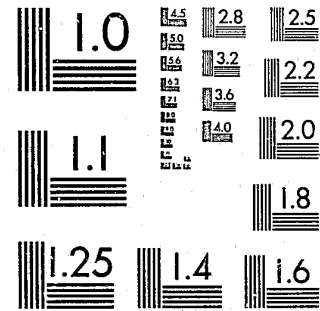


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United States Parole Commission

Federal Parole Decision - Making Selected Reprints

Volume IV

*United States
Parole Commission
Research Unit*

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October 1982

U. S. DEPARTMENT OF JUSTICE

FEDERAL PAROLE DECISION-MAKING
SELECTED REPRINTS

VOLUME IV.



UNITED STATES PAROLE COMMISSION RESEARCH UNIT

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U.S. Department of Justice
National Institute of Justice

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PREFACE

Included in this volume are five papers pertaining to parole decision-making. These papers originally appeared in various criminal justice publications. It is our intent in preparing this collection to bring together these resources in one volume for convenient reference.

U.S. Parole Commission Research Unit
October 1982

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MAY 16 1983

ACQUISITIONS

CONTENTS

1. "Presumptive Parole Dates: The Federal Approach", B. Stone-Meierhoefer and P.B. Hoffman (reprinted from Federal Probation, 1982) 7
2. "The Effects of Presumptive Parole Dates on Institutional Behavior: A Preliminary Assessment", B. Stone-Meierhoefer and P.B. Hoffman (reprinted from Journal of Criminal Justice, 1982)..... 24
3. "Females, Recidivism and Salient Factor Score: A Research Note", P.B. Hoffman (reprinted from Criminal Justice and Behavior, 1982)..... 39
4. Excerpts from The Clinical Prediction of Violent Behavior, J.P. Monahan (U.S. Department of Health and Human Services, National Institute of Mental Health, 1981)..... 44
5. "Employment, Community Treatment Center Placement, and Recidivism: A Study of Released Federal Offenders", J.L. Beck (reprinted from Federal Probation, 1981)..... 84

Presumptive Parole Dates: The Federal Approach

BY BARBARA STONE-MEIERHOEFER, Ph.D., AND PETER B. HOFFMAN, Ph.D.*

AN OVERVIEW of the goals and structure of the U.S. Parole Commission's approach to parole release decisionmaking is presented in this article; and the relationship of this approach to the philosophical concerns of equity and determinacy is highlighted.¹

The statute governing Federal parole release consideration sets forth the following criteria for parole release: (1) that an eligible prisoner has substantially observed the rules of the institution(s) to which he has been confined; (2) that release would not depreciate the seriousness of his offense or promote disrespect for the law; and (3) that release would not jeopardize the public welfare. In addition, the statute mandates the use of paroling policy guidelines as a flexible means of structuring decisionmaking discretion to provide equity among groups of similar offenders without removing the opportunity to consider individual case factors.²

In developing the guidelines and other rules and procedures to implement this statutory mandate, the Parole Commission has attempted to pursue three general objectives. First, the specific guidelines established must reflect the parole release criteria set forth in the statute. Second, to provide equity, the guidelines established must be sufficiently explicit and detailed to permit consistent decisionmaking among similarly situated offenders. Third, unnecessary uncertainty as to the date of the prisoner's eventual release is to be avoided through early notification of the tentative date of release from prison; yet this procedure must be flexible enough to permit modification of a release date should there be significant change in circumstances. The Parole Commission has attempted to reach these objectives through the development of a system of explicit guidelines for decisionmaking combined with a presumptive release date procedure.

*Dr. Stone-Meierhofer, research associate, Federal Judicial Center, was with the research unit of the U.S. Parole Commission when this article was written. Dr. Hoffman is research director, U.S. Parole Commission. Opinions expressed in this article do not represent statements of policy of either the Federal Judicial Center or its Board.

Setting a Presumptive Release Date

The first stage of the decisionmaking process takes place within 120 days after incarceration when almost all Federal prisoners are eligible for an initial parole hearing.³ At this hearing, the paroling policy guidelines are calculated, the case is assessed against these guidelines, and the prisoner is notified of a presumptive date of release.⁴

The Concern of Equity

The paroling policy guidelines calculated at the initial hearing are an important tool in the overall parole decisionmaking process (see Appendix A).⁵ These guidelines provide a scheme for classifying offenders into groups which are similar in relation to (1) the seriousness of the offense they committed; and (2) the risk of recidivism presented to society.

The determination of offense seriousness is accomplished with reference to a severity scale which classifies examples of common Federal offense behaviors into seven categories of seriousness. A "risk" of recidivism determination is then made with the aid of an actuarial device containing six items (primarily concerning prior criminal record) which, taken together, have been found to relate to the likelihood of recidivism. This device, known as the "salient factor score," is used to classify cases into one of four risk categories.⁶ The guideline matrix then sets forth a customary range of months to be served in prison

¹U.S. Parole Commission regulations are published at 28 C.F.R. §§2.1-2.60 (1981) (as amended by 46 *Federal Register* 35635-35640 (July 10, 1981)).

²18 U.S.C. §§4201 et seq. See particularly, 18 U.S.C. §§4203 and 4206.

³The procedures governing initial parole hearings are at 28 C.F.R. §2.12, 2.13 (1981). The exception to the early hearing provision of the presumptive date plan is that prisoners with minimum sentences of 10 years are not heard until just prior to their eligibility date. This stems from the regulation which prohibits the setting of a presumptive release date in excess of 10 years from the date of the hearing.

⁴The only prisoners not given a presumptive release date at the initial hearing are those for whom the Commission does not feel release within 10 years from the date of the hearing would be appropriate. These prisoners are continued for a 10-year reconsideration hearing at which all of the facts of the case are reevaluated against the guidelines calculated at the initial hearing for possible setting of a presumptive release date. It is to be noted that any presumptive release date must be set within the limits of the judicial sentence imposed (i.e., it may not be set earlier than the judicial minimum sentence, if any, nor later than the mandatory release date).

⁵28 C.F.R. §§2.20 and 2.21 as amended by 46 *Federal Register* 35635-35640 (July 10, 1981). For an overview of the development of the paroling policy guidelines, see: D.M. Gottfredson, L.T. Wilkins, and P.B. Hoffman, *Guidelines for Parole and Sentencing*, Lexington, MA: Lexington Books (1978).

⁶For a summary of the method used in construction and validation of the salient factor score, see: P.B. Hoffman and S. Adelberg, "The Salient Factor Score: A Non-Technical Overview," 44 *FEDERAL PROBATION* (1980) pp. 44-53.

for the applicable combination of offense severity and parole prognosis assessments. This guideline range presumes good institutional behavior.

Calculation of the appropriate guideline range is the necessary first step in decisionmaking; however, the actual release decision rests on an assessment of each individual prisoner's case against the guideline parameters. The guidelines do not prescribe the *only* factors which are to be taken into account at the initial parole hearing. Rather, they specify that offense severity and risk are primary factors that must be considered in every case. Other case-specific factors are then used to place the actual release decision at a point either within or outside of the appropriate guideline range. If the circumstances surrounding an individual case contain no substantial aggravating or mitigating circumstances, a decision within the guidelines will be chosen. If, however, there are significant aggravating or mitigating circumstances surrounding the case, departure from the guideline range—either above or below—would not only be permitted but would be called for.⁷ When aggravating or mitigating factors sufficient to warrant a decision outside of the guideline range are found, specific written reasons for the departure must be provided.⁸

The Concern of Determinacy

The outcome of the initial parole hearing is the setting of a presumptive date of release, the aim of which is to place a particular offender in fair relation to other prisoners with respect to offense severity and risk of recidivism. This presumptive release date may be set up to 10 years from the date of the hearing, and may be either a presumptive parole date or a decision to continue the prisoner to the expiration of his/her sentence less institutional good time (when the prisoner's sentence is not long enough to accommodate the particular time-served decision deemed appropriate).⁹ If confinement of more than 10 years is indicated, the prisoner will be rescheduled for a full reconsideration hearing in 10 years. Actual release upon the presumptive parole date is contingent upon maintenance of a good conduct record and development of an acceptable release plan.

⁷The U.S. Parole Commission's Procedures Manual (Appendix 4, Section V) provides examples of the more common factors which may warrant a decision outside the guidelines.

⁸18 U.S.C. 4206(c).

⁹When a prisoner is denied parole, this does not mean that he or she will serve the entire sentence imposed by the court in prison. Release from prison in the absence of parole is effected upon expiration of the full term sentence less statutory good time (up to 10 days a month) and earned extra good time (up to an additional 5 days a month). Upon release, a prisoner denied parole will be supervised as if on parole up to the full term date of the sentence (less 6 months).

¹⁰28 C.F.R. §2.14 (1981).

¹¹28 C.F.R. §2.28 (1981).

By making a presumptive release decision early in the parole process, the prisoner is given certainty to the extent that a release date has been set which cannot be taken away except for specified reasons. However, the early notification provision also has the effect of limiting the information considered in making this initial decision to that which is known at the time of commitment. Those relevant release factors which come to light during the course of imprisonment are the focus of subsequent reviews.

Modification of a Presumptive Release Date

After a presumptive release date is set, there are two types of regularly scheduled parole considerations. First, approximately 6 months prior to a previously set presumptive parole date, a record review is conducted to ascertain whether or not the conditions of the presumptive date (i.e., satisfactory conduct and an acceptable release plan) have been met. Second, by statute, prisoners are given an in-person parole review hearing every 18 months (prisoners with sentences of less than 7 years) or 24 months (prisoners with sentences of 7 years or more).¹⁰ In addition to these regularly scheduled considerations, there is provision for the reopening of a case at any time upon receipt of new and significant information.¹¹

Decisionmaking at these subsequent considerations focuses on whether there have been any changes in circumstances since the previous hearing significant enough to warrant a change in the presumptive release date. In designing the presumptive date process, the Commission has attempted to balance the sometimes competing aims of determinacy and equity. That is, allowance of any change in a presumptive date, by definition, decreases the certainty of the initial release decision. However, it is the Commission's position that offenders, through institutional behavior or other changes in circumstance, may need to be differentiated from those to whom they were considered "similar" when the presumptive date decision was originally made. It would therefore be inequitable if the previously set release date could not be adjusted to reflect these differences when they occur.

To consider all of the relevant information demanded by equity, while still eliminating unnecessary uncertainty, the Commission has specified the factors which may be relied upon to modify a presumptive date, and has established decisionmaking guidelines for the two most commonly occurring change factors: institutional misconduct and superior program achievement.

Postponement of a Presumptive Date

Disciplinary Infractions

As stated earlier, the Commission's paroling policy guidelines assume good conduct while confined. A record of serious disciplinary infractions does not meet this presumption, nor would it meet the statutory requirement that the Commission consider whether the prisoner has substantially obeyed the rules of the institution in which confined. Therefore, poor discipline is considered by the Commission as good cause to rescind a previously set presumptive parole date provided the infraction(s) has been adjudicated under the Bureau of Prisons's Institutional Disciplinary Committee procedures.¹²

The purpose of the Commission's rescission guidelines (see Appendix B)¹³ is to facilitate consistent decisionmaking in sanctioning rule infractions. These guidelines specify the customary period of prison time to be added to the original presumptive release date for prisoners who commit various types of disciplinary infractions. It should be noted that rescission guidelines only apply to those prisoners to whom the Commission has given a presumptive or effective date of parole. If the Commission has continued a prisoner to the expiration of his/her sentence less institutional good time, the sanctioning of disciplinary infractions is within the authority of the Bureau of Prisons which may take away the prisoner's institutional "good time" and, thus, delay the prisoner's mandatory release date.

The Parole Commission's rescission guidelines classify infractions into three categories. The first category, "administrative infractions," includes conduct prohibited by institutional rule, but which is not a criminal law violation.

A second category includes escape or attempted escape. The rescission penalty for escape depends on the type of institution from which the prisoner escaped and the length of time in escape status. The "escape" penalty is separate from that to be applied for any other criminal acts that may be committed during the escape.

A third category includes conduct which constitutes new criminal behavior (other than escape). A distinction is made as to whether the new criminal behavior occurred in an institutional setting, or whether it occurred while the prisoner was

¹²28 C.F.R. §2.34 (1981). The Bureau of Prisons may handle disciplinary infractions in an informal manner (through which only minor sanctions may be imposed) or may refer consideration of infractions to an Institutional Disciplinary Committee hearing. Only the latter method of adjudicating infractions provides the standard of due process required of the Parole Commission when considering rescission of a parole date.

¹³28 C.F.R. §2.36 (1981).

¹⁴28 C.F.R. §§2.12 (d) and 2.28 (e) (1981).

¹⁵28 C.F.R. §2.28 (c), (d), and (f) (1981).

actually in the community (e.g., on furlough, on work status from a community treatment center, or in escape status). In each case, the rescission penalty is determined by assessing the seriousness of the new criminal conduct using the severity scale of the paroling policy guidelines such that the more serious the rescission behavior, the more time is added to the previously set date. If the criminal behavior occurred while the prisoner was in the community, the risk of recidivism dimension (salient factor score) is also recalculated. The result is that the rescission guidelines for new offenses committed in the community call for the most additional prison time to be served.

Failure To Establish a Suitable Release Plan

A previously set presumptive parole date may also be retarded if the prisoner fails to establish an acceptable release plan. A release plan is initially proposed by the prisoner and his/her caseworker. It is then sent to the probation office located in the proposed area of release for verification of the details of the plan and the probation officer's recommendation as to its suitability. The plan is then submitted to the Parole Commission for approval. An acceptable plan will generally include a place to live and a place of employment (or schooling). If a prisoner has limited community resources, an effort is made to release such prisoner through a community treatment center (halfway house) for the purpose of obtaining employment and housing.

If an acceptable release plan has not been approved by the time of the parole date, the parole date may be retarded while efforts are made to secure an approved plan. If, after 120 days, the prisoner is still without an approved plan, a hearing must be held to discuss the problem and explore alternative solutions.¹⁴ The case must then be reviewed at least every 30 days so that continuing efforts to secure release can be monitored.

Prior to actual release, the prisoner must sign the release certificate agreeing to the general, and any special, conditions of parole supervision. Failure to sign this certificate will result in waiver of parole.

New Adverse Information

A presumptive parole date is given under the assumption that all relevant information is known and has been accurately presented to the Commission. If any significant information adverse to the prisoner comes to the attention of the Commission subsequent to the granting of a presumptive date, the case may be reopened for another hearing from which a more adverse parole decision may result.¹⁵

Advancement of a Presumptive Date

Superior Program Achievement

In addition to assuming good conduct, the paroling policy guideline range also assumes good program performance. However, where a prisoner subsequently demonstrates exceptional positive institutional achievements over a sustained period of time, the previously set presumptive date may be reduced according to a schedule of limited rewards under the superior program achievement guidelines (see Appendix C).¹⁶

The superior achievement guidelines provide a specified normal maximum limit (in months) by which a previously set presumptive date may be advanced. This maximum limit is purposely kept small so as not to reintroduce the gross uncertainty which the presumptive date system was designed to eliminate. Additionally, keeping the potential reward small reduces the likelihood of prisoners participating superficially in programs merely to impress the Parole Commission in the hope of obtaining a substantially earlier release date.¹⁷ By limiting the impact of positive institutional achievements, the Commission is also stating a philosophical position that, although positive institutional behavior is appropriately considered in making the ultimate release decision, considerations of offense severity and risk should remain primary.

The objective of the superior program achievement guidelines is to specify small but meaningful incentives to reward prisoners who choose to spend their prison time in an exceptionally constructive manner. While in some respects the superior program achievement guidelines represent the counterpart to the rescission guidelines, there are several important differences between the two.

First, the size of the potential reductions is tied to the total length of prison time to be served as established by the original presumptive date. This tie is important because the perceived size of the

reward is, of course, relative. For example, in light of the above discussion a potential 6-month reduction may seem an appropriate incentive for a prisoner with a 50-month presumptive date. But the same potential 6-month reduction would appear excessive for a prisoner with a 12-month presumptive date. This relationship between the superior program achievement guidelines and the length of time required by the original presumptive date is in contrast to the structure of the rescission guidelines, which set customary penalties for specific misconducts to be served in addition to, and independent of, the length of time required by the previously set presumptive date.

Second, the superior program achievement standards state that accomplishments in any area of activity or job performance can be considered, and that a clear conduct record is, in itself, not sufficient. The guidelines do not, however, describe the specific types of behavior which can be considered by the Commission to be "superior." To do this would be an overwhelming task because the definition of what is considered "superior program achievement" for one prisoner may not be considered "superior" for another. Prisoners enter the Federal prison system with a wide array of talents and weaknesses. To complete five college courses while in prison may not be that exceptional for a prisoner who already has a law degree. However, this accomplishment may indeed be considered "superior" for a prisoner who previously has been unable to succeed in the educational system.

Therefore, application of the superior program achievement guidelines involves a substantial amount of subjective judgment. Yet, by limiting the maximum amount of time to be awarded, potential inconsistency is minimized; and program participation, while encouraged, is not coerced.

Other Exceptional Circumstances

In addition, a previously set presumptive date may be reduced for other exceptional factors such as severely deteriorating health; isolated acts of unusual responsibility or courage, which though not "sustained" nonetheless merit recognition (e.g., helping others during a fire); or the receipt of new favorable information concerning the circumstances of the case not previously known.¹⁸

Summary

The U.S. Parole Commission's procedures have evolved over the years from a system which a decade ago made parole decisions on a case by case basis with essentially no structure, through a

¹⁶28 C.F.R. §2.60 (1981). It should also be noted that the Parole Commission does not punish lack of program achievement. The previously set presumptive date sets the outside release decision assuming the discipline and release plan conditions are met. However, a prisoner, while incarcerated, may not remain idle. In addition to elective activities, a prisoner is required to perform a job assignment within the prison. A prisoner's refusal to work may be cited as a disciplinary infraction. If serious enough to be referred by the Bureau of Prisons to an Institutional Disciplinary Committee, the Parole Commission may consider this behavior under its rescission guidelines for administrative infractions.

¹⁷While the Commission is cognizant of the potential for problems with "game-playing," it is believed that this potential is much reduced under the now published procedures of Federal parole decisionmaking which allocates only limited weight to institutional program participation. Furthermore, the atmosphere which gave rise to the initial critiques of consideration of program participation in release decision-making was one where prisoners were given virtually no idea of when they might be paroled until right before their actual release. In addition, no specific criteria were published and available as to how parole decisions were actually being made. This informational vacuum is not present in current Federal parole procedures.

¹⁸28 C.F.R. §§2.14 (a)(2)(ii) and 2.28 (a) (1981).

period where structure was developed for paroling decisions in the form of paroling policy guidelines, to the current system which combines the use of guidelines with the setting of presumptive dates.

Throughout this evolution, a change in one part of the process has allowed for or necessitated changes in other parts of the system. The development of guidelines based on preincarceration factors allowed the development of the presumptive date procedures. Presumptive date procedures, in turn, necessitated a restructuring of the process used to consider institutional (and other postincarceration) factors in the release decision.

The current Federal parole procedures have developed in a piecemeal fashion. Yet what has

emerged is a conceptually simple system which provides for the early setting of a tentative date of release based on factors known at the time of commitment (offense severity and risk assessment) with provision for the modification of that release date based on factors of significance which become known during the period of confinement (e.g., retardation for disciplinary infractions; advancement for exceptionally positive accomplishments). Furthermore, the system is designed to contain sufficient structure to provide consistent decision-making for similarly situated offenders, yet to be flexible enough to accommodate significant differences among individual offenders.

