716 | 40.784 | 50.284 | 51.734 | 52.822 | 53.914 | 58.224 | 50.524 |

average weekly wage is positively related to success and significant at the 1 percent level. In the first month, the level of significance drops to 10 percent, but the effect is still positive. The effects of the financial aid on survival, though, are less clear-cut. In the first month, there is a large effect on postprison survival, although it is not strongly significant, not quite reaching the 5 percent level. In the second month, there is a slight positive effect. In no other month can we ascertain an effect significantly different from zero. Note that in the first and second months where the treatment efforts appear operative, the overall explanatory power of the estimated equations is low. Performing a likelihood-ratio test suggests that one should reject the hypothesis, on the basis of the low chi-squared value, that the logistic function with its included independent variables would predict survival rates better than the mean survival rate for the sample.

Until now, we have argued that exclusion of other variables like expected wages biases upwards the coefficient of the financial-aid variable. A further complaint arises when we include expected wages--and their implied component of unemployment--without taking into account the inherent simultaneity of participation in crime with participation in work. ^{1/} A third specification is implied here.

Recidivism depends upon expected wage. The expected wage, though, depends upon hours worked (i.e., unemployment). The greater

^{1/} This complaint does not arise in our analysis of hours worked in chapter 2 because we estimated a reduced form relationship.

the average weekly hours worked, the higher will be the average weekly wage earnings. But hours worked depend upon time spent in crime. To the extent that people combine work and crime, this is no constraint. But what about the people who get caught and go to jail? Being incarcerated reduces the hours available to work and thus, ceteris paribus, lowers the expected wage. To complete this model a final equation is needed to determine days spent in jail per week. Those who get rearrested are more likely to spend days in jail than the survivors. Thus there is a simultaneous equation system from which it is possible to estimate separately the recidivism and work-disincentive effects. These results are displayed in table 3.9.

As we hypothesized, higher wages reduce rearrest; longer hours worked increase weekly wages; days in jail restrict hours worked; and higher rearrest rates increase days in jail. The separate effects of the financial assistance are everywhere of the same sign as the right-hand-side endogenous variable. Thus the unemployment insurance lowers rearrest, raises wages, reduces hours worked, and increases days in jail. This is problematical because the net effect of the treatment is no longer unambiguous. And here is where the work-disincentive effect is seen most clearly. One would need to work more hours to raise wages and thereby reduce recidivism. But the unemployment insurance tends to reduce hours worked. To assure that the insurance benefit actually results in reduced crime, we must show that on balance the positive wage effects offset the negative work reduction effects.

Table 3.9

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Instrument Variable Estimates of Postprison Outcomes
(t-statistics in parentheses)

| Independent Variable | Rearrest Equations | | Wage Equations | | Hours Worked Equations | | Days in Jail Equations | |
|----------------------|--------------------|-------------------|----------------------------------|---------------------|------------------------|--------------------|------------------------|-------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Constraint | .982 (5.608) | .864 (13.356) | -76.347 (-2.807) ^a | -44.398 (-2.554) | 23.440 (6.017) | 25.540 (9.437) | .427 (.509) | .332 (.998) |
| Treatment | -.057 (-1.225) | -- | 6.208 (1.305) | -- | -1.798 (-2.168) | -1.877 (-2.277) | .007 (.056) | -- |
| Wage | -.005 (-2.750) | -.006 (-5.502) | -- | -- | -- | -- | -- | -- |
| Hours Worked | -- | -- | 3.682 (2.941) | 3.796 (5.100) | -- | -- | -- | -- |
| Days in Jail | -- | -- | -- | -- | -2.893 (-1.923) | -4.221 (-4.119) | -- | -- |
| Rearrest | -- | -- | -- | -- | -- | -- | 1.114 (1.723) | 1.354 (2.991) |
| Education | -.014 (-1.168) | -- | .132 (.110) | -- | .382 (1.736) | .366 (1.700) | -.001 (-.030) | -- |
| Convictions/Arrests | .018 (.227) | -- | 2.653 (3.58) | -- | -2.699 (-1.649) | -3.250 (-2.145) | -.456 (-1.933) | -.395 (-1.728) |
| Time Served | -.010 (-.877) | -- | .572 (.542) | -- | .124 (.592) | -- | .011 (.336) | -- |
| Paroled | .020 (.334) | -- | 6.299 (1.117) | -- | -.822 (-.743) | -- | -.221 (-1.302) | -- |
| Job Arranged | -.021 (-.361) | -- | -.123 (-.017) | -- | 4.048 (4.275) | 3.658 (4.157) | .087 (.533) | -- |
| Age | -.005 (-1.184) | -- | .564 (1.313) | -- | .037 (.425) | -- | -.007 (-.488) | -- |
| Race | .113 (1.540) | -- | 9.625 (1.491) | -- | -.582 (-.423) | -- | .298 (1.455) | -- |
| Experience | .001 (.679) | -- | .339 (1.809) | .471 (2.922) | .036 (1.002) | -- | -.008 (-1.728) | -.009 (-2.079) |
| Skilled Blue Collar | -- | -- | 9.479 (1.168) | -- | -- | -- | -- | -- |
| Living with Family | -- | -- | -- | -- | -1.390 (-1.583) | -- | -- | -- |
| Unskilled | .077 (1.449) | -- | -- | -- | -- | -- | -- | -- |

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With a little effort one can solve the odd-numbered equations in table 3.9 simultaneously for the rearrest rate and then differentiate the resulting value with respect to the treatment variable. One discovers then that

$$\frac{\partial t^*}{\partial I} = \frac{1}{1 - \gamma_1 \gamma_2 \gamma_3 \gamma_4} (\alpha_1 + \alpha_2 \gamma_1 + \alpha_3 \gamma_1 \gamma_2 + \alpha_4 \gamma_1 \gamma_2 \gamma_3)$$

where t^* is the rearrest rate, I is the unemployment insurance variable, and

α_1 = unemployment (treatment) coefficient in rearrest equation

α_2 = unemployment (treatment) coefficient in wage equation

α_3 = unemployment (treatment) coefficient in hours equation

α_4 = unemployment (treatment) coefficient in jail equation

γ_1 = wage coefficient in rearrest equation

γ_2 = hours coefficient in wage equation

γ_3 = jail coefficient in hours equation

γ_4 = rearrest coefficient in jail equation.

A little arithmetic reveals that the treatment effect is about -0.06 in the same order of magnitude estimated in the rearrest equation and displayed in column 1 of table 3.9.

Note that

$$\alpha_1 = -0.057$$

$$\alpha_2 = 6.200$$

$$\alpha_3 = -1.790$$

$$\alpha_4 = 0.007$$

$$\gamma_1 = -0.005$$

$$\gamma_2 = 3.680$$

$$\gamma_3 = -2.890$$

$$\gamma_4 = 1.110$$

so

$$\frac{\partial t^*}{\partial I} = \left[\frac{1}{1 - (-0.005)(3.68)(-2.89)(1.11)} \right] \times \left[(-0.057) + (6.20)(-0.005) + (-1.79)(3.68)(-0.005) + (0.007)(-0.005)(3.68)(-2.89) \right] = -.0581.$$

Immediately we realize that many of the coefficients used to arrive at this figure are insignificant. In particular, the coefficient of the treatment effect in the rearrest equation is insignificant at the 5 percent level. The model, therefore, was reestimated omitting all variables with coefficients insignificant at the 5 percent level. The same computation was performed to arrive at the net-treatment effect. Now the effect of unemployment insurance is to increase rearrest rates.

Dropping the zero coefficient yields, from the even-numbered columns of table 3.9:

$$\frac{\partial t^*}{\partial I} = \left[\frac{1}{1 - (-.006)(3.79)(-4.22)(1.35)} \right] \times (-1.87)(-.006)(3.79)$$

$$= .0489.$$

There is an intuitive way to see this. The even-numbered columns of table 3.9 show that the only direct effect of the treatment is on hours worked. Here, unemployment reduces the average weekly hours. By lowering hours worked, we depress the wage, which in turn increases rearrest, thereby raising days in jail and further reducing hours worked. A multiplier effect is operative here, with insufficient offsetting effect to keep from increasing crime. This is clearly the most extreme case of work disincentive. Briefly we inspect the total reduction in arrests in the previous cases and this one.

