THE COURT EMPLOYMENT PROJECT EVALUATION

Final Report

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INTRODUCTION

From the early 1960s, when the bail reform movement began, through the latter part of that same decade when the first pretrial diversion programs drew national attention, efforts at reform of the criminal justice system have to a considerable extent been focused on the pretrial period. The reform goals have been many and varied -- they have ranged from saving resources by routing cases out of the system at the earliest point, to rehabilitating defendants and enhancing the quality of justice.

However, despite considerable attention to pretrial reforms at the federal, state and local levels and despite the development of a wide variety of new programs and processes at substantial (often federal) expense, efforts at evaluation have not produced definitive results. Do these programs have an impact? If so, what and why? Researchers' attempts to answer these questions about both bail reform and pretrial diversion have been plagued by methodological difficulties -- particularly the problem of establishing comparison groups that afford reliable indicators of program impact.

The research presented in this report represents a continuation of previous efforts to assess the impact of pretrial diversion. Prominent researchers who had looked at the burgeoning pretrial diversion movement (notably Mullen 1974; Rovner-Pieczenik, 1974; and Zimring, 1974), concluded that controlled research was essential if policy and program development were to progress in this field.

Their call for the application of such a rigorous research design reflected the general recognition that, although other research on diversion had produced a lot of information, the record was inconclusive about the outcome of court cases without diversion, the impact of diversion on recidivism and personal stability, and the relationship to these outcomes of the social services aspect of diversion programs. A controlled evaluation design was seen as the logical culmination of previous diversion research and program development, and as the next step in informing policy-making in this area.

In 1975, the Vera Institute proposed an extensive evaluation of the Court Employment Project (CEP), one of the first pretrial diversion programs in the United States. Launched in 1968 by the Vera Institute as a demonstration project funded by the U.S. Department of Labor, CEP has continued to provide pretrial diversion services in the New York City Criminal Courts. Since 1971, it has done so as an independent, not-for-profit corporation under contract to New York City's Human Resources Administration. With the cooperation of CEP and of prosecutors, judges and New York's Legal Aid Society (which provides defense counsel to the majority of the city's criminal defendants), the Vera Institute proposed an evaluation of CEP which had at its core an experimental design with the following major characteristics: the concurrent and random assignment of defendants eligible for pretrial diversion to experimental and control groups; the creation of a research population large enough to permit adequate analysis of program impact; a follow-up period of at least one year for all experimental and control group members, including program

dropouts; and the development of an extensive data base, including material from personal interviews as well as official records.

The proposed research was funded by the National Institute of Law Enforcement and Criminal Justice under its Innovative Research Program in 1975 and got underway in 1976. The study was delayed by New York City's deepening fiscal crisis, which forced CEP to stop diversion intake for seven months that year, but in January 1977, CEP resumed its diversion of defendants facing felony charges and the controlled research began. By the end of October that year, Vera researchers had selected 666 defendants for the research population, of whom 410 were assigned as experimental subjects (diverted) and 256 as controls (normal court processing). By November 1978, the full year follow-up had been completed; criminal history record data had been assembled on nearly the entire research population and personal interview data were complete for most of them.

This report presents the results of our analysis of these data, and attempts to put those results into proper context. Thus even before presenting a detailed account of the CEP program as it was in 1977 (Chapter II) and before turning to methodology (Chapter III) and findings (Chapters IV-VI), Chapter I explores the development of the "diversion" concept (or, rather, the variety of notions and programs that are subsumed under "diversion"). We think it is important to understand the larger context that has influenced the research questions dealt with in this evaluation. In designing a program evaluation, researchers typically discuss the nature, structure, and aims of the specific program to be studied with its

staff, administrators, and perhaps its funding source. In this manner the goals of the program are established against which its performance is to be assessed. This evaluation is no exception. However, CEP is unique. It was not only the first pretrial diversion program,* it was the model for many similar programs which spread across the country in the early 1970s in what has been called both a "movement" and a "fad." Although not all pretrial diversion programs have followed CEP's initial model, and although CEP itself has evolved in both structure and operations, the program has continued to be at the center of the debates and disputes over the merits of this reform. The 1973 evaluation of CEP by Franklin Zimring (1973 and 1974) became part of the debate about whether pretrial diversion was really the diversion of criminal defendants *from* prosecution or only the diversion of "Boy Scouts and Virgins" *to* social services (Morris, 1975). Then CEP became a central point of reference in the often quoted testimony of Professor Daniel J. Freed before the U.S. House of Representatives (1974) in which he urged the legislators not to leap into the diversion movement until more rigorous research permitted confidence about diversion's impact, and in which he detailed the serious and pervasive legal, ethical and empirical questions plaguing the reform.

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* Although the Citizen's Probation Authority in Flint, Michigan, was begun in 1965, CEP and Project Crossroads in Washington, D.C., were the first pretrial diversion programs sponsored by the federal government (1968); positive experiences with these two programs are typically credited with the continuation and expansion of federal involvement in pretrial diversion over the next decade.

As a consequence of CEP's unusually prominent position in the debate about diversion, the research issues confronted in this Vera Institute evaluation were selected not only to address the current programmatic concerns of CEP but also to address questions raised repeatedly in the wider debate. A word of caution is in order, however. CEP is a unique program; it operates in a unique city; and its history and structure as a pretrial diversion program do not exactly parallel other programs. Thus, quite different results might have arisen if this research design had been implemented in one of the many other pretrial diversion programs around the country. Nevertheless, while the specific findings cannot be generalized to any other pretrial diversion program, this research powerfully suggests that many of the doubts about pretrial diversion raised by both friends and critics of the idea cannot be ignored. Among the problems that emerge from the CEP data are: the diversion of defendants who would otherwise be treated leniently; the difficulty of assuring voluntary participation; and the lack of measurable impact on recidivism and lifestyle. No matter how distinguishable CEP is from other diversion programs, these must now be even more troubling and important questions for all such efforts.

On the other hand, the results of this research suggest that, during the last decade, CEP may have played an important role in encouraging the New York City criminal justice system to expand and use *other* diversionary options; after all, it was within the context of New York City's very diversionary criminal justice system of 1977 that CEP was found to have only a limited impact on disposition. And

while the data reveal no measurable impact of social services on the behavior of the clients who were diverted, CEP, as a social service agency with a commitment to criminal justice clients, has responded to this research finding by freeing itself from the constraints of the pretrial diversion model and by shifting to other methods of intake they expect will bring clients who can make better use of the program's services. Insofar as CEP's early pretrial diversion activities may be characterized as a "transitional reform" (that is, a stimulus for system change during a limited time period), the research suggests that the agency's traditional goal of affecting case disposition by social service intervention was no longer being met through pretrial diversion. Consequently, in 1979, CEP sharply curtailed its pretrial diversion activities and began to shift the focus of its efforts to affect the dispositional process by establishing stronger formal relationships with defense counsel and with judges, rather than relying on prosecutors to select clients. In addition, CEP began to expand its service activities to include new clients from the criminal justice system for whom social services may be of value without intervention in the dispositional process.

CEP has used the difficult process of intensive evaluation to encourage an internal reassessment of its decade-old operating principles and to stimulate experimentation. We hope the report of this research, its results and the process of change it stimulated will encourage other pretrial diversion programs to examine their own

impact in the context of their own jurisdictions; to assess the extent to which they are actually diverting from prosecution, criminal conviction, and harsh sentence; and to explore the question of whether their social services are likely to have an impact on their clients' lives when they are selected through the classical pretrial diversion mechanisms. The CEP research suggests that pretrial diversion programs should not simply *assume* that they are meeting their goals in these areas. This report also suggests that rigorous assessments of current operations may enable existing pretrial diversion programs to find new ways to bring their service capacity to bear upon the myriad problems of criminal defendants and to provoke further change in the criminal justice system. Finally, the report may help identify jurisdictions in which introduction of a pretrial diversion program would make sense and those in which the effort would be wasted because the diversion concept has already been absorbed in dispositional practice.

CHAPTER I

PRETRIAL DIVERSION/INTERVENTION: THE CONCEPTION AND DEVELOPMENT OF A REFORM

INTRODUCTION

It has been more than ten years since the much heralded President's Commission on Law Enforcement and Administration of Justice recommended the "early identification and diversion to other community resources of those offenders in need of treatment, for whom full criminal disposition does not appear required" (1967: 134). Since that time, concepts such as "diversion," "pretrial intervention," "deferred prosecution," and others have been widely discussed and used to describe a variety of new programs and processes implemented in local criminal justice systems across the country. Despite general usage, however, these concepts and the assumptions and rationales underlying them are not always clearly specified. Indeed, despite replication, some institutionalization, and the development of national standards and goals, there appears to be little uniformity of structure or procedure among programs.

This initial section of the report examines the development of diversion concepts and rationales over the last decade and the historical events forming the framework for this pretrial reform. The purpose of our inquiry is to examine the larger context within which the Court Employment Project has been evaluated, particularly to identify the important, disputed issues that face all pretrial

diversion programs. Many of these issues have arisen out of structural "dilemmas" -- to use Mullen's apt phrase (1974) -- resulting because diversion has been justified as a reform of many, often disparate problems faced by criminal justice systems.

DEFINITION OF PRETRIAL DIVERSION

Before exploring its history, it is important to identify the central aspects of diversion. The idea of "diversion" from traditional criminal justice processing has a long and varied background extending far beyond its role as a pretrial reform.[1] However, as a formal pretrial option introduced into numerous jurisdictions over the last twelve years, diversion has come to mean the pre-adjudication channeling of selected adult defendants out of the normal process by which criminal charges are disposed and into some type of intervention program offering services and/or supervision. Since the adjudication of guilt is deferred (hence the term "deferred prosecution" is also used), defendants completing program requirements typically receive a dismissal of the pending charges (or its equivalent) while those who do not are returned to court to continue the normal dispositional process. As suggested above, however, different jurisdictions and programs divert defendants using a wide

[1] Police, courts and probation in the case of juveniles have long been moving individuals out of the criminal justice system at early stages (Cicourel, 1968; Skolnick, 1966; Wilson, 1970) and prosecutors have always screened cases out by use of dismissals, nolles, or declinations to prosecute (Nimmer, 1974).

variety of procedures and offer somewhat different rationales and expectations as to their impact. Despite this diversity (or more likely because of it), pretrial diversion/intervention (PTD/I)[2] spread rapidly between 1967 and 1977, generating enough national interest and controversy to be variously identified as an innovative alternative to prosecution and incarceration, an important rehabilitative reform, and an unhealthy expansion of state control.

THE ORIGINS OF PRETRIAL DIVERSION/INTERVENTION AS A REFORM

The initiation and support of pretrial diversion/intervention as a criminal justice reform following the President's Commission Report was one among many different programmatic responses to the increased social concern with poverty and crime that characterized the 1960's.

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[2]Recognizing the conceptual and terminological confusion in this area, we have nevertheless had to select a phrase to refer to this reform effort and the programs it has generated. We have selected "pretrial diversion/intervention" (PTD/I) because it incorporates more of the assumptions typical of such programs than does the more common "pretrial diversion" alone. The definition of diversion offered in the National Association of Pretrial Service's (NAPSA) first draft of its standards and goals (drawn up in 1977) recognized that pretrial diversion generally involves more than an alternative to traditional case disposition. It has typically required defendant participation in a program of services and/or supervision. However, in its final standards and goals (1978), NAPSA broadened its definition of "pretrial diversion." It states that "though pretrial diversion *programs* are encompassed by this definition, not all pretrial diversion is effectuated *via* the program model popularized by the Manpower Administration pilot programs, i.e., not all are characterized by delivery of services and supervision by workers formally identified as diversion staff. Rather this definition applies equally to summary pretrial probation practices and post-charge mediation-arbitration procedures who do not feature programmatic components." (p.6) The current report concerns a traditional pretrial diversion *program* and thus concentrates on the evaluation of the procedures, assumptions, rationales, and outcomes long considered appropriate for it and similar efforts. Therefore, to underline those traditional concerns and to

Continued.../

Unlike earlier government responses that tended to explain social problems at the individual level, this period saw an increased emphasis on structural explanations (Rovner-Pieczenik, 1973), for example, that economic and occupational deprivation were not solely a function of individual failure and that criminal behavior did not result from individual pathology alone. Increased direct federal involvement in the Civil Rights Movement was a recognition at the highest policy levels that legal and social barriers to full racial equality were pervasive phenomena; the war on Poverty and Great Society programs also recognized that lack of mobility had roots in structural barriers to opportunity. So too, criminal justice reforms began to emphasize structural problems, particularly focusing on ways the system itself might act as a barrier to the reduction of crime and the rehabilitation of criminals.

The development of PTD/I as a criminal justice reform in the 1960s evolved in the context of increasing concern with crime, recidivism and the overloading of courts and correctional institutions.[3] As an approach to these problems, it reflected a complex interweaving of structural explanations of both crime and poverty. One influential reform thesis was that the criminal justice system

Continued.../distinguish it from other, newer forms of "pretrial diversion" (e.g., mediation), we will use the term pretrial diversion/intervention (PTD/I).

[3]Silberman in his recent book *Criminal Violence, Criminal Justice* (1978) suggests that although the United States historically has been a "violent" society, the decades of the 1940s and 1950s were characterized by unusually low levels of violent crime; consequently, he believes, the reemergence of high levels of criminality in the 1960s (as well as high levels of violent crime visible to and affecting the middle class) was greeted with particular public concern.

was "brutal, corrupt and ineffective," not only demonstrably unable to rehabilitate and deter but itself criminogenic (Vorenberg and Vorenberg, 1973: 154). While not a new idea, it gained additional theoretical credence in the 1960s through the work of several sociologists from the symbolic interaction school. The idea of "differential association" suggested that some individuals *learned* to be criminals in the same way that others learned conforming behavior. (See Sutherland and Cressey, 1960; Cressey, 1960; Jeffery, 1965; Becker, 1953, 1963.) It could be assumed therefore that youthful offenders' association with more hardened criminals or delinquents already in the system would increase the likelihood of their adopting deviant modes of behavior. A companion idea emerged from "labeling theory." It suggested that the reaction of formal institutions to individuals accused of deviant behavior structured and encouraged such behavior. (See Lemert, 1951, 1967, 1970, 1971; Wheeler and Cottrell, 1966; Schur, 1971, 1973.) It was thought that the official or even informal use of terms such as "criminal" or "delinquent" encouraged both the individual and others (e.g., schools and employers) to identify and label the individual as a deviant, and this would further block his/her ability to develop a legitimate career pattern. From the standpoint of criminal justice system reform, therefore, it seemed appropriate to route certain defendants, particularly youthful ones, out of the system as soon as possible to avoid negative social relationships and the stigmatizing effects of official labels.

This perspective on criminality resonated with the growing concern with developing strategies to overcome the structural disadvantages of race, class and age faced by youthful offenders. In 1969, Daniel Glaser voiced an increasingly prevalent (if not new[4]) hypothesis concerning the relationship between crime and poverty: "...unemployment may be among the principal causal factors involved in recidivism of adult male offenders" (Glaser, 1968, cited in Rovner - Pieczenik, 1973). This hypothesis also characterized federal manpower efforts to extend the provisions of the Manpower Development and Training Act of 1962 (MDTA) to criminal offenders.[5] This policy change occurred in 1966 as an indentification at the federal level of the "criminal offender as a manpower resource" (Rovner - Pieczenik, 1973:7). It reflected the growing belief that the major mandate of the MDTA to provide disadvantaged groups with

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[4] "I will guarantee to take from this jail, or any jail in the world, five hundred men who have been the worst criminals and lawbreakers who ever got into jail, and I will go down to our lowest streets and take five hundred of the most abandoned prostitutes, and go out somewhere where there is plenty of land, and will give them a chance to make a living, and they will be as good people as the average in the community." Clarence Darrow, 1902 Address to the inmates of the Cook County Jail, in A. Weinberg, *Attorney for the Damned*, Simon & Schuster, New York, cited in Bellassai, 1978:15.

[5] The specific theoretical underpinnings for many of the intervention efforts launched by the U.S. Department of Labor (DOL) in the 1960s are found in sociological theories that emphasized structural barriers to opportunity; see Merton's theory of anomie (1957:131-160); Cloward (1959); and Cloward and Ohlin, *Delinquency and Opportunity* (1960).

increased skill and access to the competitive labor market could not be adequately fulfilled without formal recognition that the Act's target population (particularly youth) was involved in the criminal justice system.

Finally, the impetus for the development of PTD/I as a criminal justice reform was also related to growing concerns with "over-criminalization." The problem of overcrowded courts combined with a steady increase in violent crime encouraged the perception that the courts were not focusing sufficiently on serious crimes and dangerous criminals because they were overloaded with violations of law stemming from personal disorganization (e.g., alcoholism or mental illness) and crimes having no victims (e.g., certain sexual, gambling, and drug use crimes).[6] To reduce the effects of over-criminalization, intermediary mechanisms were thought necessary to reduce criminal sanctions after arrest until the slower legislative process removed these behaviors from the criminal statutes.

THE EXPANSION OF PRETRIAL DIVERSION/INTERVENTION AS A REFORM

It was in this broad context of concern with the problems of crime, poverty, and criminal justice system failure that the 1967

[6] It has been suggested that in some jurisdictions the increased arrest of working and middle-class as well as lower-class youths for minor drug offenses encouraged wider social acceptance of drug decriminalization than might otherwise have occurred because this group's future employability could be seriously affected by criminal convictions; if so, then it also encouraged PTD/I efforts which were designed to get youthful defendants out of the system without criminal convictions or records.

President's Commission recommended increased development and utilization of mechanisms to route more defendants out of the criminal justice system, particularly preadjudication. The availability through the DOL of federal MDTA moneys for manpower services to offender populations made implementation of this broad recommendation possible without cost to local criminal justice agencies. In 1968 DOL funded the first early or pretrial diversion programs, the Court Employment Project in New York City and Project Crossroads in Washington, D.C. Both projects incorporated in their structures the three highly influential themes discussed above: selected defendants were identified early and diverted out of the normal dispositional process prior to adjudication; they were routed into short term programs providing manpower services; and successful participants maintained their clean records because the pending charges were dismissed.

After a demonstration phase (1968-1970), these programs became prototypes for DOL's further expansion of PTD/I manpower programs. In 1971, DOL funded an additional seven diversion programs and ten more in 1975, the latter funded under the Comprehensive Employment and Training Act (CETA). In 1971, the Law Enforcement Assistance Administration (LEAA) also began funding PTD/I programs with more reform emphasis, however, on their potential effect on the criminal justice system.

Instead of manpower moneys, the major source of federal support for diversion after 1971 was criminal justice dollars authorized under the Omnibus Crime Control and Safe Streets Act of 1968. In 1973, in

conjunction with the President's Office for Drug Abuse Prevention, LEAA expanded its funding of PTD/I programs to include the diversion of drug addicted defendants. The influence of LEAA's infusion of money into pretrial diversion is evident in the rapid increase in programs after 1971. Whereas only four PTD/I programs were in operation in 1970, the American Bar Association's 1974 *Directory of Pretrial Intervention Projects* identified 57 projects, and by the 1976 edition, the list had expanded to 148. In 1978, the Pretrial Services Resource Center believed there were over 200 PTD/I programs operating around the country.

Such growth is indicative of the considerable acceptance PTD/I experienced at the state and local levels. While the majority of these programs operated informally on the local level, several states formalized the process. In 1970 the New Jersey Supreme Court promulgated Rule 3:28 which authorized PTD/I programs, and in 1975 formal guidelines were adopted. In 1972, the Pennsylvania Supreme Court also enacted rules authorizing the process of PTD/I. In the early 1970s, California and several other states enacted legislation calling for the diversion of drug addicted defendants. By 1974 at least three states had legislated the diversion of non-addicts and a few others were developing PTD/I guidelines under Executive Branch requirements for the establishment of criminal justice standards and goals. Despite such state activity, however, most PTD/I continued to operate on the local level without benefit of formal legislative, judicial or executive support.

As the number of PTD/I programs increased during the 1967-1977 period, the concept received formal endorsement from important national sources:[7] from the President's Commission on Prisoner Rehabilitation and from Chief Justice Burger in his discussion of community involvement and rehabilitation (1970); from the American Bar Association in its Standards Relating to the Prosecution Function and the Defense Function (1971); from the American Correctional Association and the American Law Institute in its Model Code of Pre-Arraignment Procedures (1972) and from the National Commission on Marihuana and Drug Abuse, the National Advisory Commission Criminal Justice Standards and Goals and the National Advisory Commission on Courts (1973).

Recognizing that PTD/I programs were an increasingly widespread reform, the DOL awarded a grant in 1973 to the National Bar Association to establish a Pretrial Intervention Service Center. LEAA continued such support by giving two grants in 1976 to the National Association of Pretrial Services Agency (NAPSA) which had been formed in 1972 as a national professional association of individuals and organizations working in bail and pretrial diversion reform. The first of these grants enabled NAPSA to establish a Pretrial Services Resource Center and the second funded a project to develop national standards and goals for PTD/I programs.

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[7] See discussions by Bellassai (1978); Johnson (1976); and Gorelick (1975).

Despite such extensive and lively federal support, the expansion of PTD/I as a criminal justice reform has not proceeded without problems. While the Federal Speedy Trial Act created two types of pretrial service agencies on an experimental basis in ten District Courts, all attempts to mandate PTD/I in the federal courts through legislation have been unsuccessful. First introduced in 1972, S.798 was passed in the Senate but failed to gain House support. Although different versions of this bill have been reintroduced since then, legislation has not resulted and the most recent (S.1819) still awaits hearings in the House.

While the reasons for this failure are complex, two stand out: first, the general lack of enthusiasm from the U.S. Department of Justice (see Testimony of Deputy Associate Attorney General Meissner in *Federal Criminal Diversion Act: Hearings*, 1977:190-203) and second, the inconclusive, often equivocal results of evaluative research on the impact of diversion (see the testimonies of Freed in *Pre-Trial Diversion: Hearings*, 1974:144-157 and Crohn in *Hearings*, 1977:75-77; and Kirby, 1978).

In addition, an important stimulus for the growth of PTD/I programs -- federal program moneys -- has lessened. DOL no longer funds PTD/I and since 1975 LEAA's overall budget has been drastically reduced. It is not clear whether federal encouragement in the form of LEAA program funds will continue but its diminution has already created grave fiscal problems for existing and new programs.[8] While

[8] See Testimony of Brownstein, *Hearings*, 1977:39ff.

some of the early programs funded by the federal government have continued under local support (most notable, New York's CEP program; Minneapolis' de Novo; and the Dade County Florida program), others have folded as federal moneys were withdrawn (e.g., Boston's Court Resources Project and New York City's Youth Counsel Bureau). With the drastic federal cutbacks in LEAA's budget heralding the beginning of PTD/I's second decade, its growth is likely to slow. Since most programs require substantial levels of funding to sustain their social service or supervision component, PTD/I programs currently in operation and those newly proposed face critical scrutiny by local agencies (criminal justice or otherwise) who themselves are experiencing budget reduction.

CONCEPTUAL RATIONALES FOR PRETRIAL DIVERSION/INTERVENTION

Whatever its future, PTD/I was a spectacular early success as a criminal justice reform. To examine the reasons for its expansion, we must look more closely at the conceptual rationales offered for PTD/I. These rationales and the early research into their validity are a vital context for the current evaluation of CEP.

PTD/I is an overdetermined reform; that is, its multiple and overlapping rationales are based upon broad, sweeping assumptions about social process directed at the complex problems of overcrowding, cost, recidivism, poverty and injustice faced by the criminal justice system: "...no word has had quite the power of 'diversion'...which offers the promise of the best of all worlds: cost savings, rehabilitation and more humane treatment" (Vorenberg and Vorenberg, 1973:151-2).

The typical terms used in reference to PTD/I are suggestive of the most important rationales for the introduction and spread of this reform. The concept "diversion" emphasizes reform of the dispositional process, namely channeling defendants away *from* traditional criminal justice processing. Viewed from the standpoint of the defendant (the impact on the individual), pretrial diversion is justified primarily as a solution to the problems of labeling and stigma which arise from prosecution and incarceration. Viewed from the standpoint of the criminal justice system, such diversion is seen as "deferred prosecution," a preadjudication alternative to more traditional forms of disposition which is justified as a mechanism to reduce the costs of prosecution and incarceration for either petty offenses or those likely to be decriminalized over the long run. Finally, the term "pretrial intervention" deals only indirectly with the dispositional process and emphasizes defendants referral to social service or supervisory programs, justifying such programs as a mechanism to encourage defendants' life stability and/or as an innovative alternative to traditional correctional strategies for their rehabilitation.

These stigma reducing, dispositional, intervention and rehabilitative justifications for PTD/I overlap considerably (e.g., while "diversion" does not directly rely on rehabilitation as a justification, it implies indirectly that recidivism will be less likely because stigma is reduced). However each emphasizes a particular aspect of the overall process and, to some extent, suggests somewhat different expectations for program impact. It is important to

note, however, that the use of one particular term by a PTD/I program is not necessarily isomorphic with its rationale for offering pretrial services. Most programs (although not all) have espoused each or all of these rationales at some point in their development and regardless of the vantage point from which the PTD/I process is viewed (defense, prosecution, or program), the rationales are intimately interwoven.

Diversion From Prosecution And Its Consequences

Diversion as a proposed reform of the dispositional process viewed from the perspective of its impact on individual defendants is most closely associated with the work of Edwin Lemert (1967). Growing directly out of the symbolic interactionist tradition discussed above, Lemert suggested that not processing some juveniles through the regular juvenile system was the best way to discourage their development of criminal careers. As noted above, the theoretical basis of this notion that official involvement should be terminated early in the dispositional process was the thesis that deviance was only encouraged by the acquisition of deviant labels and exposure to delinquent associations. While some diverted juveniles might be referred by authorities to a voluntary program of services because of their personal needs, this referral was not supposed to be linked to the disposition of the case through the diversion.

Pretrial diversion, or simply "early diversion," has been the most common term for the application of this conception to the disposition of cases against adult defendants. The popularity of the term and its underlying rationale resulted from its use by the President's Commission report in 1967. Its assumptions are similar to those in the juvenile area and, therefore, seem particularly relevant to more youthful adult offenders who have minimal or no prior criminal histories. Pretrial diversion assumes that the less deeply such defendants penetrate the formal criminal justice system, the less stigmatizing the impact of the arrest and the less likely the individual will acquire official, negative labels. Primarily, therefore, early diversion is justified as a humane dispositional alternative for defendants not likely to return to the system. Since the labeling theory upon which this rationale depends, also assumes that this short-term reduction of stigma will decrease the likelihood that deviant self-identities and career patterns will develop over the long run, diversion is further justified as a method of decreasing recidivism. Finally, to maximize its impact on the labeling process, disposition of the case via diversion occurs pre-adjudication, and thus it is also assumed to have the systemic effect of reducing court, probation, and prison costs.

As a reform, diversion *per se* was seen as an indictment of and a solution for a criminal justice system which is,

> hopelessly overloaded with cases; is brutal, corrupt and ineffective; and...therefore every case removed is a gain. (Vorenberg and Vorenberg, 1973:154.)

This theme that courts create harmful labels and corrections does not correct surfaced as a justification for several types of pretrial reform. Pretrial diversion, bail reform and decriminalization were each a slightly different response to a growing concern in the 1960s that the system was overloaded with defendants whose offenses were minimal and that it detained more defendants pretrial for want of financial resources or stable life patterns than was necessary for public protection.[9] CEP, for example, was initially conceived as a method to help certain individuals avoid pretrial detention by identifying and diverting defendants not dangerous to the community who could be provided with employment opportunities.

> It is clear that a large number of persons charged with crimes in New York City do not have their cases diverted, are not released on their own recognizance without money-bail, or do not receive summons-in-lieu of arrest ...because they lack employment.[10]

Since lack of employment was a negative factor in assessing eligibility for release without bail, helping selected defendants find employment was thought to encourage their pretrial release; their employment could

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[9] See Mullen's discussion of PTD/I as a release alternative (1974) and also Pryor (1977) and Brownstein (Testimony in *Hearings*, 1977:41ff).

[10] Quoted from the first concept paper submitted by Vera to the U.S. Department of Labor in May 1967. DOL awarded a planning grant; the official Vera application for a three-year program grant for an "Early Diversion Program" was submitted in October that year. The diversion program began operations in January 1968 and was soon called the "Court Employment Project," reflecting its goals as an employment service (Sturz, May and October 1967).

then help justify a full dismissal of the charges.[11]

Despite pretrial diversion's labeling and differential association rationales and perhaps partly because of its early association with other forms of pretrial release, adult diversion has rarely been "true" diversion as described by Lemert and others.[12] Pretrial diversion has not generally placed the defendant entirely outside the criminal justice system and its sanctions. Instead, defendants' participation in a program of services or supervision has been an integral part of the way most pretrial diversion operates. Prosecution is delayed or deferred (not declined), providing the individual with an incentive (in the form of a dismissal) to complete the pretrial period success-

[11]Although many of CEP's recent clients would not have remained in detention without diversion (partially because of the success of bail reform, Chapter IV below), other programs in New York City continued to focus explicitly on the "diversion" of detained defendants. The social service program of the Legal Aid Society of New York aims to secure defendants' pretrial release by providing referrals to service and supervisory agencies. The New York City Criminal Justice Agency (which conducts ROR reviews for the court) also runs a supervised release program. Neither, however, anticipates securing a dismissal for the defendant at the end of the pretrial period as did CEP and other typical PTD/I programs.

[12]The distinction between "true" diversion and merely minimizing the individual's penetration of the criminal justice system has been made by Cressy and McDermott (1973) with respect to juvenile diversion. "True" diversion, according to these authors, refers to the formal termination of official involvement whether or not a referral is made to a program outside the system (p.18) See also the discussion by Austin (1977) who argues that true diversion as an intervention has not been tested. The standards for research in the NAPSA Standard and Goals (1978) suggest testing the outcomes of diversion along and diversion with services. The original Vera research on CEP considered such a design but could not operationalize it in New York. However, see Chapter IV below for a further discussion of this issue in the context of the ACD law in New York.

fully. As indicated earlier, "success" was initially seen as a defendants' participation in *manpower* services; this was a result of the early linkage between the diversion concept, the notion that crime is directly related to unemployment, the reliance on employment (and other signs of life stability) as a condition of pretrial release without bail, and the initial availability of DOL program moneys for manpower services to offenders. While these manpower programs were usually (but not always) outside the official criminal justice system, the defendant nevertheless remained formally within the control of that system.

Diversion As Intervention And Rehabilitation

From the very beginning, therefore, pretrial diversion incorporated diversion *to* programs providing services. While diversion *per se* (particularly with youths or first offenders) was conceptually justified by labeling theory, adult pretrial diversion requiring participation in services that were not inherently limited to first offenders rested equally, if not more, on the rationale of intervention and rehabilitation. The interpretation of what intervention and rehabilitation means in a diversion context, however, has shifted over the last decade.[13]

[13]It is very difficult to assess with assurance what PTD/I programs around the country are actually attempting to do. As indicated above, PTD/I is neither a single concept nor a single process. Programs have typically developed within the context of local criminal justice systems and have created procedures, goals and rationales that respond to local conditions. With as many as 200 programs now operating around the country and no systematic detailed assessment of their activities, it is very difficult to

Continued.../

The early DOL programs were designed explicitly to deliver concrete manpower services, and as such they had job developers, vocational counselors, community resource specialists, etc., on staff (see Mullen, 1974). Their primary goal seems to have been the delivery of good quality manpower services. The rationale for intervening *pre-adjudication*, however, was based on the assumption that their life stabilizing consequences would justify dismissing the charges. While it was anticipated that service intervention should reduce court costs and in the long run affect recidivism, both goals were directly tied to the delivery of *manpower* services. The goal of DOL's Pretrial Intervention Project, therefore, was to "massively" influence the criminal justice system through the use of manpower related rehabilitation services. (Mullen, *Final Report*, 1974:1) The 1971 national evaluation of this federal effort identified "offender rehabilitation" as a major programmatic goal (p.4), but focused on the crucial role of manpower services as the intervention made by defining successful "rehabilitation" as *both* increased employment and decreased recidivism.

