

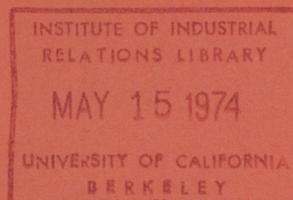
THE EFFECT OF LEGITIMATE OPPORTUNITIES ON THE PROBABILITY
OF PAROLEE RECIDIVISM

by

Philip Cook

University of California
Institute of Industrial Relations (Berkeley)
2521 Channing Way
Berkeley, California, 94720
Lloyd Ulman, Director

This report was prepared for the Manpower Administration, U. S. Department of Labor, under Research and Development Contract No. 81-06-72-01. Since contractors conducting research and development projects under government sponsorship are encouraged to express their own judgment freely, this report does not necessarily represent the official opinion or policy of the Department of Labor. The contractor is solely responsible for the contents of this paper.



Berkeley 1973?

BIBLIOGRAPHIC DATA SHEET		1. Report No. DLMA 81-06-72-01(2)	2.	3. Recipient's Accession No.
4. Title and Subtitle The Effect of Legitimate Opportunities on the Probability of Parolee Recidivism			5. Report Date June 9, 1971	6.
7. Author(s) Philip Cook			8. Performing Organization Rept. No.	
9. Performing Organization Name and Address The Regents of the University of California Berkeley, California, 94720			10. Project/Task/Work Unit No.	
			11. Contract/Grant No. DL 81-06-72-01	
12. Sponsoring Organization Name and Address U.S. Department of Labor Manpower Administration Office of Research and Development 1111 20th St., N.W., Washington, D. C. 20210			13. Type of Report & Period Covered Final	
			14.	
15. Supplementary Notes				
16. Abstracts In this paper the author analyzes the effect of improving the quality of job opportunities available to parolees on their "recidivism rate" (the percentage which returns to crime after release from prison). Original empirical results, based on a sample of 325 parolees released from Massachusetts penitentiaries in 1959, suggest that parolees who find satisfactory jobs are substantially less likely to recidivate than those who are able to find no job attractive enough to hold them more than a few weeks. It is suggested that a public employment program would be an appropriate framework within which to improve the quality of jobs available to parolees and thereby lower the recidivism rate. A complete summary of the results is included at the beginning of the paper.				
17. Key Words and Document Analysis. 17a. Descriptors				
17. Economic analysis Employment Government policies Government employees Job satisfaction Labor Literature reviews Males Rehabilitation Statistical analysis		Statistical samples Unemployment Unskilled workers		17a. Crime Recidivism Parolee
17b. Identifiers/Open-Ended Terms Public Service Employment				
17c. COSATI Field/Group Cook -- 5K				
18. Availability Statement Distribution is unlimited. Available from National Technical Information Service, Springfield, Va. 22151.			19. Security Class (This Report) UNCLASSIFIED	21. No. of Pages 106
			20. Security Class (This Page) UNCLASSIFIED	22. Price

INTRODUCTION

R. A. Gordon, Professor of Economics
University of California, Berkeley

"The Effect of Legitimate Opportunities on the Probability of Parolee Recidivism," by Philip Cook is one of the reports submitted to the Manpower Administration of the U. S. Department of Labor by members of a research group at Berkeley concerned with the design and impact of public service employment programs. We use the term "public service employment program" to refer to any policy designed to combat urban poverty through use of federal subsidies to increase employment of disadvantaged workers.

In this paper the author analyzes the effect of improving the quality of job opportunities available to parolees on their "recidivism rate" (the percentage which returns to crime after release from prison). Original empirical results, based on a sample of 325 parolees released from Massachusetts penitentiaries in 1959, suggest that parolees who find satisfactory jobs are substantially less likely to recidivate than those who are able to find no job attractive enough to hold them more than a few weeks. It is suggested that a public employment program would be an appropriate framework within which to improve the quality of jobs available to parolees and thereby lower the recidivism rate. A complete summary of the results is included at the beginning of the paper.

Readers interested in other aspects of public employment programs may wish to consult some or all of the other project reports. These include:

"The Inflationary Effects of Public Service
Employment,"
by Philip Cook and Robert Frank

"Public Service Employment and the Supply of
Labor to the Private Sector,"
by Robert Frank

"A Proposal to Improve the Design of the Public
Employment Program,"
by Laurence Seidman

"The Public Employment Program in San Francisco,"
by Michael Wiseman

"An Expanded Public Service Employment Program:
Some Demand and Supply Considerations,"
by Frank Levy and Michael Wiseman

Individual copies may be obtained for the cost of
reproduction from the Institute of Industrial Relations,
University of California, Berkeley, 94720.

TABLE OF CONTENTS

PAGE

Table of Contents

List of Tables

Acknowledgements

Abstract

<u>Section 1</u>	INTRODUCTION	1
A.	The Failure of Correctional Theory and Practice	2
B.	The Proposition	4
C.	Literature Review	5
D.	Organization	6
<u>Section 2</u>	REVIEW OF THE LITERATURE	8
A.	Theory	8
1.	Typology of Deterrence Effects of Legitimate Opportunity	9
a.	Opportunity costs	9
b.	Valuation of punishment	9
c.	Payoff to illicit activity	12
d.	Personality factors	13
2.	The Theoretical Literature	13
B.	The Empirical Literature	16
1.	Income Variables	17
2.	Unemployment	20
3.	Results	20
<u>Section 3</u>	CORRELATES OF RECIDIVISM	22
A.	Introduction	22
B.	Data	23
C.	Parole Procedures in Effect in Massachusetts in 1959	23
D.	Definition of Recidivism	26
E.	Theoretical Effect of Parolee Characteristics on the Probability of Recidivism	28

	<u>PAGE</u>
F. Empirical Results: Zero Order Correlations	31
1. Measures of Criminal Record	34
2. Time Served	34
3. Age at Release	35
4. Socioeconomic Characteristics	35
a. Married status at release	35
b. I. Q.	36
c. Prior occupation	36
d. Education	36
e. Military service	37
f. Race	37
G. Multivariate Analysis	39
H. Interpretation of the Results	40
<u>Section 4</u> PAROLEES IN THE LABOR MARKET	44
A. Introduction	44
B. Socioeconomic Characteristics of Prisoners	45
C. Labor Market Discrimination Against Parolees	46
1. Evidence for Discrimination	47
2. Evidence Against Discrimination	49
D. Job-Holding Behavior of Parolees	51
1. The Evidence: Unemployment	52
2. The Evidence: Turnover	52
3. The Evidence: Job Tenure	56
<u>Section 5</u> THE RELATIONSHIP BETWEEN PAROLEE JOB- HOLDING AND RECIDIVISM	65
A. Discussion	65
B. Job Satisfaction	68
C. Results	69
D. Causality	79
<u>Section 6</u> CREATING JOB SATISFACTION	81
A. The Manpower Training Approach	81
B. The Job Creation Approach	84
<u>APPENDIX A</u> A COMPARISON OF THREE CHOICE THEORETIC MODELS OF CRIMINAL BEHAVIOR	86

	<u>PAGE</u>
<u>APPENDIX B</u> PROBIT REGRESSION ANALYSIS	88
FOOTNOTES	91
BIBLIOGRAPHY	93

LIST OF TABLES

	<u>PAGE</u>
<u>TABLE 1</u> Dispositions of Original Felony Arrests in Three California Counties, 1969 (Percentage Distribution)	11
<u>TABLE 2</u> A Comparison of Empirical Studies of the Economics of Crime	18
<u>TABLE 3</u> Timing of Parole Revocations: Massachu- setts Sample	28
<u>TABLE 4</u> Crude Parole Rates by Prior Characteristics	32
<u>TABLE 5</u> Coefficient Estimates from Probit Regres- sions: Predicting Parole Success on the Basis of Prior Characteristics	41
<u>TABLE 6</u> Distribution of Parolees by Number of Jobs Held	54
<u>TABLE 7</u> Reason for Job Termination (Post-Release Panel Study)	55
<u>TABLE 8</u> Distribution by Job Tenures of Jobs Obtained by Parolees in the First 3 Parole Periods	59
<u>TABLE 9</u> Job Tenure Regressions (Ordinary Least- Squares)	61
<u>TABLE 10</u> The Effect of Job-Holding on the Likelihood of Recidivism: One-Way Layout	71
<u>TABLE 11</u> The Effect of Job Turnover on the Likelihood of Recidivism	73
<u>TABLE 12</u> Coefficient Estimates from Probit Recidivism Regressions Including Job Satisfaction Variables	74
<u>TABLE 13</u> Coefficient Estimates from Simplified Probit Regressions which Include Job Satisfaction Variables	77
<u>TABLE 14</u> The Effect of Job Satisfaction on the Probabil- ity of Parole Success	78

ACKNOWLEDGEMENTS

I owe a major debt to Professor Robert Evans, Jr. for supplying the raw data on which parts of this study are based. Professor Evans collected the data in cooperation with the Commonwealth of Massachusetts' Department of Corrections and Parole Board; his research was supported largely by a grant from the Penrose Fund of the American Philosophical Society and in part by the Industrial Relations Section of the Massachusetts Institute of Technology.

I thank Professor Daniel McFadden, my thesis advisor, who supported each of my several attempts to find a topic. The final draft owes much to his careful critique of a previous edition. Furthermore, I am indebted to Professor McFadden for the consistent support and encouragement he has given me throughout my career as a graduate student.

I also thank Professor Michael Wiseman for his encouragement, suggestions and amazing promptness in reading drafts of this thesis. Professors C. Bartlett McGuire and Frank Levy were also very helpful. As with any major research effort, this thesis owes much to discussions and correspondence with many others, too numerous to name here.

I thank Donald Sant for his excellent programming assistance and consultations concerning the statistical work in the thesis. The typing, which speaks for itself, is the work of Iao Katagiri.

This research was supported by a grant from the U. S. Manpower Administration.

ABSTRACT

A large percentage of felonies are committed by "recidivists" --- men who have served prison terms and returned to crime after their release. This fact suggests that if effective rehabilitation programs could be implemented for prisoners and parolees, a substantial drop in the crime rate would follow. The lack of effectiveness observed for current rehabilitation programs appears to be the result of poor technique rather than lack of funds. This thesis investigates the correlates of recidivism in an attempt to discover an approach to rehabilitation that would be effective in reforming ex-prisoners.

A review of the fast-growing economics literature dealing with criminogenic processes suggests that the rational potential criminal should be deterred by an improvement in his legitimate opportunities. Since a crucial element of an adult male's legitimate opportunity is the quality of jobs which are available to him, it is plausible that an improvement in parolees' job opportunities would lower their recidivism rate.

An original empirical study of the labor market behavior of a sample of 325 parolees released from Massachusetts penitentiaries in 1959 shows that parolees were able to find jobs; their labor market problems were not so much in the quantity but rather the quality of available work. Further empirical work shows that those parolees in the sample who were able to find satisfactory jobs were less likely to recidivate than those who failed to find such jobs.

This empirical work helps confirm the hypothesis that an improvement in the job opportunities available to parolees would reduce the recidivism rate, and thereby help to reduce the overall crime rate.

SECTION 1

INTRODUCTION

There is little question that if the public were willing to allocate more resources to crime prevention, the growth of the common crime rate could be slowed. The money could be used to finance intensive patrolling of high crime areas or to reduce police response time or to reduce the delays in judicial processing of criminal suspects --- all actions that most experts believe would have some positive impact on the amount of common crime. But there is one area of great importance in the criminal justice system in which additional resources would apparently have very little real effect, given the current state of the art --- the correctional system. The vast research effort in this area has failed to find any effective instruments for rehabilitating the 2.5 million people who are processed by the criminal justice system every year in the U. S. The correctional system does not correct, and no one knows how to modify the correctional process so that it will correct. Expenditures on prisons can, of course, be justified by pointing to the presumed general deterrence effect of the threat of punishment, and the certainty that the society at large is protected from criminals who are actually in prison. But the technology for reducing the proneness of criminal offenders is simply not available at present.

In spite of the failure of past research to devise effective correctional instruments, it is certainly too soon to despair. All possible avenues to rehabilitation have obviously not been explored as yet and the incentive for finding some solutions increases every year with the increase in reported crime. This incentive is the result of the well-known fact that ex-convicts are a high risk group who account for a substantial portion of serious crime. For example, in California 14.9% of the almost 60,000 felony defendants in 1970 had a prior prison record and an addi-

tional 30.6% had prior records of "major" crime. (Less than a quarter had no criminal record.) If the recidivism rate of ex-convicts could be reduced substantially, a substantial reduction in the crime rate would follow. Here, then, lies the incentive for continuing the effort to devise programs which would actually reform ex-convicts.

The Failure of Correctional Theory and Practice

The history of correctional theory and practice has been dominated by the view that (1) the criminal offender is suffering from a personality defect which must be corrected before he will cease illegitimate activities, and (2) there exist feasible programs of punishment and/or "treatment" which will serve to correct this personality defect in some significant percentage of all cases. A great many such programs have been tried in the last century, with a gradual evolution from a reliance on punishment (penitentiaries were originally designed to make inmates repent their evil ways by making them suffer) to a reliance on "treatment" (be it psychotherapy, group counseling, or whatever).

The one characteristic that all these attempts seem to have in common is their complete lack of effectiveness --- the percentage of released offenders who return to crime (recidivate) is independent of the nature of the correctional process they are made to experience. This claim has received increasing acceptance in the criminology literature: Leslie Wilkins, for example, asserts on the basis of his review of the literature that

It is difficult to find any reasonable grounds for disagreement with the conclusion that the major achievement of research in the field of social pathology and treatment has been negative and has resulted in the undermining of all the current mythology regarding the effectiveness of treatment in any form.
[p. 78]

James Robinson and Gerald Smith, in their recent survey of experimental studies conducted by California correctional author-

ities, draw the following conclusions:

- (1) Institutionalization and community treatment appear equally ineffective.
- (2) Chances for rehabilitation may actually decrease as the length of a prison term increases.
- (3) Group counseling in prison has no effect on the recidivism rate.
- (4) The size of a parole officer's caseload has no effect on the success of his clients.
- (5) Offenders who are released to parole supervision apparently do no better than those who are released unconditionally.

It is instructive to look more closely at one apparent exception which helps prove the rule. A large-scale experiment, the California Community Treatment Project, was begun in 1961 to test the hypothesis that intensive supervision of juvenile offenders in the community is a more effective treatment than the usual institutionalization and parole program. M. Q. Warren reported on the basis of a 15-month follow-up study that the experimental group had only a 28% failure rate, compared with the 52% rate found for the control group. This result has been widely cited as offering real hope for the corrections industry [see, for example, Levin, Taggart, Morris and Hawkins]. But a closer look at Warren's statistics has revealed that they convey a false impression --- "failure" was defined, not as a return to criminal activity, but as a return to a correctional institution. The experimental group, when compared with the control group, actually committed more minor crimes and about the same number of serious crimes. The difference in the "failure" rate was due entirely to the fact that parole officers in charge of clients in the experimental group were much more lenient in their proneness to recommend parole revocation in response to their clients' criminal activities [see Lerman, Robinson and Smith].

There is no question that personality differences have much to do with interpersonal differences in the proneness to engage in illegitimate activities. What the complete failure of correc-

tional institutions reflects is our basic ignorance of techniques that would be effective in creating long-term personality change in the desired direction. It is time to concentrate on a new approach to the problem of how to reduce the recidivism rate. The alternative to changing the personality of a criminal offender is to take his personality as largely given and attempt to change his environment. The remainder of this paper is devoted to exploring the potential effectiveness of this approach.

The Proposition

The relevant characteristics of an offender's environment at the time of his release from prison consist of a set of opportunities for legitimate and illegitimate activities. Perhaps the most important aspect of the legitimate opportunities facing an ex-convict (particularly an adult man) is the chance to make an adequate income. This observation is made plausible by the fact that approximately 90% of serious crime in the U. S. is motivated at least in part by the desire for pecuniary enrichment; furthermore, these "economic" crimes are committed largely by men who face relatively poor prospects for obtaining a decent job. At least some of these men would presumably be deterred from further criminal activity by an improvement in their chance to obtain self respect and a decent income through legitimate work.

The following Proposition is the principle thesis of this essay:

The recidivism rate of adult male parolees could be substantially reduced by a program which effectively improved their opportunity to obtain and hold decent jobs.

This Proposition could be best tested by a controlled scientific experiment. The fact that the data used here were generated by "nature" and not by experiment perhaps precludes a really convincing statistical demonstration of the validity of the Proposition. But in the absence of experimental findings, the careful analysis

of "natural" data is better than a purely speculative approach.