It is convenient to know not only the direction of the treatment effect but also its magnitude. In following the analysis of Mallar and Thornton (1977), it is possible to derive the change in rearrests attributable to the financial-aid experiments by multiplying the change in the probability of rearrest due to the experiment by the number of subjects receiving the cash subsidy, in this case 216. For nonlinear models, the probability change in question, or the partial derivative, is not a constant. I have chosen, for computational convenience, to evaluate the derivatives at the mean of the dependent variable, a procedure equivalent to evaluating the derivatives at the means of the independent variables when the estimated error

approaches zero. In linear models, of course, this evaluation procedure is not necessary.

Table 3.10 summarizes the rearrest computations for each of the three specifications suggested in the previous section. In column 1 the benchmark value derived by Mallar and Thornton is given. More than twenty rearrests are diverted by the experiment, according to their calculations.

In column 2, the rearrest reductions computed from table 3.7--the monthly and annual specifications of the conventional economic model of crime--are displayed. Noting that in some months, the treatment effect is positive and in other months it is negative, we obtain the sum for the year. This total, denoting a reduction in rearrests by 14.5, is contrasted with the reduction computed from the annual equation. When the average rearrests for the year are estimated, the reduction in rearrests due to the experiment is calculated to be 15.6. Thus the annual derivation overestimates the total of the monthly tallies by more than one rearrest among 216 participants.

In column 3, the increased number of survivors for each month is displayed. The numbers are premultiplied by -1 to reflect the fact that an added survivor is really a diverted rearrestee. The value computed for the twelfth month is essentially the annual estimated reduction. It is lower than the Mallar and Thornton value and both

Table 3.10
Estimated Reductions in Rearrests

| | Mallar & Thornton Model (1) | Conventional Economic Model (2) | Survival Model (3) | Change in Survivals (4) | Simultaneous Equation Model (5) | Simultaneous Equation Model (6) |
|----------|--------------------------------------|--|--------------------------|----------------------------------|--|--|
| Month 1 | -- | -6.2 | -5.71 | -2.83 | -- | -- |
| Month 2 | -- | -3.5 | -8.54 | 0.22 | -- | -- |
| Month 3 | -- | 1.15 | -8.32 | 8.35 | -- | -- |
| Month 4 | -- | 7.1 | 0.03 | 2.52 | -- | -- |
| Month 5 | -- | 1.0 | 2.55 | -13.64 | -- | -- |
| Month 6 | -- | -12.3 | -11.09 | -0.74 | -- | -- |
| Month 7 | -- | -2.8 | -11.83 | -0.34 | -- | -- |
| Month 8 | -- | 0.6 | -12.17 | 10.93 | -- | -- |
| Month 9 | -- | 12.5 | -1.24 | -8.58 | -- | -- |
| Month 10 | -- | -8.4 | -9.82 | -0.65 | -- | -- |
| Month 11 | -- | -0.7 | -10.47 | -2.97 | -- | -- |
| Month 12 | -- | -3.3 | -13.44 | | -- | -- |
| Total | -- | -14.5 | -- | -7.73 | -- | -- |
| Annual | -20.5 | -15.6 | -13.44 | -- | -12.55 | +10.56 |

Notes: a. From Mallar and Thornton, "Transitional Aid for Released Prisoners," Table 5.
b. From Table 1.
c. From Table 2.
d. From Table 3, all variables in.
e. From Table 4, only significant variables in.

the total monthly and annual reductions obtained in the conventional economic model.

To compute the total monthly reductions in the survival model, it is necessary to find the change in survivals from month to month. This is done in column 4. The sum of these changes, -7.73, represents the smaller number of rearrests among those exoffenders receiving financial aid. This value, it is easily seen, is more than 60 percent lower than the value estimated originally by Mallar and Thornton.

From table 3.10, the instrumental variable estimates of the simultaneous equation model of rearrest, there are two computations of the effect of financial aid. In column 5, the net reduction in arrests attributable to the experiment when all of the variables in the model are included is shown to be -12.55. In column 6, in contrast, rather than displaying a reduction in rearrests, there is shown a net increase in rearrests of 10.56 due to the experiment. This value comes about as a result of dropping the insignificant coefficients and reestimation of the simultaneous equation model detailed in table 3.10. Because the direct effects of the treatment are eliminated in all of the equations except the hours worked equation, the work-disincentive effect dominates, to create an estimated net increase in rearrests.

If one were to crudely average these alternative calculations of the effect of the experiment on rearrest, one would find that the actual reduction is more than one half that reported by earlier

analysts. Given that our estimates of the experimental effects range from a high of -15.6 to a low of +10.56, the evidence is clear that a mere respecification leads to significant reductions in the anticipated recidivism changes that can be attributed to the financial-aid experiment.

The foregoing discussion has provided a convenient vehicle for assessing the effects of employment opportunities on crime. On theoretical grounds alone it is not possible to conclude, for example, that better wages reduce crime. Similarly, lower unemployment, higher incomes, increased labor-force participation or diminished income inequality reduce crime only in special theoretical cases. While these special theoretical cases are both important and relevant in a perfectly general economic model improved economic opportunities have just as ambiguous effects as increased punishment does.

Thus the question whether (a) higher incomes or better jobs reduce crime and (b) the consequent reduction is larger vis-a-vis changes due to increased certainty and severity of punishment is an open empirical question. Until now the question could not be adequately addressed because aggregate data sets do not provide sufficient distinctions between increases of income to criminals and to legitimate labor market participants. And micro data sets like

the Bureau of Prison's lack adequate employment information. Using a rich micro data source from a experiment designed to reduce recidivism among a group of ex-prisoners in Baltimore, we can test the deterrence hypothesis against the "better wages reduce crime" view. We find strong and consistent support for the contention that better wages reduce crime. In our specification increased certainty and severity of punishment also reduce crime but the effect is so weak as to lead us to reject the hypothesis that the impact of punishment on recidivism is significantly different from zero. Although these findings are based on a specialized sample in a limited geographical area, they provide telling evidence that the case in favor of the improved employment opportunities hypothesis was not so weak after all.

D. SUMMARY

One standard economic model suggests that improved employment opportunities can affect participation in crime. In a very simple configuration, it is possible to show that higher wages and lower unemployment lead to a lower optimal allocation of effort to illicit activity. Yet, in general, the precise effects of employment on crime are ambiguous.

We examined two very different data sets on postprison recidivism. One, a sample of releases from the U.S. Federal Prison System reflects an older, predominantly white, hardened criminal population. The other, from the previously described Baltimore LIFE

experiment, is composed of a population perhaps more typical of repeat offenders released from state institutions: They are disproportionately black and young and have dismal previous employment experiences.

The image of federal prisoners as being highly educated, big-time businessmen convicted for such glamorous crimes as income tax evasion and Security and Exchange Commission violations is inaccurate. The federal releasees have lower than average educational attainment, are typically guilty of robbery, burglary and other forms of theft where the value is less than \$500, and have had relatively poor pre-prison employment experiences. In comparison to the Baltimore LIFE sample, though, the federal releasees are arguably less disadvantaged and more criminal. The label of the federal prisoner as the upper crust of the criminal echelon undoubtedly derives from this inter-group comparison.

We have estimated the effects of employment experiences on recidivism in our federal sample. More stable preprison employment is associated with lower postprison recidivism. The marginal effect of having worked more than four straight years before imprisonment is greater on black than white recidivism. However, in neither case is this effect very strong. Moreover, the added explanatory power of preprison employment is weak: Other background variables and measures of the certainty and severity of punishment explain the variance in recidivism just as well. Note that while the punishment

variables are important in explaining rearrest on parole violation, they do not have the traditional "deterrent" effects. Increased certainty of punishment increases recidivism rather than lowers it. This, it is argued, is due either to a labelling or discrimination impact, or to divergent attitudes toward risk.

We also estimate the effects of employment on recidivism in the Baltimore LIFE sample. Here preprison employment experiences again have weak effects. However, postprison employment significantly affects postprison rearrest: Higher wages and more hours worked (and therefore lower unemployment) are strongly associated with lower recidivism. Although unemployment insurance also reduces recidivism, we show that the effect is inflated by failure to appropriately take into account the work disincentive effect that unemployment insurance tends to reduce hours worked. Nonetheless, the evidence is convincing that improved employment can reduce crime.