In contrast, programs that started after LEAA began funding diversion generally focused on "counseling" as the intervention mode rather than concentrating on the delivery of specific services. (For example, see the profiles of Dade County, Florida; Project Mid-

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Continued...say what "diversion" is today. The discussions in this report are based upon the written literature, primarily of the last eight years; descriptive profiles of key diversion programs during the early 1970s (e.g., Mullen, 1974; American Bar Association, 1974; *Diversion from the Judicial Process*, 1974); and our own more recent site visits to and detailed description of seven long-term programs around the country (Vera, 1978).

way, Nassau County, New York; and the Hudson County, New Jersey program in Vera, 1978.) As described by several of these programs, counseling is "rehabilitative" because it is thought "to focus on the events leading to the criminal incident and arrest [and] to help the client see how further criminal activity is harmful to himself" (Vera, 1978:93). Therefore, counseling, the associated monitoring of behavior, and some referral to specific services outside the program, are assumed to reduce recidivism by helping the defendant develop alternative strategies for future behavior.[14]

Over the last decade, therefore, PTD/I programs that have focused on pretrial diversion as an *intervention* have emphasized slightly different approaches to this process -- either *services* as intervention or *counseling* as the intervention. Earlier PTD/I efforts, influenced by the DOL manpower programs (e.g., CEP; Boston's Court Resource Program), saw the delivery of specific manpower and

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[14] In New Jersey, this approach is considered specifically for defendants defined by the program as "amenable to rehabilitation." Court Rule 3:28 under which all New Jersey PTD/I programs operate specifically calls for the diversion of defendants who are judged by a Pretrial Intervention program to be "amenable to rehabilitation." The Hudson County Program, the state's oldest and most developed, takes this mandate very seriously in its selection procedures and defines its role in the rehabilitative process as counseling in the above described manner. Though described in somewhat different terms (e.g., "a willingness to change"), Project De Novo in Minneapolis; Project Midway in Nassau County, New York; Dade County (Florida) Pretrial Intervention; and the Bergen County, New Jersey program all offer similar notions about their attempts to counsel and help diversion clients (Vera, 1978).

vocational services as the way to intervene in clients' lives by increasing their life stability.[15] Decreased recidivism was a desirable but in some sense secondary outcome. While such programs "counseled" defendants, they tended not to do so with the same emphasis as those for whom counseling was a major activity. Somewhat in reverse, later "counseling" programs tended to think clients should be employed or in school when they left the program, but their primary focus was helping clients to understand themselves better and thereby alter the attitudes and behaviors that lead to criminality.[16]

Several reasons for the shift from a service to a counseling focus on intervention may be suggested. They are drawn primarily from the explanations offered by Project De Novo in Minneapolis which underwent this shift internally during the last eight years (see Vera, 1978:39-56).

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[15]The director of the Boston program stated in early 1977 when Vera researchers visited there, that the program was not concerning itself with the questions "do we rehabilitate people" or "do we reduce recidivism," but rather with the question "are we providing comprehensive delivery of services?"

[16]The statement that at least some (possibly most) diversion programs continued to justify their activities on the basis of rehabilitation is contrary to the *major* thrust of the 1978 NAPSA Diversion Standards and Goals, which does not consider rehabilitation a major diversion goal although it is included as a possible and desirable outcome. A consultant on the original 1977 draft said the committee had decided not to emphasize it because of widespread skepticism about rehabilitation efforts. Nevertheless, Vera researchers found most diversion staff in the programs visited believed they were engaged in some form of rehabilitation, and the programs often justified their actions using assumptions, anecdotes or court record data about recidivism. Without adequate comparison group data, however, which most programs have not created, their success cannot be assessed.

De Novo began in 1971 under DOL manpower auspices, was later funded through LEAA, and now receives support through local court services moneys. This suggests that the more widespread shift we suggested above -- from an emphasis on services to counseling -- may be related to the pattern of change in the locus of funding and sponsorship since the early 1970s[17] Social service funds are usually accounted for by showing the frequency different types of services are offered to a certain volume of clients. Courts, probation departments or prosecutors as program sponsors, however, may be more interested in the amount of supervision or behavior monitoring to which clients are subject than in the number of specific services delivered.

While De Novo itself identified changes in funding as a factor in its shift from a servicing to a counseling orientation, it is interesting that the program *sought* the change in sponsorship in order to shift the focus of its activities. First, the agency wanted to relieve itself of the burden of detailed service delivery reports which it considered very costly in time and resources. Second, it

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[17]"[I]t is significant that 40 percent of the diversion programs listed in the ABA's 1974 edition of the PTI Directory were sponsored by independent, private sector entities [possibly social service funders] while reference to the 1976 edition of the Directory shows that only 17 percent of the programs are independent or sponsored by private sector groups. In contrast, only seven percent of the programs listed in 1974 were under the administrative control of executive agencies of state or local government, whereas 36 percent of the programs listed in the 1976 Directory are so lodged. (This does not include prosecutor-administered programs, which actually declined from 23 percent of the total in 1974 to 16 percent in 1976). The other large gain for program sponsorship has been courts, which again, according to the ABA Directory, sponsored or administered 11 percent of the programs listed as of 1976 in contrast to five percent in 1974." (Bellassai, 1978:26-27.)

wanted to focus more on counseling because it felt this was a more appropriate form of help for its clients. De Novo staff had come to feel very strongly that manpower services were not helping its diverted clients change the basic behaviors which led to their arrests. "De Novo leadership and services staff were concluding from their day-to-day experience that a program that all but handed clients jobs may in fact have been discouraging them from taking responsibility for their own lives and that the services they were giving were neither crucial nor appropriate for their clients" (Vera, 1978:49-50).

It should be noted that the type of clients De Novo served heavily influenced its definition of appropriate intervention/rehabilitation strategies. De Novo's population was relatively well-educated and from middle- and working-class backgrounds. Thus its clients did not share all the employment handicaps of poor, inner-city youths without high school educations, who are served by some other diversion programs. Programs such as CEP (whose client population was predominantly inner city minority males) did not believe the heavy counseling emphasis used by programs such as De Novo or Hudson County would be effective with their clients. As some confirmation of this perception, the De Novo, Hudson and Dade County programs all reported that their counseling approaches seemed least successful with those of their clients who were poor, inner city minorities, especially males.

Pretrial Diversion/Intervention As A Prosecutorial Alternative

The two interwoven rationales for PTD/I discussed so far tend to reflect *program* and/or *defense* justifications for diversion. While both use the suspension of prosecution before adjudication as a *means* to one or more ends, neither directly addresses the primary goal of the prosecution.[18] It is the immediate task of the prosecutor to dispose of charges against defendants; in this capacity, prosecutors generally control the decision to divert and thus access to the "two scarce commodities -- nonprosecution and expensive, albeit coerced, treatment services..." (Zimring, 1974:238).[19] The term "deferred prosecution" (which is preferred by the National District Attorneys Association and by some programs sponsored by prosecutors)[20] implies a somewhat different perspective on PTD/I than those already discussed, although they are all highly interconnected.

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[18]Obviously, however, both relate to a secondary goal of the prosecution--to reduce recidivism. If stigma-reduction and/or intervention in life-style affect future criminality, prosecutors should be encouraged to use PTD/I as a dispositional alternative.

[19]There has been much discussion and debate on the proper role of "prosecutorial discretion" in the area of PTD/I particularly as it relates to judicial discretion in sentencing (see, *Hearings*, 1977; NAPSA, 1978; Johnson, 1976). Most descriptions of PTD/I programs, however, (e.g., Vera, 1978 and Chapter II below) suggest prosecutors play an extremely central role in the decision-making process. There are exceptions, particularly in New Jersey (with their Court Rule) and in Boston, where diversion occurs after a judicial finding.

[20]For example, the Citizen's Probationary Authority in Flint, Michigan, the first PTD/I program in the United States, was set up by prosecutor Robert Leonard in 1965, two years before the President's Commission; it is avowedly non-rehabilitative (Leonard, 1973).

To the extent that prosecutors emphasize deferred prosecution as an alternative disposition rather than as an intervention/rehabilitative or stigma-reducing device, diversion to a service program becomes an addition to the criminal justice system's existing repertoire of supervised mechanisms for disposing of selected criminal charges.[21] Prosecutors differ considerably in the extent to which they see PTD/I as potentially rehabilitative or stigma-reducing, depending on their personal predilections and experiences with program interventions, and their associations with particular jurisdictions and defendants. However, most who are involved in PTD/I see it at least minimally as a useful prosecutorial alternative for certain cases.[22]

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[21]Obviously, the implications of "supervision" depend upon the particular program to which defendants are referred. In some contexts (e.g., many "manpower" service programs), the referral represents subjecting the defendant to a program requiring his attendance and some willingness to identify certain problems and receive assistance. In others (e.g., certain counseling programs), the defendant is subject to more behavior monitoring; again, depending upon the particular program, this may represent considerable supervision and intensive, possibly innovative counseling (e.g., possibly Midway and De Novo) or, as in the federal system (at least according to the Justice Department research by Meissner, 1978), rather routine probation supervision.

[22]There are jurisdiction where prosecutors reject this option either because they believe it is of dubious legality (see Austin, 1977, re: San Pablo, California) or because they do not feel it is needed. There is no way to estimate how widespread any of these views are. The fact that 200 or so PTD/I programs have been established around the country can be used as evidence that many prosecutors desire such an option or that the vast majority do not want it (the "as many as 200..." vs. the "only 200..." problem). Nevertheless, it appears that where PTD/I does exist as an option, prosecutors are inclined to justify it as a dispositional alternative (see Vera, 1978 and Nimmer, 1974).

Raymond Nimmer, in his monograph *Diversion: The Search for Alternative Forms of Prosecution* (1974), has suggested a framework for understanding why some prosecutors have encouraged and supported PTD/I as a dispositional alternative. Nimmer refers (p.14) to the "dispositional dilemma" faced by prosecutors who desire to handle a case leniently because the traditional means of case disposition are inadequate and inflexible.[23] Declining to prosecute at all ("screening") may be considered inappropriate if prosecutors fear defendants will think the disposition condons the behavior which led to their arrest.[24] Yet, according to Nimmer, prosecutors often do not want to adjudicate such cases. Even followed by a lenient sentence, prosecutors may view a conviction as inappropriate for several reasons: any adjudication consumes resources; if a defendant fights a conviction, further scarce resources are used; the behavior, though wrong, may not have stemmed from criminal intent and may not justify the stigma of conviction; or available sentences may not be suitable (e.g., probation would be only an irritant, prison too severe, a fine unpayable, a suspended sentence or unconditional discharge unsupervised, a conditional discharge too difficult to monitor).

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[23]See also Freed's comment on the limitation of traditional dispositions--namely, that they are either too harsh or too lenient (1974:144).

[24]Prosecutors are not only concerned that *defendants* will respond in this manner; they also anticipate negative *public* reaction to the non-prosecution of certain "offenders." While particular acts on the part of a defendant may appear very serious to the public, a closer assessment of the facts and circumstances of the offense may lessen its seriousness in the view of the prosecutor, judge and defense counsel. This phenomenon is documented in the Vera study of felony case dispositions in New York City (Vera, 1977). A PTD/I disposition for a relatively "light" case (as seen by the prosecutor) could be justified by the D.A. as having subjected the defendant to supervision. This would be important in the event the prosecutor "gets burned" at a later date (to use the phrase offered by Willits, *Hearings*, 1977, 18ff).

In such situations, according to Nimmer, prosecutors may view pretrial diversion to social services as a positive break from these traditional options. Madeleine Crohn, Director of the Pretrial Services Resource Center, has recently said,

> ...the essential purpose of diversion... is to offer a choice...[I]ts function is to give society a choice not to punish certain people... (*Hearings*, 1977:85)[25]

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[25]Probably because PTD/I has been seen as a positive option (a "break") for defendants (all the rationales assume the defendant is advantaged by participation), advocates tend to assume prosecutors will use PTD/I as an alternative to a more severe punishment (see, for example, the NAPSA Standards and Goals, 1978). This is an empirical question, however, and some research (see below) suggests that at least some diverted defendants would have received less stringent punishments (generally no prosecution at all or a sentence involving no constraints on their behavior). For example, in the federal system (according to the draft report on diversion, Meissner, 1978:IV, 9-10), "The guidelines state that prosecutors 'may divert any individual against whom a prosecutable case exists.' In reporting to the Department, the assistant must certify for each case that it has 'prosecutorial merit with no technical deficiencies.' In the real world of a United States Attorney's office a significant number of prosecutable cases with no technical deficiencies are regularly declined not because they lack merit but because there is simply not sufficient time to pursue them given other demands. They should be prosecuted but they are comparatively less significant and so they are declined. As the guidelines are currently drawn, such cases constitute valid diversion cases."

PTD/I advocates tend to confront such data by saying that prosecutors *should* not use diversion in this manner (NAPSA, 1978); however, they provide no mechanisms to assure prosecutors *do* not. If the case is carried far enough along in the dispositional process to assure that the defendant will be convicted and receive a more onerous sentence, prosecutors often do not wish to divert the defendant (having spent so much time, they would prefer a conviction). For example, in New York City, prosecutors typically will not divert a case after a preliminary hearing has been conducted; therefore, defense attorneys have little information with which to judge the strength of the case before the diversion decision is made (see Chapter II).

Furthermore, it is possible (although difficult to test) that when the public demands more serious punishments for youthful offenders, prosecutors are more likely to ask for harsh sentences and use diversion as a form of constraint/supervision for cases they previously would have *nolled*. Without longitudinal data on the same jurisdictions, it is impossible to document such a pattern.

Defense attorneys may also view PTD/I as a position choice if they believe (and can convince the defendant) that it maximizes the possibility of dismissal and/or achieves that goal more rapidly (at least for counsel): "In diverting a defendant, the thing that [defense counsel] looks for is getting a dismissal," reported a typical New York City defense counsel. The judge may also support the choice if it appears in the interests of justice (however that is construed in a particular situation): "[Diversion] is 'justice and it is inexpensive, but is not cheap justice.' It is good, inexpensive justice" (Brownstein, *Hearings*, 1977:46).

The rationale for PTD/I as a mechansm to increase the flexibility of case disposition options, particularly for prosecutors, appears to have taken on increased prominence in recent years. This may be partly in a response to increasing skepticism about the validity of the stigma-reducing and rehabilitative rationales for PTD/I. The idea of "diversion" as a dispositional mechanism to avoid stigma assumes the defendant will penetrate deeply into the system and is predicated on the idea of keeping people *out* of the process. Yet research evidence (see below) has indicated that without diversion some and possibly many diverted defendants would not have penetrated very far into the system. Furthermore, as a practical matter, once someone is *in*, the idea of "keeping him out" does not

make much sense, particularly if the case is taken far enough to assure it is appropriate for diversion (that is, prosecutable).[26]

Accumulated program experience and research evidence, in a variety of criminal justice fields has also challenged the intervention and rehabilitation rationale of PTD/I (see, Martinson, 1976; Rutherford and Bengur, 1976; McKinlay, 1978). As Madeline Crohn has put it, again succinctly,

> We...discovered that overenthusiastic claims have deeply hurt the diversion concept. Imagine the proposed task: to reduce crime and help the courts and protect the community and successfully re-integrate an often indigent, disenfranchised segment of the population into a 'productive life style.' In other words, a panacea... (*Hearings*,1977:76).

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[26]While it is still possible that diversion helps avoid a criminal record, the labeling justification assumes the negative impact of prosecution results primarily from this lengthy and punitive process itself, not just the final legal outcome. It is also possible that the diversion process may generate new labels and new stigmas, particularly if there is little or incomplete sealing of official records (see, Gorelick, 1975:198; Freed, 1974:155). In addition, the notion of helping the first offender by shielding him from the harmful effects of the criminal justice system is not subscribed to uniformly, even by traditionally defendant-oriented people. One defense attorney, in discussing the value of diversion over more traditional lenient dispositions which also lead the defendant out of the system rapidly, made the following (not uncommon) statement:

> [Diversion] is a good thing. If you just put someone back on the street, he really doesn't know what has happened. These programs help them understand the court process, what has happened, and to recognize they could get hurt by getting arrested. People learn to recognize that they have done something that is defined as wrong and that the consequences could be serious.

It is not surprising, therefore, that the major recent justification for introducing PTD/I into the federal system is that it offers a supervised alternative to adjudication. While in his introductory remarks to the Hearings on the Diversion Act of 1977 (S.1819), the Chairman of the Subcommittee emphasized diversion as a rehabilitative and cost saving device,[27] the final Committee Report submitted to the Senate after completion of the extensive testimony narrowed the rationale for pretrial diversion considerably. It is presented as,

> an alternative to prosecution...the primary advantage to the prosecutor [being] increased flexibility of case disposition options...for a class of cases for which traditional case disposition options are unsuited. *Nolle prosequi* is not appropriate because of the seriousness of the offense and the strength of the case against the accused. Plea bargaining or full prosecution is inappropriate because, despite the seriousness of the offense, surrounding circumstances or the defendant's history indicate no benefit would result from conviction. An alternative between dismissal and punishment is needed. Diversion offers that alternative...[O]ne charge leveled against diversion is that those persons most in need of rehabilitation are screened out and prosecuted, thus insuring the 'success' of diversion. Since the committee views diversion primarily as a method to increase flexibility of response in the criminal justice system, not as a cure for recidivism, this criticism does not reach the merits of the legislation. (Committee on the Judiciary, *Report* re S. 1819: 1-5).

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[27]"Diversion has apparently been effective in reducing caseloads and court costs and in reducing recidivism on the State and local levels since the first projects were initiated in 1965." (Senator DeConcini, *Hearings*,1977:11).

This rationale for a supervised alternative to adjudication specifies only one advantage to the defendant over normal case processing: a shorter period of supervision (p.4).[28]

As with S. 1819, the 1978 Standards and Goals for Diversion proposed by NAPSA emphasize a similar primary rationale for pre-adjudication diversion. The first goal offered is that,

> Pretrial diversion should provide the traditional criminal justice system with greater flexibility and enable the system to conserve its limited resources for cases more appropriately channelled through the adversary system. (p.24)

However, the NAPSA goals also include (with apparently lower priority and certainly less clarity) the more traditional diversion and rehabilitative goals:

> Pretrial diversion should provide eligible defendants with a dispositional alternative that avoids the consequences of regular criminal processing and possible conviction, yet insures that defendant's basic legal rights are safeguarded;
>
> Pretrial diversion should advance the legitimate societal need to deter and reduce crime by impacting on arrest-provoking behavior by offering participants opportunities for self development. (p.24)

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[28]While the description of the program of supervision provided in the legislation and the report suggest that the defendant should receive services as well as supervision, the emphasis is clearly on "an intensive programmed supervision," (*Hearings*, 1977:23) or behavior monitoring. Testimony to the Committee from the Justice Department prior to submission of the Legislation indicated that defendants currently in diversion programs within various federal districts were not receiving new or creative forms of service while under supervision. According to the Meissner research (draft, 1978), diverted defendants receive the same types of supervision and services as convicted defendants under sentence of probation; the period of such "pre-adjudication probation," however, may be shorter than would be the case if they had been adjudicated.

Insofar as the more recent view of PTD/I as primarily a dispositional alternative emphasizes supervision as the treatment rather than innovative counseling or services to alter behavior, PTD/I implies a deterrence model rather than a service intervention or rehabilitation model. Furthermore, this perspective need not imply that the defendant received a more lenient sentence than he would have without diversion. As suggested earlier, prosecutors can quite legitimately suggest diversion as an alternative to prosecution in *any* case (except those technically faulty and thus not prosecutable), even cases they might otherwise have declined. PTD/I becomes even more akin to a pre-adjudication sentence than more traditional notions of "diversion from" the criminal justice system suggest (Freed, 1974:151; Morris, 1974; Diversion from the Judicial Process, 1975:93). As with any other "sentence" (probation, prison, fine, etc.), one need not assume any effect on the individual offender other than an experience of punishment.

Since PTD/I is a *pre-adjudication* sentence, it faces certain administrative and due process problems (as does, for example, plea bargaining).[29] The fact that diversion takes place *pre-trial*, therefore, has generated considerable debate about its appropriateness

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[29]While some critics of PTD/I (Nejelski, 1976) are concerned that it expands the overall pattern of non-adjudicative (administrative) handling of criminal cases, others (primarily its supporters) claim that it encourages the increased visibility and formal regulation of the discretionary powers prosecutors already exercise.

(e.g., Freed, 1974; Nejelski, 1976; Gorelick, 1975). Nonetheless, this is also what makes it an attractive option to prosecutors and, with somewhat more qualification, to defense attorneys. A process which imposes a sentence or its equivalent without the assurances provided by adjudication should assume: that supervision is not given someone who is not guilty; that the punishment is in some way proportionate to the act committed; and that due process has been observed.

As a result of these concerns, recent discussions of PTD/I have emphasized the necessity for it to meet certain conditions. Those suggested by the NAPSA Standards and Goals (1978) include: (1) cases should not be diverted pretrial unless they are prosecutable; (2) time spent in the program should relate to the minimum sentence imposed for the offense if the defendant were convicted; (3) it should be voluntary; and (4) administrative and judicial procedures should accompany the process in lieu of the due process checks provided by adjudication.

Unfortunately, as a practical matter, it is extremely difficult in any actual situation to demonstrate such standards are being met. Without carrying a case through at least the early stages of adjudication, it is difficult in many jurisdictions for anyone, particularly defense counsel, to evaluate the probability of conviction. In addition, the standard reads "prosecutable" (that is, not technically faulty) rather than "to be prosecuted"; hence it is impossible for prosecutors to use diversion to increase the number of defendants over whom the system maintains at least some control. Since the minimum sentence for the offense as charged may be longer

than the sentence for the charge to which the defendant would plead or be convicted, the period of control may also be extended for some defendants. Justification of this process as "voluntary" is also suspect because this condition is typically assumed to be met if the defendant has counsel. However, the agenda of the defendant and that of the defense counsel may differ and both may have inaccurate perceptions of possible dispositional alternatives. Truly "informed consent," therefore, may be harder to obtain than is simple agreement to the diversion. Finally, due process procedures are often cumbersome and under some conditions may be ignored because they dissipate the time and resources presumably saved by early diversion.

In 1974, at the conclusion of the DOL evaluation of the early manpower diversion programs, Mullen identified the merger of the "diversion" (or dispositional) and "intervention/rehabilitation" rationales as the central dilemma of PTD/I in its first decade as a reform:

> No longer simply a means of securing the release of appropriate defendants, these alternatives added the goals of case screening and rehabilitation to the pre-trial process... It is clear that the pre-trial intervention concept poses a fundamental dilemma acutely reflected in the evaluation literature. The basic conflict is between the delivery of services to reduce recidivism (presumably among those with enough likelihood of recidivism to make such reduction meaningful) and the provision of a humane alternative for those not likely to recidivate. In practice, the former may become unintentionally or quite purposefully subordinate to the latter as defendants must pass a number of screening tests prior to admission. In most cases, the logic of such screening is either implicitly or explicitly the selection of minimum risk defendants. (1974: 2, 29-30)

As PTD/I enters its second decade, however, the credibility of its stigma-reduction and especially its rehabilitative rationales has been eroded. While they are by no means fully disproven, programs will probably find it increasingly difficult to justify a pre-adjudication sentence on the basis of it "helping" the defendant (or the public) by reducing stigma, intervening to improve life stability, or by reducing the likelihood of rearrest. The continued use of this option to dispose of cases *preadjudication*, therefore, may rest more heavily than before on the ability of defense counsel (and perhaps the program) to assure themselves and to convince defendants that program participation will reduce the period of time defendants spend in the criminal justice system and/or the amount of its supervision.

DISPUTED ISSUES: THE ACCUMULATED RESEARCH EVIDENCE

> Diversion has grown from a long-standing but informal and low-visibility discretionary practice of prosecutors and juvenile courts; to a widely-endorsed theory and formal reform concept beginning in 1967; to the subject of a wide variety of experimental projects and self-reports in the early 1970s; to the target of intensive and critical research in the past year or two. (Freed, *Hearings*, 1974:150).

The rationales for PTD/I promise increased justice and humanity of treatment as well as a reduction of court backlogs and cost savings (particularly if the criminal justice system does not have to pay for the PTD/I services). But the accumulated research evidence as to these benefits is thin. Despite the considerable research interest in PTD/I in the mid-1970s, Kirby in his 1978 review of the diversion research literature concluded, as did Freed earlier, that

"The lack of appropriate research does not mean that diversion is a failure. Rather, it means that research does not exist to demonstrate whether or not diversion has an impact on clients" (1978:29). Both Freed in 1974 and Kirby in 1978 appropriately pointed out that this situation is common in attempts to assess social reforms. There is rarely "definitive" research because social programs are complex and respond to a variety of needs and goals, not all of which can be easily researched, and because their implementation varies from place to place in response to local circumstances.[30]

Methodological Considerations

All reviews of the PTD/I research literature agree there are serious weaknesses in the designs that have been used.[31] The major methodological problems are the lack of adequate comparison groups; small sample sizes; inability to follow-up either program participants or comparison group members after program completion; inadequate

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[30] Thus, even so-called "national evaluations" (i.e., multi-site research) of a particular program or reform are not only difficult to carry out, they often do not answer questions of impact. Even if the program concept is clear and singular, its implementation is likely to vary greatly from location to location.

[31] See for example, Mullen, 1974; Rovner-Pieczenik, 1974; Freed, 1974; Zimring, 1974; Mintz and Fagan, 1975; Galvin, III, 1977; and Kirby, 1978, who also notes similar methodological problems in related criminal justice research (p.11).

record data (e.g., subsequent arrest and conviction); and the exclusion of those who fail to complete the program in the analyses of program outcome.

The first of these is the most serious and warrants additional commentary. Research that lacks comparison groups (that is, one-group pre-post test designs) are inadequate for PTD/I evaluation (see the excellent discussion by Rovner-Pieczenik, 1974). The major problem with such designs is that the impact of the program cannot be separated from the effects of subjects' maturation or historical change. For example, diversion services might result in increased employment, but the passage of time alone might have the same effect; program participants might show an increase in re-arrest frequency, but the increase might have been even greater without the program; alternatively, they might show a decrease in arrest frequency that could also have resulted from decreased police surveillance or an unofficial change in arrest policy.

Even PTD/I evaluations *with* comparison groups have generally been assessed as inadequate because the groups were not truly comparable. (Again, see Rovner-Pieczenik, 1974.) Comparisons of program "successes" with program "failures," or with defendants who are eligible but rejected for the program, or with all defendants who fell within formal eligibility criteria have been used to evaluate PTD/I programs but their results cannot be regarded with much certainty. Non-comparable comparison groups are a *particular* problem in PTD/I research because these programs are highly selective. In most jurisdictions, many defendants may be eligible on formal, stated criteria, but screening decisions typically use

informal or hard to define criteria (e.g., "amenability to rehabilitation") applied by many different people, including the defendant, the defense attorney, prosecutors, program personnel and judges. Therefore, even when the most sophisticated matched comparison groups are used (such as in the Monroe County evaluation (Pryor, 1977), there is no way to assure that participant and comparison groups contain the same types of individuals in terms of "motivational" (however that may be defined), perceptual, psychological or unmeasured social and demographic factors. As Kirby (1978) notes in his assessment of the otherwise excellent Monroe County research,

> The matched comparison group is carefully chosen and equivalence is clearly demonstrated. However, program clients are screened by the program and district attorney [and by themselves], while the comparison group is selected by researchers. Thus, the two groups could be different because of varying selection procedures. (p.16; bracket added.)

Therefore, while "Given equal care in design and implementation, there is no reason why the quasi-experiment cannot perform significant tasks in correctional evaluation, carrying out many assignments now thought possible only by use of the controlled experiment" (Adams, quoted in Pryor, 1978:72), there *is* reason to avoid quasi-experiments in pretrial diversion research.

Despite such methodological problems, the accumulated research evidence of the last decade suggests certain patterns in the impact attributed to PTD/I programs. While not every PTD/I program may expect to achieve every outcome (see Mullen, 1974), certain program effects are suggested by the common PTD/I rationales; they are incorporated as research issues into the NAPSA Standards and Goals

for evaluation (1978),and they have been at least touched on by previous research.

Diversion From Stigma

Understanding the aspects of PTD/I related to its impact on disposition requires assessing who is diverted, that is, what risk defendants face from prosecution: the degree of penetration into the criminal justice system they will experience, their likelihood of acquiring conviction and other negative labels, and the stigmatizing consequences of such labels.

Early research expressed considerable concern as to whether cases were diverted that would otherwise have escaped prosecution, thus raising the issue of whether defendants and the system itself was actually reaping the benefits of diversion *from* normal criminal justice processing. Yet, in her review of existing PTD/I research Rovner-Pieczenik (1974:90) found only three of the fifteen available studies provided comparison data on case disposition. In these programs, 54 percent (Washington, D.C.), 30 percent (New Haven, Connecticut), and 51 percent (New York City) of the comparison group cases were *nolled*, dismissed, or acquitted without diversion. This was in contrast to 86 percent, 73 percent and 52 percent, respectively, of the diverted groups. Rovner-Pieczenik concluded that,

> Lack of confidence in the equivalence of the non-participant group against which patterns of disposition and sentences of PTI participants were assessed, and in other methodological problems, however, does not enable us to state with confidence that this apparently favorable adjudicatory treatment was due to program participation. (p.92)[32]

While more recent data are based upon somewhat better comparison groups, the earlier concern continues. Over one-third (36 percent) of the Monroe County matched comparison group were not convicted (Pryor, 1978:79) as was the case with almost one-fifth (18 percent) of the comparison group in the federal Eastern District of New York research reported by Meissner (draft, 1978). Unfortunately, in the latter, the sample size is too small (N=28) for the percentage to be at all reliable.

With respect to defendants' relative penetration of the criminal justice system, comparative data on diverted and non-diverted groups are even more scarce. While many studies assume penetration was limited without diversion because of the types of defendants diverted this assumption was typically based on the "lightness" of the charges and not on detailed comparison of evidence such as the number of court appearances, pretrial detention time, bail or trial experiences. The Monroe County data cover this issue, but the reason for the results are not clear: "The Diversion sample actually logged more court events

[32]Mullen's early research on the nine initial DOL programs (1974) also had great difficulty addressing this issue because she was not able to establish adequate comparison groups. The closest data available in the study, therefore, was a comparison of favorable and unfavorable terminations from the programs. These data suggested that about two out of ten were dismissed, acquitted, or *nolled* after leaving the program unsuccessfully; however, since disposition data were lacking on three out of ten cases, the analysis is further weakened.

(excluding preliminary hearings, trials, pleas and sentencing dates) at the lower court level than did the Control sample (Pryor, 1977:68)."

Furthermore, hypotheses from labeling theory concerning the role of diversion in protecting defendants from exposure to harmful, stigmatizing labels have not been assessed. While this is understandable given the intrinsic difficulties of such research,[33] it is somewhat surprising that research data have rarely been used to consider whether diversion helps individuals avoid *particular* labels presumed to be stigmatizing (e.g., "guilty;" "ex-con;" "divertee"). Although several evaluations provide comparative data on the proportions convicted and jailed, they typically do not do so in conjunction with data on individual's prior arrest or conviction status or with an examination of record sealing practices in the relevant jurisdiction. For example, in the Meissner data on the Eastern District of New York mentioned above (draft, 1978), a third of the diverted defendants and the control group had prior records and 75 to 80 percent plead guilty,[34] but these data sets are not cross-tabulated to show how many individuals were able to maintain a clean record because of the diversion process. The sealing of records is also not discussed either.

[33]First, although PTD/I has used the language of labeling theory as a rationale, it is not clear programs themselves are persuaded by it. Second, the collection of data to test the assumptions is difficult in most jurisdictions. Third, critics of labeling theory have pointed out that the theory itself is difficult to operationalize, i.e., testable hypotheses are difficult to construct in specific research settings (Gibbs, 1966:9ff).

[34]Unfortunately, the report is still in draft form, and the data are not only based on small samples but are not always complete.

There is evidence in the literature suggesting that, although most diverted defendants have been first offenders, it is possible that an increasing proportion *may* not be.[35] The effect of diversion on helping defendants avoid stigmatizing labels, therefore, cannot be assumed and it has not been adequately assessed.

Furthermore, while most of the literature (as well as the NAPSA Standards) suggests diversion helps defendants avoid the *consequences* of stigma, there is very little empirical evidence on the concrete problems diverted individuals experience or are likely to experience because of arrest, conviction, or prison records, and the status of record sealing in the jurisdictions under consideration has rarely been discussed. Had these issues been addressed (as is suggested by the NAPSA research standards), the data would have added valuable information to the growing body of literature from other areas on this problem.[36]

[35]For example, about half the Monroe County diversion program participants were not first offenders (Pryor, 1978:86).