Literature Review

The technical studies which have attempted to test the Proposition can be reviewed very quickly. Daniel Glaser's impressive monograph entitled *The Effectiveness of a Prison and Parole System* (Bobbs-Merrill, 1964) included a Postrelease Panel Study of federal parolees in 1959-60; this study gathered enough information through monthly interviews to allow a comparison on the basis of job-holding activity of those who completed parole and those who were returned to prison. Glaser finds that eventual successes, when compared with eventual failures, acquired their first jobs sooner and earned a higher average monthly income during the first three months on parole. Eventual successes also had a slightly better chance than eventual failures of obtaining a skilled job during the first three months on parole. Glaser's comparisons are based on a rather small sample: 108 successes and 27 failure cases. He makes the comparisons without controlling for any personal characteristics of the parolees, and the distinction between a "success" and a "failure" is never made precise in the text. In spite of these problems, Glaser's work was an interesting start in investigating the relationship between labor market success and recidivism.

Robert Evans, Jr. attempted to replicate Glaser's study on the basis of a sample of 327 men paroled from Massachusetts felony-level institutions in 1959. Evans found that eventual parole successes had a better employment experience than eventual failures. Unfortunately, Evans's study suffers from rather severe methodological problems, including (1) his distinction between failure and success reflects the terms of the parole and the behavior of the parolee (this problem is discussed in Chapter 3 below); and (2) the fact that he, like Glaser, presents his comparisons without controlling for personal characteristics of parolees. Evans's measures have the additional problem of not controlling for the

amount of time on parole --- measures such as "tenure on longest job" or "highest wage obtained" are obviously going to be influenced by the number of months the parolee was free to participate in the labor market, and parole failures could naturally be observed for fewer months on the average than parole successes. These problems make any inferences about causation very difficult.

There are apparently no other published studies of this type. But in spite of the fact that the causal link between employment experience on parole and eventual success or failure has not been adequately demonstrated, the idea is widely accepted. President Nixon's Task Force on Prisoner Rehabilitation stated the case very strongly:

A constructive member of the community, by definition, is a working member. A common characteristic of offenders is a poor work record; indeed it is fair to conjecture that a considerable number of them took to crime in the first place for lack of the ability or the opportunity --- or both --- to earn a legal living. Therefore, satisfying work experiences for institutionalized offenders, including vocational training when needed, and the assurance of decent jobs for released offenders, should be at the heart of the correctional process.
[pp. 9-10]

The popularity of this idea is also reflected in the amendments to the Manpower Development and Training Act which have allowed the recent development of a considerable number of experimental projects which provide vocational training, remedial education, and other manpower services to inmates and parolees. Evaluations of these and other recent experimental studies in the same area are reviewed in Chapter 6.

Organization

Chapter 2 selectively reviews the growing body of theoretical and empirical literature on the economics of crime; the focus of the review is on the deterrence effect of changes in legitimate opportunity. These studies generally support the view that the

criminal propensity of an individual with given tastes and criminal opportunities will decline if his legitimate opportunities improve.

Chapter 3 reviews some of the vast body of literature on the relationship between the observable characteristics of released prisoners and the probability that they will return to crime. Some original statistical results for a sample of Massachusetts parolees is also included. The main conclusion of the chapter is that recidivism rates are inversely related to the quality of legitimate opportunities (as inferred from socioeconomic characteristics of parolees), but that this relationship is weak.

Chapter 4 discusses the job-holding behavior of parolees, drawing heavily on work by Pownall and Glaser [1964] as well as presenting some original results. Two common beliefs are questioned: (1) that a criminal record is a serious handicap to finding a job; and (2) that parolees have difficulty in finding jobs. Differences among parolees with respect to job turnover and job tenure are analyzed.

Chapter 5 presents a theoretical and empirical discussion of the Proposition stated above.

Chapter 6 reviews the experimental attempts to improve job opportunities for parolees.

SECTION 2

REVIEW OF THE LITERATURE

Renewed interest in the economics of crime has produced a professional literature that covers a wide range of topics in criminology. One principle focus of this literature has been the relationship between the proneness of an individual or a population to commit crimes and the reward structure to legitimate and illicit activities. The discussion here will emphasize the theoretical and empirical results on the deterrence effect of improved legitimate opportunity. In the last section of the chapter, the theoretical discussion is extended to an analysis of parolee recidivism.

Theory

Economists have been unanimous in modelling participation in illegitimate activities as the outcome of a rational choice process; in this assumption they concur with the philosophical underpinnings of the law (people are legally accountable for their actions because it is presumed that they could have chosen to act differently). The notion of rationality has been given substance by making such further assumptions as: (1) potential criminals seek to economize on the risk of being punished and the severity of the possible punishments; (2) potential criminals act on the basis of beliefs about their legitimate opportunities and the possible outcomes of illegitimate activities, and that those beliefs are not unrelated to the "truth"; (3) potential criminals are not totally incapable of calculating their best interests, given their preferences and beliefs about their legitimate and illegitimate opportunities; and (4) potential criminals prefer more wealth to less, and less work to more.

These assumptions are debatable, and certainly do not apply

in every case. But the predictions which follow from them may nevertheless be valid in the sense that they correctly identify the systematic portion of a population's response to a change in the social environment (penalty structure, labor market, etc.). Of course, even if this rationalistic perspective provides a valid approach to studying crime, there remains the critical problem of accurately characterizing the salient features of the social environment within the context of a deterministic model. This task has barely begun, as will become clear in the course of this review.

There are several plausible arguments for the proposition that rational individuals with fixed tastes will be deterred from crime if their legitimate opportunities improve, though the effect of improved legitimate opportunity is by no means unambiguous. Not all of these possibilities have been incorporated in the theoretical literature on individual choice behavior with respect to criminal activity. I begin by presenting a typology of possible effects, and then go on to discuss the available theoretical models.

Typology of Deterrence Effects of Legitimate Opportunity

1. Opportunity costs. To the extent that criminal activity is time consuming, an increase in the legitimate wage available to the potential offender will increase the opportunity cost of engaging in criminal activity. The resulting substitution effect should tend to reduce the offender's supply of effort in illicit "work" activities.

2. Valuation of punishment. Society threatens offenders with a variety of possible punishments. The deterrent effect of these threats depends on the valuation the individual places on each possible outcome together with his subjective probability distribution over possible punishments.

a. The severity of indirect and direct punishments: it is useful in the discussion here to distinguish between direct and

indirect punishments. Direct punishments which may follow arrest include pretrial detention, bail fees, legal fees, time spent in trial, jail, prison, probation, and parole terms, fines, loss of voting privileges, etc. Indirect punishments which may follow arrest are the result of the social stigma of having a criminal record, and include loss of social status, loss of job, and difficulty in finding a job. It is argued below that arrestees of relatively high socioeconomic status are likely to suffer relatively severe indirect punishment and relatively lenient direct punishments.

Largely descriptive evidence [see Chapter 4] indicates that there is greater discrimination against men with a criminal record in the labor market for relatively good jobs than in the so-called "secondary" labor market. A potential offender who is already limited to the secondary market by other socioeconomic characteristics will suffer a negligible deterioration in his job opportunities due to an arrest and possible conviction; a white collar worker, on the other hand, is more likely to lose his job if convicted of a major offense (or even just arrested for one) and perhaps will be unable to find another primary sector job. If this empirical generalization is correct, then the secondary worker is much less likely to receive a significant punishment if arrested than is a primary worker; almost everyone who is arrested acquires a formal arrest record, a majority receive a conviction record, but only a small proportion are actually punished with a prison term.

Table 1 presents a tabulation of felony cases disposed of in 1969 in three California counties. Of these cases, 63-64% were convicted and given either felony or misdemeanor sentences. But only 5.8-10.6% were actually given prison sentences (one-third to one-half of the arrestees were committed to some institution or fined). It is plausible to assert, then, that for a worker in the primary job market the most likely "punishment" is a loss of job status; to the extent that this loss is not incurred by secondary workers who are arrested, the primary worker's expected indirect

TABLE 1

Dispositions of Original Felony Arrests in
Three California Counties, 1969 (Percentage Distribution)

<u>DISPOSITION</u>	<u>SACRAMENTO</u>	<u>SAN JOAQUIN</u>	<u>STANISLAUS</u>
Total arrests	3402	1813	1059
Percent distribution	100.0	100.0	100.0
Sentences imposed	63.2	63.4	64.0
Felony sentences ¹	31.1	25.2	37.9
Misdemeanor sentences ²	32.1	38.2	26.1
Type of commitment			
Prison	8.9	5.8	10.6
Youth Authority	2.7	2.0	2.5
Civil commitment ³	1.1	2.8	4.1
Probation and jail	18.3	10.8	24.4
Jail or fine	11.1	13.0	10.9
Probation	21.1	29.0	11.5

SOURCE: Taken from Table A-16 of "Offender-Based Criminal Statistics: Dispositions of Felony Arrests in Selected California Counties," Special Report No. 3 (March 1971), Project SEARCH Staff, California Crime Technological Research Foundation, Sacramento.

^{1,2} Under California law any person convicted of a felony offense who receives only a jail sentence or a fine is automatically classified as having a misdemeanor sentence. In offenses with alternative jail sentences as a possible punishment a defendant placed on probation may be designated by the court as receiving a misdemeanor sentence.

³ In California, defendants convicted of a criminal offense may be committed civilly to the California Rehabilitation Center for treatment because of drug involvement or to the Department of Mental Hygiene for treatment as a mentally-disordered sex offender.

punishment is relatively severe. Hence, the threat of being arrested should act as a greater deterrent to potential offenders who face relatively good job opportunities.

While they face relatively severe *indirect* punishments, wealthier arrestees are better able to post bail, hire a lawyer, and in other ways reduce their expected *direct* punishment. Banfield and Anderson [1968] present strong evidence that defendants in Cook County Criminal courts who are able to hire a lawyer (rather than a public defender) are able to postpone their trials longer and thereby considerably improve their chances of a favorable outcome.

b. Valuation of a jail or prison sentence. The threat of a prison sentence of given length would plausibly have a deterrent effect which increased with the quality of the potential offender's legitimate opportunities. This follows immediately if the prison term is valued at the loss in legitimate consumption opportunities (money income) available in prison are compared with freedom. This point can be illustrate effectively with an example: suppose two men are contemplating stealing \$1,000. Each believes that there is .1 probability that he will be arrested and imprisoned for one year, and otherwise will go free. If one has a legitimate income of \$25,000 and the other \$5,000 per year, the poorer man will be more tempted by the theft; for him, the expected value of the theft is \$400, compared with a -\$1,600 expected value for the wealthier man.¹

Obviously, many other factors besides lost income influence the valuation a potential offender places on a possible prison term (including, one suspects, at least one other observable characteristic: whether or not he is married). As long as these factors are not systematically related to legitimate income, though, it should still be true that the deterrence effect of a threatened prison term increases with wealth.

3. Payoff to illicit activity. High status jobs often carry with them the opportunity to engage in lucrative criminal activities ("white collar crime") which are not available to a man with

poor legitimate opportunities. It is possible in the extreme case that an improvement in legitimate work opportunities will not deter an offender but rather allow him to substitute lucrative and relatively safe criminal activities (e.g., embezzlement, income tax evasion, accepting bribes, false advertising, price fixing) for ordinary theft [see Gordon, 1971].

4. Personality factors. To the extent that risk aversion, "honesty preference," and the individual's time horizon vary systematically with income, participation in crime will *ceteris paribus* also change.

The typology presented above is not exhaustive, but does demonstrate that the deterrent effect of improved legitimate opportunities is not unambiguously positive, at least in the context of the current criminal justice system. This ambiguity is also present in the results available from the theoretical literature on the deterrent effect of legitimate opportunities, even though this literature takes a much narrower viewpoint than that presented above. I now turn to a discussion of several of the choice theoretic models that have attempted to develop a "rigorous" framework in which to discuss criminal deterrence.

The Theoretical Literature

G. Becker [1968] published the first modern choice theoretic model of criminal behavior. His model does not include any variable representing wealth or a legitimate work option, and is discussed here only because it stimulated the more complete models which followed. Becker viewed crime as a series of Bernoulli trials with a fixed payoff given by the monetary or psychic income from an offense; the offender is punished with probability p after an offense, with the punishment measurable as the equivalent of a fixed loss in income (fine). The individual commits a crime if the expected utility of this crime is positive.

M. Block [1972a, Chapter 6] generalizes this model by having

the individual choose an integral number of offenses rather than making sequential decisions, and by including the individual's wealth in his decision rule. Block proves that under this formulation a risk averse individual facing a positive expected payoff to crime will *increase* his supply of offenses in response to autonomous increases in his wealth if his preferences are characterized by decreasing absolute risk aversion (not an unlikely personality characteristic). This somewhat perverse result comes about because of the characterization of punishment as a fixed fine that is incurred with a fixed probability.

A somewhat more interesting characterization of punishment is presented in R. Lind and M. Block [1972] and Block [1972a, Chapter 7]. Their model is the following: suppose an individual has a utility indicator which depends on his wealth W and the length of a prison term S . Then:

$$(1) \quad U = U(W,S), \text{ where } U_W > 0, U_S < 0;$$

U_{WW} and U_{SS} are negative, and U_{WS} is nonpositive. He can commit a crime with payoff G and direct cost C . If he chooses to commit a crime, he is apprehended with probability p , in which case he keeps only a fraction z of the payoff and incurs a prison sentence S .

He commits the crime if:

$$(2) \quad (1 - p)U(W - C + G, 0) + pU(W - C + zG, S) - U(W, 0) > 0.$$

Any change in the individual's situation which decreases the left-hand side of (2) is said to have a "deterrent" effect. Lind and Block show that an increase in W will have a deterrent effect, with a possible exception in the case $zG - C < 0$. They further demonstrate the important proposition that if the direct cost of crime C is positively related to legal wealth W , the deterrent effect of a wealth increase will be greater than the pure wealth effect. In other words, if committing a crime incurs an opportunity cost of legal earnings foregone, then an increase in the wage rate will deter the potential criminal both through a pure wealth effect and a substitution effect. (Note that these results hold subject to the restrictions Lind and Block place on the utility function.)

This last result suggests that a model which explicitly considered the problem of allocating time between illicit, licit, and leisure activities would be fruitful. I. Ehrlich [1970] and M. Block [1972a, Chapter 2] have both developed such models, but they generally are unable to derive definite conclusions.

Ehrlich considers the problem of the expected utility maximizing division of a fixed amount of time between licit and illicit activities under the following assumptions: (1) licit and illicit activities are perfect substitutes; (2) the payoffs to crime and to work are positively related to the time spent in each activity, with no cross effects; (3) offenders are apprehended and punished at the end of the period with probability p , which in general depends positively on the amount of time spent in illicit activity; (4) the punishment is a fine which increases with the amount of time spent in illicit activity, but is independent of the individual's wealth. Under these assumptions, the following comparative statics result can be derived: an increase in the legitimate wage will reduce the individual's equilibrium supply of illicit activity if initially he is engaged in both licit and illicit activities *and* his preferences are characterized by constant absolute risk aversion. This last assumption is not a necessary condition, but some such restriction on preferences is necessary to obtain a deterministic result. Thus, even though Ehrlich's model is highly restrictive in its basic assumptions, the implication it yields concerning the deterrent effect of legitimate opportunity is not unambiguous. The more general formulation in Block is also unable to present determinant comparative statics results on this question.

A comparison of the assumptions made by these models² with the typology presented earlier indicates that the models have been limited in scope. Some of the models have included the opportunity cost aspect of participation in illicit activities, and all of the models have considered personality factors, particularly attitudes towards risk. Only Lind and Block, however, begin to consider the

effect of improvements in legitimate opportunity on the individual's valuation of a prison sentence, and none of the models consider interactions between the quality of legitimate and illegitimate opportunity, or even the effect of a criminal record on employability. I would judge these latter considerations to be more important than those embodied in the available theory, especially when considering the distribution of crime across the socioeconomic structure and evaluating policies that would change the socioeconomic status of potential offenders.

The Empirical Literature

Most of the empirical work in the economics of crime literature has consisted of attempts to test an economic model of crime causation (such as those reviewed above) using published statistics on population aggregates. The standard procedure is to attempt to "explain" in a multivariate statistical framework either the intertemporal variations in a particular group's crime rate or differences in the crime rates of several population groups for a single period. (To the best of my knowledge data on individuals have not been used in the economics literature except by Evans [1968].)

The basic problem with these statistical analyses is that the measures of crime rates they employ are known to be extremely unreliable. Sutherland and Cressey [1970] assert that "the statistics about crime and delinquency are probably the most unreliable and most difficult of all social statistics." [p. 25] To the extent that errors in measurement are systematically related to the independent variables, coefficient estimates are biased. Statistical results which seem to support the author's theory may in fact merely reflect this systematic error in the author's crime statistics.