Taken together, the conclusions from the two samples suggest that the dismal preprison employment experiences do not in and of themselves account for much of the variations in the postprison recidivism experience among exoffenders. This does not arise because of the homogeneity of the exoffender populations. Indeed, there is significant variation in preprison employment in both samples. Instead, it seems to arise because it is what happens in the labor market after release from prison that matters in determining return to crime, not what happened years ago. Of course, if preprison

employment experiences determine postprison employment opportunities, then the weak effect observed for the former arises because of the collinearity of the two. It is not necessary to make this assumption, though, in order to realize that postprison outcomes depend vitally on other postprison outcomes. This is what we found in chapter two in analyzing postprison employment. In addition, the preprison employment experience variable failed the statistical test in the federal sample. This could mean that preprison employment experiences are poor predictors of postprison employment opportunities, which challenges the view that the poor showing of the preprison experience variable in the Baltimore sample comes about because of its collinearity with the postprison employment variable.

CHAPTER IV. RACISM AND THE CRIMINAL JUSTICE SYSTEM

A. INTRODUCTION

There is intriguing documentation of a historical link between labor markets and the criminal justice system. Thorsten Sellin (1976) argues that the demands of the labor markets have traditionally shaped the penal system and that changes in that system through time are more closely related to changing labor market structures than to evolving theories of punishment. For example, the Romans, who perhaps held the largest number of slaves in antiquity, used prisoners to work on public projects. There was little need for prisons as we know them today because of the continuous construction of buildings and roads under the Roman rulers.

In the mid-seventeenth century, French prisoners manned the oars of the galleys. Originally, lifetime slavery at the oars had been a form of commutation of death sentences, but as the demand for rowers increased even petty criminals were sent to the galleys. The enlarged supply of galley convicts swelled, creating a major maintenance expense. At first older and infirm convicts were sent to Louisiana and the French West Indies, but they could not match the productivity of black slaves. Hence, in later years, alterations in the penal system were sought to deal with this largely economic problem. Sellin suggests that the development of the French industrial prisons was the solution.

In the United States, the crucial link between labor markets and the penal system appears to be race. The failures in the labor market--the poor, black, disadvantaged workers--are also the failures of the system of justice. Blacks have lower wages, higher unemployment, and fewer marketable skills; they are more often arrested, more likely to be convicted, and then go to prison for longer periods than whites: They are clearly disproportionately represented in prisons and jails. Sellin contends that this is no accident. It is a legacy of racism and slavery.

The story goes something like this (Sellin, 1976). 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 blacks were sold as slaves or deported. There was a significant push to make the penitentiaries occupied by the master-class criminals self-supporting, however. The costs of imprisonment represented a heavy burden on taxpayers. Why not make the prison turn a profit? In Kentucky this was tried in 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 states Louisiana and invited private firms to set up shop in the prison. Following the Civil War, though, both prison industries and convict-lease systems faced a major challenge in the South. Would these systems apply to the newly emancipated blacks? Would the master class and the former slaves be forced to work side

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 by penal servitude, private slavery would be replaced with public slavery. In part, the Thirteenth Amendment to the U.S. Constitution explicitly authorized "slavery" or "involuntary servitude" as punishment for illegal activities. Southern legislatures rushed to enact legislation and to revise their penal codes to facilitate an almost unbelievable 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 serves a somewhat different constituency than 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 many common street crimes. Most forms of robbery, burglary, larceny, auto theft, assault, rape, and homicide are prosecuted at the state or local level.

In addition, the Federal Prison System has its origins

principally in the North, the capitalist mecca that the Southern states were fighting when they devised the convict-lease system and their 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. We can easily identify the disparities, of course. In chapter III, tables 3.1 and 3.6 revealed that while background characteristics of blacks and whites differ, there are significant differences in how they are treated within the federal prison system. There are also noticeable differences in postprison outcomes.

The important question for public policy is how are these disparities linked? Can the differences between black and white rearrest rates be accounted for by diverging personal characteristics, criminal history, type of offense committed or other background variables? Or is the black-white recidivism gap due to nonracially neutral differences in treatment? These questions require an explicit examination of the sources of the racial gap in crime.

B. BLACK-WHITE DIFFERENCES IN RECIDIVISM

That there is a considerable racial gap in arrest rates for first offenders and in rearrest rates for repeat offenders is wellknown. Federal prison data reveals differences in the postprison failure rates for black and white exfelons. From table 3.6 (third row from

the bottom), we estimate that blacks are more likely to recidivate than whites. We note that 5.7 percent of blacks fail after release from federal prison while only 31.8 percent of whites do so. When one controls for any number of seemingly exogenous factors, the percentages become 32.1 and 29.3 for blacks and whites respectively (table 3.6, second row from bottom). This, of course, represents a small narrowing of the gap in recidivism, but not one of a magnitude to justify further exclusion of racism or racial discrimination as a cause of the gap. But if the cause is racism then what racism? Where is this elusive demon? In the courts, on the juries, in the prison cells, in the police stations, on the streets, in the work place?

To illustrate one method of addressing these questions, let us examine racial differences in the severity of punishment. When released from prison, blacks serve longer sentences than whites. In addition, blacks are more likely to be rearrested than whites. It might be contended that the differing rearrest rates follow from the differences in punishment. Are the observed differences in time served by blacks and whites due to differences in their ages, previous criminal records, and the types of crime for which they were convicted? Or can we assert that the differences are due to some sort of discrimination against blacks in the criminal justice system? A method has been developed in the econometric literature to compute the residual effect that race has on the outcome being investigated. Sometimes called residual discrimination analysis, the

method requires a fully specified model of how the outcome is generated, and it depends on assumptions concerning the observability of the independent variables and the lack of correlation between the error or stochastic disturbance term and the independent variables.

Suppose in our example time served is assumed to depend on the type of crime, characteristics of the offense, and prior criminal history of the offender. Then to isolate the effect of race on time served one estimates the equation:

$$TS = \sum_{i=1}^{n-1} x_i \alpha_i + x_n \alpha_n + \epsilon.$$

When $\alpha_1 \dots \alpha_{n-1}$ are $n-1$ independent variables measuring type of crime, characteristics of the offender and prior criminal history and n is a dummy variable that equals one if race is nonwhite and zero otherwise. The α_i are the coefficients to be estimated and reflect the marginal effect on time served of an increase in any one of the independent variables. Of course, it is assumed that time served is linear in its arguments and that the error term is normally distributed. Under such assumptions, ordinary least squares is an appropriate method of estimating the coefficients $\alpha_1 \dots \alpha_n$. The sample then is partitioned between blacks and whites and the time served equation is reestimated for both races dropping the race variable. Hence, we have two equations for time served:

$$TS^W = \sum_{i=1}^{n-1} x_i^W \alpha_i^W + \epsilon^W$$

and

$$TS^B = \sum_{i=1}^{n-1} x_i^B \alpha_i^B + \epsilon^B$$

where the variables are defined as before but where superscript B denotes black and W denotes white. The difference between white and black time served, $TS^W - TS^B$, would be attributable to the differences in the race-specific errors (i.e., racial discrimination) $\epsilon^W - \epsilon^B$ alone only if blacks and whites were otherwise identical both with respect to background characteristics (type of crime, criminal history, etc.) and with respect to the effects these non-race related characteristics have on time served. Not only do blacks and whites have very different characteristics, but also the effects on time served of type of crime and criminal history (among other variables) differ between blacks and whites. Suppose, however, that blacks and whites were "treated" exactly the same, so that blacks' time served could be computed as

$$\hat{TS}^B = \sum_{i=1}^n \hat{\alpha}_i^W x_i^B$$

where $\hat{\alpha}_i^W$ are the estimated white coefficients and \hat{TS}^B is the predicted time served for blacks if blacks and whites only differed with respect to the x's. Hence, the residual discrimination is

$$\hat{TS}^B - TS^B.$$

Conceptually ridding the system of this discrimination suggests replacing in the black recidivism equation TS^B with \hat{TS}^B . The question that is answered in so doing is how much of the racial gap in recidivism can be explained by discrimination in sentencing. Of course, the same logic can be applied to questions of differing pre-prison employment, parole release, criminal history, and certainty of punishment.