**APPENDIX A
GUIDELINES FOR DECISIONMAKING**

Effective 9/1/81

[Guidelines for Decisionmaking, Customary Total Time to be Served before Release (including jail time)]

OFFENSE CHARACTERISTICS: Severity of Offense Behavior (Examples)	OFFENDER CHARACTERISTICS: Parole Prognosis (Salient Factor Score 1981)			
	Very Good (10-8)	Good (7-6)	Fair (5-4)	Poor (3-0)
LOW Alcohol or Cigarette law violations, including tax evasion (amount of tax evaded less than \$2,000) ¹ Gambling law violations (no managerial or proprietary interest) Illicit drugs, simple possession Marihuana/hashish, possession with intent to distribute/sale [very small scale (e.g., less than 10 lbs. of marihuana/less than 1 lb. of hashish/less than .01 liter of hash oil)] Property offenses (theft, income tax evasion, or simple possession of stolen property) less than \$2,000	ADULT RANGE			
	<=6 months	6-9 months	9-12 months	12-16 months
			
	(YOUTH RANGE)			
	(<=6) months	(6-9) months	(9-12) months	(12-16) months

FEDERAL PROBATION
APPENDIX A (Continued)
GUIDELINES FOR DECISIONMAKING
 Effective 9/1/81

[Guidelines for Decisionmaking, Customary Total Time to be Served before Release (including jail time)]

OFFENSE CHARACTERISTICS: Severity of Offense Behavior (Examples)	OFFENDER CHARACTERISTICS: Parole Prognosis (Salient Factor Score 1981)			
	Very Good (10-8)	Good (7-6)	Fair (5-4)	Poor (3-0)
LOW MODERATE Counterfeit currency or other medium of exchange [(passing/possession) less than \$2,000] Drugs (other than specifically categorized), possession with intent to distribute/sale [very small scale (e.g., less than 200 doses)] Marihuana/hashish, possession with intent to distribute/sale [small scale (e.g., 10-49 lbs. of marihuana/1-4.9 lbs. of hashish/.01-.04 liters of hash oil)] Cocaine, possession with intent to distribute/sale [very small scale (e.g., less than 1 gram of 100% purity, or equivalent amount)] Gambling law violations—managerial or proprietary interest in small scale operation [e.g., Sports books (estimated daily gross less than \$5,000); Horse books (estimated daily gross less than \$1,500); Numbers bankers (estimated daily gross less than \$750)] Immigration law violations Property offenses (forgery/fraud/theft from mail/embezzlement/interstate transportation of stolen or forged securities/receiving stolen property with intent to resell) less than \$2,000	ADULT RANGE			
	<= 8 months	8-12 months	12-16 months	16-22 months
	(YOUTH RANGE)			
	(<= 8) months	(8-12) months	(12-16) months	(16-20) months

PRESUMPTIVE PAROLE DATES: THE FEDERAL APPROACH

APPENDIX A (Continued)
GUIDELINES FOR DECISIONMAKING
 Effective 9/1/81

[Guidelines for Decisionmaking, Customary Total Time to be Served before Release (including jail time)]

OFFENSE CHARACTERISTICS: Severity of Offense Behavior (Examples)	OFFENDER CHARACTERISTICS: Parole Prognosis (Salient Factor Score 1981)			
	Very Good (10-8)	Good (7-6)	Fair (5-4)	Poor (3-0)
MODERATE Automobile theft (3 cars or less involved and total value does not exceed \$19,999) ² Counterfeit currency or other medium of exchange [(passing/possession) \$2,000—\$19,999] Drugs (other than specifically categorized), possession with intent to distribute/sale [small scale (e.g., 200-999 doses)] Marihuana/hashish, possession with intent to distribute/sale [medium scale (e.g., 50-199 lbs. of marihuana/5-19.9 lbs. of hashish/.05-.19 liters of hash oil)] Cocaine, possession with intent to distribute/sale [small scale (e.g., 1.0-4.9 grams of 100% purity, or equivalent amount)] Opiates, possession with intent to distribute/sale [evidence of opiate addiction and very small scale (e.g., less than 1.0 grams of 100% pure heroin, or equivalent amount)] Firearms Act, possession/purchase/sale (single weapons: not sawed-off shotgun or machine gun) Gambling law violations—managerial or proprietary interest in medium scale operation [e.g., Sports books (estimated daily gross \$5,000-\$15,000); Horse books (estimated daily \$1,500-\$4,000); Numbers bankers (estimated daily gross \$750-\$2,000)] Property offenses (theft/forgery/fraud/embezzlement/interstate transportation of stolen or forged securities/income tax evasion/receiving stolen property) \$2,000-\$19,999 Smuggling/transporting of alien(s)	ADULT RANGE			
	10-14 months	14-18 months	18-24 months	24-32 months
	(YOUTH RANGE)			
	(8-12) months	(12-16) months	(16-20) months	(20-26) months

FEDERAL PROBATION
APPENDIX A (Continued)
GUIDELINES FOR DECISIONMAKING
 Effective 9/1/81

[Guidelines for Decisionmaking, Customary Total Time to be Served before Release (including jail time)]

OFFENSE CHARACTERISTICS: Severity of Offense Behavior (Examples)	OFFENDER CHARACTERISTICS: Parole Prognosis (Salient Factor Score 1981)			
	Very Good (10-8)	Good (7-6)	Fair (5-4)	Poor (3-0)
HIGH Carnal Knowledge ³ Counterfeit currency or other medium of exchange [(passing/possession) \$20,000-\$100,000] Counterfeiting [manufacturing (amount of counterfeit currency or other medium of exchange involved not exceeding \$100,000)] Drugs (other than specifically listed), possession with intent to distribute/sale [medium scale (e.g., 1,000-19,999 doses)] Marihuana/hashish, possession with intent to distribute/sale [large scale (e.g., 200-1,999 lbs. of marihuana/20-199 lbs. of hashish/.20-1.99 liters of hash oil)] Cocaine, possession with intent to distribute/sale [medium scale (e.g., 5-99 grams of 100% purity, or equivalent amount)] Opiates, possession with intent to distribute/sale [small scale (e.g., less than 5 grams of 100% pure heroin, or equivalent amount) except as described in moderate] Firearms Act, possession/purchase/sale (sawed-off shotgun(s), machine gun(s), or multiple weapons) Gambling law violations—managerial or proprietary interest in large scale operation (e.g., Sports books (estimated daily gross more than \$15,000); Horse books (estimated daily gross more than \$4,000); Numbers bankers (estimated daily gross more than \$2,000)) Involuntary manslaughter (e.g., negligent homicide)	ADULT RANGE			
	14-20 months	20-26 months	26-36 months	34-44 months
	(YOUTH RANGE)			
	(12-16) months	(16-20) months	(20-26) months	(26-32) months

PRESUMPTIVE PAROLE DATES: THE FEDERAL APPROACH

APPENDIX A (Continued)
GUIDELINES FOR DECISIONMAKING
 Effective 9/1/81

[Guidelines for Decisionmaking, Customary Total Time to be Served before Release (including jail time)]

OFFENSE CHARACTERISTICS: Severity of Offense Behavior (Examples)	OFFENDER CHARACTERISTICS: Parole Prognosis (Salient Factor Score 1981)			
	Very Good (10-8)	Good (7-6)	Fair (5-4)	Poor (3-0)
HIGH (Continued) Mann Act (no force—commercial purposes) Property offenses (theft/forgery/fraud/embezzlement/interstate transportation of stolen or forged securities/income tax evasion/receiving stolen property) \$20,000-\$100,000 Threatening communications (e.g., mail/phone)—not for purposes of extortion and no other overt act				
VERY HIGH Robbery (1 or 2 instances) Breaking and entering—armory with intent to steal weapons Breaking and entering/burglary—residence; or breaking and entering of other premises with hostile confrontation with victim Counterfeit currency or other medium of exchange [(passing/possession/manufacturing)/amount more than \$100,000 but not exceeding \$500,000] Drugs (other than specifically listed), possession with intent to distribute/sale [large scale (e.g., 20,000 or more doses) except as described in Greatest I] Marihuana/hashish, possession with intent to distribute/sale [very large scale (e.g., 2,000 lbs. or more of marihuana/200 lbs. or more of hashish/2 liters or more of hash oil)] Cocaine, possession with intent to distribute/sale [large scale (e.g., 100 grams or more of 100% purity, or equivalent amount) except as described in Greatest I]	ADULT RANGE			
	24-36 months	36-48 months	48-60 months	60-72 months

FEDERAL PROBATION
APPENDIX A (Continued)
GUIDELINES FOR DECISIONMAKING
 Effective 9/1/81

[Guidelines for Decisionmaking, Customary Total Time to be Served before Release (including jail time)]

OFFENSE CHARACTERISTICS: Severity of Offense Behavior (Examples)	OFFENDER CHARACTERISTICS: Parole Prognosis (Salient Factor Score 1981)			
	Very Good (10-8)	Good (7-6)	Fair (5-4)	Poor (3-0)
VERY HIGH (Continued) Opiates, possession with intent to distribute/sale [medium to a very large scale (e.g., 5 grams or more of 100% pure heroin, or equivalent amount) unless the offense is described in Greatest I or Greatest II] Extortion [threat of physical harm (to person or property)] Explosives, possession/transportation Property offenses (theft/forgery/fraud/embezzlement/interstate transportation of stolen or forged securities/income tax evasion/receiving stolen property) more than \$100,000 but not exceeding \$500,000	(YOUTH RANGE)			
	(20-26) months	(26-32) months	(32-40) months	(40-48) months
GREATEST I Aggravated felony (e.g., robbery: weapon fired or injury of a type normally requiring medical attention) Arson or explosive detonation [involving potential risk of physical injury to person(s) (e.g., premises occupied or likely to be occupied)—no serious injury occurred] Drugs (other than specifically listed), possession with intent to distribute/sale [managerial or proprietary interest and very large scale (e.g., offense involving more than 200,000 doses)] Cocaine, possession with intent to distribute/sale [managerial or proprietary interest and very large scale (e.g., offense involving more than 1 kilogram of 100% purity, or equivalent amount)]	(ADULT RANGE)			
	40-52 months	52-64 months	64-78 months	78-100 months

PRESUMPTIVE PAROLE DATES: THE FEDERAL APPROACH

APPENDIX A (Continued)
GUIDELINES FOR DECISIONMAKING
 Effective 9/1/81

[Guidelines for Decisionmaking, Customary Total Time to be Served before Release (including jail time)]

OFFENSE CHARACTERISTICS: Severity of Offense Behavior (Examples)	OFFENDER CHARACTERISTICS: Parole Prognosis (Salient Factor Score 1981)			
	Very Good (10-8)	Good (7-6)	Fair (5-4)	Poor (3-0)
GREATEST (Continued) Opiates, possession with intent to distribute/sale [managerial or proprietary interest and large scale (e.g., offense involving more than 50 grams but not more than 1 kilogram (1000 grams) of 100% pure heroin or equivalent amount)] Kidnaping [other than listed in Greatest II; limited duration; and no harm to victim (e.g., kidnaping the driver of a truck during a hijacking, driving to a secluded location, and releasing victim unharmed)] Robbery (3 or 4 instances) Sex act—force (e.g., forcible rape or Mann Act (force))	(YOUTH RANGE)			
	(30-40) months	(40-50) months	(50-60) months	(60-76) months
GREATEST II Murder Voluntary manslaughter Aggravated felony—serious injury (e.g., robbery: injury involving substantial risk of death or protracted disability, or disfigurement) or extreme cruelty/brutality toward victim Aircraft hijacking Espionage Kidnaping (for ransom or terrorism; as hostage; or harm to victim) Treason Opiates, possession with intent to distribute/sale [managerial or proprietary interest and very large scale (e.g., offense involving more than 1 kilogram (1000 grams) of 100% pure heroin or equivalent amount)]	(ADULT RANGE)			
	52+ months	64+ months	78+ months	100+ months
	(YOUTH RANGE)			
	(40+) months	(50+) months	(60+) months	(76+) months
	Specific upper limits are not provided due to the limited number of cases and the extreme variation possible within category.			

FEDERAL PROBATION

GENERAL NOTES

- A. These guidelines are predicated upon good institutional conduct and program performance.
- B. If an offense behavior is not listed above, the proper category may be obtained by comparing the severity of the offense behavior with those of similar offense behaviors listed.
- C. If an offense behavior can be classified under more than one category, the most serious applicable category is to be used.
- D. If an offense behavior involved multiple separate offenses, the severity level may be increased.
- E. In cases where multiple sentences have been imposed (whether consecutive or concurrent, and whether aggregated or not) an offense severity rating shall be established to reflect the overall severity of the underlying criminal behavior. This rating shall apply whether or not any of the component sentences has expired.

OTHER OFFENSES

- (1) Conspiracy shall be rated for guideline purposes according to the underlying offense behavior if such behavior was consummated. If the offense is unconsummated, the conspiracy will be rated one step below the consummated offense. A consummated offense includes one in which the offender is prevented from completion only because of the intervention of law enforcement officials.
- (2) Breaking and entering not specifically listed above shall normally be treated as a low moderate severity offense; however, if the monetary loss amounts to \$2,000 or more, the applicable property offense category shall be used. Similarly, if the monetary loss involved in a burglary or breaking and entering (that is listed) constitutes a more serious property offense than the burglary or breaking and entering itself, the appropriate property offense category shall be used.
- (3) Manufacturing of synthetic drugs for sale shall be rated as not less than very high severity.
- (4) Bribery of a public official (offering/accepting/soliciting) or extortion (use of official position) shall be rated as no less than moderate severity for those instances limited in scope (e.g., single instance and amount of bribe/demand less than \$20,000 in value);

and shall be rated as no less than high severity in any other case. In the case of the bribe/demand with a value in excess of \$100,000, the applicable property offense category shall apply. The extent to which the criminal conduct involves a breach of the public trust, therefore causing injury beyond that describable by monetary gain, shall be considered as an aggravating factor.

- (5) Obstructing justice (no physical threat)/perjury (in a criminal proceeding) shall be rated in the category of the underlying offense concerned, except that obstructing justice (threat of physical harm) shall be rated as no less than very high severity.
- (6) Misprision of felony shall be rated as moderate severity if the underlying offense is high severity or above. If the underlying offense is moderate severity or less, it shall be rated as low severity.
- (7) Harboring a fugitive shall be rated as moderate severity if the underlying offense is high severity or above. If the underlying offense is moderate severity or less, it shall be rated as low severity.

REFERENCED NOTES

- 1. Alcohol or cigarette tax law violations involving \$2,000 or more of evaded tax shall be treated as a property offense (tax evasion).
- 2. Except that automobile theft (not kept more than 72 hours; no substantial damage; and not theft for resale) shall be rated as low severity. Automobile theft involving a value of more than \$19,999 shall be treated as a property offense. In addition, automobile theft involving more than 3 cars, regardless of value, shall be treated as no less than high severity.
- 3. Except that carnal knowledge in which the relationship is clearly voluntary, the victim is not less than 14 years old, and the age difference between offender and victim is less than four years shall be rated as a low severity offense.

DEFINITIONS

- a. 'Other media or exchange' include, but are not limited to, postage stamps, money orders, or coupons redeemable for cash or goods.
- b. 'Drugs, other than specifically categorized' include, but are not limited to, the following, listed in ascending

PRESUMPTIVE PAROLE DATES: THE FEDERAL APPROACH

order of their perceived severity: amphetamines, hallucinogens, barbiturates, methamphetamines, phenylidene (PCP). This ordering shall be used as a guide to decision placement within the applicable guideline range (i.e., other aspects being equal, amphetamines will normally be rated towards the bottom of the guideline range and PCP will normally be rated towards the top).

- c. 'Equivalent amounts' for the cocaine and opiate categories may be computed as follows: 1 gm. of 100% pure is equivalent to 2 gms. of 50% pure and 10 gms. of 10% pure, etc.
- d. The 'opiate' category includes heroin,

morphine, opiate derivatives, and synthetic opiate substitutes.

- e. Managerial/Proprietary Interest (Large Scale Drug Offenses):

Managerial/proprietary interest in large scale drug cases is defined to include offenders who sell or negotiate to sell such drugs; or who have decision-making authority concerning the distribution/sale, importation, cutting, or manufacture of such drugs; or who finance such operations. Cases to be excluded are peripherally involved offenders without any decision-making authority (e.g., a person hired merely as a courier).

Salient Factor Score (SFS 81)

Register Number _____ Name _____

Item A: PRIOR CONVICTIONS/ADJUDICATIONS (ADULT OR JUVENILE).....

- None..... = 3
- One..... = 2
- Two or three..... = 1
- Four or more..... = 0

Item B: PRIOR COMMITMENT(S) OF MORE THAN THIRTY DAYS (ADULT OR JUVENILE)...

- None..... = 2
- One or two..... = 1
- Three or more..... = 0

Item C: AGE AT CURRENT OFFENSE/PRIOR COMMITMENTS.....

- Age at commencement of the current offense:
- 26 years of age or more..... = 2 ***
 - 20-25 years of age..... = 1 ***
 - 19 years of age or less..... = 0

***EXCEPTIONS: If five or more prior commitments of more than thirty days (adult or juvenile), place an "x" here _____ and score this item..... = 0.

Item D: RECENT COMMITMENT FREE PERIOD (THREE YEARS).....

- No prior commitment of more than thirty days (adult or juvenile) or released to the community from last such commitment at least three years prior to the commencement of the current offense..... = 1
- Otherwise..... = 0

FEDERAL PROBATION

Item E: PROBATION/PAROLE/CONFINEMENT/ESCAPE STATUS VIOLATOR THIS TIME

Neither on probation, parole, confinement, or escape status at the time of the current offense; nor committed as a probation, parole, confinement, or escape status violator this time = 1

Otherwise..... = 0

Item F: HEROIN/OPIATE DEPENDENCE.....

No history of heroin/opiate dependence..... = 1

Otherwise..... = 0

TOTAL SCORE.....

NOTE: For purposes of the Salient Factor Score, an instance of criminal behavior resulting in a judicial determination of guilt or an admission of guilt before a judicial body shall be treated as a conviction, even if a conviction is not formally entered.

Sec. 2.21 REPAROLE CONSIDERATION GUIDELINES.

(a) If revocation is based upon administrative violation(s) only [i.e., violations other than new criminal conduct] the following guidelines shall apply.

<i>Positive Supervision History: (Examples)</i>	<i>Customary Time to be Served Before Rerelease</i>
a. No serious alcohol/drug abuse and no possession of weapon(s) [and]	
b. At least 8 months from date of release to date of violation behavior [and]	< 6 Months
c. Present violation represents first instance of failure to comply with parole regulations of this term.	

Negative Supervision History: (Examples)

a. Serious alcohol/drug abuse (e.g., readdiction to opiates) or possession of weapon(s) [or]	6 - 9 Months
b. Less than 8 months from date of release to date of violation behavior [or]	
c. Repetitious or persistent violations.	

(b)(1) If a finding is made that the prisoner has engaged in behavior constituting new criminal conduct, the appropriate severity rating for the new criminal behavior shall be calculated. New criminal conduct may be determined either by a new federal, state, or local conviction or by an independent finding by the Commission at revocation hearing. As violations may be for state or local offenses, the appropriate severity level may be determined by analogy with listed federal offense behaviors.

(2) The guidelines for parole consideration specified at 28 C.F.R. Sec. 2.20 shall then be applied. The original guideline type (e.g., adult, youth) shall determine the applicable guidelines for the parole violator term, except that a violator committed with a new federal sentence of more than one year shall be treated under the guideline type applicable to the new sentence.

PRESUMPTIVE PAROLE DATES: THE FEDERAL APPROACH

(3) Time served on a new state or federal sentence shall be counted as time in custody for reparole guideline purposes. This does not affect the computation of the expiration date of the violator term as provided by Sections 2.47(b) and 2.52(c) and (d).

(c) The above are merely guidelines. A decision outside these guidelines (either above or below) may be made when circumstances warrant. For example, violations of an assaultive nature or by a person with a history of repeated parole failure may warrant a decision above the guidelines. Minor offense(s) (e.g., minor traffic offenses, vagrancy, public intoxication) shall normally be treated under administrative violations.

APPENDIX B

Sec. 2.36 RECISSION GUIDELINES.

(a) The following guidelines shall apply to the sanctioning of disciplinary infractions or new criminal behavior committed by a prisoner subsequent to the commencement of his sentence and prior to his release on parole. These guidelines specify the customary time to be served for such behavior which shall be added to the time required by the original presumptive or effective date. Credit shall be given towards service of these guidelines for any time spent in custody on a new offense that has not been credited towards service of the original presumptive or effective date. If a new concurrent or consecutive sentence is imposed for such behavior, these guidelines shall also be applied at the initial hearing on such terms.

(1) ADMINISTRATIVE RULE INFRACTION(S) (including drug/alcohol abuse) normally can be adequately sanctioned by postponing a presumptive or effective date by 0-60 days per instance of misconduct. Escape or other new criminal conduct shall be considered in accordance with the guidelines set forth below.

(2) ESCAPE/NEW CRIMINAL BEHAVIOR IN A PRISON FACILITY (including a Community Treatment Center). The time required pursuant to the guidelines set forth in (i) and (ii) below shall be added to the time required by the original presumptive or effective date.

(i) *Escape or Attempted Escape Without Force or Threat*

(A) Non-Secure Facility or Program (absent less than 7 days)	3-6 months
(B) Secure Facility (no force or threat used); or Non-Secure Facility or Program (absent 7 days or more)	6-12 months

Notes: (1) If other criminal conduct is committed during the escape or during time spent in escape status, then time to be served for the escape/attempted escape shall be added to that assessed for the other new criminal conduct.

(2) Time in escape status shall not be credited.

FEDERAL PROBATION

(ii) *Other New Criminal Behavior in a Prison Facility*

Severity Rating of the New Criminal Behavior (from § 2.20)	Adult Cases	Youth/NARA Cases
Low	<= 6 months	<= 6 months
Low Moderate	<= 8 months	<= 8 months
Moderate	10-14 months	8-12 months
High	14-20 months	12-16 months
Very High	24-36 months	20-26 months
Greatest I	40-52 months	30-40 months
Greatest II	52 + months	40 + months

(3) NEW CRIMINAL BEHAVIOR IN THE COMMUNITY (e.g., while on pass, furlough, work release, or on escape). In such cases, the guidelines applicable to parole violators under § 2.21 shall be applied, using the new offense severity (from § 2.20) and recalculated salient factor score (such score shall be recalculated as if the prisoner had been on parole at the time of the new criminal behavior). The time required pursuant to these guidelines shall be added to the time required by the original presumptive or effective date.

(b) The above are merely guidelines. Where the circumstances warrant, a decision outside the guidelines (above or below) may be rendered provided specific reasons are given. For example, a substantial period of good conduct since the last disciplinary infraction in cases not involving new criminal conduct may be treated as a mitigating circumstance.

APPENDIX C

Sec 2.60 SUPERIOR PROGRAM ACHIEVEMENT.

(a) Prisoners who demonstrate superior program achievement (in addition to a good conduct record) may be considered for a limited advancement of the presumptive date previously set according to the schedule below. Such reduction will normally be considered at an interim hearing or pre-release review. It is to be stressed that a clear conduct record is expected; this reduction applies only to cases with documented sustained superior program achievement over a period of 9 months or more in custody.

(b) Superior program achievement may be demonstrated in areas such as educational, vocational, industry, or counselling programs, and is to be considered in light of the specifics of each case.

(c) Upon a finding of superior program achievement, a previously set presumptive date may be advanced. The normal maximum advancement permissible for superior program achievement during the prisoner's entire term shall be as set forth in the following schedule. It is the intent of the Commission that the maximum be exceeded only in the most clearly exceptional cases.

(d) Partial advancements may be given [for example, a case with superior program achievement during only part of the term or a case with both superior program achievement and minor disciplinary infraction(s)]. Advancements may be given at different times; however, the limits set forth in the following schedule shall apply to the total combined advancement.

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(e) Schedule of Permissible Reductions for Superior Program Achievement.

Total months required by original presumptive date:	Permissible reduction
14 months or less	Not applicable.
15 to 22 months	Up to 1 month.
23 to 30 months	Up to 2 months.
31 to 36 months	Up to 3 months.
37 to 42 months	Up to 4 months.
43 to 48 months	Up to 5 months.
49 to 54 months	Up to 6 months.
55 to 60 months	Up to 7 months.
61 to 66 months	Up to 8 months.
67 to 72 months	Up to 9 months.
73 to 78 months	Up to 10 months.
79 to 84 months	Up to 11 months.
85 to 90 months	Up to 12 months.
91 plus months	Up to 13 months.

Plus up to 1 additional month for each 6 months or fraction thereof, by which the original date exceeds 96 months.

THE EFFECTS OF PRESUMPTIVE PAROLE DATES ON INSTITUTIONAL BEHAVIOR: A PRELIMINARY ASSESSMENT

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ABSTRACT

This article reports research conducted by the United States Parole Commission to examine whether the provision of greater determinacy in the setting of prison release dates, known as "presumptive parole," affects the frequency or nature of prison disciplinary infractions or program participation. An experimental design was used in which prisoners in an eligible pool of cases were randomly allocated to experimental (early notification of a "firm" parole date) and control (traditional procedure) groups.