The discussion below is limited to papers by Fleisher [1963, 1966a, 1966b, 1970], Ehrlich [1970], and Phillips, Votey and Maxwell. These works are representative of a large and rapidly growing literature [see e.g., Block [1972b], Chapman, Church, Cobb,

Grieson, Orsagh, and Reynolds]. Table 2 allows an easy comparison of the studies by Fleisher, Ehrlich, and Phillips *et al.* Each study includes several regressions, with some measure of crime as the dependent variable. The independent variables in all cases include measures of legitimate income and/or unemployment that are supposed to reflect legitimate opportunity. The percentage of non-whites in the population is also included in most of these studies as a further indicator of legitimate opportunity.

Because all the authors use aggregate data, the relationship between their regression specifications and the model of individual choice which they are attempting to test is obscured by the aggregation problem. Interpretation of the income and unemployment variables used by the authors is discussed below.

Income Variables

Ehrlich includes two income variables in his cross-section study: the median family income of the state W and the percent of families X with income less than $(1/2)W$. It can be argued that these variables are consistent with the objective of testing his model of individual choice. For example, suppose tastes and illegitimate opportunities are distributed nonsystematically across income classes, and we believe with Ehrlich that the average participation in criminal activity then increases with the absolute gap between the licit and illicit wage rates. If W is a proxy for the average illicit wage and the family's income is a proxy for its legitimate wage, then total crime should increase with the degree of absolute dispersion in the lower end of the income distribution. Ehrlich's specification is then justified by the observation that the degree of absolute dispersion increases with both W and X . [See Ehrlich, p. 46, for a similar argument.]

Fleisher [1966a and 1966b] includes two income variables in many of his regressions: the means of the second and fourth quartiles of the distribution of family income (denoted MEINC2 and MEINC4). MEINC4 is a measure of the payoff to theft and hence plays the same role as Ehrlich's X . Fleisher says of MEINC2: "This

TABLE 2

A Comparison of Empirical Studies of the Economics of Crime

REFERENCE	DATA/DEPENDENT VARIABLE	OPPORTUNITY VARIABLES	OTHER VARIABLES	EMPIRICAL RESULTS
Fleisher [1963]	Juvenile arrest rates for Boston, Cincinnati, and Chicago, 1936-1956.	Unemployment (age specific estimates for U. S. males).	1. Personnel in U. S. armed services. 2. Trend variable.	Significant positive coefficient on unemployment.
Fleisher [1966b] I	Court appearance rates of males, ages 12-16, during years 1958-1961, for 74 census tract communities in Chicago.	1,2. Mean family income of 2nd and 4th quartiles. 3. Male unemployment rate.	1. % of population nonwhite. 2. % of females 14 and over who are separated or divorced. 3. Mobility variable.	Opportunity variables are statistically significant.
Fleisher [1966b] II	Court appearance rates of males, ages 12-16, during years 1958-1961 for 45 Chicago suburbs with population exceeding 10,000.	Same as I.	Same as I.	Unemployment rate is significantly positive.
Fleisher [1966b] III	Arrest rate of males, ages less than 25, for crimes of burglary, robbery, larceny, and auto theft, 1960-1962; 101 U. S. cities with populations exceeding 25,000.	Same as I.	Same as I.	Unemployment rate usually significant. Other variables perform erratically.

TABLE 2 (con't)

REFERENCE	DATA/DEPENDENT VARIABLE	OPPORTUNITY VARIABLES	OTHER VARIABLES	EMPIRICAL RESULTS
Ehrlich [1970]	Rate of offenses known to the police for murder, rape, assault, larceny, robbery, burglary, and auto theft, by state; 1960, 1950, and 1940.	<ol style="list-style-type: none"> 1. % of families below 1/2 median income. 2. Median income. 	<ol style="list-style-type: none"> 1. Probability of an offense being punished by imprisonment. 2. Average time served by offenders in state prisons for each crime category. 3. % nonwhite. 	Significantly positive coefficients on all variables.
Ehrlich (Chap. 6)	Rate of offenses known to the police for murder, assault, robbery, burglary, auto theft; U. S. totals, 1933-1967.	<ol style="list-style-type: none"> 1. Unemployment rate. 	<ol style="list-style-type: none"> 1. Clearance by arrest rate. 2. % nonwhite. 3. % ages 15-25. 4. Trend. 5. Dummy for World War II. 	Murder and assault have significantly negative coefficients. Property crimes have significantly positive coefficients.
Phillips, Votey, Maxwell	Total U. S. arrest rates for 18-19 year old males, larceny, burglary, auto theft, 1952-1967.	<ol style="list-style-type: none"> Age and race. Specific unemployment and labor force participation variables. 		Labor force status is correlated in the expected way with crime variable.

variable is intended to stand for the economic level of a community as it affects the tendency to commit delinquent acts [p. 123]." No fuller explanation is provided, but the same absolute dispersion rationale seems to apply here as to Ehrlich's work. Ehrlich's specification is preferable because he focuses on dispersion at the lower end of the income distribution.

Unemployment

Ehrlich includes the unemployment rate in some regressions as a measure of the risk of becoming unemployed. If the risk is the same to every member of the population (an unreasonable assumption) then there is no aggregation problem and the interpretation of the estimated coefficient is clear: variations in the unemployment rate cause variations at the *intensive* margin of crime supply.

Phillips, Votey, and Maxwell include the unemployment rate for a much different reason: they assert that those men who are unemployed are going to commit more crimes per capita than those who are employed; the relationship between unemployment and crime is thus the result of variations at the *extensive* margin of crime supply. (Phillips *et al* also believed that men outside the labor force --- neither employed nor unemployed --- are more prone to commit crimes than employed men. They therefore experiment with labor force participation rates as well as unemployment rates in their analysis.)

Results

Fleisher finds that juvenile arrest rates tend to be positively correlated with unemployment rates both over time and across population groups. Ehrlich finds that U. S. aggregate rates of burglary, robbery, and auto theft vary positively with unemployment over time, but crimes of violence --- murder and assault --- tend to be inversely correlated with unemployment. Phillips *et al* find that the aggregate arrest rates of 18-19 year old males vary posi-

tively with unemployment over time, and negatively with this group's rate of labor force participation.

Fleisher's cross-section analyses occasionally produce the predicted results for his two income variables. Ehrlich's cross-section results for his two income variables are consistent with his theory throughout.

Because of the difficulties with the crime data and with the interpretation of the actual specifications employed by these economists, one is well-advised to view these results with some caution. However, it is safe to say that the available empirical results do not contradict the proposition that improved legitimate opportunities would act as a deterrent to potential offenders, *ceteris paribus*.

SECTION 3

CORRELATES OF RECIDIVISM

Introduction

The empirical work discussed in Chapter 2 was all based on data on crime rates and arrest rates for various population groups. The well-known unreliability of this type of data, coupled with the statistical problems introduced by the use of aggregate statistics, suggest that criminologists should not limit themselves to these data --- and of course they have not. This chapter reviews the results of some of the many studies dealing with the effect of certain status variables on the probability that a parolee will eventually return to crime. These studies represent another approach to explaining crime rates --- since released offenders commit a large portion of all common crimes, an explanation of why some parolees return to crime and others do not could lead the way to policies which could significantly reduce the crime rate. The detailed and relatively accurate micro data available for samples of parolees yield insights which are hopefully complementary to results stemming from analyses of aggregate crime rate data.

I present empirical results from the Massachusetts data set mentioned in Chapter 1, and compare these results with those reported in the vast literature on parole outcome prediction. Particular emphasis is placed on the relationship of several socioeconomic status variables to the probability of recidivism. The data analysis presented below is prefaced with (1) a description of the Massachusetts data set; (2) a brief summary of the legal procedures existing in Massachusetts for paroling prisoners and terminating parole; (3) a definition of "recidivism"; and (4) a brief theoretical note on the application of the theory discussed in Chapter 2 to the special case of the parolee.

Data

The new empirical results reported below are based entirely on the sample of men paroled from Massachusetts felony-level institutions in 1959 used by Robert Evans [1968]. Extensive information was available on these men from the Department of Corrections; reasonable post-release work records were contained in the running records kept by the Parole Board.

Evans describes the sample selection process as follows:

Two samples, one of 192 men and one of 135 men, were chosen from among the approximately 600 men paroled from Massachusetts felony-level institutions in 1959. Initially, every third man released to a Massachusetts address with a supervised parole period was chosen from each of five institutions. The second sample was chosen by taking the man next to every other one already chosen. The resulting two samples are generally representative of the universe of all new parolees in Massachusetts in 1959. [p. 204]

Evans chose to report the statistics from these two samples separately, but there appears to be no statistical reason for maintaining this separation. Hence, I have collapsed the two samples into one in my investigation.

Parole Procedures in Effect in Massachusetts in 1959

States use parole to a widely differing degree, and employ somewhat differing release procedures and parole regulations. The propensity to release prisoners to parole supervision (as opposed to an unconditional release) is increasing over time, and regulations are also changing. The discussion which follows³ thus pertains directly only to Massachusetts in 1959 (at which time it was about average in the percentage of releasees who were placed under parole supervision). More general discussions can be found in Glaser, Cohen, and O'Leary [1966], and in the U. S. Task Force Report, *Corrections* [1967, Chapter 6].

Under 1959 regulations in Massachusetts, a prisoner first became eligible for parole at a time which depended on both the length

and the kind of his sentence. For inmates with fixed sentences (stated minimum and maximum terms), parole eligibility was fixed at two-thirds of the minimum term, less deductions for "good conduct." (Good conduct deductions were fixed by statute at rates which increased with the length of the sentence. They vary from two and one-half days a month for inmates with a sentence of less than one year to twelve and one-half days per month for inmates with a sentence of four years or more. A prisoner who broke prison rules could lose some of his good conduct allowance.)

The release decision for the male prisoners was made by the Commonwealth Parole Board, which consisted of three men appointed by the governor. The Board made its decision in each case based on a hearing with the inmate, a behavior report from the prison warden, the results of psychiatric examinations, and other information. Inmates refused parole the first time were to be reconsidered at least once every three years. The statutory criteria governing the release decision were as follows:

No prisoner shall be granted a parole permit merely as a reward for good conduct but only if the board or officer having jurisdiction is of the opinion that there is a reasonable probability that, if such prisoner is released, he will live and remain at liberty without violating the law, and that his release is not incompatible with the welfare of society.

A prisoner was released to parole only if he agreed to observe twelve stated conditions (in some cases, special additional conditions were imposed). Violation of any of these conditions constituted grounds for parole revocation:

1. Obey the law.
2. Work diligently at a lawful occupation and support dependents, if any.
3. Abstain from the use of liquor and narcotics of all kinds.
4. Not associate with persons of "questionable character," nor with anyone having a criminal record.
5. Report to the Parole Board once a week for the first month, and thereafter once each month until the expiration of sentence.
6. Submit to medical treatment if so ordered by the Parole Board.
7. Not live with a woman other than his wife.

The following actions required permission of the Parole Board:

8. Leaving the State.
9. Leaving employment or changing residence.
10. Marrying.
11. Applying for hunting or driving licenses.
12. Corresponding with inmates of Massachusetts prisons.

If the Board decided to revoke parole for any of these reasons, the parolee was returned to the institution to which he was originally committed and became eligible for re-parole at a time determined at the Parole Board's discretion. If he was not re-paroled, he served the remainder of his maximum term less accumulated good time (as did prisoners who were never paroled).

Parole was favorably terminated if it had not been revoked by the expiration of the parolee's maximum sentence less any good time he had accumulated while in prison. The parolee's supervision could be terminated early (after a minimum of one year on parole) by unanimous vote of the Board.

In the statistical work which follows, the salient features of these procedures are as follows: (1) The Parole Board acted with complete discretion in deciding whom to parole, but was supposed to focus on the probability that the inmate had "reformed." If the Board was successful in identifying the better risks, then prisoners receiving a mandatory release in 1959 should have returned to crime in a relatively high percentage of cases when compared with prisoners released to parole supervision. This selection process may thus affect the statistical analysis of the correlates of recidivism if the sample of released offenders is limited to parolees (as in the case below). Ideally, then, the sample should be taken from the population of all releasees (about 25% of whom were released unconditionally⁴). This shortcoming applies to most recidivism studies; because of data collection problems, it is very difficult to overcome. (2) The decisions to revoke parole are highly discretionary. The implications of this point are discussed below.

Definition of Recidivism

Recidivism has been defined here as a return to serious crime within a specified time interval after release from prison. This definition unfortunately does not completely correspond to the most readily observable outcome of the parole period --- namely, whether or not the Parole Board revoked parole before the parolee was released from parole supervision. Since parole revocation is sometimes used as an operational definition of recidivism (including Evans [1968]) it is important to note that despite substantial overlap, recidivism is neither a necessary nor a sufficient condition for parole revocation. It is true for the sample of parolees that the Massachusetts Parole Board revoked parole in every case where a parolee was arrested for committing a felony. The following differences between recidivism and parole revocation should be noted:

1. Some parolees who commit felonies are never caught. Contrariwise, some parolees who are arrested for allegedly committing a felony are no doubt innocent.⁵

2. Prisoners are released to parole supervision for differing periods (in the sample, the range was three months to life). By the parole revocation criterion, a parolee who is released from parole supervision after three months would be counted a success (even if he were arrested shortly thereafter), whereas a parolee who had parole revoked after five years would be considered a recidivist. Clearly a statistically useful definition of recidivism must give every parolee the same "chance" to return to crime.

3. As is clear from the list of parole conditions given above, parole can be revoked for a variety of reasons besides arrest on a felony charge. Each parolee is subject to technical parole conditions --- typically, these include requirements that the parolee work steadily, abstain from illicit drugs and excessive drinking, report to his parole officer for regular appointments, stay away from "undesirable" companions, receive permission from his parole officer before marrying, and so on. Violations of these technical conditions constitute sufficient grounds for parole revocation if the Parole Board is so inclined. Whether or not the

Parole Board revokes parole in the case of a technical violation (or an arrest on a misdemeanor charge) depends on its evaluation of the parolee's overall history. A decision to revoke usually indicates that the Parole Board feels that the parolee is likely to return to serious criminal activity if he is left on parole.⁶

These comments imply that parole revocation should not be directly indentified with recidivism if what we wish to study is the incidence of serious criminal activity among parolees. Two operational definitions of recidivism are actually used in this study:

Definition 1. A parolee is defined as a recidivist if his parole is revoked for any reason within 18 months of his release from prison.⁷ A parolee is defined as a "success" if he remains under parole supervision for 18 months without having parole revoked, even if at some later time he is returned to prison. (Twelve parolees were released from parole supervision within 18 months of their release from prison. These parolees were excluded from all statistical analysis.) This definition resolves the second of the three objections given above. The other two problems remain.

Definition 2. A parolee is a recidivist if his parole is revoked within 18 months because he is arrested for a new crime or because he leaves town without permission (an action likely to be associated with renewed criminal activity). The definition of success remains the same as above. In this case those parolees whose parole was revoked because of a technical violation of parole conditions must be excluded from the statistical analysis.

Neither definition is completely satisfactory. The first effectively assumes that a parolee who has parole revoked for a technical violation would have committed a felony within the first 18 months if he had been left on parole. The second definition excludes the group of technical violators from consideration, thus throwing away information and perhaps biasing the statistical results. However, all statistical work is done twice, once for each definition; the results of the two analyses should bracket the "truth."

There is some question about whether the 18-month period that was selected for this study is long enough to identify most of the parolees who eventually returned to crime. Kassebaum *et al* recommended a thirty-six month followup, citing two studies which show that 75-90% of those parolees who are eventually returned to prison will be identified in this period [p. 212].⁸ I chose an 18-month followup because no data were available on the job-holding or criminal behavior of parolees after they were released from parole supervision, and many of the parolees in the sample were released from parole supervision at or shortly after 18 months.

It is true for the sample that parole revocation rates drop sharply over time, as is shown in Table 3. It appears doubtful that many parolees in the sample recidivated after 18 months.

TABLE 3

Timing of Parole Revocations: Massachusetts Sample

Months on parole	0-6	7-12	13-18
Number of parole revocations (out of 313 men initially released to parole)	84	38	23
Percentage of total	30.1%	12.1%	7.3%
Percentage of remaining parolees	30.1%	16.6%	12.0%

Theoretical Effect of Parolee Characteristics on
the Probability of Recidivism

As an introduction to the presentation of the empirical correlates of recidivism, this section extends the theoretical discussion of Chapter 2 to take account of the parolee's special status as a known offender.

An almost trivial implication of the rational criminal choice

theory is that the parolee will return to crime if his personality and opportunities remain unchanged by his prison term. If his decision to engage in serious criminal activity maximized his preferences before, it will do so again. Apparently, however, all parolees do *not* commit new felony offenses after their release. This fact may be the result of changes in personality that are associated with aging and the parolee's experiences in prison. Zimring [pp. 97-103] discusses the latter. He reviews studies which suggest that "punishment may produce in its subjects changes in attitudes toward punishment that both increase and decrease the degree to which punishment is considered worth avoiding" [p. 99]. Furthermore, punishment may lead the subject to revalue his illicit behavior --- either up (through a process of rationalization) or down (through conditioning). Finally, punishment may change the offender's degree of adherence to societal norms --- again, in either direction. The conclusion from these comments is that prison may rehabilitate some prisoners while increasing the propensity of others to engage in criminal activities.