[36]The diversion literature assumes there is considerable stigma associated with arrest, conviction, and prison records in three areas: self-identity, employment, and future arrests. What is particularly relevant to diversion is that no one can adequately assess the actual stigma and social handicap resulting from these labels. The evidence, however, is not uniformly in support of the labeling hypothesis. For example, in recent research by Bernstein et al. (1977), the effect of a prior arrest and/or conviction record on the degree of charge reduction for defendants convicted of a subsequent offense is in the opposite direction. Charge reduction is *least* for *first* offenders. The authors suggest that upon rearrest prior experience with criminal justice processing may be more helpful to the defendant than prior labels are harmful.

Recidivism And Intervention In Life Stability

Despite recent contentions that diversion is not primarily designed to reduce recidivism, virtually all evaluations have attempted to assess PTD/I programs' impact on defendants' subsequent criminal behavior. Typically they have focused on measuring the proportion of program participants rearrested either during PTD/I participation or shortly thereafter; in some cases, data on comparison populations are also provided.

Again, the findings of both early and more recent research are mixed. Rovner-Pieczenik suggests that *some* (not all) program data indicate potentially lower *in-program* recidivism rates compared to non-participant groups, although "the *extent* of this decrease in recidivism among participants...cannot be ascertained" (1974:79). As far as *post-program* recidivism, however, her evaluation of the various studies does not support their general conclusion that programs reduce recidivism: "[T]oo many uncertainties in the evaluation methodology [exist] to conclude the issue either positively or negatively" (p. 84). Mullen's analysis of the DOL programs led her to a similar overall conclusion, although in the one site that had compiled comparison group data (Minneapolis), she found "a positive -- albeit short-term effect of this project's service on the incident of rearrest among participating defendants" (1974:114). Mullen's general conclusion is that most programs select

low-risk defendants in an attempt to provide a humane alternative to the stigma of a conviction record; since this client population does not seem highly at risk of recidivism, these programs do not have a sizeable impact on rearrest.[37]

More recently, a study of rehabilitation and diversion programs in New York City (Fishman, 1975) argues that diversion did not reduce recidivism, it increased it! This spectre has been present with diversion since its inception,but has generally remained unstated (for an exception see Vorenberg and Vorenberg, 1973:177). Unfortunately, the Fishman study is so seriously flawed methodologically (Zimring, 1975; Kirby, 1978:21-22), its conclusion must be disregarded. Also unfortunately, another recent study of an inner-city urban population (Washington, D.C.) which asserted the opposite conclusion about recidivism and diversion, was also seriously flawed methodologically -- it has no comparison group (Williams, 1978).

Finally, and most significant among the recent research, is the Pryor study of Monroe County (1977; 1978). While the most methodologically sophisticated, it lacks a random selection design; the researchers conclude that,

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[37]Unfortunately, the recidivism data for the Meissner research on PTD/I in the federal system are not available as of this writing. However, in Chapter IV of the 1978 draft, the diverted population is described as a low-risk one by virtue of its characteristics: most were charged with job-related crimes and 54 percent were employed; their average age was 32; 57 percent were white, 66 percent male, 44 percent married, and 61 percent were at least high school graduates.

> the program has led to a 35 percent reduction in the one-year rearrest rate for official clients (from 37 percent to 24 percent), and a 45 percent reduction in the conviction rate on those arrests (from 22 percent to 12 percent)...[T]he biggest difference in rearrest rates between the program and comparison samples occurred within the first three months (when the program presumably has the greatest amount of control or impact on the lives of the participants). During that time, 5 percent of the program sample was rearrested, and 19 percent of the comparison sample. But, even though the differences in rates were less through the remaining nine months of the follow-up year, the comparison sample continued to have more rearrests throughout the year (e.g., 14 percent vs. six percent in the last three months (1978: 81-32).

Assessment of PTD/I programs' impact on participants' life stability (whether seen more or less as an end in itself as in the manpower programs, or as a means to reducing recidivism) has not been adequately treated in the evaluation literature. While impact on stability was a central concern in the Mullen DOL research, the analysis was limited by the lack of comparison groups. Although unemployment levels among diverted defendants generally dropped during program participation, their success appeared short-lived and quality of the jobs they obtained was poor (1974:63-68). In addition,

> Despite the fact that sixty percent of all incoming defendants were non-high school graduates with twelve percent reporting the completion of eight grades or less, program activity in developing outside educational opportunities for participants was fairly limited. (p.70)

Rovner-Pieczenik's 1974 review of early research efforts also suggests that some PTD/I programs achieved short-term (within program) employment-related gains (using one-group pretest-posttest designs),although the extent of the changes was not clear and long

range impact was impossible to assess (pp. 55-73). Unfortunately, the one recent research effort with a comparison group (the Monroe County research) did not address the issue at all.

> Attempts were also made to measure program impact in bringing about progress in various types of social or behavior problems that program clients had at entrance to the program. Some tentative initial analyses of impact in these areas were begun by the authors, but since there was no way of determining progress on such problems for the comparison samples, no real conclusions were possible. (Pryor, 1978:75)

The only study that attempted to assess the effect of different types of diversion services on different sub-groups of participants was Mullen's evaluation of the DOL program. While the analysis was sophisticated, it suffered from both the lack of comparison groups and from serious loss of cases during the follow-up period, the same problems alluded to in the Pryor research quoted above. Nevertheless, the findings are extremely interesting and deserve serious consideration. They relate to an important issue raised earlier concerning PTD/I programs' shift from "manpower" oriented service programs to those emphasizing primarily "counseling."

Mullen found that,

> program employment services can be a significant factor in improving the employment status of groups traditionally at a disadvantage in the labor market [i.e., those with long periods of prior unemployment]. We can also discern a weaker but positive effect of these employment services, presumably acting through the impact of better jobs, on recidivism among these groups...The second major finding of this section is that the impact of counseling services also is highly conditioned by the nature of the participant ...In particular, we see that a reasonably stable employment history seems positively associated with successful participation in counseling (as measured by subsequent recidivism)...Hence, we can look upon counseling services as a means of helping to ensure the good behavior of low risk participants (as are both females and employed persons) while positive change in higher risk offenders seems associated with employment services. (1974:148-159)

Finally, we cannot move to a discussion of other possible PTD/I impacts without noting the issue raised by Freed in 1974 that has not been addressed by any research on diversion. The question, however, remains important: would the *post-conviction* provision of services such as those available in good PTD/I programs result in similar reductions of recidivism and improvements in life stability (if any) for defendants now diverted *pretrial*?[38]

Diversion As A Prosecutorial Alternative

Is the overall effect of PTD/I to reduce the impact of the criminal law or to expand it? Is it an alternative to punishment or to no punishment? As a "period of supervision," is it equivalent to, less, or more than results from normal criminal processing? The juvenile diversion literature (Gibbons and Blake, 1975; Cressey and McDermott, 1974; Rutherford, 1975; Klein et al., 1975) suggests that diversion in the context of the police and juvenile courts "widens the net" of social control because juveniles who would otherwise not be officially treated are typically diverted. However, as Kirby has pointed out, the issue of expanded social control for adult offenders is extremely difficult to measure because the concept is hard to define and operationalize:

[38]This is the opposite side of the coin to the question raised earlier concerning the "diversionary" aspects of PTD/I: would diversion *without* services provide the same effect (if any) as diversion with services -- is it "diversion" or "services" that have the effect? As suggested above, there have been no tests of this question largely because pretrial diversion has been wedded to intervention from its very inception.

> To argue that diversion programs take minor cases is not the same as arguing that those cases would be dismissed in court (as many critics claim).[39] The empirical definition of social control needs further work. For example, is one day in a diversion program the equivalent of social control for one day in probation or incarceration? How should the lack of a police record be measured as a lessening of social control through diversion? (1978:23)

This issue focuses primarily on an examination of the *sentencing* impact of PTD/I. The Rovner-Pieczenik review of early evaluation studies suggested that few diverted defendants (or members of comparison groups) spent time in jail. For the three comparison groups, however, the period of supervision from probation does appear longer than the period of diversion services for the participant group (1974:90). Unfortunately, suspended sentences are combined with probation as outcomes, so the issue is hard to resolve (and it is compounded by all the methodological problems with these studies noted earlier). Nevertheless, both the more recent Meissner research and the Pryor study suggest that the length and possibly severity of sentence may be greater for non-diverted population. From the data on the Eastern District of New York (again note the

[39]A good example of this fallacy comes from Austin (1977). In examining the San Pablo, California, diversion progran run by the Probation Department, Austin notes its description of the diverted defendants:

> These people were not the kinds of persons we usually see on probation. There aren't any real crooks -- maybe only occasionally do we get a real crook and we usually reject them. (p.45)

Yet, the preliminary data on the disposition of the first 15 cases pulled in randomly assigned control group, Austin found 13 were convicted (one dismissed and one absconded); overall, more than half of those convicted received probation *plus* either volunteer work, a fine or a jail term as a sentence. The final results of the study will be interesting.

total research population was small -- 52 subjects), Meissner reports that 80 percent of those convicted received three year probation terms and 20 percent of those convicted some prison time; for the diverted group, however, the period of diversion supervision by Probation was about one year (1978 draft). Pryor also reports that only two diverted defendants in the sample (1.5 percent) received jail time compared to 18 in the comparison group (13 percent) and that nine diverted defendants (seven percent) received probation compared to 23 (17 percent) in the comparison group (Pryor, 1978:83). Again, however, this was a matched and not randomly selected sample.

One related issue not dealt with by any of these studies but of considerable concern in the PTD/I literature is that of "double jeopardy" -- whether diverted defendants who do not complete the program and are returned to court without a favorable recommendation are subject to more stringent punishment than they would have been without diversion.

Finally, diversion as a prosecutorial alternative suggests several *system* issues: how frequently is diversion utilized as an alternative (and, therefore, does it have an impact on court or prosecutor case loads) and is it more or less costly than normal court processing?

Rovner-Pieczenik (1974:89) noted that program impact data were insufficient to estimate the effect of diversion on court congestion. Mullen reported court officials in DOL program jurisdictions had uniform reactions: "projects were simply not handling sufficient

numbers of defendants to materially affect any backlog problems. In fact, some were concerned that under an expanded program, continuance requests might adversely affect court calendars" (1974:177).

More recent data do not contradict this earlier assessment. Pryor reported that the Monroe County program averaged about 300 clients a year. Although there was no data as to what proportion of the court calendar this represented, the report did suggest these 300 individuals were about 2.6 percent of the arrested defendants who met general eligibility criteria (1977:152). Also, in site visits to seven programs in 1976-1977, Vera researchers found that only the diversion program in Dade County appeared to divert a potentially significant proportion of the court case load -- 15 percent of all felony arrests in 1976 (Vera, 1978:114).

Cost analyses are also complex and the previous results were mixed. Rovner-Pieczenik's summary of the early cost-benefit analyses were extremely cautious:

> The cost/benefit analyses undertaken by Dade, Midway, MCEP and Crossroads, each concluding favorably in terms of program savings, were no more or less valid in their conclusions than were the previously discussed analyses of participant employment and recidivism. In other words, methodological limitations in the design and implementation of evaluative research hampered each program's ability, to some degree, to conclusively establish a favorable program cost/benefit ratio. The Crossroads analysis, being the most sophisticated and least open to question, suggests that cost/benefits did accrue to that program, but that the benefits outlined may not be as extensive as the program originally concluded. (1974: 102)

Mullen (1974:171-177) did not attempt cost comparisons between diverted and normally processed defendants, but found that costs per enrollee and particularly per defendant who completed the programs were high. Furthermore, "In an attempt to relate these costs to participant outcomes,...[r]ecidivism shows no relation to the amount spent by programs. Percent time employed does show a fairly strong association with cost, but the direction of the association favors low cost projects" (p. 173-194).

In contrast, however, the 1977 Pryor research on Monroe County found the program "to have a benefit-to-cost ratio of 1.3 to 1, based on one year of diversion and one year of recidivism benefits...The major portion of the program benefits was attributable to savings from reduced probation and jail sentences, reduced pre-sentence jail custody, and reductions in the number of pre-sentence investigations needed..." (1978:84).

These data suggest, therefore, that the relatively high cost of PTD/I programs would only represent *savings* in the short run if programs were successful in selecting and diverting defendants who would otherwise be subject to extensive court processing and post-sentencing periods of official supervision. If, in addition, they were successful at intervention (particularly reducing recidivism), their longer term cost position may also represent a saving. It is important, however, to note that even if processing a defendant through a PTD/I program is less or no more expensive than normal court processing, this does not necessarily mean diversion is cost-effective for the particular jurisdiction. If the program is not diverting a

sufficiently large proportion of the overall case load in the jurisdiction, the program is not likely to represent an expenditure *in lieu* of an additional court part, new jail facilities, or one or more probation officers. Rather, the program will simply be an *addition* to whatever resources are necessary to process and supervise the jurisdiction's full caseload.[40]

In summary, therefore, the accumulated research evidence does not provide a very satisfactory evaluation of specific PTD/I programs and it does not permit an *overall* assessment of the serious, disputed issues that have plagued the reform as a whole. "In short, embarking on a diversion program is pretty much an act of faith" (Galvin, 1977 III:44).

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[40] This is not always the case, however; despite its relatively small caseload, the Monroe County evaluation was able to demonstrate to the satisfaction of the county (which funds it) that diversion resulted in net savings of *marginal* costs. These funds could, therefore, be either saved or reallocated to other county uses (Pryor, 1977: 95-120).

CHAPTER II

THE COURT EMPLOYMENT PROJECT: A CASE STUDY OF PRETRIAL REFORM

INTRODUCTION

The history of the Court Employment Project in many respects parallels that of the diversion movement as a whole. During its first decade, CEP was a highly successful program. It was the model for PTD/I programs in many other jurisdictions, it expanded greatly, and it made the transition from federal demonstration to local funding with considerable ease. However, since its institutionalization, it has also faced some of the same pitfalls as other PTD/I programs. CEP began its second decade with a contraction of services resulting from cutbacks in local budgets and, in the course of two evaluations, has had to face difficult questions concerning the premises of its operations.

In this chapter we look in greater detail at CEP, its decade-old program rationales and the context within which they developed. We also describe its target population and selection procedures, its service delivery system and the problems it has faced administering those services. While the overall picture is historical, the primary emphasis in the account is CEP as it was structured and operated during calendar year 1977. The reason for this focus is that the analyses reported in the remainder of this document measure the impact of CEP's operations on Criminal Court defendants diverted

jail sentence) and the Neighborhood Youth Diversion Project in 1970 (a juvenile diversion project).

A common thread in all these efforts was the notion that certain groups could and should be removed from the criminal justice system, sometimes (as in diversion) to other forms of social intervention considered more appropriate to their personal needs and to society's interest in reducing the probability of the offense being repeated. In the early CEP project three specific goals were articulated: first, to provide employment services to people involved in the criminal process because no such services were geared to their particular needs (Sturz, October 1967:1-2); second, to give these services pre-adjudication so as to reduce detention and court processing and provide the defendant with a basis for having the charges dismissed; and third, to demonstrate that employment services could have life stabilizing results within a short period of time (initially three months) and that this could be a "step toward rehabilitation" (Sturz, May 1967), that is, reduced recidivism. The conceptual links between employment services and rehabilitation were the same as those underlying all the DOL diversion programs -- employment would give defendants an economic stake in avoiding crime and thus abort the all too casual process by which they were thought to develop criminal careers.

After three years as a DOL demonstration project, CEP spun off from Vera at the end of 1970 and became an independent, not-for-profit corporation funded by the City of New York. Since 1971 it has provided a wide variety of vocationally-oriented services under

contract from the Human Resources Administration, a city agency providing support for social, employment, and other services. Despite non-criminal justice funding, CEP continued to obtain the majority of its clients through the diversion of Criminal Court defendants. Whereas the specific procedures through which it identified defendants as eligible for diversion, secured agreement from court personnel to divert, and provide specific services changed somewhat during the ten year period, the basic premises of the agency had not changed greatly as of 1977. The experience of a decade, however, had resulted in a somewhat more modest statement of those aims than that found in the original proposal to DOL:[1]

> The Court Employment Project...seeks to provide vocational training and assistance in educational and job placement to defendants with a view both to bring about dismissal of the charges against them and to reduce their prospects of recidivism by equipping them with skills and information which would lead to a more stable position in society.... A major goal of most clients' efforts while in the program is placement in a job or appropriate educational/vocational setting....With the recognition that job procurement is not simply a matter of matching skills and people and the realization that an average four month program is insufficient for dramatic changes for most clients, the counseling staff analyzes both short and long range needs of the client [including] the ability to read a subway map, proper use of the telephone, completion of an application form [and] being a parent or informed consumer ....(Project Brochure, December, 1977).

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1 "The project hopes to demonstrate that early diversion from the criminal justice process to employment and/or job training will combat recidivism; reduce court backlogs; reduce expenditures for prosecution, trial and incarceration; increase the supply of skilled labor; and provide a tax-paying, trained asset to the community in the form of a law-abiding employee, rather than a liability in the form of a prisoner." (Sturz, October, 1967)

Since its catchment area as a social service agency was the courts[2] and diversion the mechanism for obtaining clients, CEP continued to justify the provision of services *preadjudication* through the traditional diversion rationales. First, youthful defendants were thought to need an incentive to encourage their participation in social services and a dismissal of the charges was considered a powerful incentive; and second, reduced penetration of the criminal justice system and the avoidance of stigmatizing labels were viewed as benefits in their own right. Whereas the agency also continued its commitment to the notion that needed services could be provided in a short period of time (three or four months), its definition of what those services should be and what type of intervention they represent changed somewhat over the ten year period. CEP came to see itself less an an employment service and more as a comprehensive vocational services agency (including educational, pre-vocational, health services). In making this change, CEP also recognized that its short range impact on the life stability of individual clients was likely to be modest. (See the discussion of services below.)

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2 Originally just operating in Manhattan, in 1971 CEP received funds to expand to Brooklyn and the Bronx; in 1973, it also began diverting defendants in Queens. By 1975-6, CEP had a budget of $3.4 million to service an anticipated 2,600 diverted defendants as well as other, often court-related clients. However, in 1976-77 as a result of the major fiscal crisis into which New York City was plunged in mid-1976, CEP's budget was cut to just over $1 million; while it continued to divert defendants from all four courts, its client population was reduced to about 1,000 per year.

In order to understand CEP's structure, operations and interpretations of its goals, it is important to visualize the major context within which it has always operated -- the criminal courts of New York City. New York's criminal justice system is unique in many ways, but its size and volume perhaps contribute most to that uniqueness. New York City arraigns more than 200,000 defendants each year, over 100,000 felonies, in four lower or criminal courts where the arraignment parts run day and evenings, 6 days a week. Many of these cases are both arraigned and disposed at the first court appearance (15 percent of the felonies); overall, most are disposed through a dismissal (43 percent of the felonies) or a plea bargain (55 percent of the felonies); only two percent of the felonies go to trial. On any given day, there will be as many as 2,500 defendants in detention awaiting disposition or sentencing on felony charges, overcrowding old facilities meant for much smaller numbers. The backlog of cases awaiting arraignment has been such that the time period between arrest and arraignment has been as long as 48 hours, and has ranged in recent years between 24 and 48 hours. The majority of the defendants are indigent in that they are eligible for free legal counsel through New York City's Legal Aid Society. They tend to be men and minorities, and many are also young and unemployed, as might be expected with a city minority youth unemployment rate running as high as 40 percent.

In this context, therefore, it is not surprising that a program that attempted to remove cases from the court at arraignment or shortly thereafter and that, in addition, provided manpower and other

social services to an extremely needy population, would eventually find considerable favor, yet still have only a small impact on the backlog of cases in the court. At its peak, CEP diverted about 2,600 cases a year, which was only about two percent of the total number of cases arraigned per year. Nonetheless, when it first began, the diversion of criminal defendants was a very new, untested idea. CEP did not in 1967 and still does not in 1979 have a statute or court rule authorizing diversion.[3] It has always operated entirely by informal and continually re-negotiated agreements with prosecutors and judges in four different jurisdictions. Prosecutors defer prosecution on a discretionary basis after entering criminal charges and diverting cases to CEP.

ELIGIBILITY CRITERIA

At its outset in 1967-68, CEP had very broad general eligibility criteria for diversion because of its social service aims. A large proportion of Criminal Court defendants were in need of employment and training assistance, and the project did not wish to exclude any who could benefit. However, the actual selection of defendants realistically had to reflect judges' and prosecutors' concerns about defendants' current charges and prior criminal histories in order to

[3] New York State does have a law (CPL 170.55) permitting prosecutors to adjourn a case in contemplation of a dismissal (ACD); the case is automatically dismissed at the end of a six-month period if it is not placed back on the court calendar. CEP does not and never has operated under this law since cases are not ACD'd at the time of diversion; instead, a four month adjournment of the case is obtained during which the prosecutor defers or suspends prosecution. The ACD law was, in fact, not enacted until 1971, when the idea of "pretrial diversion" had become a more accepted phenomenon, probably in part as a result of CEP and the other diversion programs that developed during this period. See the discussion in Chapter IV concerning the consequence of the ACD law for CEP's diversion activities.

encourage their agreement to divert. Therefore, initially the formal eligibility criteria excluded those charged with the most serious felonies and those who had served prior prison sentences. Individuals who were chronic alcoholics or addicts were excluded as were those already employed or charged with occupational crimes because the program did not think it could provide appropriate services. In its earliest months, the program saw itself largely as an employment service and, therefore, the CEP staff concentrated on identifying specific defendants among those who were eligible for whom the staff could immediately find suitable jobs or training openings (Sturz, October 1967).

Since its early days as a DOL manpower demonstration project, these basic eligibility criteria changed relatively little.[4] CEP continued to exclude chronic alcoholics and drug abusers and to divert only adults (i.e., those 16 or over charged in the adult Criminal Court). They did, however, expand their social service offerings and thus began to include as eligible defendants who were not immediately employable and those who were underemployed or marginally in school. This change came about because, within the first few months of operations, CEP staff realized just how educationally and occupationally disadvantaged their client population really was. Being young, lacking formal education and work experience, the defendants they screened for diversion found it difficult to hold

[4] For a discussion of the detailed shifts in eligibility critera during the first crucial three years, see Vera, *The Manhattan Court Employment Project: Final Report*, 1972.

jobs even when they had them. Therefore, it was felt that most needed substantial multiple services in order to make a successful vocational transition to employment.

Within this rather broad definition of who was eligible for its services, CEP's major changes in eligibility requirements over the decade were related to the program's *diversion* rather than social service aims. Guided by its diversion rationale, CEP gave services preadjudication and, therefore, wanted to divert and service only defendants who would otherwise be adjudicated guilty. To some extent its belief that almost *all* defendants needed vocational services conflicted with its desire to divert and service only those facing conviction. CEP attempted to reconcile this by diverting only those who faced conviction but to provide services on a voluntary basis to anyone referred from the court.[5]

Nevertheless, it has always been a very difficult problem for CEP to assure it did not "overreach" and divert cases unlikely to be convicted. This was largely because of the powerful role played by the prosecutor in the decision-making process (see below, Selection and Intake). In an attempt to assure diverted defendants were facing prosecution and conviction, CEP changed its formal eligibility criteria over the years, moving increasingly toward the decision made in January 1977 to divert only felony defendants and not to exclude anyone because of his/her prior arrest, conviction or prison

[5] The number of non-diverted clients was always substantial; in addition to family members and friends of clients, CEP has always taken non-diverted referrals from the court. At its peak of operations in 1974-75, for example, CEP gave services to over 700 such individuals.

record. In fact, however, as discussed below, because of prosecutors' unwillingness to divert repeat offenders, few defendants with prior convictions were accepted.

Before we turn to a discussion of CEP's selection and intake procedures, however, several other formal eligibility criteria should be noted. Defendants must be residents of New York City (excluding Staten Island) and have no outstanding bench warrants or pending felony charge. They must consent to diversion as well as have their lawyer's agreement. The program never required the consent of either the arresting officer or the victim of the crime (although both were apprised of the diversion possibility if they were in court). Finally, CEP did not permit diversion if it was conditioned upon restitution or the defendant's agreement to testify in the case of a co-defendant. The restitution restriction stemmed from the agency's belief that restitution implied guilt and that diverted defendants had not been adjudicated; that restitution was often unrealistic for clients who were simply too poor to make payments; and that it placed an impossible administrative burden on an agency designed primarily to deliver personal and vocational services.

SELECTION AND INTAKE

As suggested above, as a social service agency, CEP defined its population as virtually any and all defendants in the Criminal Courts who needed vocational services, but primarily those who were

young and in need of multiple support services.[6] However, in the decision as to which specific defendants CEP would service, the major actor in the selection was the prosecutor. This is because of his or her centrality in the process of disposing of criminal charges.[7] Informal negotiations must be carried out continually to balance CEP's diversion goal of giving services only to defendants facing conviction with the prosecutor's goal of case dispositions. While these goals might coincide, they do not necessarily do so. As discussed in Chapter I, prosecutors might wish to divert cases they give low priority rather than dismiss them altogether or Adjourn them in Contemplation of Dismissal (ACD, an automatic dismissal in six months). Alternatively, they might want to divert cases that would otherwise occupy more time to convict than they think appropriate. Since there was no way to know for sure what the prosecutors considered the diversion alternative

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[6] It is important to note how different this is from many programs which emphasize "counseling." They tend to define as eligible only defendants judged "amenable to rehabilitation" or "moviated to change" (Vera, 1978). They support these eligibility requirements with screening procedures designed to assess individual's motivation and needs. They also tend to primarily "counsel" clients and are often deeply involved in restitution agreements. In these important respects, CEP differs considerably from other programs, and in some cases the differences are radical.

[7] At the outset, CEP sought to have the judge make the diversion decision, "the acquiescence of the District Attorney [being] desired but not necessarily determinative" (Sturz, October, 1967). The reason for the shift over time to the prosecutor as a major decision-maker probably resulted from the fact that prosecutors are typically the only persons with any information about the cases at the early stages of processing (at or just after arraignment). Prior to any hearings which disclose facts about the case, therefore, the judge must rely heavily on the prosecutor for information.

to be and since CEP always worked with numerous prosecutors,[8] the program had an active court screening staff to identify eligible defendants and advocate with prosecutors for their diversion. Even though during 1977 (see below), CEP shifted to a system of selection by referral rather than staff screening, its court personnel continued to be active advocates for cases the agency wanted and against those they thought unsuitable for either service or legal reasons.

As it operated in 1977,[9] the first step in the selection process took place in the Complaint Room where a senior Assistant District Attorney (ADA) evaluated all felony arrests to determine if they should be prosecuted as felonies or reduced to misdemeanors at the time of a plea. Cases that were to be charged as felonies, but in which the ADA would accept a plea to a misdemeanor (or possibly even a violation) were arraigned in Criminal Court and remained there for adjudication. While this was going on, or shortly thereafter, CEP screeners examined court papers to identify eligible defendants before their arraignment. The screeners discussed CEP with the defendants and their lawyers and then attempted to secure the permission of the ADA (either an ADA liaison or the ADA in the arraignment part).

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[8] It not only screened cases in four separate jurisdictions, it also had to negotiate with each of the many different prosecutors who rotated through the arraignment parts. While CEP was able to establish a system in which a single prosecutor reviewed all potential diversion cases, such liaisons tended to change, often quite frequently.

[9] Again, while there are some structural differences compared to earlier years, the process of screening in 1977 was not much different from before. The major differences were that in its very first months (1968) CEP screened post-arraignment; since then, however, it typically screened pre-arraignment. It used to seek a two-week adjournment at arraignment to assess the defendant's needs, but this was abolished to speed up the process.

Unfortunately, it was very difficult at this early stage in the adjudication process for either CEP or the Defense Attorney (often an overworked Legal Aid lawyer) to know what the ADA was likely to do with a particular case in lieu of diversion. Under New York State Law, there is no disclosure of the People's evidence at this time, and prosecutors do not ordinarily volunteer such information. Only at a preliminary hearing would the facts of the case begin to emerge. However, prosecutors typically require defendants to waive their right to a preliminary hearing as a condition of diversion to CEP. Their logic was that diversion was, among other things, a means of conserving prosecutorial resources. In addition, they wanted the waiver in order to excuse complainants from having to appear again should the case come back for prosecution. While Legal Aid lawyers protested the waiver requirement, it continued to be enforced by prosecutors.

Legal Aid attorneys regarded the preliminary hearing as their first opportunity to evaluate the sufficiency of the State's case. Many felt then that neither they nor their clients had a truly informed basis for consenting to or refusing diversion. Nevertheless, they might agree to diversion on the basis of a general assessment as to the defendant's liklihood of conviction. CEP had some difficulty in one borough where the reluctance of some Legal Aid lawyers to divert under these conditions meant few clients. Other defense lawyers, however, justified their agreement to diversion without a preliminary hearing on the basis of the defendant's long range benefit from CEP's social services, regardless of their immediate

convictability. Overall, Defense Attorneys were generally subject to a strong temptation to divert any case they could because they knew that, if the defendant cooperated, the case would definitely be dismissed. To go into a plea bargain or trial risked conviction and without more evidence, many felt diversion to be the defendant's best option to assure dismissal. This was a particularly powerful justification when the defendant was young and without a prior record. As one Legal Aid attorney put it, "We're sometimes overprotective on this score." In most cases, however, diversion was thought to be an easier, quicker, and surer route to a dismissal for an attorney with a heavy case load that included at least some very serious cases needing a great deal of attention.

Midway through 1977, CEP began a slow transition from the screener system of acquiring cases just described to a referral system. Instead of identifying and tracking cases themselves through the arraignment process, CEP screeners waited for a defense counsel (or an ADA or Judge) to refer cases to them. They then assessed the case and advocated for those they thought appropriate. This change was made for two reasons. First, CEP had come to recognize that active recruitment was extremely costly in personnel; it began to feel this was wasting increasingly scarce resources because screeners had to evaluate many defendants who were later found ineligible, rejected by the ADA or judge, or who themselves refused diversion (see Chapter IV). Second, under a new director, CEP had begun to alter somewhat its philosophy about screening. Because CEP tended to view its primary role as that of a social service agency, it decided to play

a less active role in determining who should be diverted. The agency's new director felt *diversion* eligibility should be determined primarily through the adversarial process with CEP's role in selection primarily being to identify defendants for the defense counsel, ADA and judge who could be helped by its services.

After mid-1977, therefore, most diversion referrals to CEP were made by defense lawyers and judges, although a few came from ADAs. Referrals were made before or after arraignment. CEP court staff interviewed referred defendants to see that they were appropriate candidates for the program's services and then asked an ADA liaison for consent to divert. Unless the defense counsel or judge vetoed the diversion application, the defendant became a participant at the next court appearance. However, CEP continued its attempts to assure the agency diverted only cases facing conviction. CEP's senior staff had frequent discussions with ADAs and Legal Aid attorneys to define the types of cases they considered appropriate for *diversion*, as well as for services. In addition, court screening staff attempted to make independent assessments about referred cases and to report problems to the agency's director.

PARTICIPANT CHARACTERISTICS

The socioeconomic profile of CEP's diverted participants between 1967 and 1977 is hardly surprising, given the characteristics of most

New York City defendants.[10] While their ages ranged from 16 to over 50, half were in their teens. During its first three years as a DOL demonstration, the median age of the participating population was 19; during 1977 it was 18. The vast majority were always men (about 99 percent), of minority ethnic origins, about half black, one-third Hispanic (Spanish-speaking), and just under one-fifth were white or of other ethnic origins. Their average school level was always low -- around 10th grade. As might be expected from their age, the great majority were single. As predictable from their age and education, very few were employed at intake. Throughout most of CEP's history, only about 15 percent of its clients were employed at the time of diversion and a third enrolled in school; the vast majority were not actively involved in either vocational activity.

This statistical summary of CEP's diverted clients suggests an unusually difficult and taxing target population for social services -- young, poor,[11] minority, undereducated and under-or non-employed. They lived in ghetto areas of the city, and for the most part, spent their days "hanging out" or "hustling." Counselors reported that most clients who had not already dropped out of school were attending irregularly. Most, according to counselors, read at third-to-sixth-grade levels, and some were functionally illiterate. Clients were often bitter about the educational system and complained to counselors that

[10] As for their criminal background, in 1977, virtually all were charged with a felony and about 15 percent had a prior conviction record.

[11] Typically CEP paid clients' subway fares to and from their CEP appointments because they could not afford the 50¢ each way.

their teachers were racist. Half their families were on welfare, and, if a parent was employed, it was typically a menial job. When asked by counselors, most clients said that they wanted jobs, but counselors believed many, perhaps most, did not understand what it meant to keep a job and didn't want the responsibilities of regular employment. Two examples illustrate this point:

- CEP placed James, a 16-year-old black male, in a job with a community agency. The agency was willing to be flexible about his hours so that he could attend some classes. Because the job required that he visit the courts from time to time, the employer requested, and then insisted, that James wear regular shoes instead of sneakers. He absolutely refused and wanted CEP to find him another job where he could wear his sneakers. (One counselor noted that "these kids are serious as cancer in the terminal stage" about wearing sneakers.)