INTRODUCTION

The United States Parole Commission presently functions under what is termed a "presumptive parole date" procedure. First adopted systemwide in September 1977,¹ this procedure provides that an eligible prisoner will be scheduled for an initial parole proceeding within 120 days of reception at a federal institution. After this proceeding, under explicit parole decision guidelines, the Parole Commission will set a presumptive date of release (either by parole or mandatory release) and notify the prisoner of this decision and of the reasons for it.² A presumptive parole date is a firm parole date that is contingent upon a satisfactory record of institutional conduct and the establishment of a suitable release plan.

A presumptive parole date may be retarded or rescinded only under specified procedures for a failure to meet the above conditions.³ Similarly, advancement of a presumptive date is restricted to exceptional cases with substantial intervening changes in circumstances and to a limited award schedule for "superior program achievement."⁴

Adoption of a presumptive parole date procedure is in accord with recent and persuasive commentary on the desirability of reducing the uncertainty associated with traditional parole practice in the determination of parole release dates.⁵ Briefly, this commentary argues that the uncertainty associated with traditional parole practice has adverse psychological consequences for prisoners, increases institutional tension, and encourages superficial participation in

BARBARA STONE-MEIERHOEFER and PETER B. HOFFMAN

institutional programs merely to impress the parole board. Providing early notification of a firm release date, it has been argued, is fairer and more humane, and will tend to decrease institutional tension, facilitate realistic release planning, and reduce prisoners' participation in institutional programs for manipulative reasons. Furthermore, such early notification will assist correctional staff in making classification and programming decisions and will enhance management planning by increasing the accuracy of population forecasting.

Before considering the adoption of a presumptive parole date plan for full-scale implementation, the Parole Commission desired the benefit of experience with a pilot project. Therefore, in April 1976, the commission, with the cooperation of the Bureau of Prisons, began a pilot project in one of its five regions to examine the consequences of implementing a presumptive date procedure.⁶ While this pilot project was designed to facilitate research concerning the effects of presumptive dates on institutional discipline and program participation, the overwhelmingly favorable response of both prisoners and correctional staff to the pilot program, combined with the absence of any visible adverse consequences, led to full-scale implementation before the completion of the research analysis.⁷ That the perceived benefits warranted early implementation does not reduce the need, however, for a careful examination of its effects.

Given the nature of the research design, which included random allocation of cases from an eligible pool to experimental (cases given presumptive dates) and control (cases treated under standard procedure) groups, assessment of the granting of presumptive dates on institutional discipline is relatively straightforward. Examination can be made of the frequency and seriousness of disciplinary infractions between the two groups, both before and after the parole decision. If there is not a significantly greater frequency or seriousness of disciplinary infractions among the presumptive date cases, a finding of no visible adverse impact may be made.⁸

Examination of the impact of presumptive

dates on institutional program participation is considerably more difficult. While frequency of program enrollments, withdrawals, and completions may be measured, there are presently no readily applicable measures of program quality, or of quality of prisoner participation. A shift in program participation from therapeutic counselling to industry, for example, may indicate that a prisoner, assured of his or her parole date, is endeavoring to earn money to facilitate transition to the community. An overall decrease in program participation may indicate a return to a "custodial mentality" on the part of prisoners and prison staff. Or it may indicate that prisoners are selecting what they perceive as the higher quality programs, and that removing the "need" to impress the parole board by a showing of "rehabilitative effort" frees prisoners to ignore or drop programs found less interesting or pertinent. Given the dearth of evidence regarding the effectiveness of various prison programs, or of differential program effectiveness by prisoner type (e.g., by needs, abilities, or personalities), quality of participation, or perceptions of coercion, it would be premature to draw conclusions as to whether particular shifts in program participation are desirable, undesirable, or irrelevant. Therefore, the examination of program participation in this study must be treated as exploratory.

METHODOLOGY

This experimental project was initiated in the U.S. Parole Commission's Western Region. One of five commission regions, the Western Regional Office is located in Burlingame, California. This region encompasses 14 states and, at the time of this project, included eight federal institutions.

Sample

Beginning April 28, 1976, and continuing through August 31, 1977, a prisoner being considered for parole by the Western Regional Office was eligible to be included in

the pool of project cases provided he or she met the following criteria:

1. *Type of Hearing:* The prisoner was being considered for parole at an "initial" or at a "one-third" hearing.
2. *Type of Decision:* The prisoner was to be reconsidered for release at a time from 8 to 17 months following this hearing; the decision represented agreement between the recommendation of the hearing examiner panel and the Regional Commissioner; and such decision was within the parole guidelines.⁹

Cases included in the pool were identified by regional staff after the decision had been made, but before the prisoner had been formally notified of that decision in writing.¹⁰ Names and prison identification numbers of these cases were teletyped twice weekly to the research unit in the commission's central office. Research staff assigned the cases randomly to either the experimental (presumptive date) or control (regular procedure) group and then teletyped the designation back to the region. The group assignment yielded 112 experimental cases and 112 control cases.¹¹

Procedures

Commission procedure at the time of the project mandated that, if a parole date more than 6 months but not more than 18 months away was considered appropriate, the prisoner would be "continued for a regular review hearing" 30 to 60 days before the desired release date. The standard order sent by the commission to the prisoner (and the one sent to all control cases in the study) read, "Continue for a regular review hearing in [for example] August, 1977." While prisoners scheduled for regular review hearings could, in the absence of disciplinary infractions, expect a parole grant to result from this hearing, this was not made explicit. Moreover, occasionally prisoners were given further continuances despite acceptable institutional conduct when subsequent reviewers

substantially disagreed with the reasoning of the previous panel.

In contrast, the pilot project provided a firm release date for experimental cases. The prisoner was explicitly told that this date would be set back only for institutional misconduct. The Parole Commission order to the experimental cases read, "Continue for a regular review hearing in [for example] August, 1977. Provided you maintain a clear conduct record, your case will be reviewed by progress report during the preceding month and you will be granted parole effective September 15, 1977. If, however, it appears that you have not satisfactorily observed the institution rules, your review will be conducted as scheduled." In each experimental case, the parole date was set for the 15th of the month following the originally scheduled regular review hearing.

Data Collection

Three categories of data items were collected for each study case in the following manner. First, background items, including offense severity rating, risk score, age, sex, months in custody prior to hearing, date of hearing, group assignment, institution, date of next parole review, and date of community treatment center (halfway house) placement or release to community were coded by research staff from the regional Parole Commission file.

Second, data on discipline were coded by research staff from the regional Parole Commission file. Type, date, and disposition of infractions were recorded. Disciplinary infractions were divided into "major" and "other" categories. An infraction was deemed "major" if it appeared to represent an instance of new criminal conduct (e.g., assault, introduction of drugs, escape) and resulted in a sanction of punitive segregation, punitive transfer, or loss of "good time." Classifying in this manner is believed appropriate because some infractions (e.g., fighting) cover a wide range of behaviors (from a serious assault to a minor scuffle). Therefore, designating as "major" only those infractions which appeared to be of a

criminal nature and for which a substantial sanction was imposed provided a fairly strict definition. Lesser disciplinary infractions, including simple drug or alcohol use, were considered as "other."

Third, program participation data were obtained from the Bureau of Prisons's computerized Inmate Program Reporting Systems (IPRS).¹² Programs were categorized as educational, vocational, counseling, or industries. Recreational programs and participation in certain other activities not clearly falling into a rehabilitative category were excluded for purposes of this project.¹³ Data collected included the number of programs in which a prisoner enrolled by category, the date of enrollment and completion or withdrawal for each program, and the reason for withdrawal.

Discipline and programming data were then categorized as occurring either before or after the hearing at which the experimental/control assignment was made. For post assignment items, data were collected for the period ending with either (1) the date of the next parole review, or (2) the date of removal from the main prison system (via release to a halfway house, escape, or court order). The provision for ending the follow-up period upon placement in a halfway house was necessitated because it was not possible to obtain comparable information on discipline or programming once this transfer was effected.

ANALYSIS

A comparison of the experimental and control groups indicated that there were no significant differences in age, salient factor score, severity rating, months in custody before group assignment, or months from assignment to the date of next parole review. However, there was a difference between the groups in the number of months from group assignment to the end of the data collection period, due primarily to a differential halfway house assignment rate for the two groups.

Not all of the sample cases remained

incarcerated in the main prison system until their next anticipated parole review. Thirty-seven experimental group members and 18 control group members were released to a halfway house before their next parole review. Seven experimental group members and 4 control group members escaped from custody after their first hearing, but before their next scheduled parole review. An additional 4 experimental group members and 4 control group members were released by court order. One control group member reached mandatory release through the earning of extra good time 2 months before his anticipated parole review, and one had his case reopened by the commission and his parole review advanced, as did one experimental group member. All of these cases are included in the sample and are tracked to the date of their release (voluntary or otherwise) from prison. Overall, experimental cases averaged 8.86 months from the date of group assignment to the date of the end of the data collection period, while the control cases averaged 9.71 months ($t = -2.03, p < .05$). Months in custody before group assignment averaged 12.21 for the experimental cases and 10.36 for the control cases ($t = .43, n.s.$).¹⁴

The commission anticipated that, given the experimental cases' firm release dates (at the time a practical consideration for halfway house referral by the Bureau of Prisons), the bureau might transfer such cases to halfway houses earlier and at a higher rate. Therefore at the beginning of the study the commission requested that the bureau not transfer experimental group members to halfway houses before the scheduled parole review. However, during November 1976, it was discovered that, through a change in regional bureau practice, control cases were being afforded early transfer. Attempting to retain equal treatment for both groups, the commission then asked the bureau to remove the prohibition against transferring experimental group members to halfway houses. Nonetheless, the initial concern appears to have been justified: experimental cases were sent to halfway houses significantly more often than

were the control cases—most likely a consequence of their firmer release dates.

Given this difference, statistical control for the variable "exposure period after group assignment" was exercised on all major analyses. All means comparison data were analyzed using this variable as a covariate for post-hearing means. Similarly, "months in custody before hearing" was covaried for prehearing means on all comparisons.

Chi square frequency data were adjusted to reflect the number of persons who would have been expected to be involved in programs or discipline problems had the months in custody for the two groups been the same.¹⁵ The rate of infraction or program involvement was determined by the following formulas:

$$\text{Rate (Infractions)} = \frac{\text{Number of prisoners with one or more infractions}}{\text{Number of prisoners in group} \times \text{Months served by group}}$$

$$\text{Rate (Programs)} = \frac{\text{Number of prisoners enrolling in one or more programs}}{\text{Number of prisoners in group} \times \text{Months served by group}}$$

Multiplying this rate by the average prisoner-months for the combined experimental and control groups produces the adjusted frequencies.¹⁶

Discipline

To examine whether the presumptive parole date procedures had an effect on institutional discipline, a number of analyses were conducted.

Number of Prisoners with One or More Disciplinary Infractions. During the period of incarceration prior to group assignment (Period I), 35 percent of the experimental group and 30 percent of the control group had sustained at least one disciplinary infraction ("major" or "other"). During the period from group assignment to next parole review or release from the main prison

system (Period II), 38 percent of the experimental group and 45 percent of the control group sustained at least one infraction. Chi square analyses of both the raw and adjusted frequency distributions (see Table 1A) indicate that none of these differences is significant.

Number of Disciplinary Infractions Committed. There is also the question of whether or not there might be a differential between the groups in the total number of disciplinary infractions committed. The raw data, presented in Table 1B, showed an average of less than one infraction for each group member during Period I (.9 for experimental, .7 for control group members). During Period II, the experimental group average remained stable (.9), while that for the control group increased (1.3). *T*-test analysis showed that neither the Period I nor Period II differences between the groups were significant. Covariance analysis of the means adjusted for months in custody yielded the same results.

Number of Prisoners with One or More "Major" and "Other" Infractions. When infractions are classified as "major" or "other," 17 percent of the experimental group and 7 percent of the control group committed a "major" infraction during Period I. During Period II, the percentages were 20 percent and 24 percent respectively. The overall chi square for the raw data was significant (see Table 1C). However, when these data were adjusted for person-months, the chi square analysis of the adjusted data was no longer significant. Therefore, the difference between the groups seems to be primarily attributable to their differential months in custody. The analyses of the "other" disciplinary infractions showed no significant differences.

Number of "Major" and "Other" Infractions. Period I *t*-test analysis of the raw data yielded a significant difference between the experimental and control groups in the number of major infractions (see Table 1D), but no significant difference emerged from the *f*-test analysis of these means adjusted for months in custody. There were no differences on either raw or adjusted

TABLE 1
DISCIPLINARY INFRACTIONS

A. Number of Prisoners with One or More Major or Other Infractions

Group	Raw Data		Data Adjusted for Months in Custody					
	Period I		Period II					
	Actual	Expected	Actual	Expected				
Experimental*	39	(35.8)	43	(46.2)	35	(34.7)	45	(45.3)
Control*	34	(37.2)	51	(47.8)	37	(37.3)	49	(48.7)
	$\chi^2 = 1.00$ (n.s.)†				$\chi^2 = .009$ (n.s.)			

B. Total Number of Major or Other Infractions

Group	Raw Data		Data Adjusted for Months in Custody					
	Period I		Period II					
	Mean	Number	Mean	Number				
Experimental	.92	(103)	.88	(99)	.87		.94	
Control	.68	(76)	1.32	(148)	.73		1.26	
	$t = 1.05$ (n.s.)		$t = -1.83$ (n.s.)		$f = .40$ (n.s.)		$f = 1.39$ (n.s.)	

C. Number of Prisoners with One or More Major Infractions

Group	Raw Data		Data Adjusted for Months in Custody					
	Period I		Period II					
	Actual	Expected	Actual	Expected				
Experimental	19	(14.6)	22	(26.4)	17	(13.9)	23	(26.1)
Control	8	(12.4)	27	(22.6)	9	(12.1)	26	(22.9)
	$\chi^2 = 4.48$ ($p < .05$)				$\chi^2 = 2.27$ (n.s.)			

D. Number of Major Infractions

Group	Raw Data		Data Adjusted for Months in Custody					
	Period I		Period II					
	Mean	Number	Mean	Number				
Experimental	.27	(30)	.24	(27)	.25		.25	
Control	.10	(11)	.31	(35)	.11		.30	
	$t = 2.04$ ($p < .05$)		$t = -.88$ (n.s.)		$f = 2.97$ (n.s.)		$f = .43$ (n.s.)	

TABLE 1 (continued)

E. Number of Prisoners with One or More "Other" Infractions

Group	Raw Data				Data Adjusted for Months in Custody			
	Period I		Period II		Period I		Period II	
	Actual	Expected	Actual	Expected	Actual	Expected	Actual	Expected
Experimental	34	(31.4)	32	(34.6)	31	(30.9)	34	(34.1)
Control	34	(36.6)	43	(40.4)	37	(37.1)	41	(40.9)
	$\chi^2 = .76$ (n.s.)				$\chi^2 = .001$ (n.s.)			

F. Number of "Other" Infractions

Group	Raw Data				Data Adjusted for Months in Custody	
	Period I		Period II		Period I	Period II
	Mean	Number	Mean	Number	Mean	Mean
Experimental	.70	(78)	.63	(71)	.65	.69
Control	.60	(67)	1.07	(120)	.64	1.02
	$t = .49$ (n.s.)		$t = -1.85$		$f = 1.91$ (n.s.)	

*There were 112 prisoners in the experimental group, and 112 prisoners in the control group.
†n.s. = not significant at the .05 level of confidence.

Period II means. "Other" infractions did not differentiate the groups either during Period I or Period II (see Table 1F).

Overview of Discipline Analyses

The above analyses indicate that granting presumptive parole dates did not appear to have an adverse effect on discipline within the institution for the cases included in this project. None of the analyses significantly differentiated the groups when "months of exposure" was controlled, and what discernible trend there was pointed to the experimental cases experiencing less difficulty with discipline than the control cases.

Programming

Program data are somewhat more complicated to examine. Results are first presented for "total programs," defined as the sum of all educational, vocational training, counseling, or industry programs in which a prisoner participated. Participation is measured

by both program enrollment (i.e., those new programs started) during Periods I and II, and by the percentage of those programs which were dropped voluntarily by the prisoner. During Period I, the voluntary drop percentage is compared with the percentage of those programs enrolled in during Period I which were either otherwise terminated (i.e., completed, inmate transferred/released, or removed from program nonvoluntarily, program discontinued), or continued into Period II. For Period II, the voluntary drop percentage is compared with the percentage of those programs enrolled in during Period II or continued from Period I that were either otherwise terminated or in which the prisoner was still enrolled at the end of Period II. Results on enrollments and voluntary program drops are then presented for industries, educational, vocational training, and counseling programs separately.

Number of Prisoners Enrolling in At Least One Type of Program. During Period I, 95 percent of the experimental group and 87 percent of the control group enrolled in

at least one type of program. The figure on new programs enrolled in decreased during Period II for both groups, with 62 percent of the experimental group and 74 percent of the control group enrolling in at least one new program. The chi square analyses of both the raw and adjusted frequency data showed no differences between the groups (see Table 2A).

Number of Programs/Industries Enrolled In. During Period I, the experimental group cases enrolled in a total of 436 programs as compared to 407 total programs for the controls. During Period II, the experimentals enrolled in 193 new programs compared to 315 for the controls. A *t*-test comparison of the Period I means proved nonsignificant, while comparison of the Period II means indicated that the experimentals enrolled in significantly fewer new programs than did the controls (see Table 2B). The *f* statistic generated as a result of the covariance analysis of the Period II data also proved significant.

Percentage of Programs Voluntarily Dropped. Of those programs in which group members were involved, the voluntary drop rates for the two groups were similar (see Table 2C). During Period I, experimental group members voluntarily dropped 7.3 percent of the programs in which they were enrolled as compared to 8.6 percent for the control group. During Period II, the percentages were again close, though the order was reversed, with the experimental group members voluntarily dropping 10.1 percent and the control group members dropping 8.5 percent of the programs in which they had either enrolled during that time period or had continued from Period I. Statistical analysis showed no significant differences.

Type of Program. Table 2, Parts D through O, present statistics on program enrollment and voluntary drops for education, vocational training, counseling, and industries separately. All analyses concerning the number of prisoners enrolled in at least one new program showed no significant difference between groups. However, when considering the number of programs in which these people enrolled, a significant

difference was found for education programs. During Period II, the experimentals enrolled in significantly fewer new educational programs than did the controls. The analyses on number of programs for the other program types did not significantly differentiate the groups, though there was a consistent trend for the experimental group members to enroll in slightly fewer programs than the control group members in each program type during Period II. Similarly, no significant difference between the groups was found for "voluntary program drops"; however, during Period II the experimental group members appeared to drop a slightly larger proportion of programs in each program category than did the control group members.

LIMITATIONS

There are a number of limitations that warrant caution in attempting to generalize the findings of this research either to the presumptive date procedures as adopted systemwide or to other parole systems. First, prison staff and prisoners were informed of the pilot project procedures,¹⁷ and it is reasonable to believe that the experimental group cases, in particular, would have been aware that they were included in a special project.¹⁸ Since experimental and control group cases were housed together, it is likely that there was some communication among them as to parole status. The comparisons in this research are based upon the assumption that neither of the above conditions significantly affected the results. It is also assumed that institutional staff, cognizant of the experimental group members' parole dates, did not redefine their expectations toward these prisoners so as to produce fewer or more disciplinary reports, or heavier or lighter sanctions for disciplinary infractions.

Second, given the criteria for inclusion in the sample pool, the project cases, though randomly assigned to groups, did not represent a random sample of all prisoners.¹⁹ Further, the project sample procedures also

TABLE 2
PROGRAM PARTICIPATION

A. Number of Prisoners Starting at Least One New Program								
Group	Raw Data				Data Adjusted for Months in Custody			
	Period I		Period II		Period I		Period II	
	Actual	Expected	Actual	Expected	Actual	Expected	Actual	Expected
Experimental	106	(100.3)	69	(74.7)	98	(98.2)	73	(72.8)
Control	98	(103.7)	83	(77.3)	107	(106.8)	79	(79.2)
	$\chi^2 = 1.49$ (n.s.)				$\chi^2 = .002$ (n.s.)			

B. Number of New Programs Started								
Group	Raw Data				Data Adjusted for Months in Custody			
	Period I		Period II		Period I		Period II	
	Mean	Number	Mean	Number	Mean	Number	Mean	Number
Experimental	3.89	(436)	1.72	(193)	3.75		1.86	
Control	3.63	(407)	2.81	(315)	3.77		2.67	
	$t = .60$ (n.s.)		$t = -3.01$ ($p < .01$)		$f = 0.00$ (n.s.)		$f = 5.77$ ($p < .05$)	

C. Percentage of Programs Voluntarily Dropped				
Group	Period I		Period II	
	Voluntarily Dropped	Otherwise Terminated/Continued	Voluntarily Dropped	Otherwise Terminated/Still Enrolled
Experimental	7.3	92.7	10.1	89.9
	$(n = 436)$		$(n = 394)$	
Control	8.6	91.4	8.5	91.5
	$(n = 407)$		$(n = 542)$	
	$\chi^2 = .66$ (n.s.)		$\chi^2 = .76$ (n.s.)	

D. Number of Prisoners Starting at Least One New Education Program								
Group	Raw Data				Data Adjusted for Months in Custody			
	Period I		Period II		Period I		Period II	
	Actual	Expected	Actual	Expected	Actual	Expected	Actual	Expected
Experimental	80	(73.9)	44	(50.1)	74	(71.8)	46	(48.2)
Control	72	(78.1)	59	(52.9)	78	(80.2)	56	(53.8)
	$\chi^2 = 2.43$ (n.s.)				$\chi^2 = .32$ (n.s.)			