The parolee's licit and illicit opportunities are in general also different after his release. The official prohibition against associating with other criminals may, to the extent it is effective, reduce his ability to commit some types of economic crime profitably. The probability he will be arrested for a new offense is increased by his status as a known offender (and perhaps also by the surveillance of his parole officer); if he is arrested, he will almost certainly be returned to prison. This deterioration in the parolee's illicit opportunities appears unambiguous, unlike the effects on personality, and may be the decisive deterrent in many cases.

It is also possible that the parolee's legitimate opportunities are poorer, which would have the effect of partially offsetting the deterrence effect of poorer illicit opportunities. The stigma of a prison record may lessen the parolee's chances of finding a good job or a satisfactory social life; furthermore, the prison experi-

ence itself may cause a decline in the parolee's social and work skills. These effects may be partially offset by in-prison job training and remedial education, though these programs have not been shown to be effective.

The discussion above gives several possible explanations for the observation that some parolees do not recidivate.⁹ These explanations are compatible with the rationalistic model of criminal behavior. They do not provide any predictions regarding the identity of those who succeed on parole, however. Many studies have shown that parole success is not random, but rather statistically associated with a wide variety of observable parolee characteristics. What would we expect these results to look like?

In the discussion which follows, I make the simplifying assumption that the only change in the parolee's circumstances upon his release from prison is that the probability of arrest and punishment have increased relative to before he was incarcerated. If this is a valid assumption, then the parolees who are deterred from returning to crime will be those who were previously sufficiently close to being indifferent between committing crime or not. Then a group of parolees (defined by a particular observable characteristic or combination of such characteristics) which has a high percentage of its members close to the margin will have a high percentage of success. The reverse is also true.

Some measures of a parolee's criminal record give a direct indication of how far a parolee is likely to be from the margin. Parolees who were relatively young when first convicted, have been arrested and served prison sentences several times, and have had parole revoked one or more times have revealed their preference for criminal activity in a variety of circumstances, and are going to be relatively undeterred on the average by the parole situation.

Less clear is the relationship of socioeconomic characteristics to the probability of parole success. If nothing is known about his criminal record, then race, education, married status, possession of a work skill, etc., are all characteristics which are

strongly associated with the likelihood that a man will engage in criminal activity. But it is questionable that these characteristics will affect the *conditional* probability that a man will commit serious crimes, given that he has been observed to commit such crimes in the past. For example, a man with a high school education is less likely to be arrested than a man with only a grade school education, presumably because the former is deterred from crime by his superior job opportunities. But it is not obvious that the better educated man would be less likely to recidivate, given that they both did get arrested --- his superior job opportunities were not enough to deter him the first time in this case, so why should they be after he is released from parole?

One possible answer to this (rhetorical) question is the following: the men of higher socioeconomic class who are imprisoned may be closer to the margin on the average than other offenders. Thus, the increased sanctions which threaten parolees will deter a higher percentage of the better educated, more skilled parolees. Contrariwise, less educated parolees may decide in a larger percentage of cases that it is worthwhile returning to criminal activity despite the increased probability they will be apprehended and incarcerated as a result.

The next section presents empirical results relating observable parolee characteristics to the incidence of recidivism (as defined above). The new results based on the sample are compared with other authors' findings. Variables considered include socioeconomic variables, measures of past criminal record, and age at release.

Empirical Results: Zero Order Correlations

Table 4 presents zero order correlation results for the Massachusetts sample. These statistics will be discussed under four headings: (1) Measures of criminal record; (2) Time served; (3) Age at release; and (4) Socioeconomic characteristics.

TABLE 4
Crude Parole Failure Rates by Prior Characteristics¹

					Chi-Square Stat ²	95% Confidence Level ²
1. Release age	16-24	25-36	37-		2.85	5.99
	#141 51%	#125 52%	#47 38%			
2. Age at first conviction	8-16	17-			11.69	3.84
	#112 62%	#201 42%				
3. Prior sentences	None	One	Two	Three	11.31 ³	5.99 ³
	#69 33%	#76 47%	#51 57%	#40 58%		
4. Prior crime	Assault, Murder, Person	Sex	Robbery	Narcotics	21.1	9.49
	#52 54%	#27 22%	#84 42%	#7 14%		
5. Time served	1 year or less	More			.51	3.84
	#125 52%	#188 48%				
6. Married status at release	Married	Not married			.23	3.84
	#87 52%	#226 49%				
7. Race	White	Negro			.46	3.84
	#261 49%	#52 54%				

TABLE 4 (con't)

	Chi-Square Stat ²		95% Confidence Level ²		
8. Prior Occupation	Skilled #25 28%	Unskilled #286 51%		5.07	3.84
9. I. Q.	< 80 I.Q. #50 60%	80-109 #110 50%	110- #21 23%	.47 ⁴	5.99 ⁴
10. Education	0-7 #101 63%	8-11 #175 44%	12 #12 17%	10.04 ⁴	5.99 ⁴
11. Military service	Successful #80 35%	None or Unsuccessful #233 54%	35%	9.08	3.84

Observations
% returned % commit.
felony

¹Overall 49.5% recidivated including 17.9% technical revoke. KEY:

²The "Chi-Square Statistic" has approximately a chi-square distribution under the hypothesis that the probability of recidivism is independent of the value taken by the specified variable. This hypothesis can be rejected at the 95% confidence level if the statistics exceeds the critical value given in the last column.

³These statistics are for the trichotomy "none", "one", or "two or more".

⁴The "unknown" category is not included in these calculations.

Measures of Criminal Record

Two measures related to the duration and intensity of a parolee's criminal career are the age at which he was first convicted and the number of prior prison or jail sentences he has served. Both these and similar measures have consistently been shown to be related to the probability of recidivism, however recidivism is defined. The Massachusetts Sample is no exception in this respect. Notice also that most of the parolees have been in and out of jail or prison several times previous to the current prison sentence (over half have served two or more prior sentences).

The other measure of criminal record given here is the type of crime for which the parolee was last imprisoned. Most of the parolees in this sample and others committed a property crime --- larceny, auto theft, burglary, etc. --- and this group also has the highest recidivism rate. Almost as recidivistic is the group in the sample who committed felonious assault. Robbers do some better, and the most successful parolees are the narcotics and sex offenders. These results are not out of line with similar studies reported in Glaser and O'Leary [1966, p. 14].

Time Served

For the sample, recidivism appears to be independent of the time served in the current prison sentence (a more detailed breakdown of the time served variable does not contradict this conclusion). Other studies reported in Mulvihill and Tumin [pp. 561-567] reached mixed results on this issue. A study based on a 3-year followup of parolees released between 1963 and 1964 (using the length of the original sentence rather than the actual time served as the independent variable) shows success rates *increasing* with length of sentence. A second study based on federal parolees released in 1956 shows a slightly higher success rate for parolees who served twelve months or less when compared with others, but the success rates do not vary systematically over a longer period. A California experiment with shortening prison terms of a random

sample of prisoners found no difference between the experimental and control groups. [See Levin for a review of other studies on this issue.]

These results call into question the view that incarceration tends to increase the criminal propensity of inmates (i.e., that prisons are "factories of crime"), at least to the extent that this effect is alleged to increase with time. In any event, time served is apparently not a good predictor of parole outcome.

Age at Release

Glaser and O'Leary [p. 5] assert that "One of the most firmly established pieces of statistical knowledge about criminals is that the older a man is when he is released from prison, the less likely he is to return to crime." Yet this result does not hold for the sample. The small group of parolees who were more than 36 years old at their release are indeed more successful than the younger parolees, but there appears to be no systematic relationship between age and recidivism within the group ages 16-36. Indeed, the middle-aged (25-36) group is slightly more apt to fail on parole than the youngest group. A careful look at the three studies Glaser and O'Leary themselves review on this issue show similar results: a sample of federal parolees shows a steady decline of recidivism rates with age, but a sample of Wisconsin parolees reveals no drop in recidivism until the 35-39 age group, and the first falloff in recidivism in a New York study is reached even later (41-45).

Socioeconomic Characteristics

Six characteristics which are related to the quality of job and social opportunities are given in the table. These will be discussed in the following order: (a) Married status at release; (b) I. Q.; (c) Prior occupation; (d) Education; (e) Military service; and (f) Race.

a. Married status at release. Only about 28% of the parolees

in the sample were married at the time of their release. This attribute did not improve their chances of success on parole. This result is plausible; having a family would be expected to act as a deterrent to participation in crime for most men, but the parolees' marriages have failed as an effective deterrent at least once before and it is not surprising that they fail once again.

Other results reported below indicate that (1) being married at release *does* help the parolees for the first three months of his parole period; and (2) parolees who get married while on parole are less likely to recidivate than others.

b. I. Q. I. Q. measures were available for 55% of the parolees. No clear relationship emerges between I. Q. and recidivism. This result was also found by Glaser and O'Leary [pp. 17-18], who offer the interesting explanation that "A prisoner's intelligence test score . . . can reflect his capacity for both legal and illegal types of behavior."

c. Prior occupation. Only 8% of parolees in the sample had held skilled jobs. Only one member (4% of the 25) of this small group had parole revoked for a technical violation, compared with 19% of the unskilled parolees. There is also some difference in the percentage who had parole revoked because of a new arrest (24% of the skilled compared with 32% of the unskilled).

d. Education. The modal parolee in the sample had 8-11 years of formal education. Slightly less than 1/3 had less than 8 years, and only 12% had 12 years (none had gone beyond high school). More education is associated with lower rates of parole revocation for both technical violations and new arrests. This result could be explained by a finding that more highly educated parolees are overrepresented in other categories that are associated with high parole success rates. This possibility was checked and refuted by calculating a cross-tabulation of education with "age at release" and "number of prior sentences served." It is true for the sample that more educated parolees are underrepresented in the group of parolees who served two or more prior sentences, but they are over-

represented in the two younger age groups. More telling is the result that parolees with 8-12 years of education were more successful than others in all except one of the nine age/prior sentence categories, and the differences tended to be large. There is some evidence that education is less of a deterrent to recidivism for the youngest age group than for the rest.

From these cross-tabulations, it appears that education does act as a deterrent to recidivism. In terms of the discussion above, we can say that a relatively high percentage of more educated parolees are close enough to the margin that they are effectively deterred from returning to crime by the increased threats associated with parole.

e. Military service. One quarter of the parolees in the sample had been honorably discharged from the armed services. The remainder had either not served or been given a dishonorable discharge. The minority who had served successfully in the military had lower rates of parole revocation for both technical violations and new arrests. Successful military service should act as a job market credential which would yield relatively good job opportunities for this group; hence, this finding can be interpreted as another reflection of the causal process which yielded relatively high success rates for skilled parolees and more highly educated parolees.

Cross-tabulations analogous to those reported above for education show that the group with successful military service had a lower recidivism rate for every "age"/"prior sentences" cell except one. Military service appears most important for the group with no prior convictions.

f. Race. Blacks are greatly disproportionately represented in every measure of felonious activity: predominantly black neighborhoods have higher reported crime rates than predominantly white neighborhoods, black arrest rates are much higher than white arrest rates, the prison population is disproportionately black, etc. It is therefore of great interest that blacks in the sample are no more likely to recidivate than whites. Furthermore, this finding

is not unusual in the literature. All three relevant studies reviewed by Glaser and O'Leary [pp. 18-20] show adult male whites with slightly *higher* violation rates than adult male Negroes.

A study which reports a radically different result is reported in Kassebaum, Ward, and Wilner. their statistics are based on a followup study of 957 men released during the early 1960's from California Men's Colony-East:

Within two years of release, 34 percent of white, 22 percent of Mexican, and 21 percent of black parolees had experienced no trouble, and conversely 39 percent, 45 percent, and 51 percent, respectively, had been returned to prison. After three years, the gap had widened for blacks and had narrowed for Mexican-Americans. [p. 254]

Several studies conclude that among juveniles, blacks recidivate at a substantially higher rate than whites. The two relevant studies reviewed by Glaser and O'Leary reach this conclusion; a much more extensive study conducted by T. Sellin and M. Wolfgang gives strong affirmation of this result. Sellin and Wolfgang studied a cohort of almost 10,000 boys born in 1945 and living in Philadelphia at least between the ages of 10 and 17. Among those boys who had at least one delinquency contact (whatever the disposition), 65% of the blacks but only 45% of the whites had one or more further contacts. 30% of the blacks and 10% of the whites had five or more contacts. [These statistics are taken from Mulvihill and Tumin, p. 553.]

Recidivism studies vary along several important dimensions. A thorough review of the literature is required before a definite statement about the relationship between race and recidivism can be made; relevant dimensions (besides juvenile/adult distinction mentioned above) certainly include the year and location of the release and the definition of recidivism which is used.

Multivariate Analysis

Because the incidence of the eleven parolee characteristics discussed above are not mutually orthogonal (i.e., they tend to be correlated with each other), a multivariate analysis of the effect of these variables on the probability of recidivism may modify the empirical conclusions suggested by the zero order correlations.

The technique of multivariate analysis employed here is probit regression analysis; the reason for this choice is discussed in Appendix B, together with a summary of the properties of the probit estimator. Four probit regressions on the eleven parolee characteristics were estimated.¹⁰ The regressions differ only in the definition of the dependent variables: (1) The first regression defines all parolees who had parole revoked within 18 months of release as "failures," the remaining parolees are "successes"; (2) The second regression is based on a subset of the sample which excludes all parolees who had parole revoked in the first 18 months because of a violation of technical conditions of parole. The successes are the same as in regression 1; (3) The third regression treats all parolees who had parole revoked within three months of release as failures, and the remainder as successes; (4) The fourth regression is the same as the third, but estimated on a subset of parolees which excludes all parolees who had parole revoked for technical violations during the first three months.

The independent variables are entered in binary form. This is the natural choice for intrinsically qualitative variables (race, type of crime, etc.). Quantitative variables (age at release, number of prior sentences, etc.) could have been entered as covariates; however, a careful look at the zero order correlations suggested that these variables are characterized by threshold effects (discontinuities) on the probability of recidivism, and hence their effect is best estimated by partitioning the range of each quantitative variable and treating each segment as a qualitative variable. One segment of each partitioned variable is necessarily excluded to maintain the independence of the set of independent variable

vectors. The effect of each excluded segment is reflected in the constant term.

Interpretation of the Results

Table 5 presents the probit estimates of the four regressions described above. Interpretation of this table is most easily explained by a simplified example. The example is limited to three attributes (age, prior sentences, and race) and two hypothetical parolees.