- CEP placed Henry in a summer job at an agency on 11th Street in Manhattan. He refused to accept it because it was "too far away" from his home on 125th Street (a few miles, easily accessible by public transportation). So CEP found him another job on 108th Street. But Henry also refused that one for the same reason.

Counselors perceived most CEP clients as living entirely in the present, day-by-day. While this may be somewhat realistic given the clients' immediate circumstances, counselors saw as part of their role proding clients to think about what they must do to prepare for the kind of futures clients said they wanted. Yet the counselors said clients often had unrealistic expectations about opportunities open to them. Most CEP participants had no job skills and little education; yet they did not want menial, entry-level, and low-paying jobs because such jobs offered them no status and little money. They wanted to "make it," and they did not see taking a menial job as the way to do that.

According the CEP counselors, many clients were angry and bitter about their poverty; some felt themselves to be the victims of an unjust and corrupt social system. They wanted to get their share of the good life, but the opportunities for a college education and a good career were, generally speaking, closed to them. The obvious symbols of financial success in their communities were the "hustlers." The pimps and the higher-level drug pushers were especially attractive -- they had cars, women, and ready cash for no (or at least very little) work -- and they were widely admired as people who have "gotten over" on the system. If there was an ethos to this generation of poor minority youth, CEP counselors felt that ethos was to get what you wanted for nothing in a world that wanted you to have nothing.

Counselors felt that many of CEP's clients thought of welfare as a way to "get over" on the system, and hence most counselors were adamantly opposed to it. They believed it had as destructive an effect on clients as hard drugs, creating a cycle of dependency that became increasingly difficult to break.

CEP counselors gave the following descriptions of some of their individual clients:

- Anthony was with a friend who got into a verbal altercation with a police officer. The disagreement came to blows, and in defense of his friend, Anthony struck the officer. He was arrested for assaulting a policeman. Anthony's mother told his CEP counselor that he had been staying out late at night and had been hanging around a "very tough crowd." The counselor wanted to help Anthony, but Anthony kept him at a distance. He did not fear going to jail.

- Jorge, an illiterate Hispanic male, was arrested for stealing from a decoy police officer and for assaulting the officer who arrested him. He tended to use his physical strength instead of his brain to get along in life.

- Carl, in his late teens, was also arrested for stealing from a decoy policeman. He was utterly destitute when he came to CEP; he had nowhere to live and no money coming in. Although his counselor opposed welfare as a general matter, he thought that getting Carl on welfare was the first step on the road to recovery. Eventually Carl did obtain a job.

- Virginia was 16 years old and came from a highly religious family. She was arrested for snatching a purse, but her counselor doubted very much that she had committed the crime; he thought she had simply been in the vicinity when it happened. Virginia attempted suicide soon after the arrest, and her mother threw her out of the house. She was placed in a residential facility, where she was beaten by the other residents. The CEP counselor had considerable difficulty finding her alternative housing.

- Luke, a Vietnam War veteran, was arrested for burglary and larceny after he stole a lead pipe from an old building. It was his first offense. When he enrolled in CEP he had a part-time job and was taking correspondence courses. Soon after his entry into the program he obtained a full-time job, which he kept for several months.

- Emmet was arrested for robbing an elderly man. He insisted on his innocence, claiming that he had come along while others were robbing the man and he tried to help. The ADA found his story hard to believe -- especially as Emmet had a lengthy prior criminal history. He had just spent several months in jail and had had a severe drinking problem ever since his mother died several years before. Not long after enrolling in CEP, Emmet was arrested on burglary charges. He was terminated from the program.

SERVICE DELIVERY

Faced with clients plagued by these problems, it is not surprising that after ten years of operations CEP remained an agency devoted to expanding its social services rather than shifting, as described in Chapter I, to a more "counseling"-oriented approach. Counseling defendants to encourage changes in their behavior, in the expectation they would then avoid further criminal acts, did not seem appropriate

to most CEP staff because most of their clients lived in a social milieu where illegal activities were a regular part of life. Most were neither educationally nor socially prepared to make the transition into the world of regular employment, and certainly not in a three or four month period.

How then did CEP attempt to intervene in a life stabilizing way? It tried first to motivate defendants to keep coming to the agency during the full pretrial period so that their cases would be dismissed; it did this by providing services it hoped clients wanted and when necessary by reminding them of their pending court cases.[12] It tried to identify one or more realistic goals for the client which could be achieved in four months and which could be reported back to the court as indicative of increased life stability (e.g., back in school or attending more regularly; a part-time or full-time job; or progress in these directions such as getting working papers or a driver's license). It tried to assess special needs defendants had and refer them to specialized community services (such as health, residential, psychological, or recreational programs). Finally, it tried to give defendants a personal experience of progress or success in life -- a good relationship with an older person (the counselor), better relations with parents, increased life skills such as reading a subway map or using the telephone directory.

[12] Forty-five percent of CEP's diverted clients in 1977 failed to complete the program because they did not attend regularly.

CEP had a more complex system for delivering its services than many other diversion programs (Vera, 1978). This complexity seems to have resulted from CEP's rapid organizational expansion in the early 1970's (from operations in one to four boroughs) and from its efforts to expand the range and quality of services offered while also maintaining its policy of hiring paraprofessionals (ex-offenders) as counselors.

Between 1967 and 1970, CEP operated only in the borough of Manhattan with a staff of about 30 people at its peak. With its expansion into Brooklyn, the Bronx, and Queens, CEP opened three additional service offices. By 1975, CEP had a staff of over 220 people and was servicing over 4,000 clients of whom 2,600 were diverted from the four Criminal Courts. As the program expanded, CEP also broadened the types of services it offered. These included personal and situational counseling (in individual and group sessions); vocational counseling and preparation; in-house training programs (including basic literacy, English as a Second Language, tutoring and an extension site of Brooklyn College's School of Contemporary Studies in which some participants were enrolled); job development and placement services; referral to community programs for physical and mental health services, housing and residence, and other social services. Even though the fiscal cutbacks of 1976-77 resulted in a smaller program (one full-service office and a total staff of about 65), CEP continued to divert defendants from all four Criminal Courts and to offer many of the same services.

CONTINUED

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One consequence of the agency's rapid expansion geographically, administratively, and in the number of its clients and services was a much elaborated organizational structure. Originally, in addition to supervisory and administrative staff, CEP had only two types of direct service delivery personnel. The counselors, or "reps," were typically ex-offenders or "street-wise" people who discussed with clients their life situations, immediate and long range needs, and made one home visit to discuss CEP with the client's family. The other direct service personnel, Career Developers, generally had academic backgrounds or relevant work experience, worked with clients to find jobs, training programs, or appropriate academic settings. In the early years, Career Developers were responsible for job site development as well as career guidance and placement. However, as expansion occurred, these functions became more specialized, and by 1977 there were six major categories of staff involved in giving clients services. In addition to (1) the *Counselor* or "rep," there was (2) the *Vocational Counselor,* a more academically trained individual often with a college degree whose job was to counsel clients about their vocational and educational goals, test their reading and math levels, and prepare them for the job application and interview process. The (3) *Vocational Placement Specialist* was knowledgeable about specific educational or training opportunities in New York and about jobs available in various areas. He or she maintained networks of telephone contacts with employers and had the responsibility to actually refer clients to educational programs and job interviews.

The (4) *Job Developer* was charged with going into the community to solicit information about available jobs and interviewing clients before they were referred to specific openings. The (5) *Community Resource Specialist* maintained files on community resources and was supposed to make specific referrals to outside agencies. Finally, (6) the New York City Department of Social Services maintained a *DOSS Representative* at CEP to provide welfare and social service benefit information and referral to CEP clients and their families.

While the agency's rapid expansion was a major factor in this proliferation of specialized service personnel, an additional factor was CEP's commitment to hiring ex-offenders as personal counselors, combined with its concern that such counselors were not sufficiently experienced to always provide high quality services to clients. This commitment was as old as the agency. It was based upon the belief that,as "street-wise" people, some ex-offenders were attuned to the class and ethnic cultures of the clients and were best able to understand them and "speak their language." Such communication was seen as an essential element in assessing clients' needs and motivating them to work with more specialized service personnel. While over the last decade not all CEP's counseling personnel (new or old) were "street-wise" people, the program never backed away from its belief in their efficacy as counselors. The program, however, has not rejected the idea that college-educated, more middle-class counselors also have a role to play serving its clients.

Over the past few years, many PTD/I programs that originally employed ex-offenders as counselors have gradually moved away from

that policy.[13] Instead they have begun to hire college-educated persons or, less frequently, professional social workers. This is partly a reflection of the fact that many of these programs serve more middle- and lower-middle class clients than does CEP.

The conventional wisdom is that ex-offenders tend to be either overly sympathetic with their clients, reinforcing negative attitudes toward society and the criminal justice system, or overly harsh, bent on forcing clients to make the changes they themselves have made. Nevertheless, CEP persisted in its willingness to hire ex-offenders (though not to the exclusion of others) not least because it felt that these counselors, as successful members of the same class from which clients come, were good role models.

One consequence of this commitment, however, was a concern that the increasingly structured counseling and specialized services CEP thought appropriate for their clients could not *all* be well-delivered by such paraprofessionals. As a result, service specialists proliferated, each accumulating specialized information and each trying to establish a relationship with the client at some point in the four month service period.

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[13] See Vera (1978) especially Profile 3 (TCRP, Boston) and Operation de Novo, Minneapolis (Profile 2). Like CEP, they began as manpower-model diversion programs with a mandate to hire ex-offenders as counselors. However, in 1977, TCRP reported it had only one ex-offender on staff and de Novo "three or four." (All of those at de Novo were veterans of the program's early days, and all had obtained or were working on college degrees; the ex-offender label is no longer considered particularly relevant.) Among the other programs, Hudson PTI (Profile 4) said it had one ex-offender on staff; Dade PTI (Profile 6) had none; and Operation Midway (Profile 7) and Bergen PTI (Profile 5) both employ only probation officers as counselors.

During 1977, the process for counseling and services at CEP proceeded in the following way. Once accepted as a CEP client by the agency's court staff, a person went to the Manhattan office and was assigned to a team consisting of a personal counselor (Rep) and a Vocational Counselor (VC). These teams and their supervisors constituted CEP's basic Counseling Unit. The Rep and VC shared responsibility for the client's progress in the program. After the first three sessions (during which the Rep and VC saw the client jointly), the participant usually saw primarily either the Rep or the VC. If he had obvious, serious personal problems (for example, no place to live or an alcohol problem) he would see the Rep until the problem subsided or had been solved. The Rep also saw participants whose lives were already relatively stable and who needed only monitoring. The VC saw those who wished to get a job or go back to school and who could do so if they were given some advance preparation. To a certain extent, the roles of the VCs and Reps overlapped, and each counseling team worked out a comfortable division of labor. In some teams the Rep was dominant; in others, it was the VC. A few teams always saw participants jointly.

When and if a VC determined a participant was "job ready,"[14] he referred him to the agency's Service Unit where the participant was assigned to a Vocational Placement Specialist (VPS) or a Job Developer (JD) who would try to find him a job commensurate with his skills and interests. In practice, most participants were so unskilled that they qualified only for extremely low-level jobs.

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[14] According to the VCs, this meant that the participant was literate enough to fill out a job application, had some motivation to get and keep a job, could make a presentable appearance at an interview, and could pass whatever tests an employer might require for a given job.

Similarly, when and if the VC decided the participant was ready to be referred to a night school, college, or training program, he referred him to the Services Unit, where a VPS attempted to make the placement.

If a participant needed other kinds of help, such as medical attention or a place to live, the Rep usually referred him to the Community Resources Specialist (CRS), who made the appropriate referral. Sometimes, the Rep made the referral himself and told the CRS later.

A participant needing help with a welfare-related problem was referred by the Rep or VC to the Department of Social Services Representative (DOSSR), who determined his eligibility for benefits, informed him of the procedures he must follow, and referred him to the appropriate city office.

If Services Unit personnel (the VPS, JD, CRS, or DOSSR) did not think a referral appropriate, or failed to find an appropriate placement for the participant, they sent the client back to the counseling team along with a brief report on actions taken. If they could make a successful placement or referral, they informed the counseling team, which then resumed monitoring the participant's progress.

Counseling teams usually saw their clients once a week, although contact might be more frequent if a participant wished or a counselor believed it necessary. Very stable clients were asked to come in every other week and to call in during the week they did not visit CEP. While caseloads have been as high as 60 clients per team, a more typical level was around 50 clients per team. Because of its reduced budget,

the program did not offer group counseling in 1977, although it had in the past.

CEP participants generally stayed in the program four months. In exceptional circumstances, an additional two-month continuance was requested by CEP (or insisted upon by the ADA or judge when CEP asked for a dismissal) if the client did not have a job, was not in school, or had not made enough effort towards finding employment or education.

As indicated earlier, personal or "insight" counseling played a secondary role to service delivery in CEP's intervention efforts. Counselors were instructed not to delve into personal problems and difficulties of emotional adjustment *unless* the client had an obvious, severe problem that needed immediate attention or one that required attention as a prerequisite to vocational progress (e.g., something that rendered him unemployable).[15] Even *arrest* counseling did not play a major part in CEP's counseling activities. While the emphasis on arrest counseling varied from counselor to counselor, if he or she did not raise the arrest with the client, no one else at CEP was likely to. Those counselors who did not talk about the arrest with the client

[15] This attitude toward clients' personal lives seems unusual among the diversion programs (Vera, 1978). Most programs concern themselves to some extent with personal matters, and some of them focus on it almost exclusively -- Bergen PTI (Profile 5), for example, which sought to "strip the person" in counseling as the first step toward rehabilitation. The other program notable for the extent of its involvement in clients' lives was TCRP-Boston (Profile 3), which mandated that counselors set goals for participants not only regards employment and schooling, but also living arrangements, leisure time, and money management. Some of TCRP's counselors, however, regarded the latter two, especially, as constituting an unwarranted intrusion by the program, and they tried to avoid pursuing them in counseling.

generally took the attitude that their job was to help the client help himself (especially vocationally) and that the alleged offense was not their concern. These counselors felt it was not their role to judge clients and that clients would realize when they were ready that their behavior was a mistake and avoid trouble in the future. Other counselors avoided discussing the arrest because they didn't want to create the impression that they were part of the criminal justice system, as is a probation or parole officer, and they felt they had enough trouble dispelling that image without arrest counseling. It might also be that, as ex-offenders themselves, they sympathized with their clients and did not see their clients as entirely to blame for the offense.

Consequently, most counseling at CEP related to the individual's immediate problems and attempted to assess what actions could be taken by the client or what service given to move him forward on one of several fronts, particularly the vocational. Vocational counseling began with questions about the client's educational and employment history. The Vocational Counselor assessed his employability and job readiness based on this information, reading tests, and the client's demeanor (attitude, articulateness, and dress). He found out what job the client wanted or was willing to take and whether he had the requisite skills to get that type of job. (A similar assessment was made of clients who said they wished to go into job training or school.) Before sending the client to the special Services Unit personnel for referral, the VC asked him to fill out a job application and then "role-played" an employer-applicant interview. When he

believed the client was job ready, he referred him to a Vocational Placement Specialist in the Services Unit. (The VPS in turn, referred the client to a job if one was available for which he qualified.)[16] The VC was responsible for following up on clients after their referral to the Service Unit. Once the client found employment, the VC counseled him about any adjustment problems and encouraged him to stay on the job.

To the extent that it is possible to characterize the service philosphy at CEP during 1977, it was that CEP sought to offer clients alternatives to their street lives, to communicate through counseling and specific services, that with some effort, they could succeed (in finding a job or learning to read), but to let *them* make the choice of how they wanted to live -- not to make it for them. The counselors realized that only those clients who were receptive to this message would respond. The counselors' general attitude was that they were there to offer services to a population badly in need; they hoped the services would produce some long-term benefit to the client (either with respect to career stability or reduced liklihood of arrest), but they expressed little conviction that they would.

This tolerant counseling philosphy resulted in a fairly lenient attitude toward unexcused absences,[17] but did not preclude counselors

[16] For a variety of reasons, the VC sometimes referred a client to the Services Unit who was not job-ready. For example, a vocational counselor referred one hostile client to a VPS in hope that the client would "shape up" when she realized CEP would really try to find her a job.

[17] The penalty was usually no more serious than a scolding from the counselor, unless the problem persisted indefinitely or the client stopped showing up at all.

from prodding their clients to look for jobs or get back into school, even to the point of threatening (and carrying through) termination if clients persistently refused to comply. It was not so much that the counselors were betraying their own service philosophy, as it was that there was agency and court pressure on them to get every diverted client employed or in school (or at least attempt to do one of these), in order to justify charge dismissals. To maintain its credibility, CEP had to deliver on its promises to prosecutors to return to court as successful diversion participants primarily persons who were employed or in school.

JOB DEVELOPMENT

As one of the original manpower-model diversion programs, CEP had a job development component since the program was established. At first, one of CEP's two basic service delivery personnel -- the Career Developer -- prepared the participant for the job search, found out what type of job he wanted and was qualified for, and helped him to find suitable employment. Career Developers were expected to go out into the field to make contact with prospective employers and find job opportunities. CEP had hoped that it could establish stable "accounts" with a number of medium - and large - size businesses committed to the idea of rehabilitating these youths or at least willing to give them an opportunity to work. CEP hoped it could then refer its clients to these accounts on a regular basis. This hope was not realized.

In time, the Career Developer's role was broken down into the three separate positions discussed above. The Vocational Counselor prepared clients for a job search and found out what sort of job they wanted or were willing to take; the Job Developer made field visits to employers and tried to find job openings for CEP clients; and the Vocational Placement Specialist reviewed the preparatory work of the Vocational Counselor and made referrals to the job sites the Job Developer found.

This was how CEP's job development component was organized in 1977. However, despite the efforts of some very dedicated staff, job development at CEP was not successful. Identifying the reasons for the failure of the job development component is not difficult. Some are endemic to the task, and some have to do with internal problems at CEP. The reasons noted by program staff include: (1) the job market was poor; (2) the clients were difficult to place in jobs; (3) most of the clients didn't want the menial jobs CEP could find for them; (4) the Counseling Unit was slow in referring clients to the Services Unit for job referrals and placement; (5) the Services Unit had too small a staff to do job development adequately; and (6) the Services Unit staff members were not uniformly well-trained specialists.

The job market for minority group youths has never been very good, but CEP Services Unit staff reported that it was easier ten years ago to find job openings and place clients than it was in 1977. Several factors -- fewer job openings, a political climate hostile toward the idea of giving young criminals a break, and the lack of any work experience at all among many of CEP's youthful clients -- made recent job development efforts extremely difficult.

Statistics indicate that the unemployment rate among black teenagers in New York City was about 40 percent in 1977. It can safely be assumed that the rate was even higher for lower-class, inner city black youths who were illiterate (or nearly illiterate), unskilled, and had criminal (if only arrest) records -- in short, those who formed a substantial proportion of CEP's clientele. In addition to these shortcomings, many CEP clients, as noted earlier, were said to be unmotivated, articulated poorly, dressed inappropriately, had negative attitudes toward employment, and often didn't show up for appointments CEP Service Unit personnel made for them with prospective employers.

Another factor hampering job placement efforts, according to CEP staff, was that the counseling teams often did not refer clients to the Services Unit soon enough for the Unit to find them a job. Because participation in the program was only four months, if it took counselors two-and-a-half to three months to refer a client to the Services Unit, service personnel had only four or five weeks in which to find him a job.

Finally, CEP had only three Vocational Placement Specialists and three Job Developers during 1977 to find jobs for approximately 1,000 clients a year. They covered four of the five boroughs of New York City. This was too small a staff to do a thorough job of job development, particularly since the personnel were not all highly trained specialists.

SUCCESS OR FAILURE

CEP's own sense of its programmatic success or failure and its definitions of clients' success and failure were not necessarily parallel. With respect to *diverted* clients, the program and the client were both deemed "successful" if the client completed the four month pretrial service period so that the agency requested the court and prosecutor to dismiss the pending charges. However, even with successfully diverted clients, CEP staff was rarely satisfied that they had had the time or resources to give their clients sufficient *service* help. They felt that some clients who had their charges dismissed had made great progress in a short time, others less, and some very little indeed. The reverse was also sometimes the case. A diverted client who was returned to court with no recommendation ("unsuccessful") might have made considerable strides during the time he was in the program (perhaps he got a job), but he failed to continue to attend and therefore had to be terminated. While such cases were infrequent,[18] they did occur and serve to point out the discrepancies in CEP's own conceptions of programmatic and client "success."

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[18] In fact, it appears from project records that most terminated clients either never showed up for their appointments at all, or attended very infrequently; CEP could consider itself to have failed programmatically because it did not select the right defendants (but then, it did not try to screen for "motivation") or because it did not manage to persuade the individual to keep coming, but often the client did not give the program much chance to persuade! Whereas the program returned the case to the court docket, the number of outstanding warrants in New York City's courts was sufficently large, that the Warrant Squad of the Police Department only went after those with serious charges pending.

Whether a diverted CEP participant was "successful" at CEP in the sense that CEP recommended a dismissal of the charges depended heavily upon whether the client attended CEP with regularity. If the client had attended regularly, the VC and Rep discussed with their supervisor just before the participant's court date came up what recommendation they would make to the court. There were two options. They could recommend a dismissal of the charges or an additional two-month continuance. The latter was rarely done; such continuances were unusual and most often requested by ADAs or judges unwilling to accept dismissal recommendations after only four months of services.

If CEP decided to recommend dismissal, they wrote a summary of the participant's progress in the program, stressing concrete vocational and educational improvements rather than "personal" achievements (such as improved hygiene, better relations with family or friends, more insight into self). The reason, according to the Court Operations staff, was that the less tangible gains were unlikely to impress judges and prosecutors.

CEP employed a court report writer who used these counseling summaries, case notes, and other data in the file to write a letter to the court recommending dismissal. This letter was first brought to the ADA liaison, who had to concur with the recommendation (and so state), before it was taken to court. (A similar letter was written when the recommendation was for a continuance.) While ADAs did not always accept CEP's recommendations for dismissal, when they rejected recommendations they generally asked only that CEP continue to work with the defendant for another two months. Fifty-five percent

of the 532 diverted clients who exited from CEP during 1977 had successfully completed four months in the program and received dismissals.

The procedure for unfavorable termination of clients began with the Rep-VC decision to terminate, which they discussed with their supervisor at weekly case conferences. If the supervisor concurred, a counselor sent the defendant and his lawyer a letter announcing CEP's intent to terminate the case. CEP considered this letter a notification rather than an invitation for discussion -- and, in fact, a response from either the participant or his lawyer was rare. CEP then notified the DA's office that the defendant had been disassociated from CEP. No reason for the disassociation was stated in the letters.

Of the 532 clients existing in the program during 1977, 45 percent were terminated or administratively discharged;[19] most had not been in the program a full four months.[20] Terminations resulted primarily from participants' failure to attend counseling sessions or their rearrest (although termination for the latter was not automatic); only occasionally did they result from participants' failure to cooperate with the program (for example, not going back to scheduled job interviews or making no effort to get back into school).

[19] An administrative discharge was given to any client the program could no longer assist, either because the services CEP offered were not suited to his needs (for example, he was psychologically too disturbed) or because he moved out of the jurisdiction.

[20] In its first three years of operations, the proportion of cases CEP terminated dropped from 61 percent to 39 percent; it remained around 30 percent during the years just prior to 1977.

PROGRAM EVALUATIONS

As with many PTD/I programs (but more so than most), CEP had been evaluated during its early years of operations, first at the conclusion of the DOL demonstration phase and later by an outside consultant for the city agency which funded it. These evaluations focused on CEP's goals both as a social service agency and as a diversion program.

The first evaluation of CEP covered the demonstration period 1967 to 1970. While the paramount concerns of the project during this period were operational, research data were collected by program staff under the guidance of an outside evaluator (Vera, *Manhattan Court Employment Project, Final Report,* 1972:43-54). The data were suggestive of positive outcomes along several important dimensions, but, as with many other PTD/I programs during that period, the methodological problems were so substantial that the data could not be considered conclusive (see Mullen, 1974). Whereas the original proposal for the project had suggested a small experimental research design, the exigencies of an innovative but fledgling program made it impossible: "the experimental nature of the Project demanded initial emphasis on effective day-to-day operations, and....denying participation for the purposes of research violated the humanitarian tenets of the Project and the sensitivities of the staff" (Vera, 1972:44). Consequently, three random samples were picked: 100 dismissed participants; 100 terminated participants; and 150 comparison defendants (so-called "paper eligibles") from the three-month period just prior to the beginning of the project.

The staff experienced considerable problems obtaining complete recidivism data and, in addition, follow-up data on employment could be obtained only for the successful (dismissed) participant sample. No data on the comparison groups' case dispositions (convictions or sentences) were collected and virtually all comparisons made in the analysis were between successful participants and either unsuccessful (terminated) defendants or the comparison group, thus biasing the results in favor of the program (see Zimring, 1974:227 ff).

The evaluation concluded that "Recidivism was substantially reduced for the dismissed participant group in comparison to the terminated and control groups. Recidivism among terminated participants was approximately the same as among control group members" (p.50). It also concluded that the benefits from the increased earnings of successful participants in the fourteen months after leaving the Project "far exceed[ed] the operating cost of [the Project]" (p.53).

In 1973, Franklin Zimring submitted a report, a second evaluation, to the Human Resources Administration of New York City which then funded CEP (Zimring, 1973 and 1974). Zimring was critical of the earlier evaluation's methodology while also reporting his own difficulties designing research to measure program impact when a

controlled experimental design was not possible. Zimring's final design was unique, and an interesting approach to a perplexing and challenging problem.[21]

Zimring concluded that the program might have a small impact on detention before trial, but that the percent convicted were not affected by the program; neither were the proportions rearrested within three months and within one year after arraignment. He cautioned, however, that "These negative indications do not rule out the possibility that the Project reduces rearrests. Each comparison... involves a test group that is composed of at least 78 percent untreated subjects, which is hardly an ideal condition" (1974:235).

With respect to sentences, Zimring also found minimal impact. Few in either comparison group were sentenced to jail (5 percent and 7 percent) and few to probation (3 percent and 6 percent). Most sentenced defendants in the samples received conditional discharges (22 percent and 15 percent) or fines (6 percent and 11 percent). Zimring concluded, therefore, that

> most of the cases screened into the Project are not deemed serious enough for even probationary sentences in New York City. This places an upper limit on the degree to which the program is diverting people from the ordinary citizen's view of criminal sanctions. And if probation is given in many more serious cases, why should not diversion be available to those facing more serious criminal charges than is presently the case? Another consequence of the data...is that the Project provides more treatment and supervision than most of its participants would otherwise receive. In this sense, what the defendants are diverted into is more important than what they are diverted out of. Present emphasis is more on treatment [services] than on diversion. (1973:23)

[21] Zimring selected a sample that contained both project participants (14 percent) and other defendants who were screened as eligible but not part of the program (86 percent). He compared this with a group of eligible participants screened by the agency during a time when it did not normally operate (weekends); therefore, this second group contained no actual participants. Since project participants differed from eligible non-participants, each of the two groups was divided into sub-groups classified by age and charge. The comparison measures, therefore, were the *correlations* between the *differences* between the appropriate sub-groups on dependent variables such as recidivism and the percent concentration of actual participants in the one sub-group.

CEP took these cautionary conclusions into consideration when it decided in January 1977 to move to felony-only diversion and to remove any formal eligibility requirements relating to prior arrest, conviction or jail history.

To conclude, recognizing the limitations of even elaborate but non-controlled designs, Zimring's overall assessment of the CEP evaluation experience was that "The only cure for a poor evaluation is a good one -- in this case, large-scale and careful random assignment experimentation" (1974:241).

CHAPTER III

THE CEP EVALUATION: RESEARCH DESIGN AND IMPLEMENTATION

INTRODUCTION

As indicated in our discussion of the previous research on CEP and in our review of the disputed issues in PTD/I generally, the present research effort had three central goals. The methodological goal was to develop a mechanism to generate a randomly assigned control group of sufficient size to provide an adequate comparison with CEP participants. The NILECJ, which funded the research, identified this as a major priority, not just to answer substantive questions about diversion but to add to the growing repertoire of useful and powerful tools of criminal justice evaluation. Second, a major analytic goal of the research was hypothesis testing, that is, to subject the rationales underlying a progressive reform to rigorous investigation. As agreed upon throughout the research literature, an experimental design with randomly selected comparison groups is especially important to such assessments in the area of diversion because the complex screening of eligibles for these programs precludes the formation of adequately matched comparison groups. The third and perhaps most obvious goal of the present study was to evaluate the effectiveness of the Court Employment Project. After ten years of operation and evaluation, CEP was itself still troubled by the central questions of diversion: was it "over-reaching" by diverting

defendants who would otherwise not be prosecuted or punished; was it providing services that affected people's lives significantly; was it helping participants avoid future contacts with the criminal justice system?

To meet these three goals, an experimental design was devised and implemented. The selection procedures, sample size, data collection efforts, hypothesis construction, methods of analysis and the problems encountered in the research are documented in the following sections of this chapter. In the remaining chapters of the report, we turn our attention to a detailed discussion of the findings.

DESIGN

In any research that aims to make inferences about the effects of a program from differences on dependent variables, efforts must be undertaken to safeguard the validity of these inferences; that is the researcher must be able to assume the groups being compared were equivalent before the introduction of the experimental treatment. The basic tool for safeguarding this assumption is randomization. Random assignment of persons to conditions,(i.e., some individuals receive an intervention and others do not) ensures that each member of the pool of eligible persons has an equal chance as any other of being assigned to a given condition. This does not mean that any one individual has an equal probability of being assigned to any condition; that is, eligibles need not be equally split among conditions. Rather, the pool of eligibles might be divided into two-thirds in the experimental condition and one-third in the control group and still fulfill

the requirements of random assignment. In the above example, *each* individual has a probability of .67 that he/she will be in the experimental (intervention) group and .33 that he/she will be assigned to the control group. The crucial requirement is that the selection may, in no way, be based on the judgment of the researcher or intervention program staff. Random assignment of individuals to conditions minimizes the possibility of systematic differences between treatment groups, and assures that initial differences between groups are due to chance.

Because one goal of the present study was to evaluate the impact of CEP on its participants, an experimental design was considered essential. In field settings, however, randomization is more difficult to achieve than in the laboratory. Resistance is encountered from many sides: prospective eligible clients may object to being selected randomly, and program personnel may foresee administrative problems or object on ethical grounds. In legal settings, random selection is entwined with issues of due process and equal protection. As a result, previous research studies on diversion have resorted to the use of matched comparison groups where any comparison was attempted. The problems associated with this method have been discussed by others (Mullen, 1974; Rovner-Pieczenik, 1974; Zimring, 1974) who have demonstrated that in this area the use of matched comparison groups is not a satisfactory solution.

SELECTION OF THE RESEARCH POPULATION

The Court Employment Project evaluation originally intended to use random assignment but had to adjust this to meet some of the considerations noted above. The result -- a random time-quota selection -- is quasi-random in that experimentals and controls are selected under a quota system (non-randomly), but since the quotas are assigned during randomly selected variable-length time periods, the method has the same effect as random assignment of individuals. A complete discussion of the selection method requires a description of (1) the selection of the population and (2) the assignment procedure.

To successfully implement an experimental design, it was necessary to obtain the cooperation of CEP's screening staff, the Legal Aid Society (which represents most defendants in the New York City criminal justice system), and the District Attorneys of Manhattan, Brooklyn and Bronx.[1] There was some concern on the part of the Legal Aid Society, (and LEAA's Legal Counsel) that the originally proposed intake procedures (i.e., random assignment to condition) would deny diversion services to some defendants solely to create a research control group and that such selection would be "arbitrary" and thus deny defendants equal protection and due process. As a result of these questions, the procedures that were implemented

[1] Queens was excluded from the study for two reasons. First, the number of diverted defendants had traditionally been small, and they tended to be somewhat different in social composition from the defendants in other boroughs. Second, CEP considered the political obstacles to the research design overwhelming and did not wish to implement it.

were designed to assure that no defendant would be denied diversion services solely because of the need for a control group; nor would any individual defendant be directly subjected to randomization. This was accomplished by obtaining CEP's agreement to generate more eligible defendants than it could service (given its level of funding). During the ten months of intake into the research, CEP screeners identified more eligible defendants than the program could divert and secured the prosecutors' approval for their diversion. This pool of eligibles comprised the research population; those who were diverted were in the experimental group, and the remainder constituted an "overflow" of eligibles and were the research control group.