TABLE 2 (continued)

E. Number of New Education Programs Started						
Group	Raw Data				Data Adjusted for Months in Custody	
	Period I		Period II		Period I	Period II
	Mean	Number	Mean	Number	Mean	Mean
Experimental	2.20	(246)	0.84	(94)	2.11	0.92
Control	1.71	(192)	1.60	(179)	1.80	1.51
	$t = 1.49$ (n.s.)		$t = -2.92$ ($p < .01$)		$f = 1.04$ (n.s.)	
					$f = 5.59$ ($p < .05$)	

F. Percentage of Education Programs Voluntarily Dropped				
Group	Period I		Period II	
	Voluntarily Dropped	Otherwise Terminated/Continued	Voluntarily Dropped	Otherwise Terminated/Still Enrolled
Experimental	9.8	90.2	11.7	88.3
	$(n = 246)$		$(n = 171)$	
Control	12.5	87.5	10.0	90.0
	$(n = 192)$		$(n = 270)$	
	$\chi^2 = .86$ (n.s.)		$\chi^2 = .33$ (n.s.)	

G. Number of Prisoners Starting at Least One New Vocational Training Program								
Group	Raw Data				Data Adjusted for Months in Custody			
	Period I		Period II		Period I		Period II	
	Actual	Expected	Actual	Expected	Actual	Expected	Actual	Expected
Experimental	50	(50.9)	37	(36.1)	46	(49.7)	39	(35.3)
Control	50	(49.1)	34	(34.9)	54	(50.3)	32	(35.7)
	$\chi^2 = .08$ (n.s.)				$\chi^2 = 1.32$ (n.s.)			

H. Number of New Vocational Training Programs Started						
Group	Raw Data				Data Adjusted for Months in Custody	
	Period I		Period II		Period I	Period II
	Mean	Number	Mean	Number	Mean	Mean
Experimental	.56	(63)	.45	(51)	.54	.47
Control	.73	(82)	.55	(62)	.75	.53
	$t = -1.4$ (n.s.)		$t = -.76$ (n.s.)		$f = 3.18$ (n.s.)	
					$f = .19$ (n.s.)	

TABLE 2 (continued)

M. Number of Prisoners Starting at Least One New Industry Program

Group	Raw Data		Data Adjusted for Months in Custody					
	Period I		Period II		Period I		Period II	
	Actual	Expected	Actual	Expected	Actual	Expected	Actual	Expected
Experimental	33	(27.0)	14	(20.0)	30	(25.6)	15	(19.4)
Control	17	(23.0)	23	(17.0)	19	(23.4)	22	(17.6)
	$\chi^2 = 6.82 (p < .01)$				$\chi^2 = 3.68 (n.s.)$			

N. Number of New Industry Programs Started

Group	Raw Data		Data Adjusted for Months in Custody					
	Period I		Period II		Period I		Period II	
	Mean	Number	Mean	Number	Mean	Number	Mean	Number
Experimental	.32	(35)	.12	(14)	.30		.13	
Control	.20	(23)	.22	(25)	.22		.21	
	$t = 1.58 (n.s.)$		$t = -1.83 (n.s.)$		$f = 1.31 (n.s.)$		$f = 2.35 (n.s.)$	

O. Percentage of Industry Programs Voluntarily Dropped

Group	Period I		Period II	
	Voluntarily Dropped	Otherwise Terminated/Continued	Voluntarily Dropped	Otherwise Terminated/Still Enrolled
Experimental	0	100.0	4.9	95.1
Control	4.3	95.7	3.0	97.0
	$\chi^2 = .05 (n.s.)$ (Yates Correction used)		$\chi^2 = .04 (n.s.)$ (Yates Correction used)	

TABLE 2 (continued)

I. Percentage of Vocational Training Programs Voluntarily Dropped

Group	Period I		Period II	
	Voluntarily Dropped	Otherwise Terminated/Continued	Voluntarily Dropped	Otherwise Terminated/Still Enrolled
Experimental	11.1	88.9	18.7	81.3
Control	10.9	89.1	13.7	86.3
	$\chi^2 = .001 (n.s.)$		$\chi^2 = .79 (n.s.)$	

J. Number of Prisoners Starting at Least One New Counseling Program

Group	Raw Data		Data Adjusted for Months in Custody					
	Period I		Period II		Period I		Period II	
	Actual	Expected	Actual	Expected	Actual	Expected	Actual	Expected
Experimental	57	(56.6)	27	(27.4)	53	(55.2)	28	(25.8)
Control	63	(63.4)	31	(30.6)	69	(66.8)	29	(31.2)
	$\chi^2 = .02 (n.s.)$				$\chi^2 = .50 (n.s.)$			

K. Number of New Counseling Programs Started

Group	Raw Data		Data Adjusted for Months in Custody					
	Period I		Period II		Period I		Period II	
	Mean	Number	Mean	Number	Mean	Number	Mean	Number
Experimental	.81	(91)	.30	(34)	.80		.33	
Control	.98	(110)	.44	(49)	.99		.41	
	$t = -1.20 (n.s.)$		$t = -1.31 (n.s.)$		$f = 1.86 (n.s.)$		$f = .74 (n.s.)$	

L. Percentage of Counseling Programs Voluntarily Dropped

Group	Period I		Period II	
	Voluntarily Dropped	Otherwise Terminated/Continued	Voluntarily Dropped	Otherwise Terminated/Still Enrolled
Experimental	1.1	98.9	3.8	96.2
Control	0.9	99.1	3.5	96.5
	$\chi^2 = .34 (n.s.)$ (Yates Correction used)		$\chi^2 = .04 (n.s.)$ (Yates Correction used)	

differ from those adopted systemwide. The longest presumptive date assigned in the pilot project was set at 18 months from the date of the hearing. In contrast, under current presumptive date procedures, dates may be set up to ten years from the date of the hearing.

Third, it is to be noted that parole practices may be placed along a continuum of certainty with "traditional" parole procedures (neither explicit norms nor pre-

sumptive dates) at one end and "presumptive date" procedures at the other. Since the adoption of decision guidelines, which were phased in between 1972 and 1974, the U.S. Parole Commission has moved away from the traditional model. Thus, the comparison in this research is between a parole procedure with explicit decision standards but without presumptive dates, and a procedure with both explicit standards and presumptive dates. It is not a

comparison that included a traditional parole model.

SUMMARY AND IMPLICATIONS

The primary finding is that the prisoners given presumptive parole dates did not commit disciplinary infractions any more frequently than those in the control group, nor were differences observed in the seriousness of the infractions recorded. There is a slight indication that those with presumptive release dates may have had fewer disciplinary problems. This finding addresses the concern originally expressed by a number of commissioners and correctional personnel that the presumptive date plan might somehow produce the unanticipated consequence of a higher rate of disciplinary infractions.

In addition, the data suggest that those with presumptive dates enrolled in somewhat fewer programs—particularly education programs—than control group cases and appeared to drop a slightly higher percentage of the programs in which they enrolled. However, there appeared to be no difference between the groups as to the percentage of group members enrolling in at least one program. This may be an indication that prisoners with presumptive dates are concentrating on particular programs of interest rather than on building lengthy lists of program completions for the purpose of impressing the parole board.

The lack of dramatic change in programming behavior is not really surprising. While it seems clear that under traditional parole practice, some prisoners participated in programs in an attempt to manipulate the parole board, it is also likely that others participated in programs for the education, skills, or insight that such programs offered. Additionally, programming is likely to be viewed by some as a way of reducing boredom; or, given that prisoners are not allowed to remain idle all day, as a way of spending prison time in as pleasant a manner as possible. The choice to participate in programs has probably always been, and

will continue to be, influenced in varying degrees by all of these factors.

Furthermore, while the provision of presumptive parole dates was designed to remove the gross uncertainty associated with traditional parole practice, these procedures do not eliminate all rewards or coercion from the prison and parole system. Under presumptive date procedures as implemented systemwide, there is still a limited parole award possible for "superior program achievement" in longer term cases.²⁰ It must also be realized that the parole board is not the only dispenser of rewards in the prison setting. Prison is an inherently coercive environment, and prison staff have control over significant rewards and punishments (e.g., job assignments, custody levels, awards of meritorious pay or extra good time, institutional transfers, furloughs).²¹ To the extent that prisoners believe that the institutional staff who control these rewards desire program participation, and that such participation may influence these rewards, an incentive/coercion for program participation is likely to be present.

While it appears doubtful whether it would be possible, or even desirable, to remove all incentive/coercion for prisoner program participation, this research effort does suggest that implementation of a presumptive parole date plan can eliminate the uncertainty associated with traditional parole practice without adversely affecting disciplinary behavior or substantially disrupting programming efforts.²²

ACKNOWLEDGMENTS

This article is adapted from U.S. Parole Commission Research Unit Report Twenty-Six, September 1980.

NOTES

²⁸ C.F.R. §2.12 (1979). As initially implemented on September 6, 1977 (see 43 *Federal Register* 39808, August 5, 1977), the early hearing provisions of the presumptive date procedures applied only to prisoners with no minimum term or prisoners with sentences of less than seven years. Further, the original procedure allowed for a presumptive date to

be set only up to four years from the date of the hearing. Cases not given presumptive dates (approximately 3 percent of all initial hearings) were scheduled for Four-Year Reconsideration hearings. Effective March 5, 1979 (see 44 *Federal Register* 3404, January 17, 1979), the early hearing provisions were extended to all prisoners except those with minimum sentences of ten years or more; and the program was expanded to allow for presumptive dates to be set up to ten years from the date of the hearing.

² Use of explicit guidelines for parole decision making is mandated by the Parole Commission and Reorganization Act [PL 94-233, 90 Stat. 219 (March 5, 1976), codified at 18 U.S.C. §§4201 et seq; see particularly 18 U.S.C. 4206]. The guidelines promulgated by the commission may be found at 28 C.F.R. §2.20 (1979). For a description of guideline development and usage, see Hoffman and Stover (1978) and Gottfredson, Wilkins, and Hoffman (1978).

³ Guidelines for sanctioning institutional infractions are found at 28 C.F.R. §2.34 (1979).

⁴ Guidelines for superior program achievement are found at 28 C.F.R. §2.60 as added by 44 *Federal Register* 55002 (September 24, 1979).

⁵ See, for example, Morris (1974); von Hirsch and Hanrahan (1979).

⁶ This project was designed as a companion to a more ambitious project launched by the Bureau of Prisons at its facility in Butner, North Carolina; see Bounds and Kenan (1979). The methodology of our effort had the advantage of being somewhat more straightforward, as only one independent variable (presumptive date) was being examined. The Butner project attempted to test the effects of presumptive dates and the effects of a nontraditional program at a new institution simultaneously. In April 1977, the commission's research project was expanded to the Southeast Region. However, as presumptive date procedures were implemented systemwide approximately four months later, data on Southeast Region cases were not collected for these analyses.

⁷ See n. 1.

⁸ While it is not possible statistically to prove a finding of "no significant difference," results which indicate the absence of any difference nevertheless provide relevant information to those responsible for making policy.

⁹ In addition, the Regional Commissioner had the discretion to exclude a case from the pool (prior to designation as experimental or control) if it was felt that the offense involved was too heinous or publicized, or that the offender was too poor a risk for inclusion in the research project. This option was exercised in seven cases (less than 3 percent of the otherwise eligible pool).

¹⁰ 28 C.F.R. §2.13 (1979) provides that the panel inform the prisoner orally of its recommendation at the conclusion of the hearing. This recommendation is subject to review in the regional office. An official decision is then sent to the prisoner within 21 days of the date of the hearing.

¹¹ One additional case had been designated an "experimental," but through a clerical error received the control, rather than the experimental, written notice of action. This case was excluded from the sample.

¹² The accuracy of this data system has been called into question (Comptroller General of the United States, 1979). Nevertheless, since cases in this project were randomly assigned to experimental and control groups, group comparisons on the number of enrollments and voluntary drops remain methodologically appropriate. Unfortunately, examination of the data available on the system disclosed some missing information for the specific variable relating to actual time (i.e., number of hours) in the program, which precluded use of this potentially useful type of information for this project analysis.

¹³ For example, recreational programs, Toastmasters, and Jaycees.

¹⁴ Throughout the paper, the designation "n.s." means not significant at the .05 level of confidence.

¹⁵ This analysis requires the assumption that the rate of involvement for each group in each time period would remain constant.

¹⁶ The following "prisoner-months" figures are used in all frequency adjustments:

	PERIOD I	PERIOD II
Experimental	1,368	982
Control	1,160	1,082
Average	1,264	1,034

¹⁷ On March 17, 1976, the commission provided the Director of the Bureau of Prisons with a memorandum explaining the purpose and scope of the research project, which the director then made available to his staff. On April 5, 1976 the Western Regional Director for the Bureau of Prisons sent this memorandum to all wardens and Chiefs/Coordinators of Case Management of institutions within the Western Region. More detailed information was sent to Western Region prison personnel by the Regional Director on April 26, 1976, and again during December 1976, after feedback from prison personnel had been received.

¹⁸ It is known that participants in a research study sometimes respond to the interest being taken to study them, rather than to any specific "experimental" manipulation (the "Hawthorne Effect"). It is also noted that some of the cases in this project (both experimental and control) spent a portion of their time in custody after the systemwide presumptive date procedures went into effect. It is likely that these prisoners would have been aware of the new procedures, although these procedures applied only to hearings conducted on or after September 6, 1977.

¹⁹ The cases excluded from the project were likely the most complex or unusual.

²⁰ See n. 4.

²¹ Some of the recommendations under control of the prison staff (e.g., institutional job placements and transfers to camps or halfway houses) carry benefits beyond increased freedom of movement. For example, a job placement in industries automatically

The Effects of Presumptive Parole Dates on Institutional Behavior

carries with it the payment of a wage and the earning of a type of "extra good time" which, in the case of parole denial, provides an advancement of the mandatory release date greater than that normally provided for good institutional behavior. Placement in camps or halfway houses also carries the earning of a type of "extra good time."

²² Further research concerning the impact of the presumptive date procedures as implemented systemwide is in the planning stage.

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FEMALES, RECIDIVISM, AND SALIENT FACTOR SCORE A Research Note

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The salient factor score is an actuarial device used by the U.S. Parole Commission as an aid in assessing the likelihood of recidivism. Table 1 displays the salient factor score currently used by the Parole Commission. In actual decision-making, this device is used as part of a system of explicit decision guidelines (Hoffman & Stover, 1978). The salient score was developed and validated (Hoffman & Adelberg, 1980; Hoffman & Beck, 1980; Hoffman et al., 1978) using case records for samples of released federal prisoners—samples which were predominantly (over 95%) male. In this research, recidivism rates of released female prisoners are examined in relation to the salient factor score.

SAMPLING AND DATA COLLECTION

A sample of federal prisoners serving sentences of more than one year and one day who were released to the community during 1970, 1971, and 1972 provides the informational base for this research. All three major forms of release from the federal prison system (parole, mandatory release—with supervision, expiration of sentence—without supervision) are included. The 1970 portion of the sample consists of a 50% subsample of prisoners released during the first half of 1970; the 1971 portion of the sample con-

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CRIMINAL JUSTICE AND BEHAVIOR

TABLE 1
Salient Factor Score

Item A----- No prior convictions (adult or juvenile) = 3 One prior conviction = 2 Two or three prior convictions = 1 Four or more prior convictions = 0	<input type="checkbox"/>
Item B----- No prior commitments (adult or juvenile) = 2 One or two prior commitments = 1 Three or more prior commitments = 0	<input type="checkbox"/>
Item C----- Age at behavior leading to first commitment (adult or juvenile): 26 or older = 2 18-25 = 1 17 or younger = 0	<input type="checkbox"/>
Item D----- Commitment offense did not involve auto theft or check(s) (forgery/larceny) = 1 Commitment offense involved auto theft [X], or check(s) [Y], or both [Z] = 0	<input type="checkbox"/>
Item E----- Never had parole revoked or been committed for a new offense while on parole, and not a probation violation this time = 1 Has had parole revoked or been committed for a new offense while on parole [X], or is a probation vio- lator this time [Y], or both [Z] = 0	<input type="checkbox"/>
Item F----- No history of heroin or opiate dependence = 1 Otherwise = 0	<input type="checkbox"/>
Item G----- Verified employment (or full-time school attendance) for a total of at least 6 months during the last 2 years in the community = 1 Otherwise = 0	<input type="checkbox"/>
TOTAL SCORE-----	<input type="checkbox"/>

sists of a 30% subsample of prisoners released during the second half of 1971; and the 1972 portion of the sample consists of a 25% subsample of prisoners released during the first half of 1972. Cases were selected by last digit of prison identification number (for example, selecting all prisoners whose identification numbers

Hoffman / RESEARCH NOTE

TABLE 2
Criterion Measure

Favorable Outcome

No return to prison as a parole or mandatory release violator; and
No arrest for a new criminal offense; and
No parole or mandatory release violation warrant outstanding.

Unfavorable Outcome

Arrest for a new criminal offense; or
Return to prison as a parole or mandatory release violator; or
Parole or mandatory release violation warrant outstanding.

NOTE: Arrests for certain petty offenses such as drunkenness, disorderly conduct, gambling, and traffic infractions are not counted.

end in a particular digit produces a 10% sample). Since identification numbers are assigned sequentially, this procedure approximates random selection. A sample of 3982 cases was obtained, including 193 females. Data on case background attributes and outcome during the follow-up period were coded by research staff. For a description of data collection and coding procedures, see Hoffman and Beck (1974).

CRITERION MEASURE AND
FOLLOW-UP PERIOD

For purposes of this research, favorable outcome is defined as no arrest for a new criminal offense; no return to prison as a parole/mandatory release violator; and no parole/mandatory release warrant outstanding during the follow-up period. Table 2 shows this criterion measure. A two-year follow-up period, from month of release, is available for each case regardless of method of release. This uniform follow-up period was made possible by access to FBI "rap sheet" records.

FINDINGS AND DISCUSSION

Table 3 displays outcome by salient factor score category for male, female, and combined samples. As can be readily seen, the percentage favorable outcome for the female sample is seven points higher than for the male sample (63.2% versus 56.2%). But when an analysis of variance is used to adjust for salient factor score category (since the females in the sample tend to have higher scores than the males), this difference is reduced. For the female sample, the adjusted percent favorable outcome is 61.3% compared to 59.3% for the male sample. This difference is not statistically significant ($p > .5$, F test).

This finding, although limited by the small size of the female sample, suggests no substantial difference between male and female released federal prisoners in recidivism rate (once control has been exercised for salient factor score) when the presence/absence of a new arrest is used as the criterion measure with a two-year follow-up period for each case.*

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Note

The Parole Commission has adopted a somewhat revised salient factor score, effective August 31, 1981 (F.R., Vol. 46, No. 132, July 10, 1981, pp. 35637-35639). Repetition of the above analyses using the revised salient factor score (SFS81) produces the same finding.

TABLE 3
Percent Favorable Outcome by Salient Factor Score Category

	All Cases (0-11)	Poor Risk (0-3)	Fair Risk (4-5)	Good Risk (6-8)	Very Good Risk (9-11)
Males	56.2 (3789)	38.9 (1142)	49.0 (1001)	64.6 (990)	84.6 (656)
Females	63.2 (193)	35.0 (40)	51.2 (41)	67.2 (64)	91.7 (48)
Combined	56.5 (3982)	38.8 (1182)	49.1 (1042)	64.7 (1054)	85.1 (704)

NOTE: The first number in each cell indicates the percent favorable outcome for the cell. The number in parentheses indicates the number of cases in each cell.

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Introduction to a Controversy

Prediction in Life and in Law

Predicting who among us will commit a violent act has been called "the paramount consideration in the law-mental health system" by the President of the American Psychiatric Association (Stone 1975) and "the greatest unresolved problem the criminal justice system faces" by the President of the National Council on Crime and Delinquency (Rector 1973).

Despite its emergence only recently as the overarching concern in both the mental health and criminal justice systems, predicting harmful conduct in order to take preventive action has existed as long as law itself. Reviewing the history of prediction in Anglo-American law, Dershowitz (1974, p. 57) concluded that "the preventive confinement of dangerous persons who cannot be convicted of past criminality but who are thought likely to cause serious injury in the future has always been practiced, to some degree, by every society in history regardless of the jurisprudential rhetoric employed." "Moreover," he noted, "it is likely that some forms of preventive confinement will continue to be practiced by every society."

Far from being the occult crystal-ball activity it sometimes is made to appear, prediction is part of life. The human race would not have survived as long as it has were our ancestors not adept at predicting in some rough and intuitive way what nature had in store for them, such as that lions may bite and falling rocks crush, so it is best to avoid both whenever possible. Predictions of the movement of the stars and the rising of the tides were among the first scientific puzzles to preoccupy humankind. On a more contemporary level, much of our own lives is spent predicting how others will respond to us, and we to them, as lover, friend, or colleague. The prediction of harm is likewise pervasive: We drive through green lights only because we predict that cross-traffic will stop on the red.

* * *

Summary

The prediction of violent behavior has played an important role throughout legal history. It is currently used to assist in making a wide variety of legal decisions, from civil commitment to the imposition of the death penalty.

The term "violent behavior" appears preferable to "dangerousness." It can be defined as acts characterized by the application or overt threat of force which is likely to result in injury to people.

Three major criticisms are currently being made of the use of violence prediction by mental health professionals. It is claimed that violence cannot be predicted with any satisfactory level of

accuracy and that any attempt to do so violates the civil liberties of the persons subject to prediction. As well, many believe that the societal protection rationale underlying the prediction of violent behavior is at variance with the traditional helping role of the mental health professions.

These criticisms involve several separable moral and political issues that a society must face in making any decisions on predictive grounds. These issues concern how the criterion of violence is defined, which items to include in reaching a predictive judgment, how likely violence must be to justify preventive action, and the nature of the preventive action that is to be taken. The moral status of any prediction, it is argued, will vary along these dimensions.

* * *

Common Clinical Errors in Prediction

There are many mistakes that a psychiatrist or psychologist can forget to ascertain a relevant fact, or simply be unaware of the research findings in the area. Several sources of error, however, appear to occur so routinely in the prediction of violent behavior, even by generally competent clinicians, that it is worthwhile to single them out for special attention. The four most common "blind spots" in the clinical prediction of violent behavior appear to be: (1) lack of specificity in defining the criterion; (2) ignoring statistical base rates; (3) relying on illusory correlations; and (4) failing to incorporate situational or environmental information.

Lack of Specificity in Defining the Criterion

The difficulty of specifying an acceptable definition of violence or "dangerousness" has already been addressed. The point here is that one cannot even hope to predict what has not been defined. Some specification of a criterion—even one as simple as the FBI's four "violent index crimes" of murder, forcible rape, robbery, and aggravated assault—is essential if prediction is to succeed.