C. EMPLOYMENT, TREATMENT IN THE CJS, AND CRIMINAL HISTORY

Tables 4.1-4.4 present the results of the first-stage estimations needed to obtain the racially biasless measures used to predict recidivism.

Separate black and white logistic equations are estimated for the probability of having been employed for greater than four years prior to incarceration. As can be seen in table 4.1, 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 employment for both blacks and whites, although at different rates.

TABLE 4.1

MAXIMUM LIKELIHOOD ESTIMATES OF THE PROBABILITY OF PREPRISON

EMPLOYMENT GREATER THAN FOUR 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 |
| I.Q. | -.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 Drug or Drink | .917 (1.668) | .058 | .353 (1.324) | .024 |
| Mental Hospital | -.810 (-.759) | -.052 | -.675 (-2.048) | -.086 |
| Constant | -7.326 (-5.455) | -- | -6.448 (-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: U.S. Board of Parole.

TABLE 4.2

MAXIMUM LIKELIHOOD ESTIMATES OF THE PROBABILITY OF RELEASE ON PAROLE

(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 | -.061 (-4.573) | -.013 | -.057 (-9.166) | -.014 |
| Greater Than 5,000 | .295 (.484) | .064 | .887 (3.650) | .221 |
| Female | .575 (1.615) | .125 | .687 (2.314) | .171 |
| Grade Claimed | .046 (1.099) | .010 | .109 (4.957) | .027 |
| Married | .495 (2.044) | .108 | .488 (3.766) | .122 |
| No Drug or Drink | .950 (2.948) | .207 | .218 (1.414) | .054 |
| Mental Hospital | -.412 (-.735) | -.090 | -.719 (-3.727) | -.180 |
| Parole Hearings | .848 (7.071) | .185 | .761 (11.976) | .190 |
| Prison Punishment | -.771 (-3.218) | .168 | -.823 (-6.066) | -.205 |
| Robbery, Theft, Burglary | -.249 (-.916) | -.054 | -.658 (-4.321) | -.164 |
| White Collar | .304 (1.032) | .066 | -.221 (-1.219) | -.055 |
| Constant | -1.253 (-1.696) | -- | -.342 (-.910) | -- |
| Weighted Mean of Dependant Variable | .360 | -- | .500 | -- |
| Predicted Probability at Weighted Means of Independent Variables | .322 | -- | .502 | -- |
| Chi-Square | 131.557 | -- | 401.283 | -- |

SOURCE: U.S. Board of Parole.

TABLE 4.3

MAXIMUM LIKELIHOOD ESTIMATES OF THE PROBABILITY OF COMMITMENT
GIVEN CONVICTION (t-STATISTICS IN PARENTHESES)

| Independent Variables | Blacks | | Whites | |
|--|---------------------|---|---------------------|---|
| | $\hat{\beta}$ | $\frac{\partial \hat{\beta}}{\partial x_i}$ | $\hat{\beta}$ | $\frac{\partial \hat{\beta}}{\partial x_i}$ |
| Age | .062 (3.169) | .000 | .0792 (7.382) | .001 |
| I.Q. | .010 (.599) | .000 | .024 (1.828) | .000 |
| Female | -104.242 (-0.62) | -.000 | -14.387 (-5.533) | .211 |
| Grade Claimed | -.062 (-.715) | -.000 | .043 (-.824) | -.000 |
| Married | -.615 (-1.091) | -.000 | -.942 (-2.720) | -.013 |
| No Drug or Drink | -.061 (-.106) | -.000 | .296 (.687) | .004 |
| Mental Hospital | -222.771 (-.577) | -.000 | .240 | .003 |
| Constant | -5.230 (-3.084) | --- | -8.198 (-5.872) | --- |
| Weighted Mean of Dependent Variable | .049 | --- | .039 | --- |
| Predicted Probability of Weighted Means of Independent Variables | .000 | --- | .014 | --- |
| Chi-Square | 23.281 | --- | 70.738 | --- |

SOURCE: U.S. Board of Parole.

TABLE 4.4

ORDINARY LEAST-SQUARE ESTIMATION OF LN (TIME SERVED) AND LN (CONVICTIONS)

| Independent Variables | LN (Time Served) | | LN (Convictions) | |
|------------------------------------|-------------------|-------------------|-------------------|-------------------|
| | Whites β | Blacks β | Whites β | Blacks β |
| Age | .008 (8.00) | .009 (3.00) | .016 (16.00) | .029 (9.67) |
| Sex | -.184 (-2.52) | -- | -.527 (5.55) | -- |
| Married | .011 (.34) | -.114 (-1.84) | -.169 (-4.12) | -.165 (2.26) |
| No Drug or Drink | .027 (.73) | -.149 (-2.04) | -.195 (-4.15) | -.097 (-1.14) |
| Grade Claimed | -.010 (-1.67) | -.030 (-2.73) | -.075 (-10.71) | -.039 (-3.00) |
| I.Q. | .002 (2.00) | .004 (2.00) | .004 (4.00) | -.001 (.50) |
| Robbery, Theft, Burglary | -.185 (-5.00) | -.510 (-7.61) | -- | -- |
| Dollar Value Greater than 5,000 | -.243 (-5.40) | .098 (.62) | -- | -- |
| White Collar | .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 | 1.477 | 1.355 |
| Multiple R | .534 | .521 | .367 | .418 |
| R ² | .285 | .271 | .135 | .175 |
| Adjusted R ² | .280 | .256 | .132 | .165 |

SOURCE: U.S. Board of Parole.

finally, mental hospital confinement has no significant effect on 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 four 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 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 four years. This figure not only approaches the actual mean for whites, but it exceeds the value predicted for white exoffenders using the very same equation. What this means is that while much of the employment disparity between black and white exoffenders can be explained by differences in background characteristics, the low employment predicted for blacks is due largely to racial discrimination.

Blacks are less likely to be released on parole than whites. In table 4.2 estimates of parole-release probabilities are provided

for blacks and whites. The direction of effects of background variables on parole-release probabilities is similar for both races. Better educated, married, drug-free, younger, and female exoffenders are more likely to be released on parole whether they are black or white. More frequent parole hearings and less prison punishment result in higher parole release rates for both races. However, in many instances, these predictors are statistically insignificant for blacks. For example, while having netted over \$5,000 in the alleged crime will increase a white exoffenders chances of being released by more than 22 percentage points, it has a negligible effect on blacks. Taking account of these factors, moreover, merely narrows the black-white parole release gap from (.360-.500) to (.322-.502). However, if we predict the black probability from the white parameters, then the gap reduces to (.451-.502). Indeed, if blacks were treated exactly like whites in parole decisionmaking, but, of course, their differing background characteristics were appropriately accounted for, then blacks and whites would be released at nearly the same rates.

In tables 4.3 and 4.4 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. Similarly, when the black punishment probability is predicted using the white equation the estimated value, .032, moves closer to the actual value for whites. Blacks experience more certain punishment than whites, and a part of this can be accounted for by racial differences in how they are treated.

Blacks also experience more severe punishment than whites.

Recall from table 3.1 that the average time served by blacks is 24.7 months while whites serve only 23.7 months. Taking account of personal background characteristics and factors related to the crime, the average time served for blacks is predicted to be 19.06 months when evaluated at the white parameters. This dramatic reduction is suggestive of the same discriminatory process involving previous criminal records. On average, blacks in the sample have 6.6 previous convictions while whites have only 5.7. However, if black convictions were generated by the same process as white convictions-- if they were "treated" the same--then, appropriately taking into account black background characteristics, black convictions would total four and one half.

In summary, there are disparities between black and white federal exoffenders in (a) preprison employment experiences, (b) method of

release from prison, (c) certainty and severity of punishment, and (d) criminal histories. In every instance treating blacks like whites narrows the disparity. Some of the gap, we have seen, can be accounted for principally by differences in background characteristics. This was the true of preprison employment. But in other categories, notably release on parole, the only way to construct any significant narrowing of the gap is to effect an equal treatment of whites and blacks.