	PAROLEE		COEFFICIENT ESTIMATOR
	X ₁	X ₂	
Constant	1	1	a ₁
Age at release			
16-25 years	1	0	a ₂
26-35 years	0	0	a ₃
Prior sentences			
None	0	0	a ₄
2 or more	1	0	a ₅
Negro	1	0	a ₆

As can be seen from the matrix, X₁ is a young (16-25) Negro with 2 or more prior sentences. X₂ "scores" zero on both explicit age segments, indicating that he falls into the residual age segment (36-61). By the same reasoning he must have exactly one prior sentence and be white. If we have estimates for a₁ through a₆, we can estimate the probability of success for the two parolees by calculating the inner product of the attribute vector and the coefficient vector; the resulting sum can be looked up in a standard normal distribution table to calculate the estimated probability of parolee success. Thus, the estimated probability that the first parolee in the example will succeed is

$$P_1 = \Phi(a_1 + a_2 + a_5 + a_6),$$

where Φ is the standard normal distribution function. Similarly,

TABLE 5

Coefficient Estimates from Probit Regressions: Predicting
Parole Success on the Basis of Prior Characteristics

	18 Months: Includes Tech- nical Revocations	18 Months: Excludes Tech- nical Revocations	3 Months: Includes Tech- nical Revocations	3 Months: Excludes Tech- nical Revocations
Constant	.46 (1.4)	.73 (1.9)	1.25 (2.8)	1.85 (3.5)
Age at release				
16-25	-.19 (.8)	-.11 (.4)	.15 (.5)	.03 (.1)
26-35	-.46 (1.9)	-.66 (2.2)	.03 (.1)	-.24 (.7)
Crime for which imprisoned				
Assault, murder	.01 (.1)	.02 (.1)	.33 (1.2)	.21 (.7)
Sex	.84 (2.6)	1.13 (2.7)	-.18 (.5)	-.13 (.3)
Robbery	.41 (2.1)	.57 (2.5)	.27 (1.0)	.19 (.6)
Narcotics	.99 (1.3)	4.09 (.3)	3.76 (.2)	3.47 (.2)
Prison term less than one year	-.36 (1.3)	-.06 (.2)	-.48 (1.5)	-.48 (1.3)
Prior sentences				
None	.03 (.1)	.24 (.8)	.47 (1.2)	.33 (.7)
Two or more	-.19 (.9)	-.15 (.7)	-.47 (1.8)	-.54 (1.7)
Successful military service	.44 (2.3)	.43 (1.9)	.47 (1.7)	.56 (1.8)
Ages less than 16 at first conviction	-.24 (1.1)	-.44 (1.7)	-.49 (1.6)	-.36 (1.1)

TABLE 5 (con't)

	18 Months: Includes Tech- nical Revocations	18 Months: Excludes Tech- nical Revocations	3 Months: Includes Tech- nical Revocations	3 Months: Excludes Tech- nical Revocations
Education				
0-7 years	-.29 (1.4)	-.29 (1.2)	.30 (1.0)	.01 (.02)
12 years	.26 (.6)	.50 (.9)	-.61 (1.2)	-.17 (.2)
Married	-.21 (1.2)	-.13 (.6)	.56 (2.3)	.58 (2.0)
Negro	-.14 (.7)	.03 (.1)	-.13 (.5)	.11 (.4)
Prior occupation was laborer or operative	.25 (.8)	-.04 (.1)	-.26 (.7)	-.50 (1.2)
Intelligence				
IQ < 80	-.05 (.3)	-.13 (.7)	-.36 (1.6)	-.48 (1.9)
IQ > 110	.16 (.5)	.41 (1.0)	-.28 (.7)	0.38 (.9)
Sample size	313	257	313	300
Number of failures	149	95	45	32

(Number in parentheses is the ratio of estimates of the coefficient and the asymptotic standard error.)

$$P_2 = \phi(a_1).$$

In Table 5, the constant term reflects the following residual attribute categories: ages 36-61, imprisoned for theft, prison term more than one year, one prior sentence, no successful military service, ages more than 16 at first conviction, 8-11 years formal education, unmarried, white, skilled laborer.¹¹ For each attribute, a category with a positive (negative) coefficient estimate as associated with a greater (lesser) chance of parole success than the residual category, *ceteris paribus*.

Qualitatively, at least, the results of the two 18-month regressions are virtually identical to the zero order correlations. However, by the usual standard few of the coefficient estimates are significantly different from zero. Of the six socioeconomic attributes, only military service is significant; the education effect is insignificant and surprisingly weak, and for the first regression the skilled worker effect actually has the "wrong" sign.

The third and fourth regressions, which employ the three-month revocation criterion, yield one intriguing result; the parolees who are married at the time of their release do substantially and significantly better for the first three months of parole than those who are not married. Since the married parolees are actually less successful than unmarried parolees over the full 18-month period, one can speculate that there is a temporary "honeymoon" effect operating which is all too soon dispelled by renewed familiarity.

SECTION 4

PAROLEES IN THE LABOR MARKET

Introduction

One conclusion that follows from studies which attempt to discriminate between parole successes and failures on the basis of prior characteristics (Chapter 3) is simply that such studies, while identifying characteristics which are statistically associated with parole success, leave much of the variance among parolees unexplained. This failure to predict outcomes with accuracy can be explained in several ways: (1) the process which defines a parolee as a success or failure is subject to a large random error, and the incidence of error is to a large extent independent of prior characteristics of the parolee; e.g., some parolees may return to crime but never get caught, and thus be labelled a "success" when in fact they are "failures"; (2) there are unobservable characteristics of parolees, to some extent uncorrelated with observable characteristics, which help determine success or failure; and (3) whether or not a parolee will succeed or fail is not determined at the time of his release, but rather depends on the interaction between personality factors and the nature of legitimate and illegitimate opportunities which are supplied by the social environment the parolee is released to; furthermore, these opportunities are not completely predictable on the basis of the parolee's observable characteristics. The third explanation is a justification for the expenditure of considerable resources on the provision of parole supervision and the search for post-release programs that will deter the parolee from returning to crime. Although this search for effective programs has been so far unsuccessful, the third explanation remains plausible. This chapter describes the legitimate work opportunities which are available to parolees and the parolee's response to these opportunities; this discussion

serves as background information for Chapter 5, which attempts to show that the parolee's experiences on parole do affect the probability that he will recidivate, and for the policy discussion in Chapter 6.

Section 1 presents data on the socioeconomic characteristics of prisoners and parolees which indicate that the typical ex-convict would be limited to low-paying, low-status jobs even if he had no criminal record. Furthermore, there is evidence that the typical prisoner is drawn from a relatively more disadvantaged socioeconomic group now than in the past.

Section 2 considers in detail another sort of job market handicap suffered by parolees --- their criminal records. A review of some descriptive and statistical evidence suggests that despite the many legal limitations to employing ex-convicts in certain types of jobs and the widespread evidence of employer aversion to hiring them, parolees may fare almost as well in the job market as workers at large (after socioeconomic characteristics are controlled for). The employment problems of parolees are thus similar to those of other disadvantaged workers.

Section 3 carries on this theme by presenting evidence that, like other disadvantaged workers, parolees appear to exercise considerable discretion in their job-holding behavior. Parolees typically change jobs frequently and their typical job has a correspondingly short tenure. Observed unemployment rates and wage earnings thus reflect both labor demand and supply factors. Some parolees of course do work steadily. The significance and statistical correlates of the differences in parolee job-holding patterns are discussed in detail in this section.

Socioeconomic Characteristics of Prisoners

The approximately 200,000 prisoners in state and federal institutions in the United States are disproportionately nonwhite, unskilled, poorly educated, and in other ways unattractive to em-

employers. When they are released, these offenders will have employment problems as great as any group in the labor force. Furthermore, there is evidence that the relative disadvantage of released offenders is growing over time.

Robert Evans's review [1964] of the 1960 Census figures found that

Male prisoners aged 25 to 34 had less education in 1960 than had males of the same ages who were Negroes, Indians, Filipinos, or who had Spanish surnames and lived in the Southwest. Even among the inhabitants of Chicago's Skid Row a higher level of education was found. [p. 120]

Furthermore, Evans reported that only 9.4% of the prisoners had been professionals, managers, or clerical personnel, while 43.8% of all males were employed in these occupations in 1960. The majority of prisoners had worked as laborers or operatives.

Evans found further that the relative disadvantage of prisoners in 1960 was greater than that in 1940. While the 1970 Census figures are not yet available, it is reasonable to assume that this trend has continued. The propensity of judges to sentence convicted felons to prison terms has declined in recent years, to the point where the prison population in the U. S. actually declined every year from 1961 to 1968 (in spite of an enormous increase in felony arrests and convictions during this period). While unproven, it is reasonable to suppose that the greatly increased use of probation in the last ten years has meant that prison is increasingly reserved for the lowest socioeconomic groups. Parolees, who are drawn from this increasingly disadvantaged population of prisoners, are faced with a bleak job prospect.

Labor Market Discrimination Against Parolees

Section 1 demonstrated that prisoners and parolees are drawn disproportionately from disadvantaged socioeconomic groups in the population. Because of their lack of "human capital," then, we would expect most parolees to be confined to jobs which are rela-

tively poorly paid and offer little security or chance for advancement. Furthermore, the famous "queue theory" of unemployment (which asserts that disadvantaged workers are "last hired, first fired") would predict that parolees would have relatively high unemployment rates and relatively low labor force participation rates, as do typical non-offenders who have the same socioeconomic characteristics as the average parolee.

It has often been suggested that parolees' employment problems are intensified by the tendency of the labor market to discriminate against workers with criminal records. This assertion rests primarily on studies of employer attitudes toward ex-offenders and surveys of the legal and customary restrictions on hiring ex-offenders. The small amount of empirical work which has been done on this question, however, indicates that discrimination does not actually appear to be an important factor in the work experience of parolees. This section reviews the evidence from both types of studies and offers a tentative explanation for the apparent discrepancy.

Evidence for Discrimination

One potential source of limitation on the employability of parolees is the fact that many jobs require licensing or bonding. States establish licensing requirements for a wide variety of occupations (e.g., barbers, drivers, electricians, morticians, dentists); licensing requirements vary a good deal from state to state [see Miller, Appendix E].

The 1960 Census found that more than 7 million people were working in occupations that were licensed in some jurisdiction. A survey of licensing statutes in 1970 found that as many as half may be affected by the existence of a criminal record, with many requiring "good moral character," and other specifically excluding persons with felony records. [Taggart, p. 85]

Other jobs require "bonding" --- a form of insurance against employee theft. Private insurance firms have naturally been reluctant to bond parolees in the past. However, the Manpower Administration of the U. S. Department of Labor now has the policy of

paying for privately-issued bonds on ex-offenders to any who want it and can demonstrate that no other source is available [Taggart, p. 88]. The problem of obtaining a security clearance may also bar parolees from jobs which require it.

The *de facto* practices of employment agencies, unions, and private employers are alleged to further restrict the employability of parolees. Friedman and Pappas [p. 38] cite a study which found that 75% of the private employment agencies sampled in the New York City area both ask job applicants about arrest records and as a matter of regular and automatic procedure, refuse to refer job applicants with arrest records (regardless of whether they were followed by conviction or not).¹²

Numerous studies of the attitudes of private employers toward hiring ex-offenders have been conducted; the general finding is that a few employers have an explicit policy against hiring ex-offenders, but most are willing to hire them so long as their criminal record is not deemed too serious.¹³ The restrictions mentioned above are mainly on employment of parolees in the private sector; over 15% of the labor force now works in the public sector, and here hiring practices appear to be more discriminatory than in the private sector. The President's Task Force on Prisoner Rehabilitation reported that:

The Federal government let down its bans somewhat a few years ago; it will now hire ex-offenders on an individual basis, if the agency that wants their services presents a strong brief, and after an elaborate and time-consuming screening by the Civil Service Commission.
[p. 10]

Most public employment is at the state and local levels of government. Hiring practices at this level were studied extensively by the Georgetown University Law Center. This study reported that some jurisdictions are totally restricted from hiring persons with criminal records, and a majority are partially restricted. Information concerning an applicant's criminal record is requested on job applications and in a majority of cases checked with the FBI or local police. Nevertheless, 90% of the state, city, and county

governments contacted employed one or more persons with criminal records [Miller, p. 81].

In summary, it appears that there are relatively few jobs which are absolutely closed to parolees, but many jobs for which the criminal record would be a hindrance. A conjecture that seems warranted by the discussion above is that the incidence of restrictions (partial or absolute) against hiring ex-offenders falls disproportionately on relatively good jobs; for example, civil service positions or jobs requiring occupational licenses. This conjecture forms a partial basis for explaining the findings of the empirical work surveyed in the next section.

Evidence Against Discrimination

There have been two comprehensive studies of employment problems of parolees --- Daniel Glaser [1964] and Goerge Pownall. Both are based on several samples of parolees. These studies call into question the claim that labor market discrimination against persons with a criminal record has a significant effect on their employability.

Daniel Glaser's Post-release Panel Study consisted of a series of interviews with 194 parolees released to U. S. Probation Offices in Chicago, Detroit, Cleveland, and St. Louis in 1959-1960. Data were extracted from a total of 715 interviews with these men conducted during the first six months of their parole [see Glaser, Appendix C].

A pertinent finding from this study is that of the 145 parolees who were questioned about a spell of unemployment, only 9% ascribed their failure to find work to their criminal records [p. 356]. One explanation for this lack of perceived importance is that parolees in many cases concealed their criminal records when applying for a job. In 41% of jobs actually obtained by parolees, the employer was ignorant of the parolee's record; in another 12% of jobs, the record was only known to relatives and old friends on the job. Approximately the same statistics obtain for jobs which lasted more than two months [p. 353]. Only 4% of job terminations were the

results of the employer discovering the parolee's criminal record.

G. Pownall's Post-release Panel Survey included all new federal releasees in the Baltimore and Philadelphia Federal Probation offices released between October 1, 1965 and March 31, 1966. Pownall attempted to interview each parolee four times during his first three months on parole. Fifty-one cases were contacted in all. Pownall summarizes his data as follows: "The vast majority of releasees did not report discrimination because of criminal record in obtaining and maintaining employment." [p. 192]

The evidence cited above is intriguing, but by no means offers a conclusive demonstration of the proposition that parolee's criminal records do not affect their employability. Stronger evidence could be gleaned from comparing the unemployment rates, labor force participation rates, and earnings of a sample of parolees with the corresponding statistics for a comparable group in the population at large. I am currently undertaking such a study, using as a data base G. Pownall's 10% sample of all released prisoners under the supervision of the U. S. Board of Parole on June 30, 1964 (945 cases). Pownall's own calculations suggest that unemployment rates for parolees who have been on parole for more than six months are not much higher than a comparable group in the population at large.

What could explain the postulated unimportance of discrimination against parolees? There are at least three contributing effects: (1) parolees in a large percentage of cases are able to successfully conceal their criminal records from employers; (2) the jobs which have licensing or other formal restrictions on the hiring of ex-offenders are found disproportionately among the relatively skilled, highly paid occupations. Most parolees are not affected by these restrictions simply because they have other disadvantages (low education, poor work history) which are sufficient in themselves to bar the parolee from employment in these jobs; (3) a more speculative explanation is that employers who offer the poorer jobs for which parolees are eligible cannot afford to discriminate in their hiring decisions.

In sum, it appears safe to view parolees as having essentially the same employment problems as any group of comparably disadvantaged workers. This viewpoint characterizes Section 3 and the pertinent discussions in the remaining chapters.

Job-Holding Behavior of Parolees

Many parolees are unable to find and hold satisfactory jobs. This fact is reflected in the exceedingly high parolee unemployment rates reported in Pownall and several other studies. Two possible interpretations of these high unemployment rates are the following. First, parolees, because they tend to be poorly educated, disproportionately nonwhite, unskilled, and lacking in work experience, have great difficulty in finding any sort of employment. They are at the end of the "queue" by which employers rank job applicants, and tend to be "last hired, first fired." They are willing to work at almost any wage because of economic necessity (they typically have no savings) and because steady work is a technical condition of parole. Unemployment is thus involuntary, except perhaps in those cases where the parolee has given up and returned to crime as a source of economic support. Second, parolees, because they tend to have characteristics which employers associate with low productivity and unreliability, are largely limited to jobs which pay low wages, offer little future, and have unpleasant working conditions. This type of job is, however, readily available. Parolees have high unemployment rates because they tend to change jobs often, with typically short spells of unemployment (leisure) in between jobs. A high percentage of job terminations are quits; in some cases the parolee quits because he has found a better job, but more often it is because he discovers after working a few weeks that the job does not meet his minimum standards. He is able to exercise this discretion because short-term financial support from family or friends is usually available. The parole board rarely revokes parole just because a parolee refuses to work steadily.

The second interpretation is the "modern" explanation for observed high unemployment among disadvantaged workers living in urban areas, as developed by Peter Doeringer and Michael Piore [see Doeringer *et al* (1969), and Doeringer and Piore (1972)]. The data analyzed by Pownall and Glaser, as well as the Massachusetts Sample, tend to support the modern theory --- parolees appear to exercise considerable discretion in their labor supply decisions. The labor market limits the *quality* of their opportunities but not the *quantity*. This observation suggests that a policy aimed at reducing the unemployment rate of parolees will not be effective if it merely facilitates placement in low quality jobs (see Chapter 6). A second implication, explored further in Chapter 5, is that a parolee's job-holding behavior provides a measure of his subjective evaluation of his legitimate opportunities.

The Evidence: Unemployment

Pownall calculated a 16.7% unemployment rate for his 10% sample of all men under federal parole supervision in June 1964 [p. 61]. This rate compares unfavorably with unemployment rates typically found in slum areas of large cities. Friedlander found unemployment rates of 16 such areas ranging between 5.3% and 15.0% (his statistics, reported on page 8, are mainly for 1960 and 1966). It should be noted, however, that Pownall found a high labor force participation rate of about 95% for his sample.

Unemployment rates among parolees tend to decrease for the first few months after they are released from prison --- Pownall found that parolees who had been released for less than six months had nearly double the unemployment rate of those who had been out more than six months, though the latter group still had an unemployment rate of 12.6%. The downward trend is characteristic of new entrants and re-entrants to the labor market.

The Evidence: Turnover

The high unemployment rate suffered by parolees is largely the result of a high incidence of typically short spells of unemploy-

ment, rather than long duration unemployment. These spells of unemployment are incurred in a large fraction of job changes.

Pownall found that one-third of the parolees in his national sample had only become unemployed during the previous month, suggesting that the average duration of unemployment for this group was less than three months.¹⁴ In another study, Pownall interviewed all new federal releasees in the Baltimore and Philadelphia Federal Probation offices released between October 1, 1965 and March 31, 1966; by the end of three months on parole, a majority had held two or more jobs with intervening spells of unemployment (the sample size in this case was 38; see page 207).