It should be clear that the more inclusive the definition, the greater the predictive accuracy: Large targets are easier to hit than small ones. The data bear out this axiom. One attempt to predict "assaultive behavior" had 16 percent true positives when the criterion was defined as "homicide, all assaults, attempted murder, battery, forcible rape and attempt to rape," 22.6 percent true positives when the criterion was expanded to include "other sex offenses and kidnapping," and 53 percent true positives when assaultive behavior was construed still more loosely to encompass "all of the above plus robbery, all sex offenses, weapon offenses and disturbing the peace" (cited in Halatyn 1975). While predictive accuracy is indeed increased as definitions of violence expand, there comes a point at which it is arguable whether one is studying violence or simply any kind of lawbreaking. Including "disturbing the peace" as violence, for example, would seem to stretch the concept to its breaking point.

A good deal of the ambiguity found in current prediction research may reflect the fact that mental health professionals are often unclear about just what they are predicting will happen. Thus, Forst (1977) found the lowest rate of commitment as a Mentally Disordered Sex Offender in the California county that limited the

definition of "dangerous" behavior to physically assaultive acts and the highest rate of commitment in the county that included "psychological danger" in its criterion.

If a psychiatrist or psychologist considers "writing a bad check" to be sufficiently dangerous behavior to justify institutionalization to prevent its occurrence (*Overholser v. Russell* 1960), and if the validation researcher limits his or her definition of dangerousness to the FBI violent index crimes, it would not be surprising to find overprediction reported. Rather than overprediction, however, this would more properly be a case of unsynchronized definitions. Even if the predictions were perfectly accurate—if those predicted to write bad checks actually wrote them—the followup researcher using a less inclusive definition of violence would report them as "false positives."

Ignoring Statistical Base Rates

Probably the most common and surely the most significant error made by clinicians in predicting violent behavior is the ignoring of information regarding the statistical base rate of violence in the population in question.

The base rate, it will be recalled, is simply the statistical prevalence of violent behavior in a given group, that is, the frequency with which violence is committed in a given time period (usually 1 year).

For at least the past 25 years (Meehl and Rosen 1955), it has been known that it is virtually impossible to predict any "low base rate" event without at the same time erroneously pointing the finger at many "false positives." Livermore et al. (1968) provide a telling example of this dilemma.

Assume that one person out of a thousand will kill. Assume also that an exceptionally accurate test is created which differentiates with 95 percent effectiveness those who will kill from those who will not. If 100,000 people were tested, out of the 100 who would kill, 95 would be isolated. Unfortunately, out of the 99,900 who would not kill, 4,995 people would also be isolated as potential killers. In these circumstances, it is clear that we could not justify incarcerating all 5,090 people. If, in the criminal law, it is better that ten guilty men go free than that one innocent man suffer, how can we say in the civil commitment area that it is better that 54 harmless people be incarcerated lest one dangerous man be free? (p. 84)

Ideally, the "best" population on which to apply clinical predictions of violence is one with a base rate of 50 percent, since in this population the potential effect of the predictions in distinguishing the violent from the nonviolent will be maximized (Hanley 1979). As the base rate differs substantially from 50 percent, clinical differentiation becomes progressively more difficult. If 90 percent of a group will be nonviolent, the best prediction in the individual case is to predict them all nonviolent. If another group has a base rate of 90 percent for violent behavior, the most accurate prediction would be to predict them all violent.

It should be recalled, however, that overall accuracy is not the only factor involved in prediction. One may wish to weigh different kinds of errors differently. Thus, in mental health law (e.g., civil commitment), it appears legally acceptable to weigh a false negative (e.g., a released patient who injures someone) more heavily than a false positive (e.g., a safe person erroneously hospitalized as dangerous). In criminal law, as Livermore et al. noted, the reverse appears true.

It is clear that *knowledge of the appropriate base rate is the most important single piece of information necessary to make an accurate prediction*. This makes Kahneman and Tversky's (1973) finding that people often ignore base rates in making predictions a matter of considerable concern.

Kahneman and Tversky found that people ignore base rates when case-specific information is present. Even when the case-specific information is highly unreliable, it appears to make people forget about base rates. When no case-specific information is present, however, people will, as they should, rely on base rates.

Evidently, people respond differently when given no specific evidence and when given worthless evidence. When no specific evidence is given, the prior probabilities [i.e., base rates] are properly utilized; when worthless specific evidence is given, prior probabilities are ignored (p. 242).

Nisbett et al. (1976) provide an interesting example of how case-specific information can overwhelm knowledge about base rates:

Let us suppose that you wish to buy a new car and have decided that on grounds of economy and longevity you want to purchase one of those solid, stalwart, middle class Swedish cars—either a Volvo or a Saab. As a prudent and sensible buyer, you go to *Consumer Reports*, which informs you that the consensus of their experts is that the Volvo is mechanically superior, and the consensus of the readership is that the Volvo has the better repair record. Armed with this information, you decide to go out and strike a bargain with the Volvo dealer before the week is out. In the interim, however, you go to a cocktail party where you announce this intention to an acquaintance. He reacts with disbelief and alarm: "A Volvo! You've got to be kidding. My brother-in-law had a Volvo. First, that fancy fuel injection computer thing went out. 250 bucks. Next he started having trouble with the rear end. Had to replace it. Then the transmission and the clutch. Finally he sold it in three years for junk" (p. 129).

Logically, the case of the acquaintance's brother-in-law should simply add one more car to the thousands of cars which contributed to the base rates reported in *Consumer Reports* and therefore should have no appreciable effect on one's decision. Psychologically, however, the impact of the case-specific information far exceeds its statistical usefulness (Carroll 1979).

Shah (1978) has noted that an occupational hazard of the mental health professions appears to be a tendency to give too much weight to case information at the expense of base rates.

In fact, one might even wonder about the extent to which professional training and related clinical experiences tend to socialize (or even to indoctrinate) clinicians into practices in which exaggerated and possibly erroneous credence is given to specific information about persons in the form of various "clinical" and "pathognomonic" signs, even though the base rates involved may be low and the reliability of certain "signs" quite poor (p. 164).

Shapiro (1977), in this regard, studied the use of clinical predictions in medicine. He noted the use of "anchoring" as a prediction strategy. "Anchoring" refers to using the base rate of a condition as one's first estimate of the probability of the condition's being present in the individual case. Subsequently, the clinician will use additional patient-specific information to individualize his or her probability estimate around this anchor point.

Clearly, inaccuracy in prediction can be due either to use of an incorrect anchor point or to failure to individualize appropriately. Skill in these two aspects of prediction is acquired differently. A correct anchor-point probability may be obtained either through knowledge of the literature or by extensive clinical experience. Ability to individualize assessments to the unique characteristics of the patient is primarily a function of experience (p. 1512).

Shapiro's (1977) research showed that some physicians were poor predictors because they could not estimate base rates properly, and others, who could estimate base rates, were poor predictors because they could not individualize them in light of relevant case specific information. For further discussion of the "judgmental heuristics" involved in clinical prediction, see Kahneman and Tversky (1973), Tversky and Kahneman (1974), Ajzen (1977), and Shah (1978).

Relying Upon Illusory Correlations

An illusory correlation occurs when an observer reports that a correlation exists between two classes of events which, in fact, are not correlated or are correlated to a lesser degree or in the direction opposite to that reported (Chapman and Chapman 1969). In an ingenious experiment, Chapman and Chapman presented experienced mental health professionals with a series of responses of hypothetical patients to projective tests and paired these responses with statements about the symptoms reported by the patients. When asked what relationships they had observed in the material presented to them, the clinicians responded with relationships that "made sense" in terms of their prior biases, rather than in terms of what they had actually seen. For example, a response emphasizing the eyes in a figure drawing was consistently associated with suspiciousness and paranoia, and Rorschach responses pertaining to the buttocks were consistently associated with male homosexuality, even when the correlations did not exist in reality.

Sweetland (1972) has demonstrated how this phenomenon influences the assessment of dangerousness. Psychiatrists were surveyed to determine which personality traits they considered to be most characteristic of dangerous persons. Their six most frequent responses were: "often acts on impulse," "has no conscience whatsoever," "is addicted to heroin," "is utterly irresponsible," "fears that people are out to get him," and "resents even the slightest criticism." Following this, naive subjects were asked to examine personality descriptions which were made up of these characteristics and which were paired with the diagnoses "dangerous" or "non-dangerous." In one condition of this study, there was no relationship between the items designated by the psychiatrists as indicating a dangerous person and the diagnosis with which these items were paired. Subjects were asked after the presentation to describe what they had observed. The results indicated that, even when there was no relationship, the subjects responded as if they had observed a relationship in the materials. They consistently recalled that certain of the characteristics had appeared more frequently with the diagnosis of "dangerous," when, in fact, they were not correlated. These systematic errors of observation were consistent with the subjects' prior expectations about which characteristics implied dangerousness.

Hartogs (1970), for example, lists 48 alleged predictors of violence, including "lack of family interest, love, support, or acceptance" (p. 335) and "conflict over basic identity" (p. 333). Commenting on Hartogs' criteria, Diamond (1974, p. 443) states:

It would be difficult for an objective observer to take such claims seriously if such pseudo-scientific descriptions had not been reiterated so often that they have become part of the accepted mythology of clinical practice. I am sure that many patients have been labelled as dangerous and have been institutionalized for long periods of time upon the basis of such flimsy clinical criteria.

* * *

Summary

Several concepts facilitate understanding the process of predicting violent behavior. *Predictor variables* are the items one uses to arrive at the prediction, such as demographic factors and scores on a clinical examination. *Criterion variables* are the acts one includes in the definition of what one is predicting, such as in the case of violent behavior, murder, robbery, rape, and assault.

The accepted framework for analyzing the accuracy of predictions includes four possible outcomes: A *True Positive* is a prediction of violence that later turns out to be correct, and a *True Negative* is a prediction of nonviolence that likewise is proven correct; a *False Positive* occurs when one predicts that violence will occur and it does not, and a *False Negative* occurs when one predicts nonviolence for a person who later becomes violent.

Whether preventive action is taken on the basis of a prediction of violence depends on the decision rule that has been adopted. A decision rule involves choosing a point on a scale of violence potential above which one predicts for the purpose of intervention that violence will occur. The choice of a decision rule—which, it is argued, is a political rather than a professional choice—will determine the proportion of accurate predictions and mistakes that will occur.

The most important single piece of information one can have in prediction violence is the *base rate* for violent behavior in the population with which one is dealing. The base rate is simply the proportion of people in the population who will commit a violent act in a given time period (e.g., the annual arrest rate for violent crimes for a given group).

* * *

In the process of predicting violent behavior, clinicians appear prone to several types of systematic error, including vagueness as to what is being predicted, lack of attention to base rates of violent behavior, reliance upon erroneous predictor items, and a failure to take into account information regarding the environment in which the individual is to function.

* * *

Research on Clinical Prediction

This chapter reviews the research that exists on the ability of psychiatrists and psychologists to predict violent behavior and discusses the criticisms and limitations of that research.

* * *

Outcome Studies of Clinical Prediction

There have been at least five studies published since 1972 attempting to validate the ability of psychiatrists and psychologists to predict violent behavior. Kozol et al. (1972) reported a 10-year study involving 592 male offenders, most of whom had been convicted of violent sex crimes. At the Massachusetts Center for the

Diagnosis and Treatment of Dangerous Persons, each offender was examined independently by at least two psychiatrists, two psychologists, and a social worker. These clinical examinations, along with a full psychological test battery and "a meticulous reconstruction of the life history elicited from multiple sources—the patient himself, his family, friends, neighbors, teachers, employers, and court, correctional and mental hospital record" (p. 383) formed the data base for their predictions.

Of the 592 patients admitted to their facility for diagnostic observation, 435 were released. Kozol et al. recommended the release of 386 as nondangerous and opposed the release of 49 as dangerous (with the court deciding otherwise). During the 5-year followup period, 8 percent of those predicted not to be dangerous became recidivists by committing a serious assaultive act, and 34.7 percent of those predicted to be dangerous committed such an act.

While the assessment of dangerousness by Kozol and his colleagues appears to have some validity, the problem of false positives stands out. Sixty-five percent of the individuals identified as dangerous did not, in fact, commit a dangerous act. Despite the extensive examining, testing, and data gathering they undertook, Kozol et al. were wrong in two out of every three predictions of discovered violence (cf., Monahan 1973; Kozol, Boucher, and Garofalo 1973).

The Patuxent Institution in Maryland was similar in purpose to Kozol's Massachusetts Center. Data are available on its first 10 years of operation (State of Maryland 1973). Four hundred and twenty-one patients, each of whom received at least 3 years of treatment at Patuxent, are considered. The psychiatric staff opposed the release of 286 of these patients on the grounds that they were still dangerous (with the court releasing them anyway). The staff recommended the release of 135 patients as safe (with the court concurring). The criterion measure was any new offense (*not* necessarily violent) appearing on the FBI reports of ex-patients during the first 3 years after their release.

Of those patients released by the court against staff advice, the recidivism rate was 46 percent if patients had been released directly from the hospital and 39 percent if a "conditional release experience" had been imposed. Of those patients released on the staff's recommendation and continued for outpatient treatment on parole, 7 percent recidivated. Thus, after at least 3 years of observation and treatment, between 54 and 61 percent of the patients predicted by the staff to be dangerous actually were found to be safe. As with the Kozol et al. (1972) study, some predictive validity does seem to accrue to the psychiatric predictions (7 percent recidivism, compared with 39 to 46 percent recidivism). Still, the majority of those patients predicted dangerous were actually not discovered to be criminal in any sense. In addition, it is possible that variables other than psychiatric ones accounted for the differential recidivism rates. Those who remained until the staff considered them "cured" were older than those released by the courts against staff advice (30- versus 23-years-old). Their lower rate of recidivism may in part be attributed to their being older.

A more recent and much more sophisticated evaluation of Patuxent by Steadman (1977) concluded that "the rearrest rate for both violent offenses and all offenses of all those released to the street with Patuxent approval vary much less from those of all relevant comparison groups than prior reports have demonstrated" (p. 206). For example, the arrest rate for *violent* crime over a 3-year period for these inmates recommended by the staff for release

(i.e., those predicted not dangerous) was 31 percent, while the comparable rate for those predicted violent by the staff but released by the court was 41 percent. This 10-percent difference between the groups predicted to be violent and to be safe is much more modest than the 32- to 39-percent difference claimed in the earlier research. (see Gordon 1977 for a contrasting view of this study). Based partially on these new research findings, the Maryland legislature has abolished the "Defective Delinquent" statute under which the Patuxent program operated.

In 1966, the U.S. Supreme Court held that Johnnie Baxstrom had been denied equal protection of the law by being detained beyond his maximum sentence in an institution for the criminally insane without the benefit of a new hearing to determine his current dangerousness (*Baxstrom v. Herold* 1966). Baxstrom had received a prison sentence, and, before it was to expire, he was diagnosed as mentally disordered and transferred to a hospital for the criminally insane, where he was kept past the date his sentence had expired. The court ruled that he must be released or at least granted a civil commitment hearing at which the State would have to prove his "dangerousness." The ruling resulted in the transfer of nearly 1,000 persons "reputed to be some of the most dangerous mental patients in the state (of New York)" from hospitals for the criminally insane to civil mental hospitals (Steadman 1972). It also provided an excellent opportunity for naturalistic research on the validity of the psychiatric predictions of dangerousness upon which the extended detentions were based.

There has been an extensive followup program on the Baxstrom patients (Steadman and Cocozza 1974). Researchers found that the level of violence experienced in the civil mental hospitals was much less than had been feared, that the civil hospitals adapted well to the massive transfer of patients, and that the Baxstrom patients were treated the same as the civil patients. Only 20 percent of the Baxstrom patients were assaultive to persons in the civil hospital or the community at any time during the 4 years following their transfer. Furthermore, only 3 percent of Baxstrom patients were sufficiently dangerous to be returned to a hospital for the criminally insane during 4 years after the decision (Steadman and Halfon 1971). Steadman and Keveles (1972) followed 121 Baxstrom patients who had been released into the community (i.e., discharged from both the criminal and civil mental hospitals). During an average of 2½ years of freedom, only 9 of the 121 patients (8 percent) were convicted of a crime, and only one of those convictions was for a violent act. The researchers found that a Legal Dangerousness Scale (LDS) was most predictive of violent behavior. The scale was composed of four items: presence of juvenile record, number of previous arrests, presence of convictions for violent crimes, and severity of the original Baxstrom offense. In subsequent analyses, Cocozza and Steadman (1974) found that the only other variable highly related to subsequent criminal activity was age (under 50-years-old). In one study, 17 of 20 Baxstrom patients who were arrested for a violent crime when released into the community were under 50 and had a score of 5 or above on the 15-point Legal Dangerousness Scale. Yet the authors concluded:

For every patient who was under 50 years old and who had an LDS score of 5 or more and who was dangerous, there were at least two who were not. Thus, using these variables we get a false positive ratio of 2 to 1. . . . Despite the significant relationship between the two variables of age and LDS score and

Table 3—Validity studies of the clinical prediction of violent behavior

Study	Percent true positive	Percent false positive	Percent true negative	Percent false negative	Number predicted violent	Number predicted nonviolent	Followup years
Kozol et al. (1972)	34.7	65.3	92.0	8.0	49	386	5
Steadman and Cocozza (1974)	20.0	80.0	—	—	967	—	4
Cocozza and Steadman (1976)	14.0	86.0	84.0	16.0	154	103	3
Steadman (1977)	41.3	58.7	68.8	31.2	46	106	3
Thornberry and Jacoby (1979)	14.0	86.0	—	—	438	—	4

dangerous behavior if we were to attempt to use this information for statistically predicting dangerous behavior our best strategy would still be to predict that none of the patients would be dangerous (pp. 1013-1014).

Note that in referring to the "best strategy" on prediction, Cocozza and Steadman mean the strategy that would reduce the total error rate (i.e., false positives plus false negatives). As mentioned previously, however, some kinds of errors may be much more important than other kinds, and the "best" strategy should take into account the relative "weights" or "costs" of different kinds of mistakes.

The Supreme Court's Baxstrom decision prompted a similar group of "mentally disordered offenders" in Pennsylvania to petition successfully for release (*Dixon v. Pennsylvania* 1971). The results of the release of 438 patients have been reported by Thornberry and Jacoby (1979) and are remarkably similar to those reported by Steadman. Only 14 percent of the former patients were discovered to have engaged in behaviors injurious to other persons within 4 years after their release.

Finally, Cocozza and Steadman (1976) followed 257 indicted felony defendants found incompetent to stand trial in New York State in 1971 and 1972. All defendants were examined for a determination of dangerousness by two psychiatrists, with 60 percent being predicted to be dangerous and 40 percent not dangerous. Subjects were followed in the hospital and in the community (if they were eventually released) during a 3-year period. While those predicted to be dangerous were slightly but insignificantly more likely to be assaultive during their initial incompetency hospitalization than those predicted not to be dangerous (42 percent compared with 36 percent), this relationship was reversed for those rearrested for a crime after their release, with 49 percent of the dangerous group and 54 percent of the not-dangerous group rearrested. Predictive accuracy was poorest in the case of a rearrest for a violent crime, "perhaps the single most important indicator of the success of the psychiatric predictions." Only 14 percent of the dangerous group, compared with 16 percent of the not-dangerous group, were rearrested for violent offenses. While these data are susceptible to alternative interpretations involving the possibly confounding effects of treatment received during hospitalization (Monahan 1978), the authors believe that they constitute "the most definitive evidence available on the lack of expertise and accuracy of psychiatric predictions of dangerousness" and indeed represent "clear and convincing evidence of the inability of psychiatrists or of anyone else to accurately predict dangerousness."

These five studies are summarized in table 3.

If one takes into account that the 46 percent true positive rate reported in the first Patuxent study refers to any crimes, not necessarily violent ones, and discounts that figure accordingly, it would be fair to conclude that the "best" clinical research currently in existence indicates that psychiatrists and psychologists are accurate in no more than one out of three predictions of violent behavior over a several-year period among institutionalized populations that

had both committed violence in the past (and thus had high base rates for it) and who were diagnosed as mentally ill.

A very different perspective on the research on "dangerousness" is put forward by Gordon (1977). According to him, "The error of the critics of predictability could be characterized as assuming the prediction in question is of dangerous behavior, when it is really of the probability of dangerous behavior. In the former case the prediction might seem poor, whereas in the latter case, it might be superb" (p. 251). Mental health professionals, in his view, do not predict that violent behavior *will* occur; rather, they predict that an individual has a certain propensity to act violently. Whether he or she actually *behaves* violently will depend upon whether chance factors—factors that the clinician cannot know about in advance—trigger these propensities. Thus, for example, an individual could be predicted to be "dangerous," if it were believed that he would assault someone who cast aspersions upon his masculinity. This person would be "dangerous," *even if it happened that no one ever triggered violent behavior* by casting such aspersions. "Whether or not a released inmate recidivates may depend on chance factors such as recalling something his therapist said at the moment of temptation or falling in with the right companions" (Gordon 1977, p. 234). What this means for Gordon is that "false positives"—people predicted to be "dangerous" but not later found to have committed violent acts—may have been just as "dangerous" as the "true positives" discovered to have committed violent behavior. It is only that the chance factors that elicited violence in the latter groups were fortuitously absent in the former.

The difficulty with this position is that it makes the accuracy of prediction impossible to test. The mental health professional cannot lose: If the person predicted to be "dangerous" is discovered to have committed a violent act, he or she can say "I told you so"; if the person is not found to have acted violently, the clinician has the retort, "It's just lucky that nobody has triggered this person's dangerousness yet."

It is true, as discussed in chapter 4, that situational or environmental factors can exert a great influence on the occurrence of violent behavior. To be meaningful in predictive terms, however, these environmental or situation factors would have to be specified at the time the prediction is made and not simply fobbed off as "chance." For example, it would be quite acceptable to say that a person has a 50 percent probability of being violent, if he goes back to his old friends and a 20 percent chance if he does not. To ascertain the probability of the person actually committing a violent act, the clinician would then have to make a separate judgment on how likely the individual was to get back to his old friends. It would not be acceptable, it seems to me, for the clinician to say that the person has a 50 percent probability of being dangerous "under certain circumstances" and then not say what these situations were or how likely they were to occur.