To extend the conceptual experiment a step further, it becomes useful to replace for blacks the actual values for preprison employment, certainty and severity of punishment, criminal history, and method of prison release with the predicted "discrimination-free" values. Table 4.5 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 derivations of the predicted probability of recidivism. First in column (1) the black recidivism function from table 3.6 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

TABLE 4.5
 MAXIMUM LIKELIHOOD ESTIMATES OF BLACK RECIDIVISM AND RESIDUAL DISCRIMINATION
 (t-STATISTICS IN PARENTHESES)

| Independent Variables | (1) $\hat{\beta}$ | (2) $\partial\hat{\beta}/\partial x_1$ | (3) $\hat{\beta}$ | (4) $\partial\hat{\beta}/\partial x_1$ | (5) $\hat{\beta}$ | (6) $\partial\hat{\beta}/\partial x_1$ | (7) $\hat{\beta}$ | (8) $\partial\hat{\beta}/\partial x_1$ | (9) $\hat{\beta}$ | (10) $\partial\hat{\beta}/\partial x_1$ | (11) $\hat{\beta}$ | (12) $\partial\hat{\beta}/\partial x_1$ |
|--|----------------------|---|----------------------|---|----------------------|---|----------------------|---|----------------------|--|-----------------------|--|
| Age | .059 (-3.289) | -.013 | -.041 (-1.937) | -.009 | -.010 (-1.414) | -.002 | -.034 (-1.079) | -.007 | -.029 (-1.841) | -.006 | -.051 (-1.850) | -.011 |
| Time Served | -.003 (-1.423) | -.001 | --- | --- | -.004 (-1.885) | -.001 | -.008 (-1.494) | -.001 | -.009 (-1.556) | -.002 | -.003 (-1.411) | -.001 |
| Predicted Time Served | --- | --- | -.110 (-1.871) | -.024 | --- | --- | --- | --- | --- | --- | --- | --- |
| Female | -.508 (-1.361) | -.111 | -.786 (-1.894) | -.171 | -.737 (-1.890) | -.162 | -.811 (-1.656) | -.177 | -.444 (-1.188) | -.096 | -.545 (-1.010) | -.119 |
| Grade Claimed | -.038 (-1.952) | -.008 | -.043 (-1.090) | -.009 | -.038 (-1.963) | -.003 | -.089 (-1.370) | -.019 | -.018 (-1.419) | -.004 | -.048 (-1.601) | -.010 |
| Married | -.245 (-1.003) | -.053 | -.220 (-1.900) | -.048 | -.353 (-1.322) | -.077 | -.479 (-1.380) | -.104 | .046 (.129) | .010 | -.253 (-1.889) | -.057 |
| No Drug or Drink | -.424 (-1.553) | -.092 | -.319 (-1.160) | -.069 | -.266 (-1.987) | -.058 | -.494 (-1.723) | -.107 | -.325 (-1.114) | -.070 | -.449 (-1.294) | -.098 |
| Mental Hospital | 1.162 (2.371) | .253 | 1.177 (2.395) | .256 | 1.142 (2.315) | .251 | 1.481 (2.514) | .323 | 1.020 (2.012) | .221 | 1.191 (2.416) | .260 |
| Parole Hearings | .127 (1.185) | .027 | .656 (2.068) | .143 | .110 (1.033) | .024 | -.203 (-1.532) | -.044 | .128 (1.197) | .028 | .127 (1.186) | .027 |
| Prison Punishment | .293 (1.324) | .064 | 1.039 (2.119) | .226 | .223 (1.021) | .049 | .648 (1.465) | .141 | .308 (1.384) | .066 | .284 (1.324) | .064 |
| Robbery, Theft, Burglary | .082 (.309) | .018 | -.237 (-1.687) | -.051 | .150 (.572) | .033 | .380 (.917) | .083 | .095 (.356) | .020 | .097 (.361) | .021 |
| Release on Parole | .170 (.715) | .037 | -.572 (-1.200) | -.124 | .043 (.189) | .009 | --- | --- | .180 (.756) | .039 | .147 (.627) | .032 |
| Predicted Release on Parole | --- | --- | --- | --- | --- | --- | 2.385 (.974) | .520 | --- | --- | --- | --- |
| White Collar | .049 (.163) | .010 | -.387 (-1.917) | -.084 | .137 (.457) | .030 | .161 (.499) | .035 | .059 (.196) | .012 | .072 (.237) | .015 |
| Greater than \$5000 | -.387 (-1.592) | -.084 | -.435 (-1.664) | -.095 | -.240 (-1.379) | -.052 | -.747 (-1.999) | -.163 | -.359 (-1.556) | -.078 | -.402 (-1.617) | -.067 |
| First Offender | -.170 (-1.386) | -.037 | -.116 (-1.264) | -.025 | -.380 (-1.879) | -.083 | -.164 (-1.374) | -.035 | -.160 (-1.367) | -.034 | -.210 (-1.485) | -.046 |
| Age at First Commitment | -.043 (-1.918) | -.009 | -.044 (-1.969) | -.009 | -.068 (-3.348) | -.015 | -.041 (-1.856) | -.009 | -.049 (-2.164) | -.010 | -.047 (-2.241) | -.010 |
| Commitments/Convictions | 2.615 (4.129) | .570 | 2.534 (4.098) | .552 | --- | --- | 2.576 (4.101) | .561 | 2.586 (4.035) | .561 | 2.519 (4.111) | .549 |
| Predicted Commitments/Convictions | --- | --- | --- | --- | -.419 (-1.775) | -.922 | --- | --- | --- | --- | --- | --- |
| Convictions | .015 (.633) | .003 | .012 (.496) | .002 | -.013 (-1.567) | -.003 | .012 (.516) | .002 | .013 (.551) | .002 | --- | --- |
| Predicted Convictions | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | -.023 (-1.03) | -.005 |
| Employed More than 4 Years | -.544 (-1.217) | -.118 | -.569 (-1.274) | -.124 | -.826 (-1.866) | -.181 | .522 (-1.174) | -.114 | --- | --- | -.561 (-1.247) | -.122 |
| Predicted Employment Greater than 4 Years | --- | --- | --- | --- | --- | --- | --- | --- | -3.586 (-1.115) | -.778 | --- | --- |
| Constant | 2.219 (2.736) | --- | 3.118 (3.139) | --- | 1.851 (2.175) | --- | 1.256 (.946) | --- | 1.449 (1.278) | --- | 2.414 (1.754) | --- |
| Weighted Mean of Dependent Variable | .357 | --- | .357 | --- | .356 | --- | .357 | --- | .357 | --- | .357 | --- |
| Predicted Probability of Weighted Means of Independent Variables | .321 | --- | .321 | --- | .326 | --- | .321 | --- | .318 | --- | .321 | --- |
| Chi-Square | 86.285 | --- | 87.709 | --- | 70.092 | --- | 86.738 | --- | 86.061 | --- | 85.939 | --- |

punishment means that blacks now have lower probabilities of being punished by imprisonment; hence they are more likely to recidivate. In column (7) blacks get to be paroled at nearly the same rate as whites. But from column (1) we realize that release on parole really does not affect recidivism substantially. So equal opportunity in release from prison (or more accurately, affirmative action in release from prison) does not assure lower rearrest probabilities. Column (11) details the effects of reducing disparities in criminal histories. Since the effect of a previous conviction record is small, equalizing this factor between blacks and whites has no effect on recidivism. However, 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 (9). Although, this reduction is minor, it is seen as the only narrowing of the recidivism gap arising from a conceptual policy of reducing preprison release racial discrimination.

D. SUMMARY

Other writers have alluded to the legacy of racism in the criminal justice system due to slavery and its aftermath. Blacks are disproportionately represented in the penal system. They serve longer sentences; they are more likely to be incarcerated rather than put on probation; they are less likely to be paroled; and because they are more likely to be rearrested, they are more likely to be returned to prison. Indeed, it has been argued, this state of

SOURCE: U.S. Board of Parole.

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 slaves where private involuntary servitude had been abandoned.

Prison populations have swelled with unskilled blacks during the past two decades. Has the penal system been operating again as a labor market equilibrating device? Do long prison sentences, low parole-release rates, and high rearrest rates for blacks act to buffer the high under- and unemployment rates among members of this group? These questions cannot be answered within the context of this study. But other kinds of questions can be answered. Are there racial disparities in a system like the Federal Prison System that is less beholden to the slavery past? Are these disparities linked to one another? And, if they were eliminated, would crime rates fall?