This same pattern of frequent job changes often accompanied by short spells of unemployment characterized the Massachusetts Sample. Two statistics serve to effectively demonstrate this high turnover rate. The sample as a whole averaged .36 jobs per month on parole (a new job every three months). The successful parolees (those completing 18 months without revocation) changed jobs about once every four months, averaging 4.25 jobs per man for the 18-month period. These averages of course conceal a wide variation in behavior, but, as Table 6 demonstrates, most parolees had several jobs. After one year, the median (and also the modal) parolee had held three jobs.

This high turnover rate is indicative to some extent of a process of job "shopping" typical of new entrants and re-entrants to the labor force. The worker and employer try each other out, and employment is quickly terminated if one or the other finds the arrangement unsatisfactory. Some parolees quit only after arranging a better job, but more often this is not the case. Glaser [1964] presents a statistical summary of reasons for job termination reported by parolees in his Post-release Panel Study. About half quit their jobs; about one-third of these had another job arranged at the time (Glaser's results are reproduced in Table 7).

The willingness of parolees to quit unsatisfactory jobs was also documented by Pownall: a majority of his national sample who had become unemployed in the previous month had quit their jobs.

TABLE 6

Distribution of Parolees by Number of Jobs Held

NUMBER OF JOBS	FIRST 3 MONTHS*		FIRST 12 MONTHS	
	Number	Percent	Number	Percent
0	3	1.1	2	1.1
1	109	43.4	28	15.6
2	89	35.5	32	17.9
3	31	12.4	33	18.4
4	10	4.0	29	16.2
5	7	2.8	23	12.8
6	1	.4	10	5.6
7	1	.4	10	5.6
8	0	0.0	7	3.9
9	0	0.0	5	2.8
TOTAL	251	100.0	179	100.0

* Those completing 3 months of parole successfully.

TABLE 7

Reason for Job Termination (Post-Release Panel Study)*

<u>Reason for Job Termination</u>	<u>First Jobs</u>	<u>Jobs Lasting Over Two Months</u>	<u>All Jobs</u>
Quit as pay too low [†]	9%	----	8%
Quit as work disliked [†]	11	----	7
Quit due to conflict with employer or other person at job [†]	7	7%	5
Quit as had better job arranged	21	25	17
Quit for other reasons [†]	8	18	11
TOTAL QUITTING JOB	56%	50%	48%

Discharged when criminal record discovered	3	----	4
"Laid off" --- discharged as no more work available	36	43	35
Discharged for other reasons	5	7	13
TOTAL DISCHARGED FROM JOB	44%	50%	52%

Number of job terminations covered in above analysis	76	28	128

* From D. Glaser [1964], Table 14.18, p. 354.

† No other job arranged when quit.

A vast majority of his post-release survey had quit their first jobs; one-third of these had had better jobs arranged [see Chapter 6 of Pownall].

The Evidence: Job Tenure

High job turnover rates among parolees imply short average job tenure. There are of course considerable differences among parolees in this respect; some parolees find jobs soon after their release which last more than a year, while others never succeed in holding a job even for one month. It is argued in the next chapter that these differences in job-holding behavior have a strong effect on the probability of recidivism.

In the Massachusetts Sample, 10% of the parolees who successfully completed the first three months of parole had held no job which lasted even one month (3 of these 25 had held no jobs at all). Nine percent of parolees held no jobs during the 4-6 month period on parole. Of those parolees who obtained jobs during the first period which lasted more than one month, the modal job lasted two months and the median job lasted four months. At the other extreme, 44 parolees (one-sixth of all parolees who completed the first period) found a job during the first three months which lasted a year or more. Twenty-six more parolees had found such a job by the end of the first year on parole. Thirty-six percent of all parolees who completed a year successfully found a job sometime during that year which eventually lasted a year or more.

What determines these differences in job tenure? Since a large proportion of these job terminations are apparently voluntary, differences in job tenure among parolees (and among jobs for a given parolee) should to some extent reflect differences in job quality. This observation suggests that it might be possible to obtain a measure of the job quality effect on job tenure by doing a statistical analysis of the Massachusetts job data. Such a measure would be very useful for policymakers in the following context: if the government adopted a policy of creating jobs for

parolees (or some other group of severely disadvantaged workers) which were of better quality than jobs ordinarily available to such workers, would the quit rate for this group be reduced? The interest in this question is reflected in Peter Doeringer's widely cited regression analysis of job tenure data for disadvantaged workers in Boston (this study is discussed below). But it is doubtful that data such as I or Doeringer have can actually yield even an approximate measure of the postulated tenure effect. The problem is that there are several possible explanations for observed statistical relationships between job quality and job tenure, and it is not possible to identify the specific effect of interest here (i.e., the effect of a change in the quality of available jobs on the propensity of a given disadvantaged worker to change jobs frequently).

The first problem is simply that all job terminations are not voluntary; job tenure data hence reflects employer as well as employee behavior. There are several possible relationships between job quality and the propensity of employers to fire or lay-off workers. (1) Employers who offer seasonal or temporary jobs may have to pay extra to attract enough workers (assuming the relevant class of workers have some preference for a stable job). This effect by itself would yield a *negative* wage-tenure relationship. A second source of a negative relationship is the possibility that an employer who pays a relatively high wage (because of union pressure or whatever) can afford to be more selective in his personnel practices, both before and after hiring.

(2) If socioeconomic characteristics of workers are controlled for, then workers who find relatively highly paid jobs should be less prone to quit than those who take relatively poor jobs. This prediction follows from each of the following considerations: (a) if workers tend to exhibit a positive wage elasticity of labor supplied, they will be less prone to quit a relatively good job in order to take a "vacation"; (b) a worker who takes a relatively good job will be less likely to quit in order to take (or

search for) a better job, simply because the probability of finding such a job is low; (c) it is certainly possible that stable workers are able to find better jobs than workers who have a history of frequent job changing. In its extreme form, this hypothesis can be stated as follows: There are two groups of workers, "stable" and "unstable." Employers are able to classify job applicants between the two groups, at least in a statistical sense. Since employers have a preference for stable workers, *ceteris paribus*, they are more likely than unstable workers to obtain relatively good jobs. An observed positive wage-tenure relationship from cross-section data reflects differences in the personality types of workers rather than the tendency of a given worker to work more steadily at a more highly paid job. To identify the latter causal process, it would be necessary to control for some characterization of the worker's history (neither I nor Doeringer have such a characterization available for our samples).

The discussion above suggests that if all job termination were involuntary, the wage-tenure relationship would be negative; if all terminations were voluntary, the relationship would be positive. One approach to separating the two would be to divide the sample between jobs which terminated in a quit and jobs which terminated in a fire or layoff. This procedure is bound to be inaccurate, however, even when the necessary data are available; a worker may be fired because he stopped going to work (and thus in reality quit); he may quit in anticipation of being fired. This dichotomization was not attempted.

If a single equation regression model of job tenure as a function of wages and worker characteristics is estimated and the coefficient on the wage variable is significantly positive, one could argue that this result demonstrates that the timing of voluntary job separations is indeed related to the wage in the predicted way, and that this relationship dominates the predicted systematic effect of involuntary separations. Then the estimated wage coefficient is a lower bound of the "true" voluntary wage-

TABLE 8

Distribution by Job Tenures of Jobs Obtained
by Parolees in the First 3 Parole Periods

MONTHS OF TENURE	PERIOD OBTAINED		
	0-3 Months	4-6 Months	7-12 Months
1	13	6	9
2	42	14	13
3	41	8	4
4	17	8	13
5	16	3	5
6	8	1	1
7	11	0	3
8	14	0	2
9	8	3	6
10	3	5	0
11	4	1	1
12	5	3	0
13-24	26	10	7
25-	13	2	4
TOTAL	221	64	68

(Each parolee is represented at most once in each column. If a parolee obtained more than one job in a period, only the tenure of the longest job was recorded. Jobs with tenure less than one month are not reported.)

tenure relationship. Even if this argument is accepted, of course, we are left with the question of identifying which causal process produced this positive voluntary wage-tenure relationship.

In sum, it appears that a single equation wage-tenure model based on the type of data used by Doeringer and myself cannot be interpreted with any confidence. The statistical work which follows is of interest, then, only insofar as the reader is interested in seeing a replication of Doeringer's well-known work and/or has some strong intuition regarding the relative importance of the various factors discussed above.

(Numerical results for the models discussed below are presented in Table 9.)

Model 1. The dependent variable here is the tenure of the longest job obtained by the parolee during his first three months on parole; parolees who recidivated during their first three months or who found no job which lasted as long as one month were excluded as were all jobs for which no wage data were available.

The independent variables are three measures of job quality as perceived by the parolees. The few parolees who found jobs with former employers or relatives stayed on these jobs almost six months longer on the average than other parolees (when wage is held constant). Presumably parolees prefer jobs with former employers or relatives because they already have the good will of their employers in these cases and have an easier time adjusting to the work environment. It could also be argued that job tenure is longer when the employer is a relative because in this case the employer gives the parolee preferential treatment in making lay-off decisions. The weekly wage has a significant positive effect on job tenure.

Model 2. This model is nearly identical to that used by Doeringer [as reported in Doeringer *et al* (1969), p. 73; Doeringer (1968); and Doeringer and Piore (1971), Chapter 8 Appendix]. Doeringer's data were collected for the period September 1967 to April 1968 from a random sample of clients of an organization called Action for Boston Community Development (ABCD), which was

TABLE 9

Job Tenure Regressions (Ordinary Least-Squares)

1. Dependent variable is tenure (in months) of the longest job found by the parolee during the first three months on parole (excluding parolees who found no job lasting at least one month). There are 186 observations.

INDEPENDENT VARIABLE	ESTIMATED COEFFICIENT (t-STATISTIC)	
Constant	.94	(.30)
Job was obtained from a former employer	5.7	(1.8)
Employer was a relative	5.8	(1.8)
Weekly wage	.12	(2.3)

R = .06^a

2. Dependent variable as in 1. The total sample of 186 observations are divided into two subsamples according to the parolee's age at his release from prison. These two regressions are a replication of Doeringer, and the results for the wage variable are similar to Doeringer's findings.

INDEPENDENT VARIABLE	Regression 1 (limited to parolees ages 16-25 at their release: 87 observations)		Regression 2 (parolees ages 26-61 at their release: 99 observations)	
	ESTIMATED COEFFICIENT AND t-STATISTIC		ESTIMATED COEFFICIENT AND t-STATISTIC	
Constant	16.1	(.01)	3.0	(.41)
Age (last two digits of birth year)	-.15	(.39)	-.27	(1.6)
Education	.02	(.05)	.51	(.79)
Negro	-1.6	(.68)	.95	(.29)
Married	-2.0	(.92)	.37	(.14)
Weekly wage	-.048	(.69)	.15	(1.9)
	R ² = .02 ^b		R ² = .09 ^b	

TABLE 9 (con't)

3. The dependent variable is the tenure (in months) of a job found by a parolee during the first 12 months of probation. The sample excludes jobs which were held less than one month, jobs for which no wage observation was available, and certian other jobs.* There are 323 observations.

<u>INDEPENDENT VARIABLE</u>	<u>ESTIMATED COEFFICIENT (t-STATISTIC)</u>	
Constant	2.8	(.76)
Weekly wage	.090	(2.4)
Ages 16-25 at release	-1.3	(.69)
Ages 26-35 at release	-1.2	(.66)
Less than 8 years education	-1.6	(1.3)
12 or more years education	3.3	(1.1)
Negro	.38	(.25)
Married	-1.4	(1.1)
Skilled or professional occupation prior to incarceration	.69	(.33)
Number of months imprisoned	.064	(1.6)
Number of prior sentences is one or more	-1.4	(1.1)
IQ less than 80	3.2	(2.6)
IQ greater than 110	-.68	(.31)
Job obtained in first 3 months	1.4	(1.0)
Job obtained in second 3 months	-.34	(.21)
Was incarcerated for theft (robbery, larceny, etc.)	-2.3	(1.8)

$$R^2 = .13^a$$

* If a parolee obtained more than one job during a period, only the longest job he obtained during that period is recorded. (Remember that the first 12 months are divided into three periods: 0-3, 3-6, and 6-12 months.)

^a Significant at 95% level.

^b Insignificant at 95% level.

funded under the Concentrated Employment Program. These clients were disadvantaged worker drawn from the poorer communities in Boston. Doeringer used as dependent variable the number of weeks worked on the client's previous job. Doeringer's independent variables included those of Model 2 plus variables indexing place of birth and sex.

Model 2 uses the same dependent variable as Model 1. The data are partitioned by age into two subsamples. Ordinary least-squares estimates for the young (16-25) subsample show that the variables explain almost none of the variance. The wage variable has a negative (insignificant) coefficient. Doeringer's estimate for the 16-25 age group of ABCD clients explains 10% of the variance in job tenure and finds several variables (age, education, place of birth) are statistically significant. The signs of his estimated coefficients are the same in every case as mine with the exception of the marriage dummy variable. Model 2 appears more relevant to adult workers (ages 26 and up), though still very little of the variance is explained. The signs of the estimates are the same as Doeringer's with the exception this time of education. The interesting result here and in Doeringer's estimates is that the wage variable has a significant positive coefficient. One interpretation that has been given to this result [see Bluestone] is that an adult worker with given characteristics will choose to work longer on a better paid job than he would on an inferior job --- generalizing, we might infer that turnover could be substantially reduced among disadvantaged workers by raising their (relative?) wage. There are other interpretations that could be given to this result, however, and the evidence that can be gleaned from these single equation models is very shaky.

Model 3. The final model has as a dependent variable job tenure on almost all jobs obtained by parolees during their first year on parole. The independent variables are similar to the variables used in the recidivism regressions; the estimates of the tenure regression coefficients are strikingly similar to the results in the recidivism regressions. This similarity may simply

reflect the fact that parolees who are known to have held jobs for an extended period necessarily could not have recidivated during the first 18 months on parole. The wage variable is positive and significant. The point estimate suggests an additional month's tenure for every eleven dollar increase in the weekly wage.

These tenure regressions are difficult to interpret because of the identification problem discussed above. They are compatible with the conclusion that a worker with given characteristics (education, age, race, criminal record) will tend to stay with a higher paying job longer than a lower paying job. There is some evidence (from Model 1) that working in a friendly environment may be important. Clearly, we have learned nothing from these regressions about the job-holding behavior of younger parolees who have no relatives or former employers to hire them.

SECTION 5

THE RELATIONSHIP BETWEEN PAROLEE
JOB-HOLDING AND RECIDIVISM

How do the parolee's experiences in the labor market affect the probability that he will recidivate? An empirical answer to this question, based on the Massachusetts Sample, is presented in this chapter. The empirical results are prefaced by a brief theoretical discussion of the relationship between the parolee's experiences on parole and his decision whether or not to return to serious crime.

Discussion

The following points are developed in the discussion below: (1) the parolee's experiences, especially in the job market, have a continuous, evolving influence on his proneness to return to serious criminal activity; (2) the process which generates job opportunities is to some extent stochastic, suggesting that no amount of information about parolees at the time of their release would allow an observer to distinguish between eventual successes and recidivists with complete accuracy; and (3) the parolee's subjective uncertainty about the quality of his licit opportunities has a deterrent effect at the time of his release.

As discussed in Chapter 2, the crime decision is based on an evaluation of licit and illicit opportunities. The decision of whether or not to return to serious crime must be based on intertemporal considerations; a criminal act may result in reincarceration for some extended period of time. How much the parolee fears this possibility depends on his expectations for the quality of his life if he remains free; if he feels that he has good licit opportunities, then the opportunity cost of going to prison is greater

than if his licit opportunities are poor. Thus, an improvement in a parolee's perceived licit opportunities increases his perceived cost of returning to prison; such an improvement should thus have a deterrent effect on the parolee.¹⁵

The crucial question, then, is what factors influence the parolee's perception of his licit opportunities. Certainly he is influenced by his experiences before and during his prison term; at the time of his release, he has more or less well-formed expectations concerning available jobs and special opportunities. But surely these expectations will be influenced as well by his experiences after his release from prison — the quality of job offers actually received, his relationships with family and friends, and so on. Even if he is optimistic at the time of his release, a series of bad experiences in straight life may convince the parolee that it is not worthwhile remaining straight.

These ideas could be developed into a formal model. For example, suppose we focus on the parolee's perception of the wage income which he can earn. Suppose he is willing to stay straight if he can obtain a job which pays more than W^* . At the time of his release from prison, he is uncertain about the quality of available jobs, but has a prior probability distribution $f(W)$ with mean W^e . If W^e is much less than W^* , he will return to crime immediately; if W^e is only slightly less than W^* , and he assigned a positive prior probability to finding a job paying more than W^* , he may be willing to stay straight initially since it would be rational to sustain a small expected loss in order to ensure that he could take an exceptionally good job if he found it (i.e., if he returned to crime immediately, there is some probability that he would be returned to prison and lose his chance, small though it may be, to find a job paying more than W^*). As the parolee samples jobs, his prior probability distribution will evolve over time. For example, he may find a permanent job paying more than W^* , in which case his perception of his future wage rate would have very small variance and an adequately high mean. Or he might fail to receive

any offers as high as W^* , in which case his subjective probability of ever finding such a job would shrink to zero (and at some point he would return to crime).