The procedure used to assign specific eligible defendants to the research groups was designed to approximate a random assignment by assuring concurrent intake into the two groups and by preventing either CEP staff or Vera research staff from influencing individual decisions. The major characteristics of the mechanism were the construction of a CEP quota and the use of variable length time periods to administer the quota.

The quota system was developed to select the cases CEP would divert during a given time period such that, when added together the quotas would equal the total number of cases CEP had funds to service. Once a quota was filled, the remaining cases that were screened and approved during that time period constituted an overflow of eligible defendants to be processed normally by the court, and these individuals were assigned to the research control group.

Since the research staff controlled the mechanism for setting the CEP quota, the CEP screening staff was unable to predict whether any particular eligible defendant would be assigned to the experimental or the control group. Variable length time periods meant that the size of the CEP quota was not the same from period to period. The periods were determined by multiplying by eight (the number of hours worked per day by CEP screeners) the total number of work days in six months of research intake.[2] This product was the total number of screening hours (treated as if they were continuous) to be divided into variable length periods; the periods varied in length from 11 to 21 hours.[3] The total number of screening hours for the first six months of intake was divided into an equal number of 11, 13, 15, 17, 19 and 21 hour periods; the order of the periods was randomly determined before the start of the research.

The size of CEP's quota for each period was calculated by estimating the number of cases for which CEP was likely to secure approval during that period (based on the mean number of cases approved per hour during all preceding periods). It was assumed that CEP would be able to generate twice as many cases as it could divert and, therefore,

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[2] To obtain an adequate sample size, intake into the research was continued for an additional four months. At the end of the first six months, additional time periods were generated using the method described above.

[3] The choice of these periods was a matter of research judgment. The periods could not be too long or the groups would not be concurrently selected; they could not be too short or the sampling would too closely approximate an individually randomized decision. Eleven hours was slightly more than one eight-hour screening day, and 21 hours slightly less than three; these seemed to be reasonable periods.

the CEP quota was set at one-half the expected number of approvals for a time period. Because CEP's quota was always filled first (before the control group) and because the flow of cases through the courts was erratic, it was necessary to adjust the quota to assure that CEP diverted approximately 50 percent of the approved cases over the long run. For each time period,an adjustment factor was calculated based on the proportion of all previous cases that had been assigned to the experimental group. If this figure was 50 percent of the total cases assigned, then CEP's quota for the next period remained at half the expected number of cases. If the figure was less than 50 percent, CEP's quota was increased to bring the proportions closer to 50-50; and conversely, if more than 50 percent of all previous cases had been assigned to the experimental group, CEP's quota was reduced.

The above-described procedures for identifying and assigning defendants to the research conditions were implemented without major alteration and resulted in the assignment of 666 subjects over a ten month period. (Adjustments to the procedure and exceptions in assignment are discussed in the section entitled "Implementation.") The original design of the research called for a sample size of 800 to be taken into the research within six months and equally divided among experimentals and controls. A large sample was necessary for a longitudinal study and to assure adequate statistical power to detect any effects that might be present in the population. Because of problems with CEP's intake resulting from New York City's 1976 fiscal crisis (discussed below), it took ten months to obtain 666 subjects, divided unequally between experimentals (410) and controls

(256). Time constraints precluded continuation of intake beyond that point, and it was determined that 666 was adequate to carry out the study.

SOURCES OF DATA

The experiment was designed to cover a 12 month period with each subject interviewed three times: (1) at intake into the research population, (2) six months after intake, and (3) twelve months after intake. The three personal interviews were to be conducted with all research subjects by Vera Research Interviewers. The interviews were conducted in English and Spanish at Vera's research office, in the courts, and in the field. The interviews were designed to elicit information related to education, training, employment history, reliance on public assistance, criminal history and self-reported illegal activities, life style, and utilization of social services. (Specific items are discussed in greater detail in the presentation of results.) Informed consent was obtained, participation in the interviews was voluntary and subjects were paid stipends for each interview they agreed to give.

In addition to the three personal interviews, official record data were obtained from the New York City Police Department, the Criminal Justice Agency (CJA), and CEP's service files. These data included criminal history (of arrests in New York State), disposition of the case on which the defendant entered the research, information related to subsequent arrests, and (for members of the experimental group) information about participation in CEP. In addition,

attempts were made (where possible) to verify interview data through contacting schools, employers, and New York City's Department of Income Maintenance (public assistance). (The results of these attempts at verification are discussed in the "Implementation" section.)

DATA ANALYSIS

While the specific procedures used for each analysis are discussed in detail below, an overview of the nature of the analysis is presented here. For much of the data collected in this study, repeated measures were taken over time; analyses of these data were conducted using repeated measures analysis of covariance and analysis of partial variance. These techniques enable the researcher to control for initial differences between groups in assessing the impact of the intervention program over time. For nominal data that were assessed at only one point in time, (e.g., disposition of the intake case) two-way contingency tables and chi squares were computed. Correlational data, not subject to the experimental design, were examined in efforts to predict recidivism and success in CEP; these analyses were conducted using multiple regression. The statistical techniques used for each analysis are explained with the results.

IMPLEMENTATION: ASSIGNMENT OF INDIVIDUALS TO CONDITION

The assignment of experimentals and controls proceeded as discussed above, with two adjustments to meet CEP's organizational needs. These adjustments involved the percentage of cases in CEP's quota and the assignment of co-defendants.

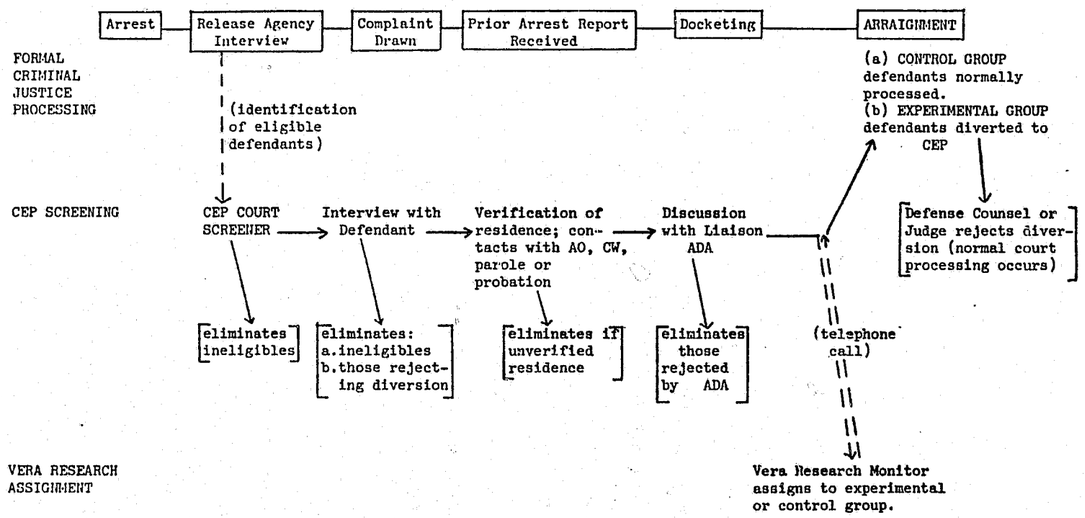
Within three months after research intake began, CEP became concerned that it was not screening a sufficient number of cases to fill its service requirement and generate an "overflow" of equal size. Therefore, the research agreed to increase CEP's quota to 65 percent of all expected cases, with an overflow of 35 percent. These percentages were in effect for the second three months of the intake period, after which CEP agreed to return to 50 percent. As a result, the experimental group (N=410) contains more cases than the control group (N=256); however, this has no effect on either the equivalence of the groups or the data analysis.

The second adjustment to the procedure involved the assignment of co-defendants. Eligible defendants who were co-defendants always received the same research assignment, (i.e., either *both* were part of CEP's quota or *both* were members of the control group). This was necessary to get the prosecutor's approval for diversion; the prosecutors expressed concern that a successfully diverted defendant would return to court after receiving a dismissal and testify in the case of the co-defendant that he and not the co-defendant was responsible for the offense.

The process of CEP screening and Vera research assignment was rather complex, involving a series of steps and a number of decision-makers. A flow chart describing this process is presented in Figure III-1. This figure serves not only to explicate the process, but also to demonstrate how exceptions occured in the assignment of subjects to research conditions.

FIGURE III-1

FLOW CHART OF CEP SCREENING AND RESEARCH ASSIGNMENT PROCEDURE*



* alternate procedure: referrals received after arraignment from Defense attorneys, judges, ADA's were screened normally by CEP staff before the defendant's next court date. After CEP secured ADA approval for diversion, Vera Monitor was called, research assignment was made, and case was either diverted (if assigned "participant") or not (if assigned "overflow") at next court appearance.

The first step in CEP's screening was conducted by New York City's pretrial release agency (CJA, formerly Pretrial Services Agency). Prior to arraignment, and after their Release on Recognizance (ROR) interview, CJA interviewers identified defendants who were eligible for diversion according to CEP's written criteria and provided CEP screeners with copies of the ROR interviews for these defendants. CEP screeners reviewed the ROR interviews to assure eligibility, and interviewed the defendants in the detention cells, where they explained the program and asked if the defendant was interested in diversion. The screeners emphasized that the defendant might not be diverted even if he/she was eligible, wanted services, and the Assistant District Attorney (ADA) approved the case. They explained that CEP interviewed more defendants than it could service, and that it was possible there would be no room in the program for the defendant when his/her case went to court.[4] After securing approval for diversion from the ADA liaison, the screener telephoned the Vera Research Monitor who recorded the names and identifying numbers of all defendants approved by the ADA and (using the assignment procedure described above) gave the CEP screener the

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[4]This was done to insure defendants understood from the beginning that there was an "overflow." Although researchers would have liked acceptance into CEP to have been contingent on a defendant's agreement to participate in the research project, this procedure was rejected because of the possibility of coercion. Consequently, as discussed below, we did not have a captive population for the intake interviews, a factor of extreme importance for the successful collection of longitudinal data based on personal interviews. See Appendix A.

research status of the subject. For cases that were part of CEP's quota, the arraignment judge was asked for a four-month continuation of the case for CEP to divert and give services to the defendant. In cases designated by research as "overflow" (assigned to the control group), the screener simply returned the papers to the prosecutor and the case was processed normally.[5]

During the ten months of research intake, CEP shifted from a predominantly court screening (solicitation) method of intake to a referral method. This did not affect the process of research assignment; however, the data were examined to assure there were no systematic differences in cliental between the two methods of selection.

While the research assignment procedure worked smoothly, there were three types of exceptions to the assignment: (1) defendants assigned to the experimental group who subsequently were not diverted; (2) defendants assigned to the control group who were subsequently diverted; and (3) defendants who were diverted to CEP but were not called into the Vera Monitor for assignment to the research. Each type of exception is discussed below.

After a defendant has been approved for diversion by the ADA liaison and was assigned to the experimental group, he/she had to

[5] As noted earlier, there was no way CEP screeners could influence the research assignment. *No one* on the entire CEP staff knew how the research was making these decisions; since the time periods shifted "erratically" (from the screeners viewpoint) and since the quota changed in size each time, there was no way CEP screeners could predict how a defendant would be assigned. Finally, the actual selection by research took place at the Vera offices, physically far removed from the courts where CEP screening took place and there was never any personal contact between the Vera Monitor and the CEP screeners except for the phone calls.

appear before a judge at which time the ADA, defense counsel, and CEP screener would jointly request a four-month adjournment for diversion to CEP. Occasionally, asserting the prerogative of judicial review, a judge would refuse to divert a case on the grounds that the defendant should receive a more lenient or a harsher disposition. In addition, a defense counsel might reject diversion if the ADA liaison had attached a condition to the diversion that was unacceptable to the attorney. Sixty-three cases (15 percent of all experimentals) assigned by the research to the experimental group were rejected (and therefore not diverted) in one of these two ways.

In 16 cases (six percent of the control group) a defendant who was assigned to the control group was diverted to CEP. This occured when a judge insisted that a case be diverted (despite its "overflow" status). To maintain good relations with the judges, CEP accepted such cases.

From the standpoint of the research design, subjects assigned to a research group remained permanent members of the research population; therefore, all exceptions to the research assignment were retained in the data collection and in the analysis of program impact which compared experimentals and controls. There were important methodological reasons for including the exceptions, non-diverted experimentals and diverted controls, in the data analysis. This approach results in more conservative tests of the hypotheses than would be obtained by excluding them. That is, with 15 percent of the experimentals not diverted and six percent of the controls

diverted, the probability of detecting program effects is lessened. However, maintaining the integrity of the experimental design (that is, the comparability of the experimental and control groups as originally assigned), permits greater confidence in the validity of any effects that are detected. It was this consideration that governed the decision to include the assignment exceptions in the analyses.[6]

In 66 cases a judge either diverted the case to CEP without the agency having screened it, or requested directly of the agency that a defendant be diverted. In such cases, CEP's Director of Court Operations or the Program Director believed the possibility of the case being rejected as an overflow would jeopardize the agency's informal relations with the court. Defendants who were diverted but not subject to the assignment procedure were not included in the research.

COMPARABILITY OF EXPERIMENTALS AND CONTROLS

To assess the success of the assignment procedure in generating equivalent groups, the experimental and control groups were compared on variables central to the analysis. These variables included

[6] To explore the possibility that program effects were being masked by the inclusion of the exceptions, many of the major analyses were also computed comparing only diverted experimentals and non-diverted controls. The results of the comparisons between diverted experimentals and non-diverted controls were not different, however, than the results described below that were obtained using the more conservative method.

demographics, such as age, ethnicity, and gender; and characteristics of the court case, such as charge severity and type. Each of the comparisons discussed below was computed using *assigned* experimentals (including those exceptions who were not treated) and *assigned* controls (including those who were diverted).

A t-test was computed to test the differences between experimentals and controls on mean age at intake into the research. The difference between the means was not statistically significant; the mean age at intake for those members of the sample for whom age data were available was 20.2. Ninety percent of the sample were males and ten percent were females. A 2x2 chi square revealed no significant relationship between gender and research status; that is, the proportions of males and females did not differ for the experimental and control groups. The sample was composed of people identified as belonging to three ethnic groups: black, hispanic and white. Blacks comprised 51 percent of the sample, hispanics 37 percent and whites 12 percent. A 3x2 (ethnicity by research status) contingency table was constructed; a chi square analysis revealed a significant relationship between ethnicity and research status ($\chi^2_2 = 12.175$; $p < .005$). It can be seen from the percentages in Table III-1 that there are a disproportionate number of whites in the control group. While approximately 65 percent of the blacks and hispanics are members of the experimental group, only 46 percent of the whites were assigned to the experimental group. The reasons for this difference are not immediately apparent. No one involved in the

screening or research assignment process itself (and no outside observers) could identify any way in which the assignment could have been influenced,or any reason for it to have been tampered with along this particular dimension,had tampering been possible. Consequently, although the likelihood that the ethnic differences are a result of chance alone is extremely small, it is the most reasonable assumption.

Table III-1

RESEARCH ASSIGNMENT BY ETHNICITY

Research Assignment	Ethnicity		
	Black	Hispanic	White
Experimental	63%	67%	46%
Control	37	33	54
(N)	(325)	(236)	(79)

Although this ethnic difference in the two samples is difficult to explain, it was not a cause for great concern from the standpoint of the analysis of CEP's impact on defendants. This is because the three ethnic groups did not differ on important impact (dependent) variables. For example, cross-tabulations were done separately for each ethnic group on the percent of experimentals and controls arrested subsequent to intake into the sample. Chi square was computed for each ethnic group and there were no statistically significant relationships between recidivism and research assignment for the three ethnic groups. In addition, to provide a more powerful test

of the possibility that the program affected the three ethnic groups differently, a series of regression analyses were computed in which ethnicity (dummy coded) was entered as a predictor. Again, there were no effects on number of rearrests, severity of subsequent arrests, number of subsequent convictions, or on a dichotomous re-arrest variable. There were also no interaction effects between ethnicity and research status on any of the recidivism variables.

Table III-2

Rearrest Post-Assignment	Black		Hispanic		White	
	Exp'l	Control	Exp'l	Control	Exp'l	Control
Yes	32	41	41	59	37	44
No	68	59	59	56	63	56
Total	100% (179)	100% (95)	100% (123)	100% (61)	100% (27)	100% (23)
χ^2	χ^2_1=2.031;n.s.		χ^2_1=0.219;n.s.		χ^2_1=0.215;n.s.	

Experimentals and controls did not differ on characteristics of the court case on which they were brought into the sample. The two variables considered were type and severity of arrest charge. The great majority of all subjects (75 percent) were arrested on charges of theft (without violence); robbery and assault charges accounted for eight percent and nine percent of the arrests, while other charges, such as conduct, forgery, weapons and drugs, occurred less frequently.

There was also no significant difference between experimentals and controls on severity of arrest charge. In 1977, for the first time, CEP restricted diversion to defendants charged with felonies. Only 11 percent of the subjects in the research were charged with a C felony (C indicating the level of seriousness of the offense which ranges from A, the most serious, to E, the least); approximately half were charged with a D felony and one-third were arrested on E felony charges.

Since employment and education are two areas that CEP attempts to affect, the experimental and control groups were tested for differences on variables in each of these areas at the time of arrest. There was no statistically significant difference between the proportion of experimentals (13 percent) and controls (11 percent) who were employed at the time of their arrests. Similarly, there was no significant difference between experimentals and controls in the proportions enrolled in school at the time of arrest (36 percent and 39 percent, respectively).

The results presented above indicate that the assignment procedure was successful in generating two groups of subjects who did not differ at the time of intake into the research population on the characteristics that were measured. While the experimental and control groups may differ on some other, untested variables, one can be fairly confident that these differences are due to chance rather than systematic bias of the research assignment.

RELATIONSHIP TO CEP's TOTAL SERVICE POPULATION

It is important to know that the experimental and control groups do not differ from each other, and it is also important to determine whether the individuals included in the research are representative of the population of defendants diverted to CEP. Using data supplied by the CEP staff, descriptive statistics were compiled on the entire clientele of CEP who were taken into the program between January 1, 1977 and October 31, 1977, the period of intake into the research population. CEP's total client population includes all members of the research experimental group, the 66 CEP participants diverted from Manhattan, Brooklyn and the Bronx mentioned above who were not subject to the research assignment process, diverted clients from Queens who were not part of the research, and finally all non-diverted clients serviced by CEP.[7] The statistics presented below include *all* defendants *diverted* to CEP during the period of research intake; thus it subsumes those included in the research.[8]

During this period, CEP's diverted clients ranged in age from 15 to 59 with a mean of 20.2 (N-679). While roughly two-thirds of

[7] CEP offers services to two groups of non-diverted clients: (1) those who, while not diverted, are referred to CEP by some actor in the courts (judge, attorney, etc.) and (2) clients referred to CEP through people outside the courts, e.g., CEP staff members, clients, or through "walk-ins." Non-diverted CEP clients are not discussed here because of our explicit focus on the impact of diversion; however, they are a group of growing importance to CEP (see Chapter VII).

[8] CEP's management information system could not at this time provide the research with data on all clients *excluding* those in the research sample.

them were included in the research (the research experimental group), it is still important to note that there is no difference between the mean age of the research experimental group and that of all of CEP's diverted clients for the period. Similarly, 90 percent of the members of the research population were male as compared to 89 percent of the population of CEP clients. Fifty-one percent of the research subjects were black, 37 percent hispanic, and 12 percent white, as compared to 49 percent, 33 percent and 18 percent of CEP's total population of diverted clients. A comparison of the proportion of CEP clients who were employed at intake into the program (15 percent) with the proportion of the research population who were employed (13 percent) reveals no difference. Nor does there appear to be a difference between the proportion of CEP diverted clients who were enrolled in school at intake (35 percent) and the proportion of research subjects enrolled in school at intake (37 percent). Because these two groups contain many of the same people (CEP could not separate out the research subjects), one cannot test the differences for statistical significance; however, the two groups show no evidence of being different. These data support two important premises: that the experimental and control groups are not systematically different from each other, and that the research population is representative of the population of CEP diverted defendants in New York City.

PROBLEMS ENCOUNTERED IN DATA COLLECTION

While the research was successful in generating randomly selected experimental and control groups, the degree of success in collecting data on these defendants and the problems encountered varied with the type of data. The data may be categorized into three types: personal interview, official record, and verification, each with its own special problems.

Interview Data

The research design called for each subject to receive three personal interviews -- at intake into the sample, six months after intake, and 12 months after intake. Because of the characteristics of the research population (e.g., its youth and concommitant instability, lack of consistent employment, street-life, etc.,) and because of circumstances of the research selection (its relation to the court case), it was often difficult to initially interview and to maintain contact with the subjects.[9] While the assignment procedure described above generated 666 cases, the research was able to interview 533 (or 80 percent) of them at intake, 466 (70 percent) six months later, and to collect 12 months of data on 441 (66 percent) of the total research population. This rate of mortality, while not unusual in longitudinal field research, requires that one investigate

[9] A detailed discussion of the problems encountered by the research interviewers is provided in Appendix A.

its effects on the changing composition of the original randomly assigned groups to assess the validity of comparisons between experimentals and controls. The drop-out of individuals from the population is clearly a process of self-selection, and it is important to know which variables differentiate those who remain part of each research group from those who drop out.

An analysis was conducted to determine whether persons who were never interviewed (N=133) differed from those who were interviewed at least once (N=533). The data on uninterviewed defendants was collected from the ROR interview conducted by Criminal Justice Agency (formerly PTSA) at the time of arraignment; data on interviewed defendants came from the ROR interview and the Vera research intake interview. The variables that were included in the analysis were research assignment, characteristics of the court case, age, gender, ethnicity, employment, and enrollment in school. The results of these analyses are presented below. In addition, because the sample size continued to drop from intake to the first and second follow-up interviews, it was necessary to determine whether there were differences on the variables mentioned above at each of these stages. The design and results of these anaiyses are also presented below.

A series of contingency tables (cross-tabulations) were constructed to compare individuals who received a research intake interview (interviewed) with those who did not (uninterviewed) to determine whether interviewed defendants, who are a self-selected group, were representative of the research population. There were no statistically

significant differences between the two groups on gender, severity or type of arrest charge; that is, uninterviewed defendants had been arrested on charges that were no more or less serious than the interviewed defendants. There were, however, differences on some demographic variables.

The population of defendants (N=666) was 51 percent black, 37 percent hispanic, and 12 percent white; however, white defendants were significantly more likely than blacks or hispanics to drop out of the research interview process before the intake interview. While 85 percent of the blacks and 81 percent of the hispanics were interviewed at intake, only 63 percent of the whites were interviewed (χ^2_2=18.387; p<.001). In addition, over the twelve months of follow-up whites continued to drop out of the interview process at about the same rate as did blacks and hispanics, so that there were significant differences on both the first (six-month) follow-up (χ^2_2=10.775; p<.005), and the second (twelve-month) follow-up interview (χ^2_2=11.213; p<.004). The percentage of each ethnic group interviewed at each of the three times are presented in Table III-3 below. It can be seen from the table that whites were more likely to drop out of the research before the intake interview and continued to drop out over the twelve months. It is possible that this difference may be attributed to the greater negative value placed on the arrest by white defendants and their families. That is, whites may have been more likely to associate the Vera research with the negative event of being arrested, and therefore, were more reluctant to participate. Because

whites make up such a small proportion of the population (12 percent), however, this difference is unlikely to affect the results of the research.

Table III-3

PERCENTAGE OF ETHNIC GROUPS RECEIVING INTAKE, SIX-and 12-MONTH INTERVIEWS*

Interview	Ethnicity Black	Hispanic	White
Intake (N=515)	85%	81%	63%
6-Month (N=450)	75%	68%	57%
12-Month (N=366)	62%	56%	42%
(N)	(325)	(236)	(79)

*NOTE: The total N (640) for this table is less than 666 because of missing data on the ethnicity variable; the amount of missing data is not large enough to affect confidence in the results. In addition, while 12-month data were collected in 441 cases, not all of them received a "12-month interview"; some individuals received their 6-month interview so late that it covered the entire twelve month period. Data for these people were included in the tests of the hypotheses, but in this analysis, they are not categorized as interviewed at twelve months, hence the smaller N (366).

More important than ethnic differences are differences between the interviewed and uninterviewed group on age (at the time of intake into the research). The two groups were not significantly different at intake, although there was a trend (p=.085) for interviewed persons to be younger (mean=20.0) than uninterviewed (mean=21.0). Older defendants were more likely to drop out of the interviewing process over

time, so that by the six-month interview there was a significant difference ($\underline{t}(616)=3.423;p<.001$)[10] between the mean age at intake for interviewed (19.7) and uninterviewed (21.4) persons. That is, the average age at the time of intake of defendants who received *both* an intake and six-month follow-up was lower than those who did not receive both interviews. (Note that the latter group is composed of individuals who received only an intake interview (N=65) and of those who were never interviewed (N=111) by the research.) Although the gap between the groups did not widen by the end of the twelve-month period, there was a significant difference ($\underline{t}(616)=3.398$; $p<.001$) on mean age at intake between persons who received all three interviews (mean=19.5; N=356) and those who did not have all three interviews (mean=21.1).

There were also differences between interviewed and uninterviewed defendants on two variables that can probably be accounted for by the age differences. The first of these, employment status at the time of intake into the research, is only weakly related to interview status. The reason for the weakness of the relationship may be that so few (13 percent) of the respondents were employed at intake. Nonetheless, employed persons were slightly ($\chi_1^2=5.876$; p=.02) *less* likely to be interviewed at intake (69 percent) than were unemployed members of the population (81 percent). The relationship remained weak but significant ($\chi_1^2=5.274;p=.02$) at six-months, and at twelve months was

[10] The degrees of freedom for a t test are N-2. The N (618) is lower for this analysis because of missing age data.

not significant. (see Table III-4.) The reasons for the relationship are not clear: it is possible that working people found it more difficult to take the time for an interview, that the stipend for the interview was a greater incentive for unemployed than for employed persons, or that older persons (who are more likely to be employed) are more likely to drop out of the research than are younger (unemployed) members of the population.

Table III-4

PERCENTAGE OF EMPLOYED AND UNEMPLOYED PERSONS INTERVIEWED AT INTAKE, SIX-and-TWELVE-MONTHS

	Work Status	
Interview	Employed	Unemployed
Intake	69%	81%
Six-Month	57%	71%
Twelve-Month	49%	60%

The interpretation that the relationship between employment and interview status is a function of age is supported by the relationship between school enrollment and interview status. Members of the research population who were enrolled in school at intake were more likely than those not in school to have an intake interview, and this relationship became stronger over the twelve month follow-up period: while the difference in percentage interviewed at intake between those

enrolled in school (85 percent) and those not enrolled in school (77 percent) was only eight percentage points, twelve months later the difference was 23 percentage points. (See Table III-5.) For each interview the comparison variable was the same, enrollment status at the time of intake; since younger persons are more likely to be in school, the effect may be accounted for by the dropout from the research of older persons.

Table III-5

PERCENTAGE OF PERSONS ENROLLED AND NOT ENROLLED IN SCHOOL INTERVIEWED AT INTAKE, SIX-and TWELVE-MONTHS

Interview	In School	Not in School	χ^2_1
Intake	85%	77%	5.905, p<.02
6-Month	80%	63%	19.584, p<.001
12-Month	73%	50%	28.617, p<.001

In summary, those persons who were black or hispanic, young, unemployed, or in school at intake into the research (characteristics reflecting the majority of the research population) were more likely to receive an intake interview and to remain in the research than were those who were white, older, employed, or not in school (characteristics reflecting a minority of the research population). These results have implications for the *representativeness* of the interviewed sample; that is, one cannot be certain that differences between

experimentals and controls found for the *interviewed* sample are present in the research population as a *whole*. While this is an important consideration, it is mitigated somewhat by the completeness of the data collected from official records. If differences between experimentals and controls on variables constructed from official record data are consistent with those from interview data (and they are), one can be more confident in the representativeness of the interview data results. For example, if interview data were to show that experimentals made a positive (or negative) change over time in employment, and official record data were to show a similar positive (or negative) effect on recidivism, one could conclude that the self-selected interview group was not grossly different from the research population as a whole.

A second, and perhaps more important issue, is the effect of the dropout or "mortality" on the equivalence of the experimental and control groups. While it was demonstrated above that the research was successful in generating equivalent experimental and control groups, the effects of mortality on that equivalence remain to be seen. To test for such effects, differences between experimentals and controls on the variables discussed above were tested for three groups: all persons who were interviewed at intake, those who received both intake and six-month follow-up interviews, and those who received all three interviews. These analyses yielded *no* significant differences between experimentals and controls on characteristics at intake among those interviewed at intake, six- or twelve-months. While roughly one-third of the defendants dropped

out of the interviewing process by the end of the twelve months of follow-up, the experimental and control groups remained equivalent. Thus, for example, while defendants who received all three interviews tended to be younger than those who dropped out of the research, the mean age (at intake) for experimentals who were interviewed three times (mean=19.6) was not significantly different from that for controls who were interviewed three times (mean=19.3).

One can conclude from the results of these analyses, therefore, that differences in changes on employment, schooling, and other self-reported variables between experimentals and controls reported in the remainder of this report are a result of program impact rather than pre-intervention differences.

Record Data

Official record data were collected from three agencies with varying degrees of success. For each member of the sample (regardless of whether or not he/she was interviewed), attempts were made to obtain Police Department records of the person's criminal history. Permission was obtained from the Identification Section of the New York City Police Department to send members of the research staff to collect copies of the "rap sheets" for each person in the sample. Rap sheets (which contain a person's New York State criminal history) were procured for most of the sample (N=612), except for those cases

that were sealed (because of favorable dispositions),[11] or in a few instances the identifier used to locate the record (NYSID number) was incorrect. Other problems with the Police Department records included illegibility and different formats used for the records at different times. The most important problem, however, stemmed from incomplete records of case disposition; the Police often fail to receive notification of case disposition from the courts. Because of this shortcoming, it was necessary to supplement case disposition data with that from the Criminal Justice Agency (CJA).

The CJA file was the most complete in its coverage of the sample (N=652) available at the time of the data collection. It was intended to use CJA records as a source of information on the intake case and subsequent arrests, including such data as number of court appearances, days in detention, bail amounts, arraignment charges, time from arraignment to disposition, and final disposition of cases. In addition, when Police Department records did not include dispositions for previous arrests, these pieces of information were obtained where possible from CJA. Two problems inhered in this course of action: (1) CJA records included only arrests that occurred in New York City, and (2) they only went back as far as 1975. Given the youth of the research population, however, collection of data proceeded satisfactorily, with CJA's computerized system allowing fairly rapid acquisition and updating of information.

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[11] A more complete discussion of sealing in New York City and its implications for diversion is found in Chapter IV.

Data on all CEP participants (i.e., members of the research experimental group) were coded from CEP's files. These files contained information on clients' expectations, counselors' evaluations, referrals to and placements in jobs, vocational status at intake and exit, etc. The major problem encountered in collecting CEP data was that the amount of information in the files varied greatly, especially with the individual's attendance at CEP; those divertees who attended CEP only a few times, once, or never had program files that were nearly devoid of information. More importantly, however, even for those clients who successfully completed the program, there were sizeable gaps in the data. While much of the missing data resulted from changes in CEP's methods of recording information, reorganization of staff over time, and variation among counselors in the accuracy and amount of information recorded, the research was hampered in its efforts to analyze what the program did to (or for) its clients.

Although each source of official record data presented some problems and had some shortcomings in completeness or accuracy, these data collection attempts were quite successful. Information was gathered on most subjects, including uninterviewed individuals for whom records represented the only information available.

The third type of data collection effort was official verification of subject's own reports about their employment, school enrollment, and public assistance status. In each interview the subject was asked to sign release forms allowing the research to attempt to verify these data; subjects generally agreed and the

verification process was completed with limited success. A number of problems arose in the verification of employment: many subjects worked "off the books," were employed under an alias, or worked part-time or for so short a time that their records were not kept by the employer. In addition, many members of the sample worked in family businesses or small, neighborhood establishments or gave the interviewer insufficient information to contact the employer. However, in over 80 percent of the cases in which the employer was successfully contacted, the subject's employment at the job during the period reported was confirmed. In very few of the remaining cases did the employer's information suggest that the subject had supplied the research with incorrect information; more often the employer simply refused or was unable to answer. Despite these problems, the research was encouraged because there was no difference between experimentals and controls in the proportion of cases in which employment could be verified and in the proportion of self-reported verified as accurate.