Psychological Tests

In a comprehensive review of the use of psychological tests to predict violence, Megargee (1970, p. 145) concluded that no test has been developed "which will adequately *postdict*, let alone *predict*, violent behavior." The literature on psychological tests published in the subsequent decade would do little to modify his conclusion.

McGuire (1976), in the most successful study predicting violent behavior with psychological tests, was able to equal Kozol et al.'s (1972) one-in-three accuracy rate in a controlled prison setting. She used a large variety of computer-combined test data (e.g., MMPI, Q-sort) to arrive at her findings. While noting that "the results do not justify the use of this approach to individual prediction in clinical settings" (p. 95), she observed that the computer analysis of relatively easily obtained test scores was considerably more economical than the intensive clinical approach. Whether her findings would obtain in the open community setting is not known.

Criticisms of the Clinical Research

The three major criticisms of the internal validity or logic of the clinical prediction studies reported to date are (1) that they are not really testing the accuracy of prediction, but rather something else, such as bureaucratic inertia or the effects of mental health treatment; (2) that it is not a fair test of predictive accuracy to measure violent behavior after a prolonged period of preventive institutionalization; and (3) that many of the people who show up in the research as "false positives" are actually committing violent behavior but have not yet been discovered.

The Studies Tested Something Other Than Prediction

It is sometimes claimed regarding the *Baxstrom* and *Dixon* patients that no one really believed that they would be violent if released—that the predictions were merely a bureaucratic ploy to keep "chronic" patients in the hospital—and so the finding that they were are not violent upon release should not be surprising. "In fact, the behavior of released patients may say more about institutional inertia than about poor predictions" (Stone 1975, p. 31).

It is difficult to respond to the criticism that mental health professionals were not telling the truth when they predicted violence so that they could facilitate their bureaucratic hold on patients. It may, unfortunately, be true that if the ticket to involuntary treatment is a prediction of violence, many psychiatrists and psychologists are willing to punch it (Monahan and Cummings 1975), regardless of whether they actually believe the patient to be violence-prone. The organizational contingencies operating upon mental health professionals to keep patients who are believed to "need" treatment, whether violent or not, may be intense.

Yet all research can do is take psychiatrists and psychologists at their word when they predict violence and assume the predictions are made in good faith. It is not an acceptable retort to the research for psychiatrists and psychologists to say, after the fact, that they did not *really* believe the patients to be violent. If bureaucratic pressure influences prediction, then that pressure is part of the social reality that should be empirically studied. And even in the case of the *Baxstrom* patients, *somebody* believed them to be violent, or else judo-training would not have been given to the staff of the civil hospitals to which they were sent (Rappaport 1973).

The Predictions that Were Tested Were Seriously Out of Date

Alternatively, it is sometimes claimed that it is not fair to test a prediction of violence that is "stale" by several months or several years. It may be that the psychiatrists or psychologists were quite accurate in predicting that the patient was violence-prone *at the time of institutionalization*. But it is unfair to test this prediction after a person has had months or years of psychotherapy or medication or is simply that much older than he or she was at the time the prediction was made. Of course many people will not be violent. In fact, the argument goes, one would hope that none would be violent. This would mean that the treatment was completely effective.

A straightforward answer can be given to the criticism that the research is not fairly testing the prediction that led to the original institutionalization: In fact, the research is not testing these predictions at all. It is more properly viewed as testing the *final* predictions that were made before the patient or offender was released, usually by the courts.

Thus, the fact that the *Baxstrom* and *Dixon* patients were largely nonviolent when released from the hospital does *not* mean that the predictions that originally sent them there were wrong. It is impossible to tell one way or the other since too much happened before the original prediction was tested (treatment may have occurred and aging certainly occurred). What the research does show is that *the predictions that kept the patient in the hospital were in error*, since in 80 to 86 percent of the cases no violence was observed when the predictions were overruled by the Supreme Court. So the research is suspect only if taken as a test of the predictions that led to the original hospitalization. It appears valid if taken as a test of the final prediction made before release.

Much Violence May Have Occurred but Not Been Detected

The strongest criticism of the existing prediction research is that it severely underestimates the extent of violent behavior committed by the individuals predicted to be violent, and thus many of those claimed to be "false positives" are actually "true positives" who have not yet been caught. To the extent this argument is valid, it seriously undercuts the thrust of the research findings.

There is no question that *some* underestimations of violence occurred in the research. The question is how much, so that a correction factor can be applied to the data obtained. Let us consider the problem in detail.

Each of the clinical prediction studies relied primarily upon *arrest* for a violent crime as its criterion measure. The Steadman studies included institutional assault and civil commitment for dangerousness along with arrest, and Thornberry and Jacoby (1979) also included civil commitment based on a dangerous act. How accurate an estimate of *violent behavior* is arrest for a violent crime, even if augmented by these other measures?

According to the National Victimization Panel (Department of Justice 1978) — a national study in which an interviewer inquires as to whether a citizen has been the victim of a crime in the past year — only 47 percent of the people who stated that they had been the victim of a violent crime reported the act to the police. In other words, 53 percent of the violent crimes reported to the interviewer was not reported to the police. For several reasons, however, this

dramatic figure appears somewhat inflated. Citizens who said they had not reported their victimization were asked the reason for not reporting. Twenty percent said that the act was "not serious enough" to report. Three percent said that it was "too inconvenient" to fill out a police form. Nineteen percent gave no classifiable reason for not reporting. As Levine (1976) has noted, "many trivial grievances which stay out of police records because people are not very upset are elevated to criminal status by the aggressive probing and searching of interviewers. . . . Since survey findings seem to include many of these trivial occurrences, the results are highly skewed and give an unrealistically grim portrayal of the crime problem" (p. 317). If one discounts those violent "crimes" that victims themselves believe are trivial, a reasonable estimate might be that of every three violent crimes committed in the United States two are reported to the police (cf. Levine 1976).

What of the violent crime that does get reported? The most recent FBI statistics (Webster 1978) reveal that the proportion of reported violent crime that is "cleared" by an arrest is approximately one-half (79 percent for murder; 52 percent for rape; 63 percent for aggravated assault; and 27 percent for robbery). One could conclude, therefore, that *of every three violent crimes that occur in the United States, two are reported to the police, and, of these, one results in an arrest.*

In terms of the criterion problem in prediction research, one could argue that since only one-third of the violent crime committed results in an arrest, it is hardly surprising that the "best" prediction studies can show only a one-third accuracy rate in predicting arrest. How could it be otherwise, since two-thirds of the criterion is hidden? Indeed, if one "corrected" for unreported and unsolved violent crime by multiplying the "true-positive" rate by a factor of 3, then instead of being only one-third accurate, the best prediction studies are in fact *perfectly accurate* in predicting arrest for violent behavior!

Several factors weigh heavily against such a large correction factor, however. The difficulty in the above argument lies in the assumption that violent behavior is evenly distributed among the population being predicted. If this were so — if, for example, each person predicted to be violent actually committed one violent act — then it would be true that a one-third accuracy rate in predicting arrest, which itself is only one-third accurate in estimating violent behavior, would in effect amount to virtually flawless prediction. There is much reason, however, to believe that violent behavior is far from evenly distributed.

Wolfgang (1978) interviewed a sample of the subjects in his Philadelphia cohort study. Offenders reported committing a mean of three "injury offenses" for each time they were arrested for an injury offense, with "recidivists" (those arrested between two and four times) reporting more than seven injury offenses per arrest. Likewise, the Rand study of habitual offenders (Petersilia, Greenwood, and Lavin, 1977) found that offenders reported committing 10 felonies per arrest.

Indeed, if we accepted Wolfgang's figure of three violent acts per each arrest and used it to "correct" for the proportion of actual violence accounted for by those people who have been arrested for violent crime, we would conclude that *all* the violent behavior in the population is committed by those people who are eventually arrested for it.

Data such as those of Wolfgang and Petersilia would support the argument that *the one-third of the individuals predicted to be violent who are arrested for a violent crime are in fact the same people who are also committing most of the unreported and unsolved violent acts*. It is not that the "false positives" are really "true positives" in disguise, but rather that the "true positives" are in fact "truer" (i.e., more violent) than we have imagined. As Shinnar and Shinnar (1975, p. 597) have stated, "The important question is who commits the 70 percent of crimes which are never solved. . . . (T)he most likely possibility is that they are committed by the same group of recidivists who commit the 30 percent of crimes which are solved."

What, then, are we to make of the criticism that the use of arrest severely underestimates the number of people who commit violent acts and thus greatly inflates the number of "false positives"? Obviously, some of the unreported and unsolved violence is committed by persons who have escaped detection and are thus mislabeled as erroneous predictions. Obviously, too, some of the people who have been apprehended and thus validated the accuracy of a prediction have also committed more violence than has been ascribed to them. Pending future research and in light of the findings of Wolfgang (1978), Petersilia et al. (1977), and Shinnar and Shinnar (1977), I would offer the conclusion that current prediction studies provide *reasonably accurate estimates* of the validity of clinical predictions of violence, at least among populations of people who have high base rates for violence since they have committed it in the past. It should clearly be noted that this conclusion applies only to the kinds of situations studied in current research. It will be argued below that in some as-yet-untested situation, such as short-term emergency commitment, the validity of clinical prediction may be appreciably higher than has been reported. Likewise, clinical prediction with persons who do not have the history of violent behavior exhibited by the subjects studied in the current research would surely be less valid than the one-in-three ratios that have been reported.

To the extent that the current research does underestimate the occurrence of violent behavior, the overlooked behaviors are most likely those that are the least serious or that are directed against family members rather than against strangers (since family victims are least likely to report such acts to the police).

It should be noted that research has not yet addressed the issue of individual differences among mental health professionals regarding their ability to predict violence. It would certainly not be surprising if some were better than others at the task. In this regard, Shapiro (1977) studied the accuracy of physicians and medical students in predicting the occurrence of various rheumatic conditions. "In general," he found, "predictive skill was closely related to level of training. Faculty scored higher than residents, who in turn outscored students" (p. 1511). When actuarial tables were compared with clinical predictions, the tables were more accurate than the less experienced clinicians, and less accurate than the more experienced clinicians. Whether such findings would generalize from physicians predicting rheumatic disease to psychiatrists and psychologists predicting violent behavior is not yet known.

Shapiro (1977) also reported that the "error rate method" of evaluating the accuracy of clinical predictions (i.e., whether a

prediction was ultimately right or wrong) was not nearly as sensitive in finding individual differences among physicians as was a mathematical "accuracy coefficient." The "error rate method" does not take into account the magnitude of the error (e.g., someone who predicts that an event has a zero probability of occurring is scored equally wrong as someone who predicts that the event has a 40 percent chance of occurring, if the event actually occurs), whereas the "accuracy coefficient" does. While "error rate" analyses could not distinguish among physicians in terms of their predictive success, "accuracy coefficients" revealed some physicians to be almost 10 times as accurate as others.

Finally, it should be recalled that the one-in-three accuracy rate discussed above is not "good" or "bad" in itself. Social values must be applied in order to evaluate the adequacy of this level of validity. Thus Gordon (1977) has written:

[P]robabilities for individuals committing dangerous crimes within three years may seldom range higher than .3 to .5 in our society. When the probability becomes higher than that, it may apply to extremely unusual phenomena such as armed desperadoes on a killing rampage, or extremely trivial cases, such as bank robbers just before they leave their hideout on the way to a bank. In short, if we inquired into the matter, we might find that probabilities that appear modest in absolute value actually describe the Babe Ruths of dangerousness, and that it is unrealistic to expect values ever to get any higher than that. When they do, the societal reaction may be to shoot first and ask questions later. What this means, then, is that if society is ever to protect itself routinely against individuals that it experiences as the most dangerous of all, it is going to have to do so at probability levels between .3 and .5 or not do it at all (p. 236).

* * *

Statistical Approaches to Improving Clinical Prediction

What steps can clinicians take to improve the accuracy of their predictions of violent behavior? At least two modifications of traditional clinical practice hold promise for augmenting predictive validity: an increased emphasis upon using statistical concepts in clinical prediction, and a heightened sensitivity to environmental or contextual variables. The former is considered in this chapter and the latter in the next. The goal in both cases will be to provide psychiatrists and psychologists with tools to incorporate in their clinical decisionmaking.

Clinical and Actuarial Prediction

The Nature of the Distinction

Much has been made in the area of prediction of the distinction between "clinical" and "actuarial" (or "statistical") methods. In what is still the leading work on the subject, Meehl (1954) distinguished the two approaches as follows:

The mechanical combining of information for classification purposes, and the resultant probability figure which is an empirically determined relative frequency, are the characteristics

that define the actuarial or statistical type of prediction. Alternatively, we may proceed on what seems, at least, to be a very different path. On the basis of interview impressions, other data from the history and possibly psychometric information of the same type as in the first sort of prediction, we formulate, as in psychiatric staff conference, some psychological hypotheses regarding the structure and dynamics of this particular individual . . . This type of procedure has been loosely called the clinical or case study method of prediction (p. 3-4).

Clinical and actuarial prediction may be thought of as differing along at least two dimensions, the *data* employed and *methods* used to turn the data into a prediction.

Actuarial tables spell out *precisely* what kinds of data are to be considered in the prediction, while the clinical approach appears to let the choice of data vary somewhat with the individual case. Thus in an actuarial table one would either *always* include or *never* include a factor such as birth order for specified types of cases, while clinicians might decide for whatever reason that birth order is relevant in one case but not in another case of the same general type. Also, there is a tendency in practice for clinicians to rely on—or, at least, to think they rely on—data at a *higher level of abstraction* than that typically used in actuarial prediction (e.g., “ego strength” rather than “age at first arrest”).

In terms of the methods used to convert the data into a prediction, actuarial approaches use *automatic* or *mechanistic* decision rules that involve mathematical manipulation of the data (frequently no more complicated than adding up a total score), while clinical approaches tend to rely more upon an *intuitive* or subjective combination of the factors deemed relevant (Elstein 1976).

In practice, clinical and actuarial approaches function very differently. Yet it is important to keep in mind that they are merely *ends of continua* regarding the collection of data and methods for transforming the data into predictions. Almost all data have some subjective element to them (“Was he *really* the first-born?”; “Do step-brothers count?”), and there are identifiable commonalities in “intuitive” clinical decision rules.

A clinician who simply memorized an actuarial table and applied it rigorously in every case would obviously produce the exact same results as the table, even though he or she would be using “clinical judgment” in choosing that particular table in the first place. Likewise, actuarial tables can be constructed that rely entirely on data that must be obtained through clinical judgment (e.g., “add ego strength score to impulse control score and subtract maternal deprivation score,” etc.).

It may be useful to distinguish the *data* and the *methods* of prediction as separate factors altogether (cf. Meehl 1954, p. 18). This would result in four “pure” kinds of prediction:

1. *Statistical data combined statistically* (e.g., age, sex, etc., in an actuarial table). Insurance company life-expectancy tables operate in this manner.
2. *Statistical data combined clinically* (e.g., a psychologist gives a prediction after looking at psychological test scores)

3. *Clinical data combined statistically* (e.g., probabilities of violence are attached to given psychiatric diagnoses)
4. *Clinical data combined clinically* (e.g., persons in certain diagnostic categories are assumed to react violently when their manhood is threatened). Many psychodynamic predictions function in this manner.

Again, most prediction in practice mixes these four types, particularly with regard to the data employed. Most clinicians no doubt take into account statistical data such as the patient's sex and age, along with clinical findings regarding diagnosis. Some actuarial tables include clinical diagnosis and demographic indices.

In virtually all of the studies that have tried to compare clinicians and actuarial tables in predicting the same events, the tables have proven the more accurate (Meehl 1954; Sawyer 1966). Indeed, so many studies have reached this conclusion that “actuarial prediction is better than clinical prediction” has become a truism in psychology. It should be noted, however, that not all accept this reading of the research. With regard to the quality of the studies upon which the actuarial-is-better conclusion rests, Holt (1978, p. 12) has stated, “No matter how impressively high it is piled, garbage remains garbage.” One problem Holt sees with the studies is that most of them were designed by statisticians who have a vested interest in the outcome of the debate.

Thus, the statistician takes advantage of the foolish boast of the clinician, “Anything you can do, I can do better,” and plans the contest on his own grounds. The clinician ends up trying to predict grade-point average in the freshman year by a “clinical synthesis” of high school grades and an intelligence test. This is a manifest absurdity: under the circumstances, how could the clinician do other than operate like a second-rate computer? If clinical judgment is really to be tested, it must operate on data that are capable of yielding insights. Moreover, it hardly makes any more sense to expect it to grind out numerical averages of course grades than to expect an actuarial table to interpret dreams. (Holt 1978, p. 27).

On Predicting an Individual's Behavior From Class Membership

A philosophical problem frequently arises in actuarial prediction concerning the legitimacy of inferring statements about an *individual case* from the fact that a person belongs to a certain *class of cases* that have *X* probability of violence.

In truth, all one can say in actuarial prediction is that the person whose behavior is being predicted has characteristics *X, Y, Z*, and that *other* persons who have been studied in the *past*, who have had characteristics *X, Y, and Z*, have committed violent acts at a certain rate.

This issue applies equally to clinical prediction insofar as one makes the inference that, for example, because in a psychiatrist's previous experience those paranoid schizophrenics whose masculinity has been threatened have been violent, *this* threatened paranoid schizophrenic patient will also be violent.

Allport, a leader of the clinical (what he calls “ideographic”) approach to assessment, has stated:

Where this [actuarial] reasoning seriously trips is in prediction applied to the single case instead of to a population of cases. A

fatal nonsequitur occurs in the reasoning that if 80 percent of the delinquents who come from broken homes are recidivists, then *this* delinquent from a broken home has an 80 percent chance of becoming a recidivist. The truth of the matter is that *this* delinquent has either 100 percent certainty of becoming a repeater or 100 percent certainty of going straight. If all the causes in his case were known, we could predict for him perfectly (barring environmental accidents). His chances are determined by the pattern of his life and not by the frequencies found in the population at large. Indeed, psychological causation is always personal and never actuarial (cited in Meehl 1954, p. 20).

Meehl (1954, p. 20) agrees with the philosophical thrust of Allport's statement but notes that "*if nothing is rationally inferable from membership in a class, no empirical prediction is ever possible*" (italics in original).

There is, in Allport's paragraph, a subtle implication that by nonactuarial methods you can predict "for sure". It is interesting to note that in spite of his dislike for actuarial concepts he begins the crucial sentence with "His chances are determined." The whole notion of someone's "chances" is, as Sarbin has emphasized, an implicitly actuarial notion (Meehl 1954, p. 20).

What is necessary to make the inferential leap from membership in a class that has in the past been violent to the prediction that this member of the same class will in the future be violent is a *theory* linking the conditions operating to produce violence in the past class of cases with the conditions operating to produce violence in this specific present case.

As Underwood (1979) has recently written:

The importance of a causal theory is not that it guarantees the continuing effectiveness of the predictive scheme, but that it suggests the circumstances under which the scheme will remain effective. A statistical correlation in data about one group of people may not hold when used as a basis for predictions about another group of people. A causal theory helps to identify any relevant differences between the two groups, or differences in the surrounding circumstances. Changes in the job market could remove a previously valid connection between lack of education and parole failure; changes in the typical employment patterns of men and women may remove the connection between male gender and short life (p. 1446).

An analogy may be instructive. If asked to predict in which direction this monograph would fall, if it were let go, the reader could technically state only that every other solid object he or she has let go in the past has (eventually) fallen down rather than risen up or remained suspended. What allows for the prediction that *this* object, if released in the future, will also fall down is that we possess a theory—gravity—that can plausibly let us generalize from the past class of cases to the current individual case. This theory also allows us to set boundary conditions on the prediction, so we know that, if the monograph were let go in space, outside the force of the earth's gravity, it would not fall but would remain stationary.

The catch, of course, is that we understand gravity much better than we understand violence and tend simply to assume that whatever conditions operated to produce violence in the past will also do so in the future. This may often be a plausible assumption, but there are exceptions, particularly if the time or situational gap

between those persons studied in the past and the person to be predicted in the future is great. The violent crime rate among those under 18, for example, has increased by about 300 percent since 1960 (Wolfgang 1978). Therefore, more weight should now be given to "under 18" as a predictor of violence than should have been given in 1960.

As Gottfredson et al. (1978, p. 54) have put it:

[U]sing an actuarial parole aid is a little like using a weather report that says there will be a 60 percent chance of rain. What the weather report actually means is that on similar days it has rained 60 percent of the time. It does not tell whether or not it will actually rain today. Nevertheless, such information can be useful in deciding whether or not to carry an umbrella.

Actuarial Studies of the Prediction of Violence

Wenk, Robison, and Smith (1972) reported three massive studies on the prediction of violence undertaken in the California Department of Corrections. The first study, begun in 1965, attempted to develop a "violence prediction scale" to aid in parole decision-making. The predictor items employed included commitment of offense, number of prior commitments, opiate use, and length of imprisonment. When validated against discovered acts of actual violence by parolees, the scale was able to identify a small class of offenders (less than 3 percent of the total) of whom 14 percent could be expected to be violent. The probability of violence for this class was nearly three times greater than that for parolees in general, only 5 percent of whom, by the same criteria, could be expected to be violent. However, 86 percent of those identified as potentially violent, were not, in fact, discovered to have committed a violent act while on parole.

The second study reported by Wenk et al. (1972) was undertaken in 1968, also in regard to parole decisionmaking. On the basis of actual offender histories and psychiatric reports, 7,712 parolees were assigned to various categories keyed to their potential aggressiveness. One in five parolees was assigned to a "potentially aggressive" category and the rest to a "less aggressive" category. During a 1-year followup, however, the rate of conviction and imprisonment for crimes involving actual violence for the potentially aggressive group was only 3.1 per thousand (5/1,630), compared with 2.8 per thousand (17/6,082) among the less aggressive group. Thus, for every correct identification of a potentially aggressive individual, there were 326 incorrect ones.