A model of this type could be made more formal and be formulated to include the many nonwage aspects of licit opportunities which affect the parolee's decision. The crux of the matter is communicated by the brief outline above, however: (1) the rational parolee bases his decision to return to crime on intertemporal considerations because the threatened prison term will last several periods; (2) therefore, the parolee's perception of the available quality of straight life during the relevant future is a crucial element in his decision; (3) expectations for the future should adjust to conform with actual experiences while on parole; and (4) if the parolee finds sufficiently good licit opportunities to earn a living and engage in rewarding social activities, he will stay straight --- otherwise, he will eventually return to crime.

Finally, it is necessary to explain what determines the parolee's evaluation of his actual experiences on parole. In the "model" above, for instance, what determines whether the parolee i actually finds a job which pays more than W_i^* (the minimum wage which will keep him from recidivating)? If parolee i is qualified for a range of jobs in the relevant labor market, which are distributed according to the density function $g(W)$, then the probability that parolee i will actually obtain a job with wage $W > W_i^*$ depends on the intensity of his search efforts and the percentage of jobs in g which pay more than W_i^* . Then if his reservation wage is low relative to his abilities, his chances are very good of obtaining a satisfactory job, and inversely. Whether or not the parolee finds a satisfactory job is, in this model, the outcome of a stochastic job search process with probability of success determined by the level of the parolee's reservation wage relative to his distribution of opportunities.

Job Satisfaction

The discussion in Chapter 4 suggests that job tenure serves as an indicator of job satisfaction. The parolee and his employer both have certain minimum standards for each other --- if the parolee ascertains that the job does not meet these standards (which would typically include the wage, social and physical aspects of working conditions, job status, etc.) he will soon quit or perhaps treat his job responsibilities so casually that he will be fired. The parolee should discover that the job is of unacceptably poor quality within a few weeks; what could be called the initial screening period. Quits or layoffs which occur after several months or more are more likely to be the result of other factors --- e.g., the parolee feels the job is acceptable but quits to take a better job, or perhaps the employer finds the parolee's performance acceptable but must lay him off in response to a downward shift in the firm's demand for labor. It is job terminations which occur within the initial screening period which are of interest here.

Whether or not the parolee finds a job sufficiently attractive to hold him beyond the initial screening period will influence his perception of the quality of straight life and hence affect the likelihood that he will decide to return to criminal activity. A parolee who finds no satisfactory jobs during his first few months on parole is likely to become more pessimistic about his chances of finding a straight life style sufficiently good to deter him from returning to crime; this assumption is warranted for the Massachusetts Sample since all of these parolees are adult males for whom work opportunities would tend to be the major determinant of their overall evaluation of straight life. Therefore, finding a satisfactory job can be viewed as being virtually a necessary condition for an upward revision of the parolee's perception of the quality of straight life.

The actual empirical indicator of job quality used in the empirical work below is the following: Did the parolee hold a job within a specified time period which lasted one month or more?

The choice of one month as the length of the initial screening period is certainly arbitrary. However, it seems to be a reasonable choice for the type of unskilled or semi-skilled job typically held by parolees, and is useful in the sense that many jobs (a majority) taken by the parolees in the Massachusetts Sample did not last this long.

An alternative approach to measuring job satisfaction would be to use observable characteristics of the job (wage, skill level, etc.) as measures of job quality and then assume that a parolee with a relatively high quality job (so defined) was more satisfied than one with a relatively low quality job. This approach ignores the fact that some parolees have higher standards than others; while it is presumably true that a given parolee would be more satisfied with a higher quality job, a direct measure of job quality is not a good measure on which to base interpersonal comparisons. Job tenure, since it is a direct behavioral indicator of job satisfaction, is more appropriate in this regard.

Other indicators of the parolee's evaluation of labor market opportunity which it would be useful to utilize include unemployment and total hours worked. These measures were unfortunately not available from the Massachusetts Sample. Indicators of the parolee's evaluation of other, nonjob aspects of his social life were not available with the exception of data on whether and when a parolee got married while on parole.

Results

The 18-month period was divided into four subperiods: 0-3 months, 4-6 months, 7-12 months, and 13-18 months. The variable J_I was observed for each parolee for $I = 1, 2, 3$, where

$$J_I = \begin{cases} 1, & \text{if the parolee held a job during period } I \text{ for} \\ & \text{one month or more} \\ 0, & \text{otherwise.} \end{cases}$$

In Table 10, the effect of J_I on the likelihood that a parolee

would recidivate in subsequent periods is calculated. Tables 12 and 13 demonstrate the statistical effect of the JI on recidivism in subsequent periods when prior characteristics of parolees are accounted for in multivariate probit regressions (see Appendix B for an explanation of the probit technique). Table 14 presents estimated probabilities of recidivism based on the regression results reported in Table 13. Finally, Table 11 presents the statistical relationship between recidivism and the number of jobs held by the parolee.

Table 10 demonstrates that whereas a majority of parolees who recidivated in period I + 1 had held a "satisfactory" job (JI = 1) in period I, it is nevertheless true that those who did not hold a satisfactory job in period I (JI = 0) had a much higher chance of recidivating in period I + 1 and subsequent periods.¹⁶

Table 14 demonstrates the reverse proposition: while steady job-holding behavior is related to parole success, too frequent job-changing increases the observed likelihood that a parolee will recidivate. For example, the probability that a parolee was going to recidivate in period 2 increased monotonically with the number of jobs he held in period 1. The pattern in subsequent periods is less obvious; however, it is true that parolees who had more than four jobs during the first six months and more than five jobs in the first 12 months, were more likely to recidivate in subsequent periods than those parolees who changed jobs less frequently.

Tables 12 and 13 present the results of a multivariate analysis of recidivism which includes the JI variables. The regressions are based on six subsamples of the Massachusetts Sample: two regressions are reported for each of the latter three periods, one including the technical revocations (as failures) and one excluding them from the sample. The estimated coefficients for prior variables are similar to those reported in Chapter 3 (note that some prior variables were omitted because they did not appear to be important in the statistical work reported in Chapter 3). Age and prior criminal record variables are occasionally significant.

TABLE 10

The Effect of Job-Holding on the Likelihood
of Recidivism: One-Way Layout

1. Job-holding in the first period vs. recidivism in subsequent periods (S = 254: all those parolees who completed the first three months on parole without revocation).

	Total	RECIDIVATES IN:			Successful
		2	3	4	
J1 = 0	25	8	2	6	9
J1 = 1	229	27	36	17	149

<u>Percentage Distribution</u>					
J1 = 0	100.0	32	8	24	36
J1 = 1	100.0	12	16	7	65

(The null hypothesis that J1 has no effect on the probability of parole success generates a chi-square statistic of 8.2. The 95% confidence level for chi-square with 1 degree of freedom is 3.84, indicating that the null hypothesis can be rejected.)

2. Job-holding in the second period vs. recidivism in subsequent periods (S = 219: all those parolees who completed the first six months on parole without revocation).

	Total	RECIDIVATES IN:		Successful
		3	4	
J2 = 0	20	7	5	8
J2 = 1	199	31	18	150

<u>Percentage Distribution</u>				
J2 = 0	100.0	35	25	40
J2 = 1	100.0	16	9	75

(The null hypothesis that J2 has no effect on the probability of parole success generates a chi-square statistic of 11.2, indicating that this null hypothesis can be rejected at the 95% confidence level.)

TABLE 10 (con't)

3. Job-holding in the third period vs. Recidivism in the fourth period (S = 181: all those parolees completing the first twelve months of parole without revocation).

	<u>Total</u>	<u>Recidivate</u>	<u>Successful</u>
J3 = 0	6	3	3
J3 = 1	175	20	155
<hr/>			
	<u>Percentage Distribution</u>		
J3 = 0	100.0	50	50
J3 = 1	100.0	11	89

(The null hypothesis that J3 has no effect on the probability of parole success generates a chi-square statistic of 7.2, indicating that this null hypothesis can be rejected at the 95% confidence level.)

TABLE 11
The Effect of Job Turnover on the Likelihood of Recidivism

# of Jobs	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
0	3	0%	33%	2	0%	50%	2	50%	50%
1	107	11	65	57	11	79	27	11	89
2	89	12	71	64	20	72	32	3	97
3	31	16	52	41	15	76	32	13	87
4	10	40	40	24	13	80	29	10	90
5	7	43	14	14	36	57	23	9	91
6	1	0	0	5	20	40	10	20	80
7 or more	1	0	0	4	50	0	19	32	68

- (1) Number of parolees holding the indicated number of jobs in the first period.
- (2) Percent who recidivated in the second period.
- (3) Percent who succeeded by the 18-month criterion.
- (4) Number of parolees holding the indicated number of jobs during the first two periods.
- (5) Percent who recidivated in the third period.
- (6) Percent who succeeded by the 18-month criterion.
- (7) Number of parolees holding the indicated number of jobs during the first three periods.
- (8) Percent who recidivated in the fourth period.
- (9) Percent who succeeded by the 18-month criterion.

TABLE 12

Coefficient Estimates from Probit Recidivism Regressions
Including Job Satisfaction Variables

INDEPENDENT VARIABLE	SAMPLE					
	S1	S2	S3	S4	S5	S6
Constant	1.22 (1.9)	1.29 (1.9)	.14 (.3)	.08 (.1)	.17 (.2)	1.57 (1.5)
Ages 16-25 at release	-1.04 (1.8)	-.70 (1.2)	-.19 (.5)	.08 (.2)	-.61 (1.4)	-2.15 (2.0)
Ages 26-35 at release	-1.32 (2.2)	-1.20 (2.0)	-.42 (1.1)	-.26 (.6)	-.81 (1.8)	-2.02 (2.0)
Crime for which imprisoned:						
Assault	-3.88 (.2)	-3.23 (.1)	-4.21 (.3)	-4.17 (.2)	-.74 (1.3)	-1.27 (1.3)
Sex crime	4.20 (.2)	3.98 (.1)	3.95 (.3)	3.74 (.2)	.52 (1.0)	1.30 (1.3)
Robbery	.30 (1.1)	.62 (1.8)	-.11 (.4)	-.07 (.2)	.74 (1.8)	1.01 (1.9)
Narcotics	3.63 (.1)	.36 (.1)	3.69 (.1)	3.40 (.1)	.72 (.8)	3.94 (.1)
Incarcerated less than one year	-.31 (.9)	-.02 (0.0)	-.56 (1.5)	-.26 (.5)	3.6 (.2)	3.85 (.1)
No prior sentences	.12 (.3)	.62 (1.0)	.50 (1.6)	.39 (1.1)	-1.10 (2.0)	-.82 (1.2)
Two or more prior sentences	-.33 (1.2)	-.40 (1.2)	.47 (1.8)	.60 (2.0)	-1.26 (2.5)	-1.27 (2.2)
Successful military service	.04 (.2)	.25 (.7)	.17 (.7)	.23 (.7)	.37 (1.02)	-.44 (1.0)
Became married since release from prison	4.57 (.2)	4.45 (.1)	.42 (1.1)	.59 (1.1)	.55 (1.2)	.76 (1.3)
Job-holding experience during parole period:						
Found job that lasted at least one month during first 3 months on parole(J1)	1.09 (3.8)	.79 (2.2)	-.08 (.2)	.28 (.7)	.33 (.8)	.21 (.4)

TABLE 12 (con't)

INDEPENDENT VARIABLE	SAMPLE					
	S1	S2	S3	S4	S5	S6
Stayed on a job at least 1 month dur- ing 3-6 month pa- role period (J2)			.88 (2.8)	.58 (1.5)	-.03 (.05)	.19 (.3)
Stayed on a job at least 1 month dur- ing 6-12 month pa- role period (J3)					2.00 (3.5)	2.13 (2.8)
Number of observations	255	242	220	205	181	168
Number of failures	35	22	39	24	30	17

(Number in parentheses is ratio of estimates of the coefficient to its asymptotic standard errors.)

DESCRIPTION OF SAMPLES

S1 The sample is all parolees who survived at least 3 months on parole, with the exception of a few parolees for whom no job information was available. A failure is a parolee who had his parole revoked for any reason during the 3-6 month period.

S2 All parolees who survived at least 3 months on parole and who did not have their parole revoked during the 3-6 month period for technical reasons. A failure in this case is thus someone who had his parole revoked during the 3-6 month period for committing a new felony.

S3 All parolees who survived at least 6 months on parole, with the exception of a few parolees for whom no job information was available. A failure is a parolee who had his parole revoked for any reason during the 6-12 month period.

S4 All parolees who survived at least 6 months on parole and who did not have their parole revoked during the 6-12 month period for technical reasons. A failure is a parolee who had his parole revoked during the 6-12 month period for committing a new felony.

S5 All parolees who survived at least 12 months on parole, with the usual exception. A failure in this case is someone who had his parole revoked for any reason during the 12-18 month period.

TABLE 12 (con't)

S6 All parolees who survived at least 12 months on parole and who did not have their parole revoked during the 12-18 month period for technical reasons. A failure is a parolee who had his parole revoked during the 12-18 month period for committing a new felony.

NOTE: All independent variables are binary. The dependent variable assigns a "1" to success and a "0" to failure. Hence, a positive estimated coefficient indicates that a parolee who has that attribute is thereby given a higher probability of succeeding on parole during the specified period.

TABLE 13
Coefficient Estimates from Simplified Probit Recidivism
Regressions which Include Job Satisfaction Variables

	Age at Release		Robbery		Number of Prior Sentences		Job Success in Preced- ing Period	Constant
	16-25	26-35	None	2 or more				
3-6 months	-1.17	-1.38	.10	.27	-0.35	1.05	1.49	
1. Technical revocations included 255 observations; 35 revokes	(2.2)	(2.5)	(.4)	(.8)	(1.3)	(3.9)	(2.6)	
3-6 months	-.85	-1.25	.47	.81	-.39	.74	1.65	
2. Technical revocations excluded 242 observations; 22 revokes	(1.6)	(2.3)	(1.4)	(1.4)	(1.2)	(2.1)	(2.7)	
7-12 months	-.33	-.41	-.20	.59	.42	.73	.32	
3. Technical included 220 observations; 39 failures	(1.0)	(1.2)	(.8)	(2.1)	(1.7)	(2.7)	(.8)	
7-12 months	-.04	-.18	-.03	.51	.54	.53	.43	
4. Technical excluded 205 observations	(.1)	(.5)	(.1)	(1.6)	(1.9)	(1.7)	(1.0)	
13-18 months	-.79	-.76	.72	-.94	-1.30	2.07	.58	
5. Technical included 181 observations	(2.0)	(1.8)	(2.0)	(1.9)	(2.7)	(4.8)	(1.0)	
13-18 months	-1.91	-1.79	.87	-.56	-1.19	2.15	1.60	
6. Technical excluded 168 observations	(2.3)	(2.1)	(1.8)	(.9)	(2.2)	(3.5)	(1.9)	

(Number in parentheses is ratio of estimates of coefficient and asymptotic standard error.)

TABLE 14

The Effect of Job Satisfaction
on the Probability of Parole Success

Description of Parolee	Probability of Parole Success during the Specified Period (calculated from entries in Table 4)		
	S1 (3-6 months)	S2 (7-12 months)	S3 (13-18 months)
One prior prison sentence, not a robber, and:			
1. Ages 26-35, no job in pre- ceding period	.54	.46	.44
2. Ages 26-35, satisfactory job in preceding period	.88	.74	.97

3. Ages 36-61, no job	.93	.63	.72
4. Ages 36-61, job	.99	.85	.996

The effect of the JI variables on the likelihood of recidivism in the next period is large, statistically significant, and of the expected sign. This result holds whether or not technical revocations are included in the sample. It is interesting to note that the effect of J1 on recidivism in periods 3 and 4 is nil when J2 and J3 are included in the regression; the same is true for the effect of J2 on recidivism in period 4. This result accords with the presumption that the parolee's most recent experiences in the job market should have the most influence on his perception of the quality of legitimate opportunities.

Parolees who got married after release from prison appear to be more successful than others, though the effect is measured imprecisely.

Table 13 presents the results of somewhat simplified regressions based on the same six samples. There are no surprises in the estimated coefficients. The estimated probabilities of recidivism for several arbitrarily chosen groups of parolees presented in Table 14 are based on the regression estimates of Table 13. The probability estimates help demonstrate the importance of the job tenure variables as predictors of recidivism in subsequent periods. For example, parolee aged 26-35 who has one prior prison sentence and whose most recent incarceration was not for robbery is virtually certain ($p = 97\%$) to succeed in period 4 if he held a satisfactory job in the preceding period; if he did not hold such a job, however, his estimated probability of success drops to 44%.