Many of the problems encountered with job verification were also encountered in verifying school enrollment. In many cases a school reported that the person never attended; however, this may have been a result of school registration under a different name than that given to the Vera research staff.[12] Often information obtained from subjects

[12] The problem of aliases, multiple names, and similar names for different people plagued the research throughout. Whereas unique identifiers (e.g., Social Security number or NYSID numbers) were available, they were not always applicable or helpful.

regarding the school name and/or address was incomplete or incorrect, making it impossible to verify reports. As a result, completed verifications of school enrollment were obtained for roughly 60 percent of those subjects who reported being enrolled in school. Of those, about 80 percent of the schools confirmed that the subjects had been enrolled on the dates reported; however, because of the New York City school system's categorization of students in truant classes, it was extremely difficult to verify subjects' reported grade levels. Nontheless, rates of confirmation of enrollment did not differ for experimentals and controls.

The third type of data to be verified were subjects' reports of receipt of public assistance. A member of the Vera research staff entered the names of all sample members into the computer files of the Human Resources Administration, Income Maintenance Division, to determine whether there was any record of welfare for that person during the period from January 1977 to October 1978. The dates of welfare receipt were recorded from the files and compared with the subject's reports to the Vera interviewers. In over 80 percent of the cases in which it was possible to verify receipt of welfare benefits, the subjects' reports were confirmed by official files. Members of the experimental and control groups were equally likely to report information that was confirmed (or disconfirmed) by these records.

The problems encountered in attempts to verify employment, school enrollment, and receipt of welfare, as well as the results of these attempts influenced the research decision to rely on self-

reports for analyzing data in each of these areas. That is, because of the inability of the research to contact all employers or schools and because of frequent use of aliases, it was not reasonable to conclude that discrepancies between subjects' reports and those of the verifying agency represented deliberate falsehoods on the part of the subjects. In addition, the similiarity between experimentals and controls on confirmation rates indicated that there was no systematic difference between the two groups in veracity or accuracy of report. For these reasons it was decided that analyses of self-reported employment, school enrollment, and welfare receipt would proceed without altering the database by inclusion of information from the verifications.

HYPOTHESES

The research was designed and the data collected to test hypotheses related to CEP's impact on case outcome, recidivism, and life style variables. The experimental design and the longitudinal nature of the study allowed the measurement of differences between the experimental and control groups over time and permitted some confidence in attributing such differences to the impact of the program. Because CEP was a diversion program that offered social services to its participants, it was expected to have an impact on the outcome of the court case on which the individual was diverted and to have rehabilitative effects.

Diversion Effects: Disposition

It is possible to measure effect on the court case in a number of ways. Most important among these is the final disposition of the case. It was predicted that members of the experimental group would receive more favorable dispositions than those in the control group. Since case dismissal was supposed to be an outcome of successful participation in CEP, all subjects in the experimental group who completed the program should have received dismissals; more control group members, on the other hand, should receive unfavorable dispositions (e.g., convictions accompanied by sentences such as fines, prison terms, probation, conditional discharge, etc.). It was also possible that CEP's impact on disposition would be mediated by prior arrest or conviction record. In addition, dismissal of the current case has different implications for those with no prior record (i.e., the maintenance of a "clean record") than it does for defendants who have prior convictions. Thus it was considered necessary to test the difference of impact on disposition between experimentals and controls while controlling for the effects of prior record. The hypothesis that experimentals would receive more favorable dispositions than controls was tested using a two-way chi square, and the prior record variable was introduced as a third (or controlling) variable.

One of the goals of CEP was to reduce penetration into the criminal justice system; therefore, a number of measures of penetration were included. It was predicted that experimentals would have fewer court appearances and spend less time in pretrial detention;

because of the four month continuance, however, the length of time to disposition should be longer for experimentals than for controls. Each of these hypotheses was tested using a t-test for the difference between two means.

Intervention Effects: Lifestyle

Rehabilitation implies a change in lifestyle in the direction of greater stability, employment, and increased education. Thus it was predicted that over time experimentals would make more progress in this direction than would members of the control group. Since CEP attempted to place clients in jobs, experimentals should be more likely than controls to be employed six months after intake into the program. Other mesures of employment on which one would expect experimental group members to improve are salary, number of months employed, number of hours worked, and number of jobs held. Because subjects were interviewed three times (at intake, six months and 12 months later), it was possible to compare experimentals and controls over time. A series of repeated measures analyses of covariance were computed to test these hypotheses. In each of these analyses, research assignment (experimental or control) is the independent variable, measurement at intake on the dependent variable is the covariate, and measurement on the dependent variable at six and twelve months is the repeated measure factor. (This method of analysis and the reasons for its appropriateness are explained in Chapter V.)

Many participants in CEP were young and undereducated; therefore, one would expect that for a segment of the population CEP would have an impact on education-related variables. It was predicted that over time a greater proportion of experimentals than controls would be enrolled in school, increase their level of education, and improve school attendance. These hypothese were tested using the method described above (i.e., repeated measures analysis of covariance). It is also possible that CEP had an impact on education only for the younger members of the sample, therefore, the effects of age were partialled out (controlled for) to ensure that if such an effect existed, it would be detected.

Other possible rehabilitative effects include an increase in vocational goal-directed activity, such as searching for a job or childcare. An index of "vocational activity level" was constructed to measure the amount of time an individual was engaged in any of the following activities: employment, school, job search, or childcare. It was predicted that experimentals would make greater improvements over time on this measure than would controls; this analysis was computed using repeated measures analysis of covariance. It was also predicted that experimentals would show a decrease from intake to six months later in the frequency of self-reported illegal activities (for which they were not necessarily arrested), an increase in the number of kinds of social services used, and improvements in living conditions. Each of these hypotheses was tested using repeated measures analysis of variance, with one between-subjects factor (research assignment) and one within-subjects repeated measures factor (the dependent variable measured at intake, six months, and 12 months).

Rehabilitation Effects: Recidivism

It was predicted that CEP would have an impact on recidivism, but it is possible to measure recidivism in a number of ways. In the present research, recidivism was operationalized in several different ways: the proportion of each group arrested within four, six, and 12 months after intake; the number of rearrests per person within a given period; and the severity and type of these arrests.[13] For each variable it was predicted that experimentals would fare better than controls. That is, if one of the goals of CEP was rehabilitation, experimentals should be arrested less frequently and for less serious crimes than members of the control group. As as ancillary analysis, an attempt was made (using multiple regression/correlation) to determine which factors predicted recidivism; although such analysis violates the experimental design, it was conducted as an exploratory, hypothesis generating analysis.

Success in CEP

A defendant's status at exit from CEP was likely to affect a number of the outcome variables under consideration in the present study. Participants who successfully complete the program are not only supposed to have their cases dismissed, but should also

[13] The fact that so few subjects spent any time in jail or prison meant that the research did not have to assess these rearrest rates according to differential periods of "time at risk" (i.e., time during which each person was not incarcerated).

be the most likely to benefit from the services offered by the program. The predictions of reduced recidivism, increased employment, and improvement in lifestyle are far more likely to be supported by the data for successful participants in CEP than they are for those terminated by the program. To preserve the integrity of the experimental design, however, all analyses described above were conducted with the experimental group intact, including successful and unsuccessful participants (as well as exceptions to the research assignment). In secondary analyses, however, CEP status (successful or unsuccessful) was used as a variable to determine whether there were any differences on the above variables between those clients who successfully completed CEP and those who were terminated from the program.

Related analysis, also outside the experimental design, was conducted using only members of the experimental group. This analysis used multiple regression/correlation techniques to predict success in CEP. In this analysis CEP exit status served as the criterion (dependent) variable and demographics, criminal history, status at intake, and CEP variables as predictors. This analysis was not intended to test an hypothesis; rather, it was designed to provide some insight into the participants' characteristics that seem to affect a defendant's success in CEP.

GENERALIZABILITY

The results of the comparison between the research population and the CEP service population indicate that one can be confident that the Vera research population is representative of the overall population diverted by CEP. It is reasonable, therefore, to assume that any impact of CEP on the research population should be felt by other diverted defendants *so long as* they are similar to CEP's client population and given the same types of services. It is important to exercise caution, however, in generalizing the results of this evaluation to other diversion programs. The New York City population is distinctive, and black and hispanic, inner city youths living in a city as large as New York are likely to be very different from defendants in other enviornments. Careful consideration of the problems and experiences of this particular population, as well as the specific nature of the Court Employment Project, should serve as the baseline for any attempts to generalize the results of the present study to diversion in other jurisdictions.

CHAPTER IV

PRETRIAL DIVERSION AS AN ALTERNATIVE TO PROSECUTION, STIGMA AND PUNISHMENT: CEP'S IMPACT ON DISPOSITION

INTRODUCTION

In this chapter we examine the experimental data relating to the diversion rationales which have been used to support CEP's commitment to providing services *preadjudiction*. As have most PTD/I programs, CEP saw the diversion strategy of service delivery not only as a method for social service intervention (see Chapters V and VI), but specifically as a way to affect the dispositional process. That is, CEP wanted to help defendants avoid prosecution, conviction, and stigma, and to help the system provide a more humane and less costly alternative to criminal sanctions. Since any PTD/I program's ability to affect case disposition is closely tied to prosecutors' perceptions about and use of diversion as dispositionai option, this aspect of CEP's diversion efforts is also discussed.

As a *pretrial* program, one of CEP's major aims has been to provide the criminal justice system with an option for helping defendants secure a dismissal, that is, to divert them from prosecution and from the stigma of a criminal conviction. Related to this, the program also wanted to reduce the amount of contact defendants had with official criminal justice agencies. This, CEP believed, would limit youthfui defendants' exposure to unpleasant (and possibly

harmful) experiences[1] and have the added system benefit of reducing the resources spent on processing cases. Whereas the original diversion goals of the program had included helping defendants avoid jail (detention at a minimum but possibly also a jail sentence), the probability of CEP's diverting such defendants *pretrial* seemed unlikely in more recent years. Bail reform, the relative infrequency with which defendants entering the system in these jurisdictions are sentenced to jail,[2] and the hesitancy of prosecutors to agree to any alternative that leads to a full dismissal for defendants otherwise likely to receive a jail sentence were thought to have limited diversion from incarceration. Furthermore, as indicated in Chapter II, ten years of experience with the New York City Criminal Courts, in conjunction with the results of the Zimring evaluation, had made CEP uneasy about the extent of its success affecting disposition and sentence. Despite the agency's concerns, in the absence of definitive data to the contrary, CEP continued to provide services *preadjudication*. This service delivery strategy was predicated upon the continued belief that CEP clients were being diverted from prosecution, conviction and their negative consequences -- criminal sanctions and the stigma of an official record -- if not from a jail term.

In the remainder of this chapter, we draw primarily upon data from the experimental design to compare the disposition outcomes of diversion to CEP with those resulting from normal criminal processing. We augment these data with materials from a random sample of cases

[1]Presumably this was not just a positive end in itself, but also a means to rehabilitate (See Chapters V & VI).

[2]See Vera, *Felony Arrest*, 1977..

eligible for the program, screened by CEP, but not approved for diversion (and thus not included in the experimental design) and with qualitative materials from research interviews with decision-makers in the dispositional process.

DIVERSION FROM PROSECUTION

The rationale for diversion and preadjudication services assumes that diverted individuals avoid three potentially harmful personal experiences: (1) the process of full prosecution and substantial penetration into the criminal justice system (including pretrial detention, bail experiences, multiple court experiences, etc.); (2) criminal convictions; and (3) the stigma of a criminal record. Mullen's analysis of the universal diversion dilemma, discussed in Chapter I, suggested that all PTD/I programs face the problem of deciding whether to divert primarily minor offenders as a humane alternative or to divert more serious offenders to facilitate intervention and rehabilitation. CEP could not fully resolve this dilemma over the years; however it adjusted its formal intake criteria on several occasions in an attempt to assure that it was not diverting exclusively minor first offenders. These adjustments represented a program attempt at least to assure diverted cases were serious enough (in the estimation of the system) to warrant prosecution (see Chapter II). A description of the cases and defendants approved for diversion during 1977 follows. It suggests the complexity of CEP's effort; subsequent analyses of data from the experimental design indicate the extent of its success.

The Diversion Population: Current Changes and Prior Records

As seen in Table IV-1, CEP predominantly diverted felony cases (97 percent), nine out of ten of which were arraigned on class D or E felonies (the least serious of five levels of severity in New York State's Penal Law). Three out of four defendants were charged with some form of theft, most frequently Grand Larceny 2° (CPL 155.35, a class D felony involving the theft of property valued at more than $1500), Grand Larceny 3° (CPL 155.30, a class E felony involving the theft of property valued at more than $250 or a credit card), and Burglary 3° (CPL 140.20, a class D felony which involves knowingly entering or remaining unlawfully in a building with the intent to commit a crime). Seven percent were charged with robbery (larceny with threat or use of force) and an additional nine percent with assault (without robbery). Fewer than one out of ten were charged with a drug, forgery, conduct, or weapons offense.[3]

[3] CJA data suggest the following distribution of charges in the Manhattan and Brooklyn Criminal Courts (combined) for the month preceding the start of this research (December, 1976):

A or B Felonies	10%
C Felonies	11
D Felonies	28
E Felonies	14
Misdemeanors	32
Violations & Other	5
TOTAL	100%
(N=5700)	

Table IV-1

TYPE AND SEVERITY OF INTAKE CASE (ARRAIGNMENT CHARGE)
TOTAL RESEARCH POPULATION
(N=659)

Type of Charge		Severity of Charge	
Theft	75%	A or B Felony	1%
Robbery	7	C Felony	9
Assault (w/out Robbery)	9	D Felony	48
Other	8	E Felony	39
		Misdemeanor	4[a]
TOTAL	100%		100%

[a] Although the program is felony-only, occasional exceptions are made.

Consequently, during 1977, CEP was not diverting cases on drug possession, morals, or other charges typically involved in de-criminalization efforts. This was in contrast to at least some other diversion programs, for example, Dade County, Florida, where from one-fifth to one-third of the cases diverted in 1977 were fairly minor drug charges (Vera, 1978). Neither were CEP's cases uniformly other types of "junk" cases -- to use the parlance of the system -- for example, fare beats charged with jumping the subway turnstile without paying their 50¢ (an A misdemeanor, theft of services). On the other hand, the charges brought against these defendants were

also not the most serious. Only one percent were charged with the two highest classes of felonies (A and B), and only nine percent with the third most serious (C) felony class.[4]

In addition,some of the charges at the D and E felony level appear more serious at first glance than upon closer inspection. A Larceny 2° (e.g., car theft) upon examination of the evidence might be only a misdemeanor (a passenger in a stolen car). A Larceny 3° might have involved a theft from a parent or guardian,or be related to an argument with a spouse over property. A Burglary 3° charge could have resulted from the arrest of teenagers found in an abandoned building, who had taken either nothing or possibly copper plumbing pipes for future sale. Furthermore, the great majority (over 90 percent) of the defendants interviewed reported they had not been carrying weapons at the time of their arrest; those few who were, reported carrying knives or clubs, not firearms. In over half the cases where there was a victim, the defendant reported knowing that person, and in three-quarters of the cases the victim was reported not hurt. In all such cases, therefore, there are not only questions concerning what charges can be sustained by the evidence and whether the victim would appear in court to press charges, but there is also the issue of how seriously the crime is regarded by the criminal justice system.

[4]While the few A and B felonies are not surprising (they were excluded under the formal eligibility criteria), the C felony cases were included as eligible; obviously, however, the agency was able to divert relatively few.

The "value" of the case combines not only convictability but the normative weight with which the illegal behavior is viewed by the system (particularly as compared to other offenses) and the sanctions deemed appropriate. In terms of arraignment charges, therefore, CEP was successful at diverting only *felony* cases and somewhat successful at assuring some were not "junk;" most, however, were also not obviously of great seriousness.

The pattern regarding the type of *defendants* diverted was similarly mixed. While six out of ten defendants approved for CEP diversion had no official prior arrest record, four out of ten had been arrested previously, and half of these more than once.[5] Only 16 percent, however, had a prior record of *convictions* and very few of these had spent any time in jail (which is not surprising since only three percent of the 16 percent had been convicted of a felony). As might also be expected, the most serious prior arrest charges were offenses similar to those involved in the current case -- theft (49 percent), robbery (14 percent), and assault (10 percent), about three-quarters at the felony level. Half of those previously arrested had had their first adult arrest early, between the ages of 16 and 17. Almost two out of ten defendants interviewed (18 percent) reported they had been arrested as juveniles (although we could not obtain confirmation since the records are sealed). Half of those with juvenile arrests said they had been arrested before the age of

[5]Sixteen defendants of those interviewed (three percent) reported they had been previously arrested as adults *outside* New York State. These arrests could not be officially verified and since there were too few to affect further analyses, they are not included in these data.

fourteen, and most of these reported more than one arrest (Median = 2.0). Half of those reporting a juvenile record also said they had been on juvenile probation and a quarter reported being in a state training school. Again, most reported the most serious charges for which they had been arrested as juveniles were theft (43 percent), robbery (16 percent),or assault (15 percent), offenses similar to those with which they were charged as adults.

In summary, therefore, while the majority of the youthful defendants CEP was able to divert were first adult offenders (and thus, for example, had at least some stake in avoiding the experience of prosecution and maintaining a record clean of convictions), a sizeable minority (40 percent) had had previous adult experinece with the criminal justice system. For some, their experiences were quite substantial and extended back into the juvenile court system as well. Finally, as will be shown in Chapter VI, about one out of three were rearrested subsequent to this involvement with the diversion process. Certainly not all these defendants could be characterized "boy scouts and virgins."

Penetration Into The Criminal Justice System

CEP's concern with limiting youthful defendants' exposure to the criminal justice system was, therefore, appropriate for a sizeable

proportion of its population, but not for all.[6] Although many felony arrests in New York City do not continue in the system past their arraignment,[7] 94 percent of the defendants in the research population were required to appear in court again after arraignment. While this was partly because all *diverted* defendants had to reappear (their cases were adjourned at arraignment for four months during which they received CEP's services), 86 percent of the control group also had to appear again. This suggests that, as indicated above, while the cases were not the most serious, they did require attention by the court if they were not diverted. Yet, since most of these defendants did not have

[6]It might be noted that there was also ambivalence in the system (and even in CEP) about the appropriateness of such attempt to "shelter" young defendants from the harsh realities of the criminal justice system. There were those (quoted in Chapter I), including defense counsel and PTD/I personnel, who felt it was an important deterrent for youths to "taste" the harshness of what would happen to them if they continued to get arrested; others denied this had a deterrent effect and claimed it only hardened young people. This debate cannot be resolved by the data in this study. While we will compare recidivism rates for diverted and non-diverted defendants in Chapter VI, it is important to recognize that all the defendants in this study (diverted or not) had experienced the harshest parts of the process before ever being considered for diversion or assigned as part of this research: all defendants were *arrested*; they were booked by police at a station house or a central facility; they were fingerprinted and photographed, and awaited arraignment in detention facilities in the precinct or courthouse. At the time of this study, the period between arrest and arraignment averaged more than 24 hours. The conditions of detention are typically poor -- overcrowded, uncomfortable "pens" with extremely limited sanitary facilities; prisoners are fed if the wait is long, but such "amenities" are minimal. Consequently, except possibly for those subject to a jail experience, the pre-arraignment arrest and detention period *before* diversion was likely to be the most difficult and potentially "traumatizing" part of these defendants' overall experience. CEP could not affect their exposure to this process, though it could, perhaps, influence defendants' later interpretation of the totality of their arrest, detention and prosecution experience.

[7]Data from Vera's study of 1971 felony arrests (Vera, 1977) show 16 percent of the cases disposed at or before arraignment; of those disposed at arraignment, 72 percent of the defendants took guilty pleas. Since 1971, however, there has been increased court pressure to dispose of cases early and the proportion disposed at arraignment by 1977 was probably higher.

lengthy or heavy prior records, few had to go through the difficult experiences of obtaining bail or bond, being detained pretrial, or returning to court after the issuance of a warrant. Bail or bond was set at arraignment for only seven percent of the defendants (nine percent of the controls and five percent of the experimentals). While an additional 32 defendants had bail or bond set at a later date, only 10 percent of the total research population spent *any* time in pretrial detention. The amount of time they spent was short, and did not differ significantly for the two groups (2.1 days for controls and 1.4 for experimentals). There was also no difference in the proprotion of experimentals and controls for whom a warrant was issued at some point prior to their last court appearance (18 percent of the controls and 15 percent of the experimentals).[8]

There were,however, two interesting differences between diverted and normally processed defendants with respect to their penetration of the criminal justice system. Experimentals took *longer* on the average to have their cases fully disposed than did controls, approximately 21 weeks compared to 16 weeks ($\pm(503)=4.442$; $p<.001$).[9] What is particularly interesting is that the average period of time *controls* spent in the

[8]In most cases these defendants missed a court appearance but appeared subsequently on their own (that is, they were not returned by the Police Warrant Squad or because of a new arrest).

[9]For the purpose of these analyses, the cases of defendants who had failed to appear as of the final date of our check of court records (December 1978, generally more than a year after the issuance of a warrant) were considered "disposed" as of the date of the last scheduled appearance at which the warrant was issued. With defendants sentenced to pay a fine, the case was not considered fully disposed by the research until the last appearance for payment.

system was exactly four months -- the official period of diversion to CEP -- but the average period of time for experimentals was more than *five* months. This is a result of the time required to prosecute the cases of defendants who were unsuccessfully terminated from the program during the diversion period. Successful and therefore dismissed CEP clients had an average of 20 weeks to disposition compared to 26 for unsuccessfully terminated clients. Despite the fact that it took longer to process diverted cases than those handled normally by the court, control cases were scheduled to *appear* more frequently in court than were diverted cases. Control cases were scheduled for an average of 3.91 appearances compared to 3.49 for experimentals ($\pm(636)=-2.017$; $p<.05$).

In summary, therefore, from the standpoint of CEP's rationale of diverting defendants from a prolonged pretrial period, from multiple sometimes painful experiences with the courts, and of conserving system resources, CEP's success was limited. Regardless of how the case was handled pretrial -- diversion or normal process -- *all* defendants faced the difficult experiences of arrest and detention prior to arraignment. Thereafter, even without diversion, few would have experienced the harsher aspects of the prosecution process -- detention, the search for bail, failure to appear or revocation of their release on recognizance. If diverted, the average period of time before disposition was prolonged, but the number of times they were scheduled to appear in court was reduced slightly.

Disposition: Dismissals And Convictions

Diversion from prosecution and particularly from criminal conviction is not only a general PTD/I rationale but has been CEP's central justification for providing its services *pretrial*. As far as many administrators and staff were concerned, this goal was second in importance only to that of providing needed services to clients. In its early years and particularly as it expanded after 1970, the program saw itself as an important informal addition to the few formal alternatives available to full prosecution. It was thought of as an additional route to favorable dispositions for clients who would otherwise have been unlikely, or much less likely, to receive them. Table IV-2 speaks to these issues; it compares the experimental and control groups on the final disposition of their cases. As the discussion that follows suggests, a full assessment of CEP's impact on case disposition in 1977 requires careful scrutiny of several, complex types of dispositions available under New York State law. A neat and simple distinction between "charges dismissed" versus "convicted" does not do justice to the concept of "diversion" as it operated in New York, nor to an analysis of the dispositional outcomes to which CEP had become an alternative.[10]

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[10]Our thanks go to Steven Mendelsohn, Esq. for his extensive help in researching and thinking about these issues, and in drafting the pages which follow. Responsibility for any errors contained therein, however, belong to the Report authors.

Table IV-2

FINAL DISPOSITION OF INTAKE CASE
BY RESEARCH ASSIGNMENT

Research Assignment	Dis-Missed	ACD[a]	CONVICTED Viol.	Misd/YO[b]	Fel.	War-rant	TOTAL(N)
Experimental	58%	14	5	9	1	12	100%(401)
Control	18%	28	23	25	1	5	100%(242)
TOTAL	43%	19	12	15	1	9	100%(643)

[a]Adjourned in Contemplation of Dismissal

[b]Youthful Offender Adjudication (79 percent of these are YO, 21 percent misd. convictions)

χ^2_5=146.44; p<.001

If one looks only at the first column of Table IV-2, it appears that, in 1977, CEP had a significant impact on defendants' chances for having their cases dismissed in the interests of justice. More than three times as many experimental cases were dismissed outright as were controls (58 percent compared to 18 percent). However, by 1977 -- ten years after CEP began -- New York State provided by statute several alternatives to full prosecution and criminal conviction *other than* a dismissal in the interests of justice. As is also evident in Table IV-2 (columns 2, 3 and 4), these alternatives were widely used by the criminal justice system in disposing of the felony

charges against diversion eligible defendants who were not diverted (controls) or who were unsuccessful in CEP (i.e., the experimentals not dismissed). These options included an Adjournment in Contemplation of Dismissal (ACD, the second column of Table IV-2); a conviction on a non-criminal violation (column three); and a Youthful Offender adjudication with a finding of guilt for a misdemeanor offense (column four). As with diversion to CEP, the existence and use of these other alternatives to full criminal conviction represented, to differing degrees, statutory recognition by the legislature and internalization by the criminal justice system of the general idea of "diversion."

As Table IV-2 indicates, 28 percent of the control and 14 percent of the experimental cases were disposed through an Adjournment in Contemplation of Dismissal. CEP 170.55 permits judges, with the agreement of the prosecutor, to adjourn action on a case without specifying a date for its return to court. If the prosecutor does not restore the case to the calendar within six months, it is automatically dismissed and becomes at this time a dismissal in the interests of justice.[11] For those members of the research population whose cases

[11] CPL 170.55 states, in pertinent part: "Upon or after arraignment in a local criminal court...the court, upon motion of the people or the defendant and with the consent of the other party, or upon the court's own motion with the consent of both the people and the defendant, may order that the action be 'adjourned in contemplation of dismissal,' as prescribed in subdivision 2."

(2) "An adjournment in contemplation of dismissal is an adjournment of the action without date ordered with a view to ultimate dismissal of the accusatory instrument in furtherance of justice. Upon issuing such an order, the court must release the defendant on his own recognizance. Upon application of the people, made at any time not more than six months after the issuance of such order, the Continued.../

were so adjourned, *none* were restored to the calendar; all were dismissed after six months.[12] It would appear, therefore, that in assessing CEP's impact on obtaining a *dismissal of the charges*, the "dismissed" and "ACD" categories in Table IV-2 should be combined in comparing the experimental and control groups.

Before concluding this, however, several other dimensions of the statutory and practical distinctions between the Dismissal in the Interests of Justice and the ACD should be discussed.

Historically, this statutory provision codified for the first time the informal dismissal on own recognizance (DOR) which had been used by prosecutors previously but which had fallen into disuse during the 1960s (see Comment, *Albany L. Rev.*,1974). This change in the statutes occurred at the same time CEP had demonstrated its ability to obtain cases to divert, and had secured local funding to expand diversion beyond New York County (Manhattan). At the same time, the legislature also provided for the ACD of persons charged

Continued... court must restore the case to the calendar and the action must thereupon proceed. If the case is not so restored within such six months period, the accusatory instrument is, at the expiration of such period, deemed to have been dismissed by the court in furtherance of justice."

[12] From all evidence the research could obtain both from agencies keeping court and criminal justice records and from interviews with relevant parties, most cases receiving an ACD are not returned to the calendar by the prosecution. Consequently, the ACD would appear to be used in cases where prosecutors intend a dismissal, regardless of what occurs during the six month period. Whereas the judge might place some condition upon the defendant during the six month adjournment period, it is typically an admonition to "stay out of trouble" or away from a complainant. It is interesting to note that, even in those situations where a defendant in the research population was arrested on a new charge before the expiration of the ACD, the original charges were not restored to the calendar.

with marijuana possession offenses under a separate statute with its own record sealing provisions (CPL 170.56). This effectively carried out partial decriminalization of minor drug offenses. Both ACD provisions were responsive to the same concerns as CEP and the diversion movement generally: to increase the availability of formalized mechanisms to move more defendants, particularly those charged with lesser criminal offenses, out of the system without a criminal conviction.

While the ACD leads to a dismissal in furtherance of justice, the procedure is legally different from the outright dismissal in two respects. First, it cannot be initiated without prosecutorial consent, whereas the direct dismissal can be accomplished on the court's own motion over prosecutorial objection. Second, the ACD dismissal does not become final until some time has passed. The non-legal consequences of these differences are somewhat harder to specify but do relate to how the ACD is used (or perceived to be so) as a disposition in contrast to the immediate dismissal.

The necessity for prosecutorial consent suggests the ACD is intended for use in cases where there is legal sufficiency to prosecute the case but where, because of the nature of the case, the prosecutor believes non-prosecution to further justice -- the almost classic situation described in the diversion literature for "pretrial diversion."[13] The delay in the official declination to

[13] Of course, ACDs like other "diversionary" mechanisms may be used by prosecutors if a case is flawed by evidentiary weaknesses; there is no way to assess this at present.

prosecute, however, suggests that the pendancy of criminal charges, even in the limbo of adjournment, is supposed to have some deterrent effect on the defendant. Certainly it appears from research interviews with prosecutors in the system that they view the ACD in this manner despite their recognition that ACDs rarely, if ever, are restored to the calendar. The ACD and the outright dismissal, however, may be more alike in practice than either the law or the "intent" of prosecutors suggest since: (1) ACDs are almost never returned to the system for prosecution; (2) there is no contact between the court and the defendant either during the six month adjournment period or at the time the ACD is officially dismissed (the defendant does not reappear in court); (3) many in the system believe that defendants do not understand the distinction between having the charges dismissed outright and receiving an ACD since both are discharged by the court and told not to return; and (4) the possibility (as assumed by many defense counsels) that without the ACD, the charges would eventually be dismissed anyway because of insufficiency of evidence or the non-appearance of a witness.

Certainly, the ACD is akin to a dismissal resulting from formal or supervised diversion.[14] Both delay the official declination to

[14] The Standards and Goals for Diversion issued by NAPSA in 1978 include statutes such as CPL 170.55 under the official definition of pretrial diversion, and in fact mention the ACD in New York State (1978:11). This is a change in their position, since the draft standards of 1977 excluded such "diversion without supervision or services" as true pretrial diversion.

prosecute and both result in a full dismissal. As to whether an ACD should be considered a "harsher" disposition than diversion to CEP for four months will be considered below when the sentences received by members of the research population are examined. For the moment, however, from the perspective of obtaining a dismissal of the charges, it seems appropriate to consider the ACD and the outright dismissal as pragmatically equivalent categories, so long as in its actual use the ACD is not often restored to the calendar (even under conditions of rearrest on new charges). Their equivalence is further supported by the fact that under the criminal history sealing statute (CPL 160.50 discussed more fully below), the ACD is fully the same as the outright dismissal: the same official records are to be sealed and all photographs and fingerprints are to be returned. As far as actual recordsealing *practices* are concerned, the two dispositions seem the same, except that the dismissal is sealed immediately and the ACD at its six-month expiration date.

Assuming the correctness of this conclusion, Table IV-2 shows that seven out of ten experimentals (72 percent) had their charges dismissed or received an ACD as did nearly five out of ten controls (46 percent). While the differences between the two groups are *statistically* significant, it is clear that the positive impact of CEP on disposition is substantially less than if dismissals alone are compared (column one of Table IV-2). An assessment of whether this difference is *substantively* or *programmatically* significant (i.e., legitmates CEP maintaining *pretrial* services) cannot be made from these data alone; the data which follow in this and subsequent chapters are also relevant. CEP's own assessment of the programmatic implication of these results is found in the concluding chapter of the report. Let it suffice to say here that CEP was not at all satisfied with the *degree* of its impact on disposition and subsequently changed the pretrial focus of its service program. The specific programmatic changes made by the agency are also discussed in Chapter VIII,

The issue of CEP's impact on the likelihood of a defendant's *conviction* goes beyond the discussion of how many defendants receive dismissals. A further question concerning "what kind of convictions" defendants receive must also be addressed. Under New York State statutes, not all "convictions" are the same and their legal as well as practical consequences deserve attention in any complete assessment of CEP's effect on defendants through its impact on the disposition of their cases.