The final study reported by Wenk et al. (1972) sampled 4,146 California Youth Authority wards. Attention was directed to the record of violence in the youth's past, and an extensive background investigation was conducted, including psychiatric diagnoses and a psychological test battery. Subjects were followed for 15 months after release, and data on 100 variables were analyzed retrospectively to see which items predicted a violent act of recidivism. The authors concluded that the parole decisionmaker who used a history of actual violence as his sole predictor of future violence would have 19 false positives in every 20 predictions, and yet "there is no other form of simple classification available thus far that would enable him to improve on this level of efficiency" (p. 399). Several multivariate regression equations were developed from the data, but none was even hypothetically capable of doing better than attaining an eight-to-one false positive to true positive ratio.

The Department of Corrections of the State of Michigan (1978) has recently implemented an actuarial prediction device, the Assaultive Risk Screening Sheet, for use in program assignment and parole decisionmaking. Data on 350 variables were collected for

Figure 1

Michigan Department of Corrections
ASSAULTIVE RISK SCREENING SHEET

RESIDENT'S NAME _____ NUMBER CSO-353 12/77
 SCREENED BY _____ LOCATION _____ DATE _____

INSTRUCTIONS: Starting at left, check "yes" or "no" at each item. This directs you to next item. When a risk category is reached at right, circle that category. If information is missing or conflicting, circle insufficient information box and refer to classification director. See definitions on reverse side.

ASSAULTIVE RISK CATEGORY

VERY HIGH
 ASSLT. RISK

HIGH
 ASSLT. RISK

MIDDLE
 ASSLT. RISK

LOW
 ASSLT. RISK

VERY LOW
 ASSLT. RISK

INSUFFICIENT INFORMATION

NOTICE OF HIGH OR VERY HIGH RISK:
 Not applicable
 Sent _____ Date _____ Signature _____

NOTE: IF HIGH OR VERY HIGH risk, notice of risk screening MUST be given to resident within 30 days

2,200 male inmates released on parole in 1971. Statistical analyses were performed on the data for half the subjects to derive an actuarial table relating to arrest for a new violent crime while on parole. The followup period was a mean of 14 months. The resulting factors were then applied to the other half of the subjects to validate the predictive accuracy of the scale. The scale is presented in figure 1, and the results of the validation study are in table 4.

Table 4—Violent recidivism rate of Michigan assaultive risk categories

Risk category	Recidivism rate*	Percent of sample
Very high risk	40.0%	4.7%
High risk	20.7	6.6
Middle risk	11.8	45.5
Low risk	6.3	23.5
Very low risk	2.0	19.7

*Base rate for violent recidivism = 10.5 percent.

Note that 40-percent accuracy on the basis of simply checking off the type of crime committed, the nature of institutional behavior, and whether an arrest occurred before the inmate's 15th birthday provides a higher degree of predictability than most of the clinical studies have been able to achieve after months of extensive (and expensive) examinations. Note, too, that such a degree of predictability applied to less than 5 percent of the sample.

As to why the Michigan study produced results so superior to the California studies, several factors are involved. Wenk et al. (1972) reported base rates of violent behavior of 5 percent, 2.5 percent, and 0.3 percent in their three studies. This compares with a base rate for violence of 10.5 percent in the Michigan research—between 2 and 35 times higher than the California base rates. Part of these differences may be accounted for by variations in the meticulousness with which the recidivism data were collected. But the major reason accounting for the largest difference in base rates is that Wenk et al. (1972, Study 2) used *convicted and returned to prison* as their criterion, whereas the Michigan researchers used *arrest* for a violent crime as their index of violence. Since a large number of factors having nothing to do with violent behavior affect arrested individuals who are convicted and sent to prison (e.g., plea bargaining, prison overcrowding), the Michigan study may have the more accurate estimates of actual violence committed, despite the fact that the use of arrest overestimates violence to the extent that some, but few, innocent persons are included (Heumann 1978; see also Murphy 1980).

Major Actuarial Correlates of Violent Behavior

What factors have most consistently been related to violence in the research?

Past Crime, Particularly Violent Crime

If there is one finding that overshadows all others in the area of prediction, it is that the probability of future crime increases with each prior criminal act.

Following his cohort of Philadelphia males until they were 30, Wolfgang (1978) found that, if a person is arrested four times, the probability that it will happen a fifth is 80 percent. If a person is arrested 10 times, the probability of an eleventh arrest is 90 percent

and the probability that the offense will be a serious or "index" offense (although not necessarily a violent one) is 42 percent. The PROMIS Research Project in Washington, D.C., analyzing arrest data on over 45,000 criminal defendants, found that the probability of rearrest for a person with five or more prior arrests "began to approach certainty" (Shah 1978a). Steadman et al. (1978) found that virtually all the violent crime committed by released mental patients is committed by patients who had an extensive criminal record before going into the mental hospital.

Further, the amount of crime attributable to repeat or chronic offenders, as mentioned previously, appears to be a substantial portion of the crime committed in society. Fifty-three percent of all crime committed by Wolfgang's (1978) birth cohort was committed by the 6 percent of juveniles who had five or more arrests. By the time they were 30, this group of chronic offenders had risen from 6 percent to 15 percent of the sample.

The 49 habitual offenders in the Rand study (Petersilia et al. 1977) reported committing over 10,000 crimes. Over a 20-year criminal career, they averaged 20 serious crimes per year of "street time" (i.e., time not spent in jail), with two of those crimes being violent ones. They admitted to committing 10 felonies for each time they were arrested. The PROMIS Research Project (1977) in Washington, D.C., likewise found that persons with a record of previous violent crime committed a disproportionate amount of violence. This study also found a significant degree of nonspecialization among offenders: "Today's petty larceny defendant may have been involved in a past robbery case and might be the subject of a future homicide prosecution or simple assault arrest" (p. 13).

Age

At the extremes, the relationship between age and crimes of violence is self-evident: Infants do not mug, nor do geriatric patients rape. It is the precise configuration of the inverted U-shaped relationship between age and crime that is at issue, and it clearly varies by the type of crime and by many other factors. The general thrust of recent research, however, is that the curve is strongly skewed toward the young and is becoming even more skewed.

In 1975, males between 15 and 20 years of age represented 8.5 percent of the American population and 35 percent of the arrests for violent crimes (Zimring 1978). Juvenile violence appears to be increasing more than twice as fast as that of adults, almost tripling between 1960 and 1975 (Wolfgang 1978). Not only one's current age, but the age at which one first comes in contact with the police, appears to relate strongly to criminal behavior. The Philadelphia cohort study (Wolfgang et al., 1972) found that the probability of being an adult offender was three and one-half times greater if one had been a juvenile offender than if one had not.

The average age at which the habitual offenders in the Rand study (Petersilia et al. 1977) committed their first serious offense was 14, with first arrest following a year later. The parole guidelines used in Michigan (1978) distinguish between *high* risk for assaultive recidivism and a *very* high risk for such conduct solely on the basis of whether one was *arrested* for any crime before his 15th birthday. The violent recidivism rate for Michigan parolees with an arrest record by the time they were 15 was 40 percent, almost double the 21 percent violent recidivism rate for those without such an arrest.

As violence feeds on the energy of youth, so age mellowes even the most habitual offender. The Rand study found that habitual offenders committed an average of 3.2 serious crimes per month as juveniles, 1.5 per month as young adults, and 0.6 as adults. William Butler Yeats had said it earlier, "The years have put water in my blood and drowned the wildness within it."

Boland and Wilson (1978) concluded that "the best evidence now available suggests rather strongly that juveniles, especially chronic juvenile offenders, commit a far larger portion of serious crimes than arrest reports had previously led us to believe [and] that the rate at which they commit these crimes declines as they get older"

* * *

Violent Behavior Among Former Mental Patients

An interesting pattern exists in the data on violent crime rates of former mental patients. Almost without exception, studies performed in the 1950s and earlier found that released patients had a *lower* rate of arrest for violent behavior than the general population (Ashley 1922; Pollock 1938; Cohen and Freeman 1945; Brill and Malzberg 1954), while studies performed in the 1960s and 1970s have consistently found a *higher* rate of violent behavior among former patients than among the nonpatient population (Rappaport and Lassen 1945; Giovanni and Gurel 1967; Zitrin, Hardesty, Burdock and Drosaman 1976; Durbin, Pasewark, and Albers 1977; Sosowsky 1978). What accounts for this wholesale shift in the research findings?

According to Coccozza, Melick, and Steadman (1978; see also Steadman, Coccozza, and Melick 1978), the apparently increased crime rate among former patients reflects "the changing clientele of state hospitals." They examined the arrest records of almost 4,000 patients released from New York State mental hospitals in 1968 and 1975 using a 19-month followup period. Particular attention was paid to whether or not the former patient had ever been arrested *prior* to being sent to the hospital.

A striking pattern of results emerges. While it is true that former patients, as a group, do have a substantially higher arrest record for all types of crime than does the general population, patients without an arrest record *prior* to going to the hospital have a *lower* arrest rate than the general population. Patients with *one* arrest prior to going to the hospital have a slightly higher than average arrest rate for violent crime once they get out of the hospital (except for sex crimes which are substantially higher). Patients with *two or more* prior arrests have a drastically higher violent crime rate than the general population. Thus, compared with the general population, the higher rate of violent crime committed by released mental patients can be accounted for entirely by those patients with a record, particularly an extensive record, of criminal activity that predated their hospitalization. This is consistent with the literature on violent crime among criminal populations: A record of past violence is the best predictor of future violence.

But why the *increase* in violent crime rates among released patients in recent years? Steadman, Coccozza, and Melick (1978) compared their findings with those reported by Brill and Malzberg

(1954) on a comparable population of New York patients released in 1947. The results of the two studies are almost identical except that only 15 percent of the 1947 patients had a prior arrest record while 40 percent of the 1975 subjects did. As Brill and Malzberg noted 25 years ago:

Arrests in the ex-mental hospital patients were largely concentrated in a relatively small, rather well-demarcated group of persons with a previous criminal record, and their anti-social behavior was clearly correlated with well-known factors which operate in the general population and was not correlated with the factors of mental illness except in a negative way . . . [An] attack of mental illness with hospitalization does not tend to leave an inclination toward criminal activity greater than that which existed prior to the illness and . . . does not produce such a tendency if it did not previously exist . . . (pp. 12-13).

Rabkin (1979 p. 25) came to a similar conclusion in her exhaustive review of every study published on the topic:

At the present time there is no evidence that [released patients'] mental status as such raises their arrest risk; rather, antisocial behavior and mentally ill behavior apparently co-exist, particularly among young, unmarried, unskilled poor males, especially those belonging to ethnic minorities.

The real issue, therefore, is not what psychological factors account for the increased crime rate among released mental patients, but rather what sociological and economic factors underlie the administrative and political decision to send more criminals to mental hospitals in the first place. As chronic-geriatric patients—who have a very low crime rate—are being “deinstitutionalized” from mental hospitals into nursing homes, the proportion of beds that are being filled by younger and more violent persons—who in the past might have been sent to jail or prison (Stone 1975)—is rising. As Steadman et al. (1978, p. 820) have noted, “if one were to gather a group of men of whom 40 percent had previously been arrested, from the general population, it is quite likely that the arrest rates found among the 1975 former patient group would be duplicated or exceeded.”

In terms of specific psychiatric diagnoses, the New York study found a significant association between patients diagnosed as drug or alcohol abusers or “personality disorders” and future criminal behavior. While no more than 8 percent of any other diagnostic category was subsequently arrested, 18 percent of patients with alcohol or other drug-related diagnoses were arrested as were 28 percent of those diagnosed as “personality disorder” (Steadman, Cocozza, and Melick 1978). With the substitution of “sociopathy” for “personality disorder,” these are the same three factors identified in Guze’s (1976) study of mental illness in a prison population. As was the case with sociopathy, it is unclear what “personality disorder” means in this context and how independent it is from a history of past criminal behavior.

As stated by the President’s Commission on Mental Health (1978, p. 56), “The sporadic violence of so-called ‘mentally ill killers’ as depicted in stories and dramas is more a device of fiction than a fact of life. Patients with serious psychological disorders are more likely to be withdrawn, apathetic, and fearful. We do not deny that some mentally ill people are violent, but the image of the mentally ill person as essentially a violent person is erroneous.”

The Dominance of Clinical Prediction in the Law

If actuarial or statistical prediction has advantages over the clinical approach in terms of precision, reproducibility, or efficiency, why has clinical prediction dominated in the legal system? Kastermeier and Eglit (1973) offered several reasons to account for the primacy of the clinical approach: (a) the view that legal decisions are intrinsically individualized; (b) the fact that actuarial prediction explicitly acknowledges that errors will be made (and therefore decisionmakers may feel more responsible for the mistakes, even though they may be fewer than a clinical approach would produce); and (c) the view (see below) that some important case-specific factors will not be considered in statistical formulae. Carroll (1980) added two other reasons, “(d) uneasiness over stating some reasons for decisions that are not part of the statistical predictions (e.g., public opinion, personal impressions, and private attitudes), and (e) concern over loss of status or even loss of job in competition with statistical formulae.” One final reason for preferring clinical to actuarial approaches might be called (f) uneasiness over stating some reasons for decisions that are part of the statistical predictions (e.g., the inclusion of such socially sensitive variables as race and sex in prediction equations). It is for this reason that clinical prediction sometimes functions as a “laundering” of actuarial prediction by hiding the nature of the variables used in the prediction from public view (see chapter 1).

The above six reasons for preferring clinical to actuarial prediction are primarily of a negative sort. They refer to weaknesses in the legal system or in human decisionmakers that lead them to prefer one method over the other. Are there any *good* reasons for preferring clinical to actuarial prediction? At least three possibilities arise.

Clinical Prediction and the Rare Event

It is true that some important case-specific factors may be overlooked in the actuarial approach (reason (c) above). Meehl (1954) gives the example of predicting whether “Professor A” will attend the movies on a given night. Presume that an actuarial table has been developed that predicts with a probability of .90 that the professor will attend the movies. The clinician, however, knows that, in addition to fulfilling all the criteria in the table for a .90 probability, the professor has just broken his leg. “This single fact is sufficient to change the probability of .90 to a probability of approximately zero” (p. 25). Note that one could not incorporate such rare contingencies as breaking a leg into the actuarial table, since, precisely because they are rare, they would not appear as statistically significant in a large prediction study.

In other words, such a factor does not appear as statistically important in the mass event, but if the clinician knows the fact in the case of Professor A, he (correctly) allows it to override all other data in the Table . . . [T]hese rare cases furnish one of the respects in which the human brain can be a very sensitive indicator (Meehl 1954, p. 25).

So there may indeed be some case-specific factors that could allow a human being to make a more accurate prediction than an actuarial table in a given individual case. Some formal prediction schemes such as the one used by the U.S. Parole Board allow for just such a "clinical override," when the persons responsible for the prediction believe that the results of an actuarial table are inaccurate in a given case (Gottfredson, Wilkins, and Hoffman 1978). Yet elsewhere Meehl (1973, p. 85) cautions that "clinicians should beware of overdoing the broken leg analogy."

There are at least four aspects of the broken leg case which are very different from the usual "psychodynamic" reversal of an actuarial prediction. First, a broken leg is a pretty objective fact, determinable with high accuracy, if you care to take the trouble; second, its correlation with relative immobilization is near perfect . . . ; third, interaction effects are conspicuously lacking—the immobilization phenomenon cuts neatly across the other categories under study; fourth, the prediction is mediated without use of any doubtful theory . . . (p. 85)

It may be, Meehl states, that clinical prediction as a whole is less accurate than actuarial prediction, but that for a subset of cases for which clinicians express high confidence in their predictions, the clinicians are more accurate. "Once having proved this, we could thereafter countermand the formula in cases where the clinician expresses high confidence in his head" (1973, p. 89). We should note, however, that such proof has not yet been reported (see Shapiro 1977).

Insufficient Time for Actuarial Analysis

A second reason for preferring clinical to actuarial predictions of violence is that situations may arise in which time does not exist to permit a review of the individual's record and his or her scores on the other variables that may be included in an actuarial table. It is difficult to imagine, for example, how much actuarial information could be collected in the context of an emergency 72-hour civil commitment evaluation. While one could judge a person's sex, and estimate age and intoxication status, many other potentially relevant variables could be ascertained only from external sources that are not available in the context of the "emergency" situation. At least until more complete actuarial information can be compiled, "intuitive" clinical judgment (taking into account, e.g., the vehemence of shouted threats) may be the only feasible short-term prediction strategy (Meehl 1973, p. 170).

The Unavailability of Actuarial Data

In addition, as argued previously, there exists little actuarial knowledge concerning what variables predict violence in short-term "emergency" situations. We do not know what to look for, even if we had the time to find it. In situations where no actuarial data exist, reliance upon clinical expertise is the only approach available, if decisions are to be made on predictive grounds. Meehl (1973, p. 89), in this regard, asks rhetorically whether professionals will use clinical or actuarial techniques in making predictive decisions. He answers: "Mostly we will use our heads, because there just isn't any

formula . . ." " 'Clinical experience' and 'common sense,' " he notes, "must be invoked when there is nothing better to be had" (p. 59).

The Clinical Use of Statistical Data

Perhaps too much has been made in the past of distinguishing actuarial and clinical methods and not enough of how each might contribute to the other. From the beginning, clinical methods have been pitted against actuarial ones in the academic equivalent of a cockfight. Recall that the title of Meehl's 1954 book was "Clinical Versus Statistical Prediction." The tone of much of the actuarial writing (except for Meehl himself, 1973) was not chosen to win psychiatric friends or influence psychological colleagues.

To the practitioner, dealing every day with life-and-death decisions, the message of much of the [actuarial] work is, "Your judgment is not nearly as good as you think it is," which is a threat to the security, self-esteem, and even the professional identity of many clinicians. Small wonder that they find it easy to ignore work that lies largely outside their field, seems of dubious relevance, and is clearly still embroiled in controversy (Holt 1978, p. 16).

Yet clinical prediction, as noted, may take into account actuarial tables, and actuarial prediction may incorporate clinical judgments. One possible strategy for improving clinical prediction, therefore, suggests itself. It is to provide clinicians with as much actuarial information as possible, to see if this affects their predictions.

On the first point, Hoffman et al. (1974) presented actuarial prediction tables to parole board members reviewing the files of adult male inmates for parole consideration. The board members were then asked for their own clinical predictions and for a decision on whether the inmates should be paroled or kept in prison. They found that the correlation between statistical risk estimates based on the actuarial tables and the board's clinical risk estimates was 0.74 when the actuarial tables were presented to board members before they made their clinical judgments and 0.53 when the tables were not provided. The correlation between risk estimates and the outcome of the parole decision was 0.30 when the actuarial tables were provided and 0.18 when they were not. The provision of actuarial data, therefore, affected both the clinical judgments of the parole board and its parole decisions in the predicted direction.

A complicating fact is that Hoffman et al. (1974) also found that actuarial data were more likely to result in increased clinical predictions of unfavorable parole outcome (when the actuarial data suggested such an unfavorable outcome) than they were to result in increased predictions of favorable outcome (when the actuarial data were in the favorable direction). This could mean even more false positives.

The reason that actuarial estimates indicating violent behavior may have more of an effect upon clinical prediction than actuarial estimates indicating nonviolence may involve the social consequences of each type of error for the clinician doing the predicting.

If one overpredicts violence, the result is that individuals who will not be violent are institutionalized. This situation is not one likely to have significant public ramifications for the individual responsible for the overprediction. But consider the consequences for the predictor of violence should he or she err in the other direction—underprediction. The correctional official or mental health professional who predicts that a given individual will not commit a dangerous act is subject to severe unpleasantness should that act actually occur. Often he or she will be informed of its occurrence in the headlines (“Freed Mental Patient Murders Mother”) and will spend many subsequent days fielding reporters’ questions about professional incompetence and institutional laxity. As Steadman (1972) noted, “There may be no surer way for the forensic psychiatrist to lose power than to have a released mental patient charged with a serious crime in the district of a key legislator.” Given the drastically different consequences of overprediction (or “type 1 errors”) and underprediction (or “type 2 errors”) for the individual responsible for making the judgment, it is not surprising that he or she should choose to “play it safe” and err on the conservative side. Note that if the clinician adopted the strategy of simply providing estimates of the likelihood of future violence and left it to others in the legal system (e.g., judges) to decide whether the likelihood exceeds the threshold necessary for taking preventive action, these potentially biasing social contingencies might be attenuated (see chapter 1).

In practice, therefore, if *either* clinical or actuarial estimates indicate violence, the prediction is likely to be that violence will occur, while it may take both actuarial and clinical estimates of safety to result in a prediction of nonviolence.

How, then, is the clinician to improve the accuracy of his or her prediction by taking statistical data into account? Several steps appear advisable:

(1) Making Base Rates of Violence a Prime Consideration

If the base rate of violent behavior in a given population is very low, prediction becomes an extremely difficult task. As Megargee (1976, p. 18) has it, “(m)ental health professionals should limit themselves to predicting dangerous behavior in high base-rate populations such as those who have already engaged in repeated violence.”

It should be noted that the “population” for which a base rate is estimated should be as specific and relevant as possible (Meehl 1973, p. 38). The base rate of violent behavior for a person brought to a mental health center by the police as potentially “dangerous to others” is *not* the base rate of violence in the general population, or even the age- and sex-adjusted base rate of violence in the general population. It is *the rate of violent acts committed by other people who have been referred by the police as dangerous*. This base rate (which to my knowledge is not available and therefore would have to be estimated) may be very different from that of the general population.

Carroll (1979), in a series of ingenious studies, examined what factors influence whether decisionmakers take base-rate information into account in making predictive decisions. Subjects in several parole prediction studies were more likely to make use of statistical data when these data were explicitly associated *with the individual case* whose behavior was being predicted rather than in terms of

group rates (see the discussion of predicting from class membership earlier in the chapter). As Carroll (1980) notes:

Subjects . . . were presented with the information that a group of parolees had a known recidivism rate, and that each case they examined was drawn from this group. They apparently failed to complete the syllogism by saying “therefore, each case has an expected risk of recidivism equal to that of the group”

Clearly then, the reasoning process is difficult and not immediately obvious to subjects. The completion of this reasoning process . . . by simply assigning a risk level to the individual case, does result in use of the risk information . . . These results are consistent with very recent work showing that base-rate information will be used if a *causal connection* is apparent between the characteristics about which the base-rates are given and the events to be predicted (Tversky and Kahnemann, in press).