We conclude that in the Federal Prison System, seen through the lens of nearly 2,500 exfelons released in 1972, there are significant racial disparities in treatment. And there are apparent gaps in post-prison outcomes. Although there are only minor differences in pre-prison employment experiences, equalizing these represents about the only means of reducing the racial gap in recidivism through eliminating other racial disparities. Blacks and whites experience differing certainty and severity of punishment. Equal treatment will not close the racial group in postprison failure. Blacks and whites

are treated differently in the prisons; blacks are decidedly less likely to be released on parole. Yet, equal treatment will not close the gap in postprison failure. Blacks and whites have different criminal records. Unfortunately, equalization of previous criminal histories does nothing to close the racial gaps in rearrests. Equal treatment in preprison employment, we have found, will reduce the postprison recidivism gap, though by only a small amount. Thus we reach the following pessimistic conclusion: Eliminating racism or racial discrimination as it manifests itself in experiences of offenders before or during imprisonment will have little impact on postprison failure. We explore further the implications of this conclusion in a final section.

CHAPTER V. POLICY IMPLICATIONS AND CONCLUSIONS

A. INTRODUCTION

At the April 1978 Hearings on Unemployment and Crime before the Subcommittee on Crime (of the Committee on the Judiciary, U.S. House of Representatives), then Secretary of Labor Ray Marshall testified that he had no doubt that unemployment causes crime. He stated that if the goals of the Humphrey-Hawkins Bill were met crime would fall. Since the hearings, unfortunately, there has not even been an approach made toward meeting the ambitious employment goals of Humphrey-Hawkins, partly due to the Carter administration's anti-inflation objectives. Whether a drop in crime after a full-fledged implementation by Humphrey-Hawkins would have implied that unemployment really does cause crime is a debatable conjecture. At those same hearings, Secretary Marshall conceded when pressed by Congressman John Conyers, chairman of the subcommittee, that other employment programs for exoffenders have failed. Further research was apparently needed in order to devise employment programs that met the needs of offenders and exoffenders.

The research undertaken in this project was neither intended nor designed to yield new or better strategies for reducing recidivism, eliminating unemployment, or eradicating crime. Rather, the principal interest was in determining the validity of the view that employment opportunities affect participation in crime. An additional objective was to examine why exoffenders fail in the

labor market.

The premise that unemployment causes crime is a gross oversimplification of a complex interaction of the criminal justice system with labor markets. The hypothesis that exoffenders are discriminated against in the labor markets is an artifact of casual observations--supported by extensive data--suggesting that because of licensing restrictions and other barriers there are numerous jobs exoffenders are prohibited from holding. A rigorous test of the hypothesis is virtually impossible without information not now readily available.

What we have sought to do in this research, rather than prove nearly unprovable conjectures or propose never-before-proposed public policies, was to raise nagging questions that must be answered before workable policies can be designed.

The questions raised are largely empirical ones. To address them, two distinctly different data bases were chosen. One, relating to individuals released from the U.S. Federal Prison System, has the image of representing the cream of the crop of formerly incarcerated offenders. The other, drawn from the Maryland State Penitentiary, is a sample of acutely disadvantaged former criminals. More representativeness of these samples should not be claimed than is warranted, however. The federal sample is elite only to the extent that it includes fewer blacks and has a handful of criminals

convicted of "attractive" white-collar crimes. The Maryland sample is predominantly black, much more so than is true nationwide, but certainly as black as other prison samples drawn from states with large metropolitan areas. And, in many respects, the disadvantage of blacks in such prison populations is similar to the disadvantage of blacks not in the prison population.

F. SUMMARY OF RESULTS

To explore the determinants of employment and unemployment among exoffenders, the Maryland sample--part of an unemployment insurance experiment in Baltimore--was relied upon. The questions posed were whether there is a general effect of preprison disadvantage and specifically an effect of criminal history or previous employment experience on postprison employment. Does previous employment experience lower the postprison probability of being unemployed? Does it matter if the previous experience is before or after imprisonment? Does a more extensive criminal history mean that one will more likely be unemployed?

When postprison employment opportunities are captured by postprison unemployment probabilities, it is found that previous employment experience does affect employment outcomes. Although preprison work experiences have some effect, postprison employment has the really important effect. The higher the unemployment rate at release from prison the higher unemployment will be in subsequent months. Criminal history, moreover, has only a weak effect on

postprison employment; this is true whether the history is current or past and whether the measure of employment is the probability of unemployment or is hours worked.

When looking explicitly at hours worked as a measure of employment experience, the following results emerge. Background variables have strong effects on employment. In particular, race has a consistently strong impact on labor-market success. Blacks are found to work fewer hours than whites. However, family background variables capturing criminal behavior among relatives explain little of the variation in black hours worked while they are a significant determinant of white hours worked. Generally, as we have noted before, criminal history has little explanatory power for the employment experiences for either blacks or whites. Yet, preprison employment experience does. Additional months of work experience before incarceration raise employment for both blacks and whites after prison, although more so for whites than for blacks. For blacks, the dominating factor influencing employment immediately following release from prison is whether a job was arranged. Relative to what happens after release from prison, experiences before prison mean practically nothing. Postprison outcomes significantly affect black exoffenders' employment success, even above and beyond any possible effect preprison disadvantage may have. White exoffenders, on the other hand, are molded by their background. Virtually nothing after prison release seriously alters their previously established pattern of employment experience.

The findings of the effects of employment on recidivism are consistent in both data sets analyzed. Better preprison employment experiences have a weak and almost insignificant effect on postprison rearrest or parole violation. Does unemployment or lack of employment opportunities or low wages cause crime? In theory, the answer is ambiguous. From a crude examination of our results the answer in practice is yes and no. The federal sample suggests that if postprison employment (which is unobserved) is perfectly correlated with preprison employment, then better employment does not reduce crime, as measured by recidivism rates. The Baltimore sample, on the other hand, suggests that better wages do reduce crime. Moreover, the Baltimore sample reveals that preprison employment is at best weakly correlated with postprison employment, so the no in the federal sample might be yes after all. Further, in the Baltimore sample lower unemployment means higher hours worked leading to increased wages. Thus the "unemployment reduces crime" view is indirectly supported.

Using the federal data, we addressed ourselves to the question: Are racial differences in recidivism a result of treatment differentials in the criminal justice system? There are, of course, found to be significant racial differences in recidivism rates. There are also racial differences in how offenders are treated in the federal criminal justice system; in some instances, these differences are substantial. Further, there are specific differences in

preprison measures as employment and criminal history. But when controlling for differences in background characteristics and criminal history, the racial gap in recidivism tends to narrow. When one goes even further to conceptually rid the criminal justice system of residual discrimination, there is no further narrowing of the gap.

There is, however, an employment impact. If black-white preprison employment discrimination were eliminated, then the racial gap in recidivism would narrow an additional amount. Indeed, there would be a convergence of the predicted recidivism for blacks toward the actual value observed for whites. Controlling for the differences in background characteristics between them, however, still leaves a minor difference in black and white recidivism rates.

C. IMPLICATIONS

What do all of these findings mean? Why is there no effect of criminal record on employment after prison? What is the significance of the finding that background characteristics and general disadvantage may or may not explain postprison employment depending on whether one is white or black? Why should postprison employment experiences significantly affect employment among blacks but not whites? If employment disadvantage does not explain recidivism, then what does? And finally, how does one explain that only antiemployment discrimination policies will reduce the racial gap in recidivism? We attempt to address these questions individually.

1. NO EFFECT OF CRIMINAL RECORD ON EMPLOYMENT

Suppose that we had hypothesized that there is discrimination against exoffenders. Then, the finding that there is no effect of criminal record on postprison employment could be evidence against that hypothesis. But clearly we have not performed an adequate test. Such a test would require information on both criminals and noncriminals. Those individuals with no criminal records--if discrimination were operative--would experience more favorable employment outcomes. Among exoffenders alone, however, the only insight that can be learned about discrimination by examining criminal record is whether discrimination is based on degree or seriousness of a record and not whether discrimination is based on the existence of a record. The evidence is moot concerning whether having a criminal record reduces employment prospects. Furthermore, there does not seem to be support for the view that there is increased discrimination according to the degree or seriousness of a record.