As suggested above, Table IV-2 indicates that five out of ten defendants in the control group avoided any conviction compared to seven out of ten in the experimental or diverted group. Expressed alternatively, the table shows that 15 percent of the experimentals group were "convicted" compared to 49 percent of the controls.[15]

[15] Overall, nine percent of the research population was neither convicted nor not convicted of the charges in the intake case; 12 percent of the experimentals compared to five percent of the controls absconded and thus were never adjudicated. It might be noted here that, hypothetically, had all defendants in the sample been adjudicated, no matter what the outcomes, the distributions of the experimental and control groups on case outcome would remain statistically significant; the absolute differences (and thus their substantive significance), however, would have been narrowed.

However, the third column of Table IV-2 indicates that not all those convicted were convicted of *crimes*. Almost half the convicted controls were found guilty of *violations* (for example, disorderly conduct) which are not *criminal* offenses (CPL 10.00(3) in the same way that other "petty offenses," such as traffic infractions, are not (CPL 1.20(39)). Furthermore, the vast majority of the remainder of those convicted (75 percent) were "adjudicated youthful offenders" (YOs) after a finding of guilt for a misdemeanor offense. Under CPL 720.10 (4, 6), youthful offender "adjudications" are "substitutions for convictions" rather than convictions; the record is sealed from public access and the individual is not considered by law to have been "convicted."

This YO statute, originally enacted in 1943, has been amended frequently; in 1971, at the same time as the ACD/diversion laws were passed and CEP/diversion was expanding, YO status was made *mandatory* for all 16, 17, and 18 year old youths guilty of misdemeanors. This legislative amendment also reflected a desire, similar to that underlying various PTD/I efforts, to protect youthful and not yet "hardened" offenders. While the statute permits others in this age group to be treated as youthful offenders with its protections, it does not make such treatment mandatory. New York City prosecutors and judges interviewed by Vera researchers, however, claim that the YO statute is quite frequently invoked for youths with prior convictions who are charged with less serious crimes but that its use varies with changes in the "political climate" toward the punishment of young offenders. However, since 76 percent of the

defendants in the research population found guilty of misdemeanors were first offenders, they were automatically "adjudicated YOs" and their records sealed, as were 13 percent of those few members of the sample with prior convictions. Overall, therefore, only 3.8 percent of the entire research population were convicted of *crimes* on the intake case, and 6.6 percent of the controls.

Since a major goal of virtually all diversionary efforts (CEP, the ACD or the YO) is to help the defendant obtain a *non-criminal disposition*, we may conclude from Table IV-2 and our discussion of it, that CEP succeeded in this attempt only somewhat better than did normal court processing. The explanation for CEP's minimal impact on avoiding criminal dispositions is obvious: New York State provides numerous other options for decision-makers that achieve the same result, and these other options were considered appropriate for the types of cases selected for CEP.

Recognizing that CEP's overall impact on disposition is limited, it might be added that the effect of diversion is similar for those with and without prior conviction records (Table IV-3) and for those with and without prior arrest records (Table IV-4). Although the proportions shown in Table IV-3 appear to suggest members of the experimental group who had prior convictions reap greater benefits from CEP than do experimentals without prior convictions (by virtue of the 45 percent reduction -- 20 percent versus 65 percent -- among those not convicted previously as compared to the 30 percent reduction -- 15 percent versus 45 percent -- among those convicted), this is probably a result of differences in sample size. For each sub-table in Table IV-3, Cramer's Ø was computed as a measure of the strength

of the association; Ø for those without prior convictions was .46 while for those with prior convictions it was .49. We are led to conclude, therefore, that the relationship between research assignment and case disposition is equally strong (or weak) for defendants with and without prior conviction records; based upon the Cramer's Ø in Table IV-4, the same may be said for those with prior arrest records.

Table IV-3

FINAL DISPOSITION OF INTAKE CASE,
BY PRIOR CONVICTION RECORD AND BY RESEARCH ASSIGNMENT

Prior Convict.	Res. Assign.	Dism.	ACD[b]	CONVICTED Viol.	CONVICTED Misd/YO[c]	CONVICTED Fel.	War-rant	TOTAL (N)
NONE[a]	Exp'l.	58%	15	5	9	1	11	100%(332)
	Control	17%	31	23	21	1	6	100%(207)
CONV.	Exp'l.	60%	9	10	10	-	10	100%(67)
	Control	24%	12	18	47	-	-	100%(34)

[a] Includes defendants whose official arrest records contain no dispositions as well as those without arrests or with favorable dispositions.

[b] Adjournment in Contemplation of Dismissal.

[c] Includes Youthful Offender adjudications as well as misdemeanor convictions.
Cramer's Ø (rows 1 and 2) = .46
Cramer's Ø (rows 3 and 4) = .49

Table IV-4

FINAL DISPOSITION OF INTAKE CASE,
BY PRIOR ARREST RECORD AND BY RESEARCH ASSIGNMENT

Prior Arrests	Res. Assign.	Dism.	ACD[a]	CONVICTED Viol.	CONVICTED Misd/YO[b]	CONVICTED Fel.	War-rant	TOTAL (N)
NO	Exp'l.	57%	19	4	8	1	10	100%(239)
	Control	15%	36	22	20	1	6	100%(163)
YES	Exp'l.	59%	7	9	11	1	13	100%(164)
	Control	23%	13	24	34	1	4	100%(82)

[a] Adjournment in Contemplation of Dismissal.

[b] Includes Youthful Offender adjudications as well as misdemeanor convictions.
Cramer's Ø (rows 1 and 2) = .48
Cramer's Ø (rows 3 and 4) = .44

Despite the number of issues discussed, we have not fully exhausted the implications of CEP diversion as contrasted to alternative non-criminal dispositions. One of CEP's central goals, like those of other PTD/I programs, was to protect defendants from the *stigma* associated with prosecution and conviction -- to help them maintain a "clean record." In order to assess CEP's relative success in this area, two additional elements of criminal justice system processing must be taken into consideration. First, whether or not the defendants diverted to CEP had *already* lost their "clean records" because of

prior arrests and second, whether the different types of dispositions discussed above are different in the protection they offer from the stigmatizing consequences of arrest and prosecution. While recognizing the difficulty of such comparisons, let us turn to a discussion of the stigmatizing consequences of criminal records both in the criminal justice system and in such areas as employment, the attempts of the system itself to limit these through sealing statutes and practices, and the role of CEP's diversion efforts in this complex area.

DIVERSION FROM STIGMA: MAINTAINING A "CLEAN RECORD"

As we have already shown, many defendants eligible for CEP diversion had a stake in obtaining a disposition that protected their previously "clean" records.[16] Sixty percent of the research population had no official prior arrest record, and 84 percent no official prior conviction record. However, as with providing options for non-criminal dispositions, New York State has also been concerned with

[16]Not all these defendants verbalized personal concern about "getting a record" or avoiding it. Defense attorneys interviewed noted that they often had to convince defendants it was in their interests to avoid an official record. Whereas in research interviews, 58 percent of interviewed experimentals wanted CEP diversion to help them obtain dismissal, this appears to represent more of a concern with avoiding immediate punishment than with avoiding an official record.

creating statutory protections against the stigmatizing consequences of arrests and youthful convictions for some defendants. While New York has incorporated in statute several mechanisms to reduce such stigma, there are many ambiguities in the laws and in their applications. As with pretrial diversion, therefore, measuring their actual consequences for defendants is extremely difficult. The two particular statutes most relevant to interpreting the impact of CEP are CPL 160.50, a general criminal record sealing statute effective in 1976, and CPL 720.10, the youthful offender statute mentioned above with its more limited sealing provisions. Section 295 (15) of the New York Executive Law combined with Article 23-A of the Corrections Law which restrict the discriminatory use of a criminal conviction record in hiring for public and private employment are also important. But they are of somewhat less immediate concern to the diversion-eligible defendants in our sample because few were convicted of crimes on this arrest. Each of these statutes deserves some discussion concerning its scope, limitations, and actual application before assessing their overall importance to a comparison of diverted and non-diverted defendants' ability to avoid an official record and its potential stigma.

Individuals not convicted in New York State by virtue of diversion or any other dispositional process are offered statutory protection under CPL 160.50. This section calls for the automatic sealing of all official records of the arrest and criminal proceedings and the return of all fingerprints and photographs to the defendant or attorney of record when criminal proceedings are terminated in the favor of the person, unless the individual has another criminal action pending or

the district attorney demonstrates to the satisfaction of the court that the interests of justice require otherwise.[17] There are several important additional aspects of this sealing statute relevant to our discussion.

[17]CPL Sec. 160.50 states: (1) "Upon the termination of a criminal action or proceeding against a person in favor of such person,...unless the district attorney upon motion...demonstrates to the satisfaction of the court that the interests of justice require otherwise, or the court on its own motion...determines that the interests of justice require otherwise...the court wherein such criminal action or proceeding was terminated shall enter an order, which shall immediately be served by the clerk of the court upon the commissioner of the Division of Criminal Justice Services and upon the heads of all police departments and other law enforcement agencies having copies thereof, directing that: (a) every photograph of such person...in regard to the action or proceeding terminated, except of dismissal pursuant to...170.56 or 210.46...shall forthwith be returned to such person, or to the attorney who represented him at the time of the termination of the action or proceeding...(b) Any police department or law enforcement agency, including the Division of Criminal Justice Services, which transmitted or otherwise forwarded to any agency... outside the State of New York copies of any such photographs...and fingerprints, including those related to actions and proceedings which were dismissed pursuant to Sec. 170.56 or 210.46...shall forthwith forward a request in writing that all such copies be returned to the police department or law enforcement agency which transmitted or forwarded them...and upon such return such department or agency shall return them...(c) All official records and papers...relating to the arrest or prosecution...shall be sealed and not made available to any person or public or private agency, and (d) Such records shall be made available to the person accused...and shall be made available to (i)...(ii) a law enforcement agency upon *ex parte* motion in any superior court, if such agency demonstrates...that justice requires such records be made available to it or (iii) any...agency with responsibility for the issurance of licenses to possess guns..."

Subd.2 or Sec. 160.50 goes on to ennumerate the instances where an action shall be considered "terminated in favor" of the defendant (see CPL Section 160.50(2) (1-j). Section 160.60 "Effect of Termination of...Actions in Favor of the Accused" next goes on to say: "Upon the termination of a criminal action or proceeding against a person in favor of such person...the arrest and prosecution shall be deemed a nullity and the accused shall be restricted, in contemplation of law, to the status he occupied before the arrest and prosecution. The arrest or prosecution shall not operate as a disqualification of any person so accused to pursue and engage in any lawful activity, occuption, profession or calling. Except where specifically required or permitted by statute or upon specific authorization of superior court, no such person shall be required to divulge information pertaining to the arrest or prosecution." (For non-discrimination provisions see Exec. L. Sec 296(14-15); Correct. L. Secs. 750-755).

First, an ACD is considered a "favorable termination" for purposes of sealing. Therefore, whatever statutory protections result from sealing obtain equally to individuals receiving acquittals, ACDs, and dismissals (whether or not they resulted from diversion), since all are non-convictions.[18] Second, 160.50 does not specifically include non-criminal violation convictions in the list of "favorable terminations" to be automatically sealed. Prosecutors typically contend this means that such records should *not* be sealed and that fingerprints should be retained in official arrest files. While this issue is currently before the Appellate Division,[19] it appears that since 1976, violations have *not* been routinely sealed under 160.50.

[18]If the official records are *actually* sealed, the record of the arrest and diversion to CEP would be unknown to future prosecutors should the individual be rearrested, since it is the fact of the dismissal not the method by which it was obtained that triggers the sealing.

[19]Trial court cases decided under the statute thus far go both ways. Cases holding violation convictions to be included base their conclusion on the statutory purpose to relieve from stigma those persons not convicted of "crime," the fact that case law under the former but now repealed expungement statute (Civ. Rights L. Sec. 79-e) construe the favorable termination concept to include violations convictions and convictions for presumptively included traffic infractions. Cases finding against sealing and the return of photographs and prints in these circumstances tend to base their decision on the various permissible uses of violation conviction information as bearing upon legislative intent, and upon the fact that 160.50 unlike the predecessor statute, sets forth a fairly comprehensive list of dispositions constituting "favorable"terminations. (Among the cases favoring sealing and return are *People* v.*Flores*, 90 Misc. 2d 190 (1977); *People* v. *Hyll*, 90 Misc. 2d 101 (1977); contra. *People* v. *Blackman*, 90 Misc. 2d 977 (1977); *People* v. *Casella*, 90 Misc. 2d 442 (1977); compare. *People* v. *Miller*, 90 Misc. 2d 399 (1977) (return but no sealing).

Violations convictions are also not automatically covered by CPL 720.10, *et seq.*, the special provisions for handling cases of some youthful offenders adjudicated guilty of criminal charges (*People* v. *Caruso* 92 Misc. 2d 559 (1977.) CPL 720.10 contains parallel sealing provisions to those of 160.50, but they are not as inclusive. (YO adjudications are also not "favorable terminations" within the meaning of 160.50, *People* v. *Dugan*, 91 Misc. 2nd 239 (1977).) Somewhat ironically, therefore, first offenders guilty of non-criminal violations are not as protected under law by having that official conviction record *sealed* as are first offenders found guilty of a criminal misdemeanor offense who automatically should have that record sealed under 720.10. However, while the official records of the arrest and YO adjudication are sealed from the public and are not legally permitted to bar the individual from public employment or licensure, they are open to New York criminal justice agencies under a variety of situations, including the individual's subsequent arrest.

Obviously, therefore, YO sealing provisions do not fully restore even a first offender to the status of having a "clean record." His/her prints and photographs are retained by the authorities for future identification and the circulation of the record is not restricted within the criminal justice system, at least not during prosecution for a new arrest. For the purposes of public or private employment, however, the individual is considered under law not to have been convicted, and in New York State no one is supposed to be restricted in employment for an arrest record alone. Furthermore, the YO record is not officially available to any agency, public or private, for the purposes of employment review.

Even though this is less protection than is offered under 160.50, the latter also does not really restore the arrested but "favorably terminated" individual "to the status he occupied before the arrest and prosecution" (160.60), even if he was a first offender. The flaws in 160.50 as a protection from stigma for those non-convicted are several, even if one assumes the sealing procedures to be rigorously adhered to in practice (which, of course, one cannot!).

First, by referring to "sealing" rather than "expunging," the statute permits the maintenance of records, and only restricts their circulation; however, it does not specify how long they must be maintained or what type of "sealing" should be used. Furthermore, the mere existence of such records creates the possibility that unauthorized access might occur.[20] Second, the law allows for exceptions to the sealing in particular cases (160.60(1)) upon the motion of the district attorney but give no guidance as to the conditions under which such exceptions are appropriate.[21] Third, as we shall see below, the FBI obtains copies of arrested felons' prints and New York State cannot require them to be returned; however, it is apparently the FBI policy to do so in at least some cases.[22] Fourth, the potential flaw in the

[20] People in the system are able to offer examples of this; how widespread such violations are is unknown.

[21] And people in the system do not seem to know what these exceptions tend to be or how frequently they are exercized.

[22] Again, people are not sure how frequently or under what conditions prints are returned or what happens to the full FBI record.

statute in terms of the protections from the stigma of arrest in non-penal matters, especially employment, comes from Section 160.60:: "The arrest and prosecution shall not operate as a disqualification of any person so accused to pursue and engage in any lawful activity, occupation, profession, or calling. Except where specifically required or permitted by statute or upon specific authorization of superior court, no such person shall be required to divulge information pertaining to the arrest or prosecution." The key word is "permitted;" the potential exceptions are enormous.

Finally, while the statute can authorize people to treat their own arrests as nullities, it cannot oblige others to do so, nor -- and this is more poignant -- can it immunize arrestees against accusations of lack of candor arising out of their innocent misinterpretation of the state's statute.[23] As indicated above, while it is illegal for employers in New York State to ask about prior *arrests*, there are no systematic means for monitoring or assuring compliance. It is well known that employers ask prospective employees many things they should not, if not on written applications, then in interviews, the content of which is much harder to document in a complaint of discrimination.

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[23] One judge interviewed reported a case of an individual whose New York City case had been dismissed and sealed; however, when the individual applied for United States citizenship, she claimed she had never been arrested and the application was denied on the grounds that she had "lied."

See also *Dover* v. *Poston*, 76 Misc. 2d 721 (1973); lying on application as to prior *conviction* constituted misrepresentation justifying disqualification.

The complexities of these statutes and their interpretations suggest they cannot offer full protection from the potential stigma of arrest with or without diversion. Clearly, it is best simply not to be arrested. However, if arrested for the first time, it appears that a favorable disposition in the form of a non-conviction covered by the sealing provisions of 160.50 is the most advantageous outcome possible whether it occurs via diversion or normal court processing. If "convicted," the desirability of various types of convictions is more ambiguous, but in general it would seem that a conviction for a non-criminal violation, even though the record is not sealed, offers more formal protection under statute than does a YO "adjudication" for a misdemeanor offense sealed according to 720.10. First, the YO is based upon a finding of guilt for a *criminal* offense, even if that is not a matter of public record and cannot be used to bar the person from most types of opportunities. While the violation is *conviction*, it is for a much more minor offense and, although it does not specifically come under the protections of Section 295 (15) of the Executive Law against discrimination on the basis of *criminal* convictions (discussed below), it is *not legally* such a conviction. Second, once adjudicated as a youthful offender, such treatment is no longer mandatory if the individual is rearrested; that is, he has "used up" his right to a YO and may have its protection again only with the discretion of the court. The first offender who receives a violation conviction still must be adjudicated a YO if subsequently convicted of a misdemeanor.

Finally, the least desirable outcome is a conviction for a crime, and particularly conviction for a felony. Such convictions leave individuals most vulnerable to heavier statutory (and non-statutory) penalties upon a subsequent arrest and to potential stigma in non-penal areas such as employment. It is interesting to note, however, that even here, New York State has created some formal protections to mitigate against nonpenal stigma. Section 296 (15) of the New York Executive Law (added L. 1976, c.931, sec. 6) provides: "It shall be an unlawful discriminatory practice for any person, agency,bureau, corporation or association,...to deny any license or employment to any individual by reason of his having been convicted of one or more criminal offenses, or by reason of the finding of the lack of 'good moral character' which is based upon his having been convicted of one or more criminal offenses, when such denial is in violation of the provisions of Article 23-A of the Corrections Law." Article 23-A (Correct. L. Secs. 750-755), says (Sec. 751) "The provisions of this article shall apply to any application by any person who has previously been convicted of one or more criminal offenses...to any public agency or private employer for a license or employment, except where a mandatory forfeiture, disability or bar to employment is imposed by law, and has not been removed by an executive pardon..." Section 752, however, goes on to identify a series of exceptions to which no

definitive scope has yet been accorded.[24] Presumably, employers or agencies granting licenses are given under Section 752 some discretion in deciding when "unreasonable risks" are posed in hiring those who have been convicted of crimes. Moreover, as with 160.50, the state cannot exercise control over what federal employers or federal licensing entities, or private employers outside the state do.

In light of this discussion, let us return to the data on the research population to assess CEP's impact on first offenders in terms of helping them maintain, if not fully "clean" records, then those which are as legally advantageous to them as possible. Table IV-5 (which is taken from Table IV-3 above) shows that half of those first offenders in the research population who are normally processed by the court (controls) had their cases "favorably terminated" and thus were subject to 160.50. This same condition applied to three-quarters of the diverted group, a statistically significant difference. Nonetheless, virtually all the remaining controls (and experimentals as well) avoided *criminal* convictions by having their cases disposed either as non-criminal violations or as YO adjudications. Half the controls who were convicted (22 percent of the total) were convicted of violations. Twenty percent of the controls compared to eight percent of the experimentals who were first offenders were mandatory YOs, thus sealing their records, but "using up" their mandatory youthful offender treatment.

[24]Sec. 752 "Unfair Discrimination against Persons Previously Convicted... Prohibited" states: "No application for any license or employment, to which the provisions of this article are applicable, shall be denied by reason of the applicant's having been previously convicted of one or more criminal offenses, or by reason of a finding of lack of 'good moral character' when such finding is based upon the fact that the applicant has previously been convicted...unless: (1) there is a direct relationship between one or more of the previous criminal offenses and the

Continued.../

Table IV-5

FINAL DISPOSITION OF INTAKE CASE FOR DEFENDANTS WITH NO PRIOR ARRESTS, BY RESEARCH ASSIGNMENT

Research Assignment	Dism.	ACD[a]	CONVICTED Viol.	YO (Misd)[b]	Fel.	War-rant	TOTAL (N)
Experimental	57%	19	4	8	1	10	100%(239)
Control	15%	36	22	20	1	6	100%(163)

[a]Adjournment in Contemplation of Dismissal.

[b]Mandatory Youthful Offender status for a Misd. adjudication.

χ^2_5=92.928; p<.001

Before we leave this rather lengthy discussion of the protections from potential stigma offered by various outcomes of any arrest, it is extremely important to reiterate that our discussion has been based upon the assumption that sealing really takes place -- that records are, even in this limited sense, "made clean." This does not appear to be an accurate assumption, although it is extremely difficult to know for certain how inaccurate it is. Criminal history records and fingerprints are kept by a multiplicity of autonomous and semi-autonomous

Continued...specific license or employment sought; (2) the issuance of the license or the granting of the employment would involve an unreasonable risk to property or to the safety or welfare of specific individuals or the general public..."

agencies at several levels of government with different types of recordkeeping and sharing systems; in addition, the systems themselves are in flux. Consequently, the practical outcomes of sealing add additional ambiguity to the discussion above. Not only, as we have said, do employers ask about arrests and convictions when they should not, and not only do they interpret such things in ways the law does not, but some records may well remain unsealed within New York City's and State's own manual and computerized files.

The central State repository for criminal history information, the New York State Identification System of the State's Department of Criminal Justice Services (DCJS), is currently attempting to assure that the recording of case dispositions (and therefore sealing) occurs as required by law. Pressure was brought to bear on DCJS by a Legal Aid Society suit (*Tatum* v. *Rogers*, Fl Sugg. - 75 Civ. 2782 (SONY 1979). DCJS has undertaken to retrospectively update records going back ten years within the next two to three years. While it is said that entry of dispositions and sealing are up-to-date with current cases, it is very difficult to know how complete actual sealing is. Prosecutors and Legal Aid attorneys are not fully satisfied that procedures meet the legal requirements. However, their perception of what is currently on RAP sheets is probably affected by the lack of dispositions overall. However, even when the sealing is actually carried out by the appropriate record-system, many types of errors are possible. For example, none of the systems seal unless there is an explicit court order to do so, and errors may well occur at this level. Errors also occur when, by

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mistake or by intent, someone in the system provides unauthorized access to sealed information. As noted above, without expungement, this can be controlled but not eliminated.

In conclusion, even with sealing provisions administratively realized, there are exceptions, loopholes, and ambiguities involved in any attempt to help arrested individuals regain officially "clean records." They really never do. However, as suggested in the discussion above, there are differences in law (though probably less in practice, particularly in the employment sphere) among those whose records have been tarnished in different ways, and CEP's diversion efforts did assist some first offenders who would not have otherwise become eligible for protections that are offered by 160.50, whatever they may actually be. Nonetheless, for most first offenders, diverted or not, the legal advantages of sealing, youthful offender treatment, and the reduction of charges to a non-criminal offense exist already not only in statute but in practice.

DIVERSION AS A PROSECUTORIAL ALTERNATIVE

As discussed in Chapter I, diversion must be considered akin to a preadjudication sentence and, therefore, partially justified (to use Crohn's terms cited earlier) as a "choice not to punish." As a "sentence" imposed preadjudication, the discussions of PTD/I standards suggest several conditions which should obtain for the imposition of supervised diversion by prosecutors to be appropriate. The justification for these have already been presented in Chapter I. First, the PTD/I option should not be used for cases prosecutors would

otherwise decline to prosecute (even if they could legally sustain a conviction); while there is disagreement concerning this, particularly from prosecutors, most PTD/I programs (and the NAPSA Standards, 1978: 29-30, 35) have taken this position. Second, the defendant's period of participation in the diversion program should not exceed the amount of time he or she would be under supervision if traditionally sentenced; while admittedly difficult to measure, the condition is fairly widely agreed upon.[25] Third, participation in diversion should be voluntary. Fourth, structural provisions for assuring due process must be made; that is, mechanisms must exist to assure decisions about admitting defendants to the program or terminating them as unsuccessful are not made in an arbitrary fashion. Fifth, a defendant's return to court without a favorable recommendation for case dismissal from the PTD/I program should not prejudice the final disposition of the case.

CEP as an agency has generally subscribed to these principles as operating parameters for providing services preadjudication. Although we recognize that many, if not all, these conditions are difficult to measure, the research has collected both quantitative and qualitative data which bear upon how well CEP has managed to fulfill them.

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[25]The NAPSA Standards and Goals (1978:54-56) suggest balancing the need for time to effect change and the need to assure the case can still be prosecuted, incorporating the criminal penalties if convicted in this assessment.

Diversion As An Alternative To Declination

We have already dealt with this issue at length in a preceding section of this chapter. As seen in Table IV-2, almost five out of ten defendants in the control group (46 percent) received a dismissal or ACD. Clearly, therefore, either the prosecutor could not secure a conviction in these cases or chose for reasons of time, resources or "justice" to decline to prosecute.[26] It may be concluded that CEP was not fully successful in assuring that diverted cases were those prosecutors would otherwise pursue to conviction. Insofar as CEP represented a behavioral requirement by the prosecutor that a defendant spend four months supervised by a program prior to a dismissal, diversion represented at least some extension of official control over the defendant that would not have been exercised in the absence of the program.[27]

In discussing CEP with Assistant District Attorneys (ADAs), those who had considerable experience diverting cases to the agency told researchers that "without CEP, probably most CEP-type defendants [would] plead guilty to misdemeanors or violations; maybe a few [would] get ACDd." Certainly the ADAs who made these assessments believed defendants they diverted would have been prosecuted to conviction.

[26]The mean number of court appearances (including arraignment) for control cases that were dismissed was 4.2, and it was 3.7 for those receiving an ACD.

[27]Data based upon interviews with defendants in the research population suggest that few of the control group were diverted to programs other than CEP when excluded from that program because of lack of room. Only 13 defendants reported being in some court or police-related rehabilitation program either prior to or after intake into the research.

Yet the data suggest their estimates are somewhat exaggerated; part of the reason was ADAs' own lack of full information about many cases they consider for diversion because the decision was made at the earliest stages of prosecution.[28] This lack of information to predict the likely outcome of a case is suggested (though not proved) by data collected by the research on cases eligible and screened for CEP but not approved and thus not included in the process of research selection into the experimental (diverted) and control groups.[29] Eighteen percent of a random sample of such eligible but unapproved cases were rejected by ADAs because of the nature of the case or the characteristics

[28]The NAPSA standards (1978:8, 27-36) emphasize the necessity for the diversion decision to be made only *after* the formal filing of charges because "it is only then that the investigatory stage of the criminal case has come to completion and the government has decided whether or not to prosecute the accused. Only by reserving the diversion decision until this point in the processing of a criminal case can the criminal justice system and society be confident that diversion will not be used as a device to retain in the system unprosecutable cases or meritorious cases which, because of their minor nature, otherwise would not be prosecuted" (p.8).

As discussed in subsequent sections, the assumption that this process has been satisfactorily completed in New York City at the time of arraignment is not necessarily correct in all cases, despite such processes as early case assessment by senior prosecutors.

[29]Three quarters of all the cases CEP screened during the first six months of the research selection period (see Table 1, Appendix B) were explicitly rejected by one of six major decision makers in the system, including the defendant him or herself. To examine the selection procedure in greater detail by looking at those rejected (since the experimental design looked only at those accepted and approved), researchers randomly selected 21 days during a six month period and examined in detail the 594 cases rejecting or rejected for CEP on those days. The source of the data was CEP records kept on all defendants screened explicitly for the purpose of identifying who rejected them and for what reason. Case outcomes on the sample cases were obtained from CJA computerized records. The data from this collateral data collection effort are presented in tabular form in Appendix B.

of the defendant.[30] Eighty-six percent of these defendants were denied CEP diversion because they either had too many or too serious priors, or the case was considered too serious. ADAs rejected only six out of 107 cases because they were viewed as too minor for diversion. While recognizing that ADAs' early judgments do not automatically correlate with convictability, it is interesting that in 38 percent of the cases where the priors or charges were considered "too serious," the defendant received a dismissal, ACD or discharge. Consequently, in cases where the disposition of the case was not related to diversion, prosecutors' early assessments were not particularly good predictors of the outcome.

An alternative explanation for the use of diversion in lieu of a declination were offered by other ADAs interviewed. Several recognized that (or would admit that) more than the occasional defendant was diverted by an ADA knowing that he/she would receive a dismissal or ACD anyway. One ADA offered the reason that "ADAs don't like the ACD because it does not tell either them or the defendant where the defendant will be over the next six months; CEP did." Another ADA reported that some cases which might have received an ACD were diverted because the ADA, the Defense Attorney or CEP thought the defendant needed CEP's services. When asked why the Defense Attorney would agree to this disposition, the ADA said that "the Legal Aid attorney can't know that this [ACD] is likely at this stage [in the processing of the case].

[30]An additional five percent were rejected by ADAs because they transferred the cases directly to the jurisdiction of another court or body (e.g., Family Court, mediation).

Only the DA can offer the ACD and if they don't, Legal Aid can't second guess the outcome. As long as there is CEP, the DA won't accept an ACD." As indicated in Chapter II, CEP had very little control in this decision-making situation because it had no official standing in the case and because it also had little information about the facts of the case. CEP screeners did not routinely discuss the details of the arrest with the defendant but concerned themselves primarily with the overall severity of the charges and the defendant's prior record to assure it was worth their time pursuing diversion. This situation always made the agency uneasy, a feeling expressed in their continual concern with "over-reach." Ultimately it was a factor in CEP's decision (discussed in Chapter III) to become less involved in initial selection of cases by moving to a referral system in which the defense counsel would be heavily responsible for identifying potential diversion cases.[31]

Diversion As An Alternative To Punishment

Despite continual references to PTD/I as "diversion from incarceration" (for example, throughout the 1978 NAPSA Standards and Goals), CEP had for some time been skeptical that it, or any short-term and particularly *pretrial* diversion program in New York City, could be an alternative to a jail or prison sentence. Table IV-6

[31]Interestingly, shortly after this change in procedure, data on incoming cases indicated that defendants were significantly younger and charged with less serious offenses than previously. CEP then again became more actively involved in scrutinzing the nature of the cases referred, attempting informally to assure referrals for CEP diversion were otherwise likely to be prosecuted and convicted.

confirms this assumption by comparing the final outcome and sentence imposed in the intake case for all members of the research population. While the difference in the distributions of sentences for experimentals and controls is statistically significant, the major conclusion to be drawn from the data on the control group is that, in lieu of diversion, the majority of defendants were sentenced to other formally available alternatives to harsh punishment. Fewer than two out of ten controls (15 percent) were sentenced to the harshest alternatives -- probation or a period of incarceration. One out of ten (11 percent) was sentenced to serve probation time (the average length being 1.5 years); of those sentenced to a period of incarceration (four percent), six (2.4 percent) were to serve a jail sentence and five (two percent) served only that period they had already spent in pretrial detention. The remaining defendants in the control group were discharged (23 percent) or paid a fine (11 percent), the median amount of which was $50.

Table IV-6

FINAL DISPOSITION AND SENTENCE OF INTAKE CASE BY RESEARCH ASSIGNMENT

Disposition/Sentence	Assignment Status: Experimental	Control
Dismissal/ACD	72%	46%
Discharge	7	23
Fine	2	11
Probation	3	11
Jail/Time Served[a]	4	4
Warrant Outstanding	12	5
Transfer to Family Court	.5	-
TOTAL (N)	100%(405)	100%(246)

[a] Includes jail sentences (2.4 percent) and sentences to time already served pretrial (two percent).

$X^2=155.468$; $p<.001$

Part of the explanation for the infrequent use of jail and probation as sentencing alternatives lies in the fact that, while charged originally as felonies, only one percent of the defendants in the control group were eventually convicted of felonies. As already shown in Table IV-2 above, the majority took pleas to non-criminal charges (violations such as disorderly conduct or trespass) or were adjudicated youthful offenders for misdemeanor findings of guilt (for example, petit larceny or criminal possession of stolen property).[32] This reflects the fact that, in reviewing cases for CEP, ADAs screen out cases likely to get probation or jail sentences. In the random sample of cases eligible but rejected for diversion, 32 percent of the cases rejected by ADAs were sentenced to probation or jail.