In addition to giving predictions in individual rather than group form, Carroll (1980) also found that statistical information that was stated in *verbal* form was more influential in affecting clinical judgment than statistical information stated in *numerical* form. If subjects were told that “the computer” revealed that a person had a “good” parole prognosis, they were more influenced than if told that the computer concluded the person had a “75 percent” chance of parole success. Indeed, when presented with numerical risk statements of 35 percent, 55 percent, and 75 percent chance of parole success, the subjects’ clinical predictions distinguished between 35 percent and the latter two values, but did not distinguish between 55 and 75 percent success. That is, as Hoffman et al. (1974) found, statistical information was used to increase one’s prediction in an *unfavorable* direction, but it was ignored when it indicated a favorable outcome. When the statistical data were translated for the subjects into verbal terms such as “good” or “poor” risk, however, subjects did distinguish between a favorable and a neutral prediction. Thus, “a set of verbal categories in which to present statistical risk predictions appears to be the most effective presentational mode currently available” (Carroll 1980).

(2) Obtaining Information on Valid Predictive Relationships

Clearly, the clinician is better off with no statistical information than with erroneous information. One purpose of this monograph is to disseminate the results of recent research on factors predictive of violent behavior. Yet, in an area as rapidly developing as this one, “continuing education,” particularly self-education, is a clear necessity. Clinicians need to be alert and sensitive to illusory correlations. Given the tendency for such correlations to persist, continuing education and inservice training programs need to emphasize such sources of error in clinical judgments.

Also, *more* information does not necessarily lead to better predictions. In fact, a surplus of information may reduce predictive accuracy. Bartlett and Green (1966) studied the ability of psychologists to predict student grades. In one condition, psychologists were given four pieces of information (e.g., high school rank), and in another they were given the same four items plus 18 additional ones (e.g., father’s education). In every case, the psychologists predicted more accurately with fewer items of data. Disturbingly, however,

they were more confident of their predictions the more data they had available to them.

Focusing on a limited number of *relevant* and *valid* predictor items, therefore, is more important than an exhaustive examination that yields much irrelevant and ultimately confusing information.

(3) Not Overreacting to Positive Associations

There is little that can be said here other than to exhort clinicians not to overreact to one positive index of violence at the expense of overlooking several negative indices.

A *balanced* search for information on factors that would decrease an individual's propensity for violent behavior (e.g., strong family support), as well as factors that would increase violence proneness, should be undertaken. In addition, it should be noted that simply because a pattern of positive and negative evidence appears to be highly "representative" of future violent behavior does not mean that such behavior should be predicted to occur (Hahneman and Tversky 1973). The base rate and the reliability of the available evidence must also be considered.

For example, if only 10% of a particular group are expected to engage in future violent behavior on the basis of prior probabilities (base rates), and if the specific evidence concerning the predictions is of poor reliability (e.g., clinical assessments and certain psychological test indices), then the predictions should remain very close to the base rates. The greater the move away from the base rates under the above conditions, the greater will be the probability of error (Shah 1978a, p. 229).

Summary

One of the most promising avenues for improving the accuracy of clinical predictions of violent behavior appears to be an increased emphasis upon incorporating statistical concepts into clinical decisionmaking.

* * *

The clinician who wishes to improve the accuracy of his or her predictions by incorporating statistical information can best do so by making the base rates of violent behavior a prime consideration, obtaining data on factors that actually relate to future violence, and not overreacting to a positive indicator of violence at the expense of overlooking several negative ones.

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Employment, Community Treatment Center Placement, and Recidivism: A Study of Released Federal Offenders.

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THE FEDERAL PRISON SYSTEM, with the cooperation of the United States Probation System and the United States Parole Commission, is presently conducting an evaluation of the impact of Federal prison programs on postrelease adjustment. Among the issues being addressed are the employment needs of Federal offenders, the impact of prison programs on employment, and the relationship between employment and recidivism. This article reports the results of the first phase of that research. An overview of postrelease employment for Federal offenders is presented (with particular emphasis on the minority offender), and the effect of Community Treatment Center placement on postrelease employment and recidivism is examined.

Sample Selection

Two samples are used in this research. For the employment section of the study, the sample consists of 974 randomly selected parolees released during the first half of 1978. Releasees to detainees, for deportation, and reparolees are excluded. Only parolees are included because the information on employment after release was collected by interviewing the supervising probation officer. This precluded the possibility of collecting information for subjects not under supervision. Data on the various employment measures used range from 95 percent to 99 percent complete and were collected for the first year after release from prison or from a Community Treatment Center. Additional data collected included demographic and offense information from the Federal Prison System and the Salient Factor Score from the United States Parole Commission data system.¹ The Salient Factor Score, a statistical device used to measure risk of recidivism, was unavailable for 13 cases.

The sample for the recidivism analysis consists

*The opinions stated in this article are those of the author and do not necessarily represent the official position or policy of the Federal Prison System.

of 2,108 randomly selected offenders released during the first half of 1978 and includes all types of release (i.e., parole, mandatory release, and expiration of sentence).² Releasees to detainees, for deportation, and rereleases are excluded. Short sentence cases (offenders with a sentence of 1 year and 1 day or less) are also excluded because they are generally ineligible for CTC placement. Data on rearrest were collected on parolees by interviewing the supervising probation officer and for all other cases through the FBI.³ Rearrest information was 97 percent complete. In addition, the Salient Factor Score from the United States Parole Commission was used as a statistical control and is available for 91 percent of the sample.⁴

Findings and Discussion

Employment Problems Faced by Federal Offenders.—For the purpose of examining employment after release, offenders with a "legitimate" reason for being unemployed (e.g., students, retired persons, housewives, or the medically disabled) are excluded from the analysis. The results at 12 months after release (see table 1) show an overall unemployment rate among Federal parolees of almost 25 percent, an average of 184 days worked, and median earnings of \$6,025.⁵ (Full time employment is considered to be 240 days per year.)

As a point of reference for these figures, during 1978 (the period covered by most of the present data) the national unemployment rate was 6 per-

¹The Salient Factor Score (see U.S. Parole Commission, 1977) is a predictive instrument used by the U.S. Parole Commission to assess risk of recidivism and measures such variables as prior convictions, prior incarcerations, heroin addiction, and employment. The Salient Factor Score used is the actual score calculated by the Parole Commission at the time of the parole hearing. Two different (but closely related) versions of the Salient Factor Score were used by the Parole Commission for the study sample.

²This sample includes the 974 parolees discussed in the employment section of the report. Both the sample for the recidivism analysis (N=2,108) and for the employment analysis (N=974) were chosen by selecting all cases whose prison identification number ended in an even digit. As prison identification numbers are assigned sequentially on admission, this procedure is assumed to provide a close approximation of random selection.

³For parolees not under supervision for the entire year after release, arrest information was collected through the FBI. All cases have a uniform followup period of 1 year after release for both employment and arrest information. For example, followup for a case released during April 1978 would extend through April 1979.

⁴Cases with and without Salient Factor Scores have nearly identical recidivism rates so the missing data should not unduly bias the results.

⁵A median figure for money earned was used rather than a mean or average because the mean was biased by a few individuals earning very large amounts of money.

FEDERAL PROBATION

TABLE 1.—Postrelease Employment of Federal Parolees Released in 1978^a

	TOTAL	WHITE	MINORITY	SIGNIFICANCE (White/Minority)
A. Employment at Release:				
Percent Unemployed.....	25% (N=907)	25% (N=522)	26% (N=385)	N.S.
B. Employment at 6 Months:				
Percent Unemployed.....	18% (N=864)	14% (N=500)	24% (N=364)	.001
Mean Days Employed.....	95 days (N=864)	100 days (N=500)	89 days (N=364)	.001
Median Money Earned.....	\$3,000 (N=856)	\$3,510 (N=497)	\$2,552 (N=363)	.001
C. Employment at 12 Months:				
Percent Unemployed.....	24% (N=856)	20% (N=498)	29% (N=358)	.01
Mean Days Employed.....	184 days (N=853)	194 days (N=496)	170 days (N=357)	.001
Median Money Earned.....	\$6,025 (N=846)	\$7,200 (N=491)	\$4,942 (N=355)	.001
D. National Employment in 1978 ^b				
Percent Unemployed.....	6.0%	5.2%	11.9%	N.S.

^aWhite Hispanics are included in the minority group. Cases with legitimate unemployment (e.g., medically disabled, students, housewives) are excluded from the analysis.

^bU.S. Department of Labor, 1979

cent (U.S. Department of Labor, 1979) and the poverty level for a family of four was \$6,700 in the city and \$5,700 in rural areas. Although national figures provide an inexact comparison, it is safe to say that ex-offenders tend to be underemployed and that many are living at or near the poverty level. There was, however, a great deal of variability in the postrelease employment success for the offenders studied. Perhaps the strongest differences occurred between ethnic groups. In table 1, the disparity in employment success for minority offenders compared to white offenders (excluding white Hispanics) is also shown. Minority offenders include Hispanics, Blacks, and Native Americans. Those individuals with a "legitimate" reason for being unemployed are again excluded.

It is apparent that minority offenders are at a considerable disadvantage in seeking employment. As members of a group (i.e., ex-offenders) already at a disadvantage, minority offenders seem to have their employment problems compounded. At 1 year after release, for example,

minority offenders in the present study have a 9 percent higher unemployment rate compared to white offenders and are earning over \$2,000 less in salary. It is important to note that white and minority offenders are equally successful in securing employment at the time of release from prison. About 75 percent of both groups had a job at the time of release.

Part of the difference in employment is due to the fact that minority offenders have a somewhat higher rearrest rate. Minorities show a 27 percent rearrest rate at 1 year after release compared to 19 percent for white offenders. However, even when those who were reincarcerated are excluded, minorities still show significantly worse postrelease employment. The results, in fact, are largely unchanged by excluding those reincarcerated.

Another characteristic found to have a strong influence on employment was age. Young offenders (less than 25 years old) had greater difficulty in securing employment than older offenders. And

A STUDY OF RELEASED FEDERAL OFFENDERS

TABLE 2.—Postrelease Employment by CTC Placement Adjusted for Salient Factor Score Risk Category^a

	CTC		NON-CTC		SIGNIFICANCE ^b (CTC/NON-CTC)
	WHITE	MINORITY	WHITE	MINORITY	
A. Employment at Release:					
Percent Unemployed	13%	16%	45%	51%	.001
	(N=323)	(N=296)	(N=199)	(N=89)	
B. Employment at 6 Months:					
Percent Unemployed	14%	20%	17%	31%	.05
	(N=305)	(N=281)	(N=195)	(N=83)	
Mean Days Employed	104 days	96 days	89 days	72 days	.001
	(N=305)	(N=281)	(N=195)	(N=83)	
Mean Money Earned	\$4,354	\$3,209	\$3,941	\$2,045	.01
	(N=301)	(N=280)	(N=196)	(N=83)	
C. Employment at 12 Months:					
Percent Unemployed	20%	24%	23%	33%	.05
	(N=305)	(N=276)	(N=193)	(N=82)	
Mean Days Employed	198 days	183 days	178 days	145 days	.001
	(N=305)	(N=275)	(N=191)	(N=82)	
Mean Money Earned	\$8,817	\$6,474	\$8,395	\$4,279	.05
	(N=300)	(N=273)	(N=191)	(N=82)	

^aWhite Hispanics are included in the minority group. Cases with legitimate unemployment (e.g., medically disabled, students, housewives) are excluded from the analysis.

^bFigures are adjusted using Analysis of Covariance.

among the young, minority offenders were again the most disadvantaged. Among offenders less than 25 years old, minorities had an unemployment rate of 43 percent and earned only \$3,638 at 1 year after release, compared to 28 percent unemployed and \$5,280 earned for white offenders (differences are statistically significant). These findings reflect trends in the general population that are more pronounced among ex-offenders. For example, among those less than 20 years old, the national unemployment figures in 1978 were 35 percent for minorities and 14 percent for whites (Job, 1979).

Although the data are not presented here, differences were also found when sex and prior record are considered. Female offenders and those with extensive prior records have severe problems in finding and holding employment.

Impact of CTC Placement on Employment.—Although Federal parolees show a fairly dismal work record, placement in a Community Treatment Center has been found to significantly improve postrelease employment (see table 2). Because there are marked differences in employment success between white and nonwhite offenders, the results are reported for CTC and non-CTC releasees divided by ethnic group. By defini-

tion, CTC releasees include only those who successfully completed the CTC program. To adjust for any background differences between those released through a CTC and those who were not, the Salient Factor Score was used as a statistical control (analysis of covariance). The Salient Factor Score was chosen as a control because it was found to be the single most potent nonprogram predictor of both rearrest and postrelease employment.

The results in table 2 show that those released through a CTC have significantly better postrelease employment. (The figures exclude those with a "legitimate" reason for being unemployed and are statistically adjusted to control for background differences.) Releasees through a CTC show significantly more days employed, more money earned, and lower unemployment. In addition, these findings hold true for both white and nonwhite offenders. Release through a CTC tends to increase the employment success of both groups of offenders.

The most striking finding, however, is in the percent having a job upon release to the community. Overall (i.e., not controlling for Salient Factor Score), 83 percent of all CTC releasees had a job at

FEDERAL PROBATION

TABLE 3.—Percent Rearrested or Warrant Issued at 12 Months After Release Adjusted for Salient Factor Score Risk Category^a

	CTC	NO CTC	SIGNIFICANCE ^b
	RELEASE	RELEASE	
WHITE	20.7%	18.6%	N.S.
	(N=676)	(N=463)	
MINORITY	25.0%	35.7%	.001
	(N=598)	(N=300)	

^aWhite Hispanics are included in the minority group.

^bFigures are adjusted using Analysis of Covariance.

release compared to 49 percent of non-CTC releasees.⁶ In all probability the employment advantages enjoyed by CTC releasees all reduce to this one variable: CTC releasees were much more likely to have a job at release and the advantage this provided continued, with somewhat diminishing returns, during the first year after release.

Although CTC placement improved the total amount of employment for both white and minority offenders, the quality of the employment (measured by average daily wages) was upgraded only for minority offenders. After controlling the Salient Factor Score, white offenders released through a CTC earned an average of \$42.01 a day while employed during the first year after release compared to \$43.57 for white offenders not released through a halfway house. The difference is not statistically significant. The same figures for minority offenders, however, show that releasees through a halfway house earned an average of \$31.58 a day while employed compared to \$27.04 for minority offenders not released through a halfway house. This difference is significant at the .05 level. These results, then, indicate that for minority offenders released through a halfway house both decreases the amount of time a person is out of work and improves the quality of the employment obtained.

The results on employment support the findings of an earlier study conducted by the Federal Prison System (Beck, et al., 1978; Beck, 1979). The findings of the two studies, in fact, are nearly identical. There is solid evidence that CTC referral greatly increases the chance that an offender will have a job at release and that he will show

^aThese figures include those "legitimately" unemployed.

significantly greater earnings during the first difficult months after release.

Impact of CTC Placement on Recidivism.—For the purposes of this article, recidivism is defined as a new arrest or a violation warrant issued during the first 12 months after release from prison or from a CTC. Arrests for minor crimes such as drunk, vagrancy or disorderly conduct are excluded. For the sample studied, 23.7 percent were rearrested or had a warrant issued.

The results show that, overall, CTC placement does not have an effect upon recidivism. After statistically adjusting for differences in the two groups using the Salient Factor Score (Analysis of Covariance), CTC releasees had a rearrest rate of 22.5 percent compared to 25.5 percent for offenders not released through CTC. The difference is not statistically significant. There is, nevertheless, evidence that CTC referral reduces the rearrest rate for minority offenders (see table 3). Among white offenders, CTC releasees had a rearrest rate of 20.7 percent compared to 18.6 percent for non-CTC releasees (not significant). Among minority offenders, however, CTC releasees had a rearrest rate of 25.0 percent compared to 35.7 percent for non-CTC releasees. This finding is significant at the .001 level. In other words, CTC referral (after controlling for risk of recidivism) substantially reduces the rearrest rate for minority offenders but has no impact on the recidivism rate for white offenders.

An alternative method for analyzing the data is shown in appendix A. The results in appendix A are reported separately for the four risk categories identified by the Salient Factor Score. As in table 3, the results show that CTC placement has no effect on recidivism for white offenders but that

A STUDY OF RELEASED FEDERAL OFFENDERS

minority offenders have lower recidivism rates if released through a CTC.

The inevitable question, of course, is why does CTC referral reduce recidivism for minority offenders but have little impact on the recidivism rate for white offenders? Analysis of the employment information has shown that release through a CTC improves the postrelease employment of both white and minority offenders, so more appears to be involved than a simple correlation between employment and recidivism. Assuming that white and minority offenders are selected for CTC placement according to the same standard,⁷ one explanation may be that crime among minority offenders is more likely to be economically motivated than among white offenders. If that is true, any program that improves employment will be more likely to reduce recidivism for minority offenders than for white offenders. A second explanation may be that improving employment up to a certain level or threshold will reduce recidivism, but beyond that point will have diminishing returns. Although CTC placement improved the employment success of both white and minority offenders, white offenders started at a much higher level. The relative increase in employment for white offenders may not have been as critical as it was for minority offenders.

There is one source of evidence that tends to support the explanation that there exists a certain minimal level of employment below which recidivism tends to increase for both white and minority offenders. Employment information available for the parolees only indicates a relationship between employment and recidivism when money earned during the first 6 months after release is compared with the percent rearrested during the second 6 months.⁸ The results show that offenders earning less than \$3,000 in the first 6 months had a rearrest rate of 19 percent during the second 6 months (19 percent for white offenders, 20 percent for minority offenders), while offenders earning more than \$3,000 later had a rearrest rate of only 7 percent (6 percent for white offenders, 9

percent for minority offenders). Likewise, the figures measuring the quality of the employment indicate that offenders averaging less than \$25 per day while employed in the first 6 months had a rearrest rate of 17 percent during the second 6 months (16 percent for white offenders, 17 percent for minority offenders) while offenders averaging more than \$25 per day later had a rearrest rate of 10 percent (9 percent for white offenders, 11 percent for minority offenders). In either case, after controlling for either the total earnings or the quality of the employment obtained, the effect of CTC placement on recidivism disappears.

It has been well established that most offenders leave prisons with very meager resources (see Irwin, 1970; Taggart, 1972; Erickson, *et al.*, 1973). In theory, CTC placement supplements these resources by providing short-term support in the form of room and board. With this support, offenders are able to locate employment without the financial stress faced by many offenders at release of needing to locate immediate employment. It can plausibly be argued, therefore, that release through a CTC reduced recidivism for minority offenders by giving them the chance to locate more lucrative employment and increasing the proportion earning more than \$3,000 during the initial months after release. The halfway house experience had no effect on recidivism for white offenders because they were more successful in finding employment and most were earning more than \$3,000 during the first 6 months even without the aid of a CTC. Whatever the explanation, the results indicate that there is a link between employment and criminal behavior and that improving employment performance can reduce recidivism for some offenders.⁹

Results—All CTC Participants

In analyzing both employment and recidivism, only those who successfully completed the CTC program are included in the CTC release group. An additional 4 percent of the sample participated in a CTC program at some point during their incarceration but were not actually released through a CTC. If those individuals, however, are included in the CTC group, the results on employment and recidivism remain essentially unchanged.¹⁰ For example, if you include all CTC participants and adjust for differences in the groups, minority offenders referred to a CTC had a rearrest rate of 26.3 percent compared to 33.2 percent for non-CTC minority referrals (significantly at the .05 level).

⁷It is at least conceivable that minority offenders with a high risk of recidivism are somehow excluded from CTC placement in a way that white offenders are not excluded. To effect the results of the study, the factor identifying high risk offenders would have to be a variable correlated with recidivism but not already measured by the Salient Factor Score. There does not appear to be any evidence supporting this explanation.

⁸Offenders arrested during the first 6 months after release are excluded from this particular analysis.

⁹For this report, minority and white offenders were studied separately because the data indicated minority offenders experienced severe employment difficulties. Partial data collected after the study was completed indicated that the most powerful predictors of postrelease earnings were employment history prior to incarceration and age at the time of the offense leading to incarceration. The finding that minority offenders have poor employment is due in large part to the fact that minority offenders were younger at the time they committed their offense and showed poorer employment prior to incarceration.

¹⁰Another 5 percent of the study sample failed to complete a stay at a CTC at some earlier point during their incarceration but were still released through a CTC. These individuals are included in the CTC group for all analyses.

FEDERAL PROBATION

Summary

The findings of the study indicate that a substantial number of Federal parolees encounter employment difficulties. At 1 year after release, the unemployment rate was 24 percent and the median earnings were only \$6,025. In addition, minority offenders, particularly those under 25 years old, experienced more severe employment difficulties than white offenders.

On the positive side, however, release through a CTC was found to significantly improve the postrelease employment success of both white and minority parolees. Releasees through a CTC showed lower unemployment rates, more days worked, and more money earned. Relating these findings to recidivism, CTC release was found to be significantly associated with reduced recidivism for minority offenders but not for white offenders. Further analysis supported the hypothesis that release through a CTC was more effective in reducing recidivism only for minority offenders because of the greater disadvantages minority offenders face in gaining employment.

In conclusion, the results of the study support an

earlier report (Beck, 1979) which found that CTC referral increased the employment success of offenders after release from prison. There is strong evidence that Community Treatment Centers are providing a useful service in the area of employment and this help can reduce recidivism for minority offenders, a group found to have great employment needs.

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APPENDIX A.—Percent Rearrested or Warrant Issued at 12 Months After Release by Salient Factor Score Risk Category^a

	POOR RISK	FAIR RISK	GOOD RISK	VERY GOOD RISK
WHITE				
CTC RELEASE	43.1% (N = 72)	32.6% (N = 141)	19.1% (N = 199)	8.4% (N = 227)
NO CTC RELEASE.....	45.8% (N = 48)	24.3% (N = 74)	17.6% (N = 108)	7.3% (N = 164)
MINORITY				
CTC RELEASE	44.2% (N = 95)	28.1% (N = 128)	19.0% (N = 211)	13.3% (N = 128)
NO CTC RELEASE.....	60.0% (N = 55)	49.2% (N = 65)	29.2% (N = 72)	14.5% (N = 62)

^aWhite Hispanics are included in the minority group.

SIGNIFICANCE (ANALYSIS OF VARIANCE)

WHITE/CTC versus no CTC = N.S.
 MINORITY/CTC versus no CTC = P<.001

END