Testing whether there is an effect of criminal record on employment is consistent with tests of other hypotheses, of course. Criminal record might be a proxy for prior participation in crime. If there is some sort of accumulation of criminal human capital arising from prior participation in crime, then through time the gains to crime will be higher for those with more extensive criminal records. Relative to participation in legitimate activity, therefore, crime would be more attractive. Thus the lower hours

worked, if found for more extensive criminal histories, would mean that more active criminals choose not to allocate larger fractions of their time to legal pursuits. Since we did not find that a more extensive criminal history lowers hours worked, this may mean

(a) that those with more extensive criminal histories do not necessarily accumulate additional criminal human capital or (b) that the gains to illegitimate activities through accumulation of criminal human capital do not outweigh any losses to legitimate activity when criminal records are lengthened, or (c) that criminal record is not a satisfactory proxy for prior participation in crime, or even (d) that the decision to engage in crime is unresponsive to changes in relative returns to crime.

Another hypothesis is that criminal history is a proxy for time out of the labor market. While more time engaging in crime may not increase criminal human capital, it may result in more time in court, in jail, or in prison. This means time not working. Time out of the labor market may represent deterioration of work skills, less experience, and therefore may lower probabilities of having a successful employment profile in the future. By this reasoning, our finding that preprison employment experiences have a weak impact on postprison employment is consistent with the finding that criminal history does not affect employment.

While policies and programs designed to eliminate licensing restrictions and other employment barriers posed by criminal records

are commendable in their own right, there is nothing in the findings reported here to suggest that these initiatives will affect the postprison employment prospects of exoffenders. Expungement of criminal records, for example, would make sense in order to reduce the criminal-labeling effect on rearrest, but the evidence does not reveal whether such an effort would reduce the unemployment rates of offenders. Indeed, if the only significant use of criminal records is made by law-enforcement agencies or prosecutors' offices, a legitimate complaint could be raised that eliminating access to this information on an individual's past may not only reduce crime-solving efficiency but may also reduce the deterrent effectiveness of criminal sanctions. In the absence of a strong finding that criminal records diminish employment prospects, the case for the substantial beneficial effects of expungement is weakened.

2. NO EFFECT OF DISADVANTAGE ON BLACK EMPLOYMENT

Varying background characteristics and degrees of preprison disadvantage explain little of the variation in postprison employment experiences of blacks, we discovered. We also note that having controlled for these factors--which explain much of the variation in postprison employment experiences among whites--there is among whites little added explanatory power of such postprison events as having been employed or rearrested during the first six months out of jail. One explanation for this result could be that the process by which blacks are arrested, convicted, or incarcerated is a random one (i.e., without regard to actual

participation in crime), and that the process affecting white involvement in the criminal justice system clearly discriminates among crime-prone and noncrime-prone individuals. Since being disadvantaged and an exoffender as opposed to being disadvantaged and a nonoffender is somewhat a matter of chance for a black, luck plays much more of a role in determining whether blacks get hired after prison than in the case of whites. Those blacks who are lucky enough to have a job arranged when they leave prison or who are lucky enough to find a job within a few months after release can expect to have more favorable subsequent employment experiences than the unlucky ones. This has nothing to do with relative disadvantage, criminal record, or previous employment experience. It is consistent with the view that their exoffender status is not strongly predicted by their backgrounds or experiences. It is also predictive of the view, discussed earlier, that criminal records, or other measures of exoffender status, are poor predictors of employment performance.

3. POSTPRISON EXPERIENCES DETERMINE BLACK, BUT NOT WHITE, EMPLOYMENT

The explanations as to why background variables do not explain black postprison employment can also be marshalled to explain why postprison experiences do. But we can go beyond these explanations and look at how preprison employment experiences affect white postprison employment. This examination will suggest why postprison experiences do not affect white offenders. Recall that preprison employment experience and having had a good job before imprisonment

tend to increase postprison employment for white exoffenders. Moreover, among whites, having a family member ever in prison reduces employment. Experience and background count among whites. Perhaps employers, at least for whites, go beyond workers' recent pasts. Perhaps, they look for indications of stability and prior successful job performance. Yet, even if they do not, white workers with successful pasts appear more apt to start out successful upon release. This means that any explanatory power of postprison variables in determining exoffenders' employment arises because of the correlation of these variables with preprison experience and background characteristics.

It should be easy to see that there are both demand-side and supply-side effects of background and experience on postprison employment. White exoffenders with more favorable experiences and backgrounds may be more willing to work. And their prior success--in spite of their current exoffender status--makes employers more willing to hire them. If previous experiences and background have a sufficiently strong effect on exoffender labor-supply decisions or if employers base their exoffender hiring decisions strongly upon information on prior-work history and family background, then there is little wonder that these variables predict postprison employment well. But generalizing this result beyond the Baltimore sample should be avoided because white exoffenders represented a small fraction of the participants in the LIFE experiment, and they appear to be somewhat more disadvantaged as a group than white exoffenders

generally:

There are a number of more explicit reasons why post-prison employment can be so significantly affected by blacks' early post-prison outcomes. The first has to do with affirmative action. Assume that through time there is a lessening of discrimination against blacks as a group that results in general improvement of the economic well-being of blacks. Then those blacks who have been out of the labor market because of incarceration may enjoy improved employment conditions relative to their preprison conditions, even taking into account their current exoffender status. This phenomenon does not explain why their background characteristics or preprison experiences do not explain much of the variation in their postprison employment experiences in the first place. But, it suggests why the postprison outcomes matter for blacks but not for whites.

A second related reason is that some firms, engaging in affirmative action, may seek to kill two birds with one stone by hiring workers who are both exoffenders and black. This makes sense if implicit subsidies are offered to firms hiring minorities or exoffenders. Enough is not known about the job market in Baltimore at the time to be any more than suggestive; but numerous training and job assistance programs for both blacks and exoffenders existed side by side in Baltimore during the 1970s. Indeed, in one CETA program providing job placement services for disadvantaged workers, it was found that exoffenders received higher wages and more job

offers (Phillips and Myers, 1978). If black exoffenders are perceived to be more productive than black nonoffenders who participate in the CETA program, then it makes further sense to hire them.

A third reason, derived by use of opposing logic, may be that black exoffenders are more likely to quit and thereby would supply a continuous flow of labor for the firm that hires them but which does not wish to invest in their specific human capital. The higher quit rates among blacks are consistent with the fewer hours they work, as compared to white exoffenders. And, the high turnover rates would be consistent with the secondary labor market jobs they hold. Note, too, that those blacks who had previously held white-collar jobs are less likely than other blacks to be employed and work fewer hours as one would expect if this preferential hiring practice applies only to jobs in the secondary labor-market.

Other reasons why postprison outcomes affect black postprison employment but not that of whites include: (1) employers need additional evidence of satisfactory performance among blacks, and this evidence must be recent; and (2) white criminals are very different from whites generally and background characteristics adequately mirror these differences; but black criminals are more like other blacks generally, so their backgrounds tell little about their likely performance.

Obviously, we have not exhausted all of the possible reasons. But, the finding is significant for policy because it suggests when labor-market intervention is likely to be effective in reducing exoffenders' unemployment and where such policies should be targeted. Our findings suggest that policies designed to improve the employment performance of exoffenders should best be implemented immediately upon release from prison. They also suggest that the people who are most likely to be responsive to employment intervention strategies are minorities and those with the lowest wages and highest unemployment--that is, the most disadvantaged of exoffenders. An example of this follows from our examination of the unemployment insurance experiment. In theory, we found that the work disincentive effect of the unemployment insurance benefit would be lowest when unemployment probabilities were high and wage rates were low. Empirically, this situation corresponds to the plight of the blacks in our Baltimore sample. We found that the insurance benefit did not reduce hours worked for blacks while it did for whites.