Causality

What is the causal significance of these regression results? More specifically, the crucial question in interpreting these results is the following: Do they allow us to infer a prediction about the effect on recidivism of a program which improved the legitimate opportunities of parolees? Certainly if the job-holding

variables had proved unimportant and statistically insignificant as explanators of recidivism, it would be justifiable to be pessimistic about the potential effectiveness of such a program. The inverse proposition that positive results (actually obtained) necessarily warrant optimism about the potential effectiveness of such a program does not hold, however. These results could have been generated by a process such as the following: There exists some Factor X which at any point in time determines both his will to work steadily and his will to remain straight; furthermore, Factor X is not influenced by his actual experiences in the job market. Then the statistical relationship between job success and parole success is the result, not of a direct causal connection, but of the effect on both of Factor X. In this case there is no reason to suppose that a program to improve job opportunities for parolees would lower the recidivism rate.

In rebuttal to this interpretation, it can be pointed out that the multivariate statistical analysis controls for several of the possible candidates for Factor X. More important perhaps is the intrinsic plausibility of the model presented at the beginning of this chapter; if one accepts the view that the labor market provides the major alternative to crime, and that there is a certain amount of randomness in the outcome of the parolee's job search, then the Factor X interpretation of the results is inappropriate.

Ultimately, the type of data used in the empirical work here cannot be used to demonstrate conclusively the direction of causality. The empirical results reported here are at least compatible with the prediction that an improvement in legitimate opportunities for parolees would reduce the recidivism rate, and further investigation of this proposition is warranted.

SECTION 6

CREATING JOB SATISFACTION

How can the parolee's job opportunities be effectively improved so that he has an increased chance of finding a satisfactory job? On this question I can offer no new evidence, but it is instructive to review the growing body of literature on the subject.

Disadvantaged workers (including parolees) can be assumed to share a taste for higher wages and fringe benefits, more pleasant working conditions, job security and perhaps a chance for promotion. The probability that a parolee will find a job satisfactory thus increases with an improvement in these characteristics. There are two basic approaches to helping the parolee find a satisfactory job. (1) Since the most attractive jobs usually have more stringent hiring standards, it is plausible to assert that the parolee's opportunities could be improved by giving him vocational training, remedial education, counseling, and job placement services (which could include aid in obtaining bonding and union membership in some cases). (2) A more direct approach is simply to create an acceptable job for the parolee (either through public employment or through subsidies in the private sector), and then perhaps provide him with on-the-job training to qualify him after the fact. Both approaches have been tried and evaluated. A review of the relevant literature follows.

The Manpower Training Approach

The effort to rehabilitate prisoners and parolees through developing their work skills has grown enormously in recent years, with the primary thrust coming from the Federal government under the Manpower Development and Training Act. Inmate skill training

projects (begun in 1967 under the MDTA) are in operation in 60 correctional institutions around the country. In fiscal 1972, the Department of Labor was budgeted to spend \$30 million on offenders. [For a description of programs, see Taggart and Manpower Report, pp. 70-72.]

Most of the programs that have been funded so far are in some sense experimental, even though they are typically not designed to allow rigorous evaluation. Followup evaluations suffer from this lack of a preplanned experimental design and a lack of relevant data on outcomes. Both these problems are evident in the Abt Associates, Inc. evaluation of 25 inmate training projects funded from 1968 to mid-1969 under MDTA (Section 251). These programs offered vocational training, remedial education, and counseling to inmates who volunteered for the training and survived a screening process which often included interviews by prison officials [Vol. III, p. 8]. "Control" groups were chosen in a rather haphazard fashion, apparently not from among volunteers to the training program. Thus, the experimental group would be expected to perform better independent of any manpower services they received, both because of the screening process applied to volunteers and the presumption that volunteers as a group would be more highly motivated to succeed than the nonvolunteers who made up most of the control group. Abt attempted to collect employment and recidivism data on each experimental and control subject at three months following his release, and again at six months. Abt's initial findings were that there was no difference between the two groups in either employment or the recidivism rate. Some attempt was then made to correct for an alleged bias in the data introduced by nonrespondents; after this correction, it was estimated that experimentals were slightly more successful at avoiding parole revocation than controls during the first six months, the difference being 4.7 percentage points [p. 48]. This difference was statistically insignificant. The statistical insignificance of the results, together with the methodological weaknesses of the study, leave the question of whether in-prison training is effec-

tive in considerable doubt.

The Federal Offenders Rehabilitation Project, conducted from 1965-1969 by the Rehabilitation Services Administration and three federal correctional agencies, appears to have utilized a better experimental design and better data collection techniques. The results of this study are unequivocally negative. It eventually involved eight satellite projects in seven state vocational rehabilitation agencies, with the primary purpose of testing the effectiveness of providing intensive vocational rehabilitation services to federal offenders. The experimental group (S = 623) received some combination of the following services: training, tools, medical services, counseling, and income maintenance.

Since the comparison of experimental to control clients did not reveal significant differences in outcome, either in the areas of recidivism or employment, it is necessary to conclude given the basis of FOR data and analytical limitations, that vocational rehabilitation services do not have a positive impact on the performance of public offenders. [p. 95]

A third experimental project was limited to parolees. Taggart describes it as follow [p. 21]:

Project Develop, operated from 1966 to 1968 under a grant from the U. S. Department of Labor to the New York State Division of Parole, attempted to measure the effect of post-release manpower services. It provided vocational guidance, work orientation, counseling, education, training, support, placement, and followup assistance to young (17 to 23 years old), undereducated, and underemployed parolees with above-average intelligence, at a cost of \$2,400 per person completing the program. Within the 2- to 10-month period involved, the proportion violating parole or rearrested for a new crime was 15 percent among participants, compared with 23 percent among the control group, and the proportion sent back to jail was halved (6 percent for the experimental group, 12 percent for the control) . . . these differences are not statistically significant . . .

I have been unable to locate the original report, and am not in a position to evaluate the experimental method employed by Project Develop.

The three studies discussed above warrant considerable pes-

simism regarding the efficacy of offering manpower services to criminal offenders at either the pre- or post-release stage in the correctional process. Final judgment should of course be withheld pending further experimental results. So far, however, there is nothing that would lead us to prefer this approach to the discredited "people-changing" approach to rehabilitation discussed in the introduction.

It should be noted that neither the FOR study nor the Abt Associates study given any evidence to refute the association between job satisfaction and recidivism that I have postulated. Both studies found that (1) the experimental treatment had not significant effect on job-holding; and (therefore) (2) the treatment had no significant effect on the recidivism rate.

The Job Creation Approach

The most direct approach to improving the job opportunities of parolees is to place them in a good job and *then* provide them with on-the-job training and counseling. A recent experiment funded by the Manpower Administration reveals both the difficulties and the potential efficacy of this approach.

"Operation Pathfinder" was an experiment conducted by the Mentec Corporation in Los Angeles during the period September 1969 to February 1972. 173 juvenile parolees over the age of 18 were placed in semi-skilled, production type jobs paying in the range of \$1.65 to \$4.17/hour. The parolees were more or less randomly assigned to three experimental groups and a control group. Experimental subjects received social reinforcement (SR) from supervisors on the job and/or from Mentec counselors off the job. The SR technique is supposed to modify behavior through verbal rewards. Mentec found that this technique was effective at improving the job performance of parolees when it was applied by job supervisors. When compared with the others, parolees receiving SR from supervisors were superior with respect to productivity, quality of

work, absenteeism, and tardiness. The experimental group was characterized by a significantly higher employment rate and longer job tenure than the control group. Most important, 24.3% of the control group recidivated, while only 3.6% of all experimental subjects were lost as a result of recidivism.

The success of Operation Pathfinder and the apparent failure of ex-convict training programs suggest that the most direct approach to improving parolees' opportunities --- job creation with on-the-job training --- may also be the most effective. It suffers from two rather serious problems, however: (1) It is doubtful that the private sector would be willing to create the necessary number of jobs together with special on-the-job training programs, even if the government offered to subsidize most of the expense incurred. Mentec contacted 1141 firms in the Los Angeles area and were only able to find 14 which were willing to cooperate with their whole program. It is possible that a public service employment program could be designed to absorb many parolees. (Placing parolees in public sector jobs would require the removal of the institutional and legal barriers which currently prevent ex-convicts from taking many public sector jobs [see Miller]. (2) The public reaction to a large-scale job recreation program for parolees may be negative, since it would have the appearance of a reward for criminal activity. This objection could be most effectively quashed if the program was demonstrated to be successful in reducing crime.

APPENDIX A

A COMPARISON OF THREE
CHOICE THEORETIC MODELS OF CRIMINAL BEHAVIOR

KEY: B = Becker [1968];
L & B = Lind and Block [1972];
E = Ehrlich [1970].

Measure of Criminal Activity (choice variable).

1. B, L&B: Does the individual commit an offense?
2. E: Amount of time t_1 the individual spends in illegitimate activity.

Characterization of payoff to crime.

1. B: Monetary or psychic income from an offense G .
2. L&B: Monetary payoff G , which is gross of the direct cost of committing the crime C .
3. E: Payoff which depends on time spent in crime $G(t_1)$.

Characterization of the probability of apprehension.

1. B, L&B: Fixed by the environment --- crime viewed as a series of Bernoulli trials.
2. E: Models three possibilities. In the text he assumes p is independent of the amount of criminal activity committed during the period. Two appendixes suggest the alternatives $p = p(t_1)$ and p as a function of expenditures on self-protection.

Characterization of sentence.

1. B: Monetary equivalent f .
2. E: Monetary equivalent $f = f(t_1)$ which is paid at the end of period.
3. L&B: Sentence s (number of months in prison) which is not directly measured by monetary equivalent. Offender also loses some portion of his illicit payoff if he is apprehended and convicted.

Legitimate opportunities.

1. L&B: Total wealth W (sum of human capital and monetary wealth).
2. E: Nonhuman wealth W' plus some labor income $w_1(t_1)$.
3. B: No discussion.

Preferences.

All three assume that criminals maximize their expected utility.

1. L&B: $U = U(W,s)$ where U_{WW} and U_{ss} are negative, U_{Ws} is nonpositive.
2. E: $U = U(\text{total income}, t_c)$, where t_c is leisure time.

Decision rule.

1. B: Commit crime if $pU(G - f) + qU(G) > 0$.
2. L&B: Commit crime if $qU(W - C + G, 0) + pU(W - C + zG, s) - U(W, 0) > 0$, where z is the portion of the payoff the criminal keeps if he is convicted.
3. E: Choose t_1 and t_1 to maximize the expression $qU(W' + G(t_1) + w_1(t_1), t_c) + pU(W' + w_1(t_1) - f, t_c)$.

APPENDIX B

PROBIT REGRESSION ANALYSIS

It is not possible to predict with certainty at the time of his release whether a given parolee will eventually recidivate. However, a statistical analysis of data such as those available for the Massachusetts Sample can yield information on the *likelihood* that a parolee with certain characteristics will recidivate; that is, one can make a statistical prediction of the percentage of parolees with given personal characteristics who will recidivate in a given legal/institutional/social environment.

To generate estimated probabilities from a sample, it is necessary to make some assumptions about the nature of the data. A basic assumption which lies behind the statistical work here is that the sample of parolees can be viewed as a set of independent Bernoulli trials. Whether or not parolee i fails (recidivates) does not influence the probability that parolee j will fail. Further, it is assumed that the probability of success for parolee i , denoted p_i , is some function of his observed characteristics (denoted by the vector X^i). Hence,

$$p_i = P(X^i) .$$

Mathematical convenience in fact requires a restrictive form for P :

$$P(X^i) = \psi(\beta'X^i) ,$$

where $\beta'X^i$ is a linear combination of the observed characteristics (or some transformations of these observed characteristics). With J. Tobin [1955], we can think of $\beta'X^i$ as an index of characteristics.

Finally, it is necessary to choose a form for the ψ function. One possibility is to assume ψ is simply a scalar multiple (which without loss of generality can be assumed to be 1). This assumption yields the "linear probability model":

$$p_i = \begin{cases} \beta'X^i, & 0 \leq \beta'X^i \leq 1 \\ 0, & \beta'X^i < 0 \\ 1, & \beta'X^i > 1 \end{cases} .$$

This model was not employed because it seems unreasonable to assert that there is a critical value of the index above which the probability of success is 1 (and a second critical value below which the probability is zero). Furthermore, there is no known statistical technique for generating consistent estimates for β within the linear probability model. In particular, ordinary least-squares estimates are inconsistent (and biased).

A more reasonable assumption about ψ is that it approaches the value one asymptotically as the index becomes large, and approaches zero asymptotically as the index decreases. There are many transformations ("ogives") that have this property; I have chosen the probit transformation because it is inexpensive to estimate [see McFadden (1972) for a discussion of other similar transformations].

The probit assumption is that $\psi = \Phi$, the cumulative distribution function for a standard normal variable. Then,

$$P(X^i) = \frac{1}{\sqrt{2\pi}} \int_{-\infty}^{\beta'X^i} \exp\left(-\frac{1}{2}u^2\right) du .$$

Under this assumption, an estimate $\hat{\beta}$ of β can be obtained through the maximum likelihood technique. $\hat{\beta}$ is consistent, asymptotically efficient, and asymptotically normal; in general, $\hat{\beta}$ is biased for finite samples. The asymptotic normal distribution of $\hat{\beta}$ has mean β and variance-covariance matrix given by the inverse of the "information matrix" (defined as minus the expected value of the matrix of second derivatives of the log likelihood function). The information matrix can be estimated. The estimate $\hat{\sigma}_{\hat{\beta}_i}$ of the asymptotic standard error of $\hat{\beta}_i$ is a measure of the precision with which β_i is estimated. A common practice is to assume that

$$\frac{\hat{\beta}_i - \beta_i}{\hat{\sigma}_{\hat{\beta}_i}} .$$

has approximately the standard normal distribution; this permits the usual test of significance for the $\hat{\beta}_i$ [see Tobin].

FOOTNOTES

1. There is some evidence that practicing burglars earn more (in an expected value sense) committing burglaries than they could earn working. See Cobb [1971].
2. Other theoretical essays of interest include Banfield [1968] and Tullock.
3. Based on "The Rules of the Parole Board" for the Commonwealth of Massachusetts, 1959.
4. Estimated from Table 9, *Statistical Report of the Commission of Correction*, 1959.
5. In statistical analyses which use parole revocation as a proxy for recidivism, this problem does not introduce a bias in the measured effect of various independent variables on the probability of recidivism unless the error in measurement is systematically related to one or several of these variables.
6. The parole revocation decision in such cases, because it is highly discretionary, is influenced by the biases of the members of the Parole Board. (See Kassebaum *et al* for a discussion of parole conditions and the revocation decision in California. Robinson and Takagi's study explores the effect of a parole board member's background on his decision whether or not to revoke parole.)
7. To be more exact, I dated the parole revocation as the time when the parolee committed the violation which led to revocation, rather than the somewhat later date at which parole was officially revoked. The difference is due to administrative delay in making the decision.
8. For a similar conclusion, see Task Force Report, *Corrections* [p. 68], and Mulvihill and Tumin [p. 549].
9. Another explanation may hold in some cases. Some crimes are the result of special circumstances in a parolee's life which do not recur while he is on parole. Murder is in many cases this type of crime.
10. If the data set had been larger, it would have been interesting to explore the effect of interactions between the variables as well as the main effects.

11. All missing observations are also assigned to the residual category.
12. E. V. Sparer, *Employability and the Juvenile "Arrest" Record*. (New York: University Center for the Study of Unemployed Youth, 1966).
13. Several of these studies are reviewed in Friedman and Pappas [pp. 40-41]. See also John McKee *et al*, *Barriers to the Employment of Released Offenders*. (Elmore, Alabama: Rehabilitation Research Foundation, 1970), and G. M. Farkas, "Industrial Employer Attitudes toward Hiring Men with Criminal Records," *Personnel Administrator*, 6, No. 4, (July-August 1961).
14. If the probability of leaving unemployment in any given month was one-third (independent of how long the parolee had been unemployed already), then the mean duration of a spell of unemployment would be 3 months. In fact, the probability of leaving unemployment decreases as a spell of unemployment becomes longer; it can be shown mathematically that in this case the mean duration of a spell of unemployment is less than 3 months. See H. Kaitz [1971].
15. This statement is based on the assumption that the perceived improvement in licit opportunities is not coupled with an improvement in illicit opportunities and/or an increase in the marginal utility of illicit activity.
16. There is one exception. Parolees who held a satisfactory job in period 1 were more likely to recidivate in period 3 than those parolees who did not hold a satisfactory job in period 1.

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