4. RECIDIVISM UNEXPLAINED BY EMPLOYMENT DISADVANTAGE

In both the Baltimore and federal samples, we found that preprison employment experience has little impact on postprison rearrest. Lack of a stable job or extensive work experience is indication among exoffenders of employment disadvantage. Numerous writers have argued that this disadvantage reduces the attractiveness of legitimate employment relative to criminal activity and thereby leads to intensified pursuit of crime. But our findings do not

strongly support this view. Instead, in the federal sample criminal-history variables are highly correlated with recidivism. Individuals with more-extensive criminal records tend to get rearrested more often. If we ignore criminal history, to be sure, previous employment experience does matter. The more disadvantaged one's preprison employment performance, the more likely it is that one returns to crime. But is it legitimate to ignore criminal history? The standard economic model of crime, moreover, suggests that the subjective measures on the certainty and severity of punishment must be entered into any acceptable specification of the crime supply function. Ignoring these factors artificially inflates the importance of previous employment experiences. Admittedly, previous employment and previous criminal activities are related. By similar logic, postprison employment and postprison criminal activity are related. We observe in the Baltimore sample just what relation there is--higher wages due to increased hours worked tend to reduce crime. Taking account of current employment opportunities, as is done with the Baltimore sample, reveals that the relative importance of criminal history thereby diminishes. Could it not be, for lack of available measures of postprison employment in the federal sample, that the importance of criminal history is exaggerated? Because there are minor differences in how criminal record and certainty and severity of punishment are measured in the two samples, the answer is unclear. But from the policy point of view, the conclusion is clear: A dismal employment record before prison does not doom an exoffender to failure in the labor market.

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Instead, the limited evidence implies, labor market intervention strategies can be effective in reducing crime among exoffenders even if their preprison records are seriously blotted.

5. ELIMINATING RACIAL EMPLOYMENT DISCRIMINATION WILL REDUCE BLACK-WHITE CRIME DIFFERENTIALS

We performed a number of conceptual experiments of eliminating racial discrimination in the criminal justice system and in the labor market. When performed using the federal data, the experiments yielded provocative results: Elimination of racism in the criminal justice system would not appreciably reduce black recidivism rates and therefore would have no narrowing effect on the black-white gap in crime. Conceptual elimination of racial discrimination in preprison employment, however, yielded qualitatively different results. Black employment rises when blacks and whites are treated equally in the labor market. The increase in employment, based on our coefficient estimates, lowers recidivism. Thus the black-white crime gap is narrowed. While it is certainly true that these results come from a model where the significance of preprison employment is low, the exercise is useful nonetheless in questioning the likelihood that anti-discrimination employment policies would be effective in reducing crime. Essentially, it could be argued, participation in crime depends on the relative attractiveness of work. If blacks are paid less than whites, work will be less attractive to them than to whites, assuming that the returns to crime are the same for both. Thus blacks engage in proportionately more crime. But eliminating

the racial gap in legitimate employment, so the argument goes, diminishes the relative price differential between black and white criminal activity. And, naturally, black crime falls. This argument is an extremely oversimplified example of the many ways in which labor markets and the criminal justice system may interact. The interaction in this case is by way of racial discrimination in the labor market. The interaction in Sellin's paradigm is by way of racism institutionalized in both the penal system and in the economy.

D. FINAL COMMENTS

We began our concluding comments by indicating that this research was not intended to supply policy-makers with new and better strategies for fighting crime or for reducing unemployment. The performance by policy-makers on both accounts has been dismal. Martinson (1974) has chronicled the failures in crime-reduction efforts; the dismal failure of manpower programs is well documented. In exploring how crime and employment interact, however, we have stumbled across promising means by which to both improve the postprison employment performance of exoffenders and to reduce their recidivism. Given the failure of previous attempts to deal simultaneously with crime and unemployment, the insights gained from our research findings can be useful in addressing future program attempts.

One of our strongest findings concerning recidivism was that higher wages, attained by way of increased hours worked, tend to

reduce postprison crime. Haveman and Christainsen (1978) categorize two forms of direct job-creation programs that could have the impact of raising wages or employment among a disadvantaged group. Public employment, including training programs, special public-service jobs and sheltered employment, is one form. Another is wage subsidies.

The Supported Work Experiment described by Piliavin and Gartner (1980) and other public-service employment schemes have a common flaw: They often do not train workers for comparable jobs in the private sector. Due to this flaw, whether the targeted group consists of exoffenders or other disadvantaged workers, the long-run employment-creation effects are often minimal. Moreover, earned income or wages in public-service jobs are not always comparable to potential earnings in private sector jobs. Thus the relative attractiveness of crime may still be high in spite of the lower unemployment experienced by enrolling in these employment expansion programs. The many findings of improved employment among exoffenders in public training and employment programs are not matched by findings of reduced recidivism. For example, Jenkins and his colleagues (1973) reported that while inmates released from Alabama's Draper Correctional Center who received manpower development training were more likely to be employed after release, they were no less likely to be rearrested than a control group. Our findings that preprison work experience, on-the-job training, education, and work release are unrelated to recidivism supports this view. Therefore, we would argue that training programs, public work jobs, and

sheltered or other public service employment may be ineffective in reducing recidivism.

Wage subsidies, on the other hand, represent a more promising strategy for reducing crime. Just as public-service employment expands job opportunities, so too do wage subsidy schemes. There are a variety of wage subsidies. There are wage-rate subsidies and wage-bill subsidies. Wage-rate subsidies are typically paid directly to the worker in the form of an earnings supplement. A payment may also be made to the employer, often for employing specific categories of workers or for hiring new workers (in which case it becomes a recruitment subsidy). The wage-bill subsidies can be in the form of tax credits or direct payments to the employer. California is now experimenting with a tax-credit wage-bill subsidy for the hiring of disadvantaged workers. This program, administered with CETA funds, includes a significant number of exoffenders. Although the program has yet to be evaluated, our results suggest that the scheme is likely to reduce recidivism, particularly if workers' wage incomes are raised.

The attractiveness of the wage subsidy, whether paid to the employer or the employee, is that it rewards employment. The difficulty with unemployment insurance is that it implicitly rewards unemployment and can inadvertently raise recidivism rather than lower it. In the unemployment insurances' favor is its ease of administration. The formal mechanisms exist for distributing

benefits to exoffenders just as they are distributed to other individuals out of work. By contrast the wage-bill subsidy, particularly when applied to specific disadvantaged groups, is difficult to design and even more difficult to administer. If there are fewer problems with wage-rate subsidies paid directly to workers, these may not be offset by the possibly higher unemployment if market wages are inflexible. This does not argue for giving the subsidy to the employer, however. There can be peculiar effects in dealing with groups with specific disabilities, such as those who have criminal records. Under the worker-paid subsidy, exoffenders could conceivably conceal their criminal records. Under the employer-paid subsidy their employment would depend upon revealing their past, which might entail transmitting more information than is optimal for maximizing expected wages. If the number of subsidized job openings is smaller than the number of eligible workers, the criminal record could serve as a negative signal, potentially lowering the exoffenders' chances of being hired, thus lowering expected wages. And, it is higher wages that can be expected to reduce recidivism.

The problems with the implementation of wage subsidies to reduce recidivism would also be found in attempts to reduce exoffender unemployment. But the prospect of using public-service employment is more promising in this regard. Our findings suggest that the most important variables in determining postprison employment success--and this is particularly true for black exoffenders--are whether a job

was arranged and whether the exoffender worked upon release from prison. Here is where recruitment subsidies and public-service jobs can play a crucial role. At the very time when exoffenders need a job the most, when they are released from prison, many find it the hardest to readjust to the "real" world. This, of course, was the logic of providing the unemployment insurance. But this logic does not extend to all exoffenders. Unfortunately, in theory at least, those who are least likely to be responsive to the cash cushion that unemployment insurance can provide, are also less likely to be dramatically affected by such postprison aids as public-service employment. The task for future research, understandably, is to examine the costs and benefits of providing direct employment aids as opposed to unemployment insurance or other cash benefits.

The most compelling task for future analysis is the assessment of racial differences in the effectiveness of employment strategies to either reduce recidivism or to reduce exoffender unemployment. Greater and deeper cognizance of the historical roots of racism in both the criminal justice system and in labor markets will need to be acquired to develop strategies that work. That task is beyond the scope of a report like this that asks what, beyond econometric exercises that ask how; that task must reach for the seemingly unreachable and ask why.

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