Clearly such reduction of initial felony charges provides a problem for judging whether or not four months of participation in CEP was a longer or harsher alternative when considering "the criminal penalties that *could be* imposed were the defendant to be found guilty at trial" (NAPSA, 1978:55, emphasis added). Given the criminal sanctions *as charged*, namely D and E felonies, the *maximum* that could be imposed under the state Penal Law would be one year in jail. Data from 1971 on the actual deterioration of charges from arrest to conviction, however, show that seven out of ten D and E felony arrests in New York City that eventually led to a conviction took misdemeanor pleas (maximum

[32] While the majority of these defendants were treated as Youthful Offenders, that status is somewhat less important with respect to sentence than it is to the issue of stigma and criminal conviction. Generally, YOs may be sentenced to the same sanction as anyone convicted of the same offense who is not treated as a YO; however, the sentence cannot exceed those for an E felony notwithstanding the level of conviction for which a YO finding was substituted.

sentence 90 days to one year) and another one or two out of ten plead to a violation or infraction (maximum sentence 15 days) (Vera, 1977: 13). As we have seen from Table IV-6, defendants charged with D and E felonies who were approved for diversion by the prosecutor (controls) were very likely to receive minimum rather than maximum sentences in lieu of diversion. Even though it would appear that, compared to other PTD/I programs across the country, CEP's four month period of participation was *short* (NAPSA, 1978:55, reports that most programs are between six and 12 months, and that 12 months is typical of felony programs), it was nonetheless *long* compared to the period and amount of supervision meted out to equivalent New York City defendants who were not diverted.

Assessing the relative harshness of a preadjudication "sentence" to CEP in contrast to specific other sentences is even more difficult. While the requirements of the program were not intensive, the defendant did have to appear at least once a week for four months and had to demonstrate some level of cooperation with the counselors and service personnel by appearing on time, calling when late, and making efforts to participate in CEP's service activities. If he/she did not, his/her case was returned to court.[33] Discharges (even when conditional) and suspended sentences clearly represented less restriction and supervision than did CEP. According to judges and prosecutors in New

[33]See below for a detailed discussion of the consequences of such "failure" with respect to the defendant's court cases; since they are not particularly serious, diversion to CEP cannot be regarded as a harsh alternative. However, it is also the case that "failure" to perform under *other* sentences (including conditional discharges, ACDs, fines,or probation) also does not produce serious consequences in New York City, limiting how "harsh" they can be regarded.

York City, conditions are not monitored by the court and violations are very rarely reported back and the case returned to the calendar. Fines were generally small and, although this was a poor population, it is rather common that the parent, not the defendant bears the payment burdens of such a sentence. Only probation and a period of incarceration intuitively seem more restrictive; indeed, one prosecutor commented that "ADAs don't like [probation] much either, because if they allow themselves to think about it, they realize that probation is giving these defendants no supervision at all; CEP did." Consequently, while the average length of probation supervision was longer than CEP, the extent of that supervision (defined only as what the defendant is asked to do and how often) was limited. Furthermore, prosecutors report that if an individual violated probation, this typically had no immediate consequence, even if the person was re-arrested on a new charge shortly thereafter. Consequently, while probation may or may not have been a more irritating sentence to defendants than CEP,it certainly does not seem to have placed as many behavioral conditions on them.

Finally, although the period of incarceration, either pretrial or post-conviction, was on the average shorter than the four month diversion period, the negative quality of that experience must be viewed as harsher than CEP. Incarceration, therefore, was the only sentencing alternative indisputably more behaviorally restrictive (and in this sense harsher) than CEP diversion, though it may also be argued that fines and probation were harsher in the sense of being more irritating.

Diversion As Voluntary

"This concept is so fundamental a consideration that it is included as a matter of definition" (NAPSA, 1978:38). It is also an extremely difficult concept to define and measure. Often the major criterion for judging the "voluntariness" of a defendant's decision is the presence of a defense attorney; certainly this is a major dimension of NAPSA's consideration of the concept. In diverting a defendant to CEP, defense counsel's consent was a formal requirement of the program and one adhered to on a routine basis. This was partly a function of the timing of the formal diversion -- generally at arraignment after filing of the charges. Nevertheless, the issue of whether the defendant's consent was voluntary is more complex and subtle, a fact recognized in NAPSA's broader interpretation of "voluntary" to mean "informed choice" (1978:38). Defining what this is and how it is assured, however, is a difficult matter. The essential issue is not just the presence of counsel, but the amount and type of information in the possession of both counsel and defendant and the timing of their decision.

As described in Chapter II, defendants were first contacted by CEP screeners in the detention pens before arraignment. The program was described to them -- its service offering, requirements, and the likelihood of a dismissal of the charges at successful completion. Under these conditions, many defendants did *not* want to participate. From the random sample of formally eligible but not diverted defendants mentioned earlier, it was determined that a third of the defendants contacted by CEP themselves refused diversion (generally prior to even talking with their attorney). Either they did not want

CEP's services or they wanted to pursue their case to disposition without diversion.[34] This is virtually *de facto* evidence that, for these defendants, there was no sense of "compulsion" to participate. The decision *not* to select diversion, therefore, appeared to be voluntary.

It is not clear, however, that the decision *to* participate was as voluntary, though our data on this issue are suggestive rather than definitive. Defendants who rejected CEP themselves were considerably older and had more prior arrests and convictions than were those diverted to CEP. Thus, those who agreed to diversion had less prior experience with the criminal justice system to use as a basis for making informed judgments about the potential outcome of their cases. This is supported by data obtained in research interviews with defendants in the sample. Four out of ten respondents (42 percent) said that, while in detention prior to arraignment, they had believed they would receive jail sentences when their cases were disposed; another one out of ten expected probation. In contrast, one out of four reported expecting a dismissal or ACD; only one out of ten had no expectations as to their potential case outcome.

At least some defendants interviewed by the research felt these misconceptions stemmed from what CEP screeners had told them while they were in the pens:

> They tell you if you don't come in they're going to give you two, three years. That's the way they

[34]The decision also seems to have been reasonable: 42 percent received a dismissal or ACD and 59 percent a dismissal, ACD, or discharge.

> talk to you over there. They scare the shit out of you...Then when you get out you find out that they're all full of shit, that they were just scaring you. Everything they said was not true.

While this may be true (and we have no independent confirmation), defendants' own lack of experience with the system noted above was probably equally or more important. This is suggested by data on charge reductions in the Bronx Criminal Court reported by Bernstein, *et al*, (*Charge Reduction*, 1977). They found that defendants with no prior arrests were less likely to have their charges reduced than were defendants with prior arrests, who were less likely than those with prior convictions. The explanation of this surprising pattern offered by the authors is the impact of defendant's differing amounts of experience with the process on their ability to negotiate a disposition.

One conclusion which may be drawn from these materials (which supports the NAPSA standards, 1978:36) is that CEP probably should not have discussed diversion with the defendant prior to arraignment and the defendant's release from custody. The pressure on young and inexperienced defendants while in detention may have been substantial. Nevertheless, while the pressure on defendants may be reduced by discussing diversion after their release, it still does not answer the problem of their lack of knowledge about the potential outcome of their cases. However, our data also suggest that simply providing defendants with counsel does not solve this problem of *informed* consent.

As already discussed (Chapter II), most of CEP's potential clients were represented by Legal Aid attorneys who were often overworked and pressed to make a rapid decision about diversion under far from ideal conditions. First, they were under heavy pressure from the court to dispose of cases rapidly, at arraignment if possible. Second, the defendant may or may not have any trust in the Legal Aid attorney; not only did they have virtually no time to talk with one another before arraignment, the defendant may not even see this "public" attorney as being "his" lawyer as contrasted with an arm of the court which appointed him (Silberman, 1978). Third, the defense attorney had to make his judgment about diversion without benefit of any prior legal procedures which disclosed the facts of the case; not only did he have only a brief moment of discussion with his client before arraignment, but he was always denied a preliminary hearing by the prosecutor if the case was to be diverted.[35] Despite such conditions, Legal Aid attorneys did not often turn down an opportunity to divert to CEP. Only 12 percent of the cases eligible but not diverted in our random sample were rejected by the defense attorney, most often because they thought they could get a better deal for the client. In those relatively few cases, the choice appears to have been reasonable since 55 percent were dismissed or ACDd without CEP. This suggests, however, that many Legal Aid attorneys may have been inclined to divert cases to CEP because it was a relatively easy way to maximize the

[35] "It is unfair to ask a defendant to waive his right to a preliminary hearing and indictment in a felony case..." (NAPSA,1978:35). Nonetheless, despite arguments by Legal Aid lawyers and CEP, this was a firm policy of the prosecutor's office in recent years. Having both diversion and a preliminary hearing was considered to be "having your cake and eating it too," according to one ADA interviewed.

possibility the defendant would receive a full dismissal of the charges. (Note that the disposition data support their *overall* judgment!) At the same time it helped "dispose" of cases rapidly and provided defendants with services and assistance that they might need.[36]

In summary, therefore, an inspection of the decision-making process suggests that while defendants' decisions to participate in CEP were formally voluntary, there is considerable evidence that they were not fully informed. The agenda of Legal Aid attorneys, while reflecting deep concern for defendants' well-being, sometimes appeared different from that of their clients. Partly because attorneys were not in full possession of the facts of the case and partly because

[36]This social service concern of some Legal Aid attorneys, particularly those who deal heavily with youths (16-19), appears prevalent and often stems from a very personal concern for these defendants as disadvantaged individuals. "I think the program is good. Psychologically, it's also good for there to be someone to respond to the kid and to help him, give him support and to be responsive. It's important that it's someone out of his own neighborhood. Legal Aids feel the clients are helped by CEP." The same attorney, however, without being fully aware of it, also identified a problem with this perspective which stems from the fact that many defendants do not want such services. The defense attorney and the "social worker" orientation may sometimes be in conflict, particularly if the defense attorney wants the defendant to get services (and thinks this will help him avoid coming back to the courts), but if he also feels a favorable disposition might be possible without diversion. "If there were not some reward like dismissal of the charges," said the same Legal Aid quoted above, "the defendant would be less likely to want to go into a diversion program. Most defendants are not looking for 'help;' it's a way out of trouble. The Legal Aid waives someone back on the street, he really doesn't know what has happened. These programs help them understand the court process, what has happened, and that the consequences could be serious."

they had the interests, concerns and pressures discussed above, Legal Aid lawyers reported (as did ADAs) that the great majority of cases they diverted would have been fully prosecuted. They reported that without CEP in the courts, "not many [would] be ACDd who would previously have been diverted." This widespread assumption enabled attorneys to legitimate the diversion decision as a good defense strategy, and resulted as much from a concern with defendants' needs for help as from a concern with finding the most rapid route to a favorable disposition. While not totally incorrect (49 percent of the controls were "convicted" as described earlier, mostly for violations or under YO provisions), there is a substantial degree of error in the assumption.[37]

Diversion and Due Process

As discussed in Chapter I, PTD/I has drawn considerable critical comment because, as a pretrial "sentence," it lacks the protections characteristic of the adjudication process. Even recognizing that few non-diverted defendants have their charges adjudicated by trial does not lessen concern with due process in administrative decisions such as diversion; indeed it may heighten such concerns (Nejelski, 1976). PTD/I

[37]Note that this is not a unique finding. A recent INSLAW study on plea bargaining in Washington, D.C. found that most of the guilty pleas were to the most serious charge and that this was about the same for defendants convicted at trial. "Despite the findings of the study, Mr. Hickey (director of the Public Defender Service of Washington) said his experience shows there is a 'general expectation that the defendant who plea bargains is getting a break for both expressing his culpability and for saving the court system's time and money." The study director replied, "Maybe the defendant and the attorneys don't really understand what is going on. Maybe they think they are getting a break where statistics prove they are not." ("Plea Bargaining," 1978.)

was seen originally as a formalization of previous types of informal diversion; yet most programs have operated on an extremely informal basis, both legally and procedurally. Over the last few years particularly, diversion advocates have expressed concern about tightening these procedures to assure due process, especially in decisions about entry to and exit from diversion programs (see NAPSA, 1978). In contrast, many diversion programs and prosecutors in their jurisdictions are concerned about maintaining "flexibility" at these points. Nonetheless, the issue of arbitrariness remains.

In the case of CEP, few formal procedures existed in the process of deciding which defendants to divert. There is no "right" to diversion in New York State; the decision to delay prosecution for four months while the person attended CEP was entirely an informal procedure. While there have been always formal, stated criteria for admission to the program (developed by CEP with the participation of prosecutors), these have defined only broad categories for *exclusion* (e.g., at the time of the research, no misdemeanors, no A or B felonies, and no one with an outstanding felony case). Within the remaining categories of cases, informal criteria of eligibility have always developed; they have varied from prosecutor to prosecutor and even from CEP screener to screener. Attempts to establish more uniform and detailed criteria of suitability, acceptability, or eligibility were made difficult by several factors. The first is the size of the jurisdictions in which the program has operated and the very large number of prosecutors making decisions about diversion-eligible cases. Very little control over these decisions has been possible even when

supervising ADAs have attempted it. For example, when "liaison ADA's" were appointed in a given borough (through whom all CEP eligible cases were supposed to be screened), the resulting control typically lasted only a few weeks (or months at best) after which the ADA would change and a new one would be appointed; or the liaison would be often unavailable or overworked so the number of ADAs making decisions would begin to increase informally. Consequently, it was structurally difficult to impose uniformity(to say nothing of review or appeal) even when it was attempted.

In addition, however, such uniformity was not always considered desirable. CEP itself wanted to maintain a certain amount of "flexibility" in order to allow it to negotiate case-by-case for "more serious" cases to divert. CEP was cautious about more formal criteria, seeing them as possibly restricting diversion to cases the ADAs would otherwise ACD or dismiss. Likewise, prosecutors also wanted flexibility so they could use diversion as they saw fit. Furthermore, because of CEP's social service orientation, it always wanted the ability to divert a case because of the defendant's particular service needs.

Consequently, informal negotiations have been the rule in deciding whom to divert. While informal norms developed, these were always subject to change. For example, prosecutors periodically resist the informal norms concerning which defendants have a "right" to an ACD. As one ADA put it,"Legal Aids think they always have a right to an ACD for a first offender but since 160.50 [the sealing statute] we don't always

want to give the defendant a free ride." Similarly, the informal groundrules for diversion also changed as structural conditions in the court changed (for example, the amount of backlog in arraignment), or as personnel changed (especially ADAs), or as organizational policies at CEP or the District Attorney's office changed (e.g., CEP making a felony-only policy or the DA beginning to emphasize prosecuting youths).

While such flexibility enabled "the system" to use CEP diversion in ways that reflected complex changes at all levels, prosecutors were always the major decision-makers. Therefore, they not only set the tone but held an unchallengeable position in decision-making about individual cases. While CEP would occasionally have to maintain good court relations by overriding a screener's decision not to divert a particular person if a prosecutor or judge wanted diversion, the opposite was not true. Prosecutors might be persuaded by CEP to divert some cases they had initially rejected, but there were no practical consequences if they did not do so.[38]

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[38]Judges do not appear to have been very active in the diversion process, at least not directly. CEP court staff reported making few attempts to get judges to override negative decisions by an ADA. Our random sample of formally eligible cases not approved for diversion showed that only five percent of the sample cases were rejected by a judge after diversion had been agreed to by all other parties. Interestingly, in half of this very small number of cases the judge felt the cases were too insignificant for diversion and should be dismissed or ACDd. Therefore, while the judges did act to check the diversion of cases unlikely to be prosecuted, they did not do so often. It was somewhat more typical for a judge to suggest diversion in a case not initially screened by CEP. While in a few of these situations, the program did not want to divert the defendant (e.g., the few misdemeanor cases in the research population) or the district attorney was not anxious for this to happen, much more frequently, according to CEP, the program had simply overlooked the defendant, not had time to screen the case, or misjudged the likelihood of a successful negotiation.

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Formal mechanisms to reduce arbitrariness and assure consistency in decision-making were also lacking at exit from CEP; however, this was somewhat less of an issue for the agency than were intake decisions. While CEP's criteria for a determining who was "successful" (and thus recommended for a dismissal) were as general and informal as their intake criteria, the agency had greater control over their application and received fewer prosecutorial challenges. Prosecutors and the court wanted reports from CEP that demonstrated defendants' increased vocational activity (school, employment, training, and related activities). However, detailed expectations for particular clients were not established in any formal way.[39] While this left every exit decision open to question, the agency handled problems that arose to its satisfaction. Every client for whom CEP recommended a dismissal received one (or, in a few cases (six percent), an ACD). Prosecutors occasionally wanted the program to work with a client beyond the

Continued...The reasons for this lack of judicial activity are several. Certainly the desire to quickly dispose of cases was important during the time of the research and, as we have seen, CEP cases did stay on the court calendars longer than regularly processed cases. But there are other factors as well. Prosecutor's are the only actors with substantial information about the cases at the very early stages of adjudication. The judge, therefore, probably relies on the prosecutor more at this stage than any other in deciding how to proceed with the case. In addition, judges differ considerably in their philosophy(and behavior) concerning how active a role to take in influencing the disposition of a case prior to sentencing.

[39]It may be that the agency avoided the "contract" system of defining a client's responsibilities because it was difficult to assess quickly which of the *many* things these very difficult clients needed were feasible in a short-range intervention. Under such circumstances CEP may not have wanted to lock the client or themselves into a requirement that the person *actually* "have a job," "be in school full-time," or "be in a training program" as a rigid prerequisite for dismissal of the charges.

initial four months, but this happened rarely and was always handled by informal negotiations between the client, defense attorney, CEP and the ADA.

The explanation for this limited controversy over exit status lies in an understanding of whom the program typically recommended for dismissal and whom they terminated. Those given favorable recommendations were clients who *attended* the program with regularity.[40] Since those clients may have needed few or direct vocational services from CEP (e.g., attendance at CEP was highly correlated with having a job at program intake and having fewer prior arrests), CEP was generally able to document this cooperation and describe their vocational activity to the court. Those unfavorably terminated (i.e., CEP made no recommendation to the court) were clients who had ceased program attendance and could not be successfully contacted by CEP staff. CEP did not like to terminate clients and generally made considerable effort to personally talk with them about returning. These contacts included multiple

[40]A regression analysis predicting exit status from CEP (successful/dismissal recommended vs unsuccessful/no recommendation) was carried out and is reported in Appendix C. The most important single predictor of success in the program was attendance. Secondarily, the number of months the individual was employed in the six months prior to intake and the person's salary during that period were also important predictors. Finally, if the client's counselor had specifically identified the client as having "court-related" needs (that is, being concerned about having his case dismissed), and if the client had few prior arrests, successful participation was more likely. (The multiple R was .56 and the R^2 was .32; for further detail, see Appendix C.)

These predictive factors are not surprising. Not only has research on other diversion programs linked positive pre-diversion characteristics (such as employment) with success in such programs (Mullen, 1974; Pryor, 1978), but in the context of CEP, these factors form a consistent pattern. Clients who were already the most stable and had the most to protect (jobs and income and little in the way of a prior

Continued.../

telephone calls, telegrams, and often home visits. Typically, however, once clients became "ghosts" (to use program parlance), they could not be contacted.[41]

Eighty-four percent of CEP's unsuccessful participants were terminated because they failed to keep appointments, according to the Agency's client records. Either they had not attended CEP at all, or they had dropped out after limited attendance. Consequently, the issue of formal termination hearings was academic. While the defense attorney was notified by mail of the impending termination (using a standard form letter), CEP's records show they rarely replied on their client's behalf; this is understandable given the limited relationship typical of Legal Aid attorneys and their clients in relatively less serious cases. When they did reply and if they were helpful in renewing clients' program attendance, CEP was almost always willing to continue working with the client. But this was rare.

Continued...record), were those most likely to want to maximize the likelihood of receiving a dismissal and to articulate that concern to their counselors. Knowing that the program looked heavily to attendance as a measure of cooperation, they attended, and, therefore, they successfully completed the four months.

[41]For a discussion of the research staff's parallel problems locating, personally contacting, and encouraging defendants to "cooperate" in something (a research interview) when, for whatever reason, they were little inclined to do so, see Appendix A.

Program Failure As Prejudicial

Central to the concern with PTD/I as a preadjudication sentence is whether diverted defendants were put in additional jeopardy if they decided not to continue their program participation or if the program terminated them as "unsuccessful." Since CEP rarely made negative decisions about clients who continued to attend, "failure" in the program was typically a result of a defendant's own decision to stop coming. While "due process" issues seem less compelling under such circumstance, the potentially negative consequence of this decision to withdraw on the outcome of the pending case remains of concern. This concern may be viewed from two perspectives. First, does termination without favorable recommendation prejudice the defendant's case? According to general PTD/I standards, it should not for diversion to be an appropriate preadjudication alternative. Second, however, is whether diversion makes it additionally difficult for prosecutors to continue prosecution if defendants return to court.

As we have already noted, successful CEP participants in the experimental group always received a dismissal from the court (94 percent dismissed outright and six percent ACD). Unsuccessful participants, however, received a wider range of dispositions. To address the issues raised above, however, we have compared the dispositions of unsuccessful (terminated) participants in the experimental group with those of the central group (Table IV-7). While successful participants had a 100 percent chance of a favorable disposition, unsuccessful ones had less than a 50-50 chance (the proportion dismissed or ACDd is 41 percent). Three

out of ten (31 percent) unsuccessful participants were convicted or adjudicated a YO, and almost three out of ten (29 percent) absconded, that is, failed to appear for a court hearing and a warrant for their arrest remained outstanding.

Table IV-7

INTAKE CASE DISPOSITION FOR CONTROLS AND CEP TERMINATIONS

INTAKE CASE DISPOSITION	Controls	CEP Terminations
Dismissed	15%	31%
ACD	30	10
Convicted-Violation	23	9
Convicted-Misd/YO	26	19
Convicted-Felony	1	3
Warrant	5	29
TOTAL	100%	100%

$\chi^2_s = 78.090$; $p<.001$

Assessing these outcomes from the perspective of the defendant, Table IV-7 suggests that terminated defendants were *not* more harshly treated by the court than if they had not been diverted. Comparing unsuccessful experimentals with controls, 41 percent of the CEP terminations had their cases dismissed or ACDd compared to 45 percent of the controls and fewer of them were convicted/YO (three out of ten compared to five out of ten). However, significantly more of the un-

successful defendants absconded and therefore could not be prosecuted: 29 percent compared to five percent of the controls. Nevertheless, if hypothetically only the same proportion of unsuccessful participants had absconded as did controls (five percent) and if the remaining 24 percent had been convicted, the proportions of the two groups convicted would not have been significantly different.[42]

The data are harder to interpret from the perspective of the prosecutor. As already noted, since three out of ten unsuccessful participants failed to appear, prosecutions could not be completed in more of the experimental than control cases. While four out of ten terminated cases were either dismissed or ACDd by the prosecutor (three out of four dismissed outright), we do not know whether these dismissals resulted from: (1) the prosecutor's inability to convict because the cases had been weakened over time (e.g., lost complainants or witnesses); (2) the prosecutor's lack of success obtaining a conviction because of technical weaknesses in the original cases; or (3) the prosecutor's unwillingness to prosecute because of the relative insignificance of the offenses. While some prosecutors contend the

[42]Of course, continuing our hypothetical situation, unsuccessful participants who absconded could be convicted of a more serious charge than were controls, for example,if they were arrested on a new charge and prosecuted on the earlier (intake) case. However, as of December 1978, (in most cases more than a year after their last scheduled court appearance), their warrants were still outstanding, even for some who had been rearrested. Prosecutors indicated this was not uncommon since record-checking systems do not always identify the outstanding warrant. Even if the warrant was identified, the defendant would probably have both cases disposed together, or the first case would be dismissed. Although an outstanding criminal court warrant may be "stigmatizing" if the individual comes through the system again, that is apparently not necessarily the case.

first was most likely, the fact that almost five out of ten control cases were also dismissed or ACDd lends weight to the interpretations suggested in either (2) or (3) above. That is, the prosecutors' handling of these unsuccessful diversion cases was probably less affected by defendants' program failure than by the characteristics of the original cases(which, of course, prosecutors themselves had helped select for diversion). The control group shows that individuals typically eligible and approved for CEP diversion had slightly less than a 50-50 chance of an ACD or dismissal when normally processed by the court. While that probability doubled to 100 percent for those who successfully completed the program, it declined only very slightly (to 40 - 60) for those who were not successful.

Diversion As Resource Conservation

One reason given for the desirability of PTD/I programs as a prosecutorial option is that they "conserve energies" within the criminal justice system for more serious cases (NAPSA, 1978). According to our estimates (see Table I, Appendix B), CEP diverted less than two percent of all the defendants in the Manhattan and Brooklyn Criminal Courts who were potentially eligible according to formal criteria. This is considerably less than two percent of all those cases arraigned in these courts.[43] This suggests that the

[43]In the Zimring evaluation of CEP, it was estimated that CEP diverted between 1.2 and 2.0 percent of all arraigned defendants in Manhattan during 1971, a time when CEP had more funds and a larger caseload than during the period covered by our figures, Spring 1977.

program's client volume in relationship to the size of the court caseload was too small for it to be expected to have much, if any, system impact on conserving the time of prosecutors, Legal Aid attorneys, or judges for other cases on the calendar. Interviews with various people in the system confirm this conclusion. Most agreed that screening some cases for diversion took longer to handle because of disagreements over whether to divert but that in other diversion cases dispositions were speeded up because no hearings and only one additional adjournment was necessary.

On the average, therefore, some believed diversion delayed the overall process *very slightly* and others believed it speeded it up also very *slightly*. Both perspectives may in fact have been correct. As shown earlier, control cases remained on court calendars for a shorter period of time than did diverted cases, but they required slightly more court appearances. However, diversion to CEP only reduced by approximately 1/2 the number of appearances scheduled in the Manhattan and Brooklyn Criminal Courts. Since these courts scheduled more than 200,000 appearances a year, CEP does not appear to conserve many resources by reducing this type of penetration into the system.[44] As far as conserving other system resources, such as Probation Officers' time, jail or detention space for use with other, more serious cases, the data on sentences suggest that 32 more people

[44]Unfortunately, we do not have data that permit an estimate of what, if any costs are saved by CEP diversion through reducing the length of court appearances or the number and type of personnel who must appear.

would have served probation or jail time if the pattern of sentences found for the control group had been applied to the 397 defendants in the experimental group. Most of these 32 would have been on probation, hardly a large addition to the caseload of a system with over 25,000 cases under supervision.

Finally, as one prosecutor put it, recognizing that relatively few cases are diverted, "CEP is a qualitative phenomenon, not quantitative," meaning that it was important in providing a more appropriate (and perhaps, to use Judge Brownstein's terms quoted in Chapter I), a more "just" outcome for certain (but not necessarily many) cases. This is of course an almost impossible potential system impact to measure empirically because it implies value judgments and assumes some consensus about those values. However, the data on case outcomes presented in this chapter, both with respect to conviction and sentence, raise questions about the misuse of CEP/diversion at least as this may be assessed from the perspective of those who do not want preadjudication options to create *any* extension of state control or supervision no matter how limited or how benevolent.

CHAPTER V

PRETRIAL DIVERSION AS INTERVENTION: CEP'S IMPACT ON LIFE STABILITY

INTRODUCTION

As suggested in Chapter I, PTD/I programs are not designed simply as diversion *from* normal court processing -- prosecution and sentencing -- but also as diversion *to some* type of social service or counseling program. The rationale behind diversion to a program is intervention -- intervention in the individual's current life activities for the purpose of helping to stabilize vocational, family, health or other difficult circumstances. Such intervention is also seen as having consequences for the individual's future criminal activity. While Chapter VI will focus on CEP's impact on recidivism, this chapter examines the consequences of participation in CEP for defendants' lifestyles, particularly their vocational activity.

As a social service agency, CEP has continued over the years to provide primarily employment and vocationally-relevant assistance to clients. It has maintained this focus rather than shift to a counseling-orientation which seems more prevalent among recent PTD/I programs. It has based this position on a belief that providing direct and concrete services, combined with appropriate but limited counseling, is the most effective method of intervening in the lives of a population of extremely youthful inner-city minorities. The data that follows suggest CEP has not been very successful at encouraging measurable changes in the lives of these defendants. There are both

organizational and social structural reasons for this lack of impact that are not likely to be overcome simply by an increased emphasis on personal "counseling."

In this chapter two sets of variables are discussed. First, variables directly related to CEP's service orientation were analyzed to test hypotheses about program effects on life stability in the areas of employment, education, and other forms of vocational activity. Second, lifestyle variables (e.g., use of services, living arrangements, type of residence) were examined because they might be affected by CEP's referral activities. To test hypotheses about both types of variables, the research used a longitudinal follow-up of respondents for one year after intake. Continuous activity data were collected through a series of three interviews. To maximize the accuracy of the data, verification of self-reported activities were conducted whenever possible.

In each of the three interviews, data were collected on respondents' activities. The intake interview covered the period beginning with the defendant's intake into the research and going back 12 months prior to intake; the first follow-up interview covered the period beginning with the intake data and ending with the date of administration of the first follow-up interview; finally, the second follow-up went from the date of the first to the date of the second follow-up interview. The data were collected in such a way that continuous activity information was recorded from one year prior to intake through the date of the second interview. This was done using a "timeline" -- the respondent was told, "We want to draw a line through

every month you worked or went to school, or did the other things listed on the left side of the page. Let's start with (school/work)." The interviewer began with the month and week of the interview and worked back in two-week intervals to the previous interview date. This method was used to record school, employment, job training, job search, military service, time spent engaged in making money illegally, keeping house or childcare, and time spent in jail, ill, or disabled. For each of these activities, the start and end dates, as well as hours worked and salary earned, were recorded. This method was especially useful because it enabled the research to create equal length time periods for all respondents, regardless of the number of months between interviews. For example, while some people received their first follow-up interview eight, nine, ten months or more after intake, the timeline format enabled the research to create variables that included only the first six months after intake.

For an analysis of respondents' activities, three six-month periods were calculated: (1) the "intake period" extended from six months before intake into the research to the date of intake; (2) the "first follow-up period" covered the time from intake into the research to six months later; and (3) the period from six months to twelve months after intake was covered by the "second follow-up period." The length of participation in CEP was four months; our use of six month intervals reflected factors intrinsic to the population. First, the maximum length of time on which we could collect data on all respondents was twenty-four months, twelve months prior to intake and twelve months subsequent to intake. Second, as will

be seen in the results below, the pattern of activity for this population is quite erratic; therefore, comparisons of activity over six month periods (e.g., six months prior and six months subsequent to intake) are more reliable than those collected at single points in time (e.g., at intake to and exit from the program). Finally, to do the most appropriate type of analysis, we wanted three periods of equal length. Therefore, the first follow-up period may be considered as representing "in program" effects for experimentals, although it actually covers the time spent in the program and shortly thereafter.

Two types of variables were constructed from the timeline data. The first type measured some characteristic of an activity during a given six month period; one such variable was the average monthly salary during the six months prior to intake. The second type of measurement was made at a *point* in time, e.g., whether the respondent was employed on the date that was six months after intake. In the discussion of results, the type of variable being used is indicated, as are the strengths and weaknesses of that type of variable.

Knowing which members of the research population are included in the analysis is as important as knowing what behavior is being analyzed. This requires consideration of two features: (1) the composition of the experimental and control groups, and (2) the determination of which cases should be included, that is, coverage of what length of time constitutes "complete" data. First, as discussed in Chapter III, the experimental group includes all cases *assigned* at intake to the experimental group and the control group contains all

cases assigned at intake to the control group, regardless of whether they actually were diverted. The second process is somewhat more complex; as was discussed above, first follow-up (or six-month) interviews were often not conducted exactly six months after intake and second follow-up (or twelve-month) interviews often were not conducted exactly one year after intake. In most cases the first follow-up interview contained information covering eight, ten, or over twelve months; therefore, in many cases it was necessary to extract "12-month data" from the first follow-up interview. The structure of the timeline enabled us to collect the information from whichever timeline contained it. To avoid confounding missing data with lack of activity, however, data that spanned the period from six months after intake to twelve months after intake were included for a case only if the information on the case covered at least 11.5 months after intake. Thus, those cases included in the analyses have data for three complete periods of six months each: the six months prior to intake, the first six months subsequent to intake, and the second six months after intake.

When studying a population for whom the possibility of incarceration is real, it is necessary to consider the potential impact of time spent in jail on availability for participation in other activities. That is, if large numbers of respondents in either the experimental or the control group spent substantial time in jail, their availability for other activities would be seriously limited and might therefore produce artificial differences between groups. Using official record data (Police Department criminal records and CJA data), the

number of months spent in jail on any case during each six-month period was computed for each defendant. A total of 18 defendants (12 members of the experimental group and 6 members of the control group) spent time (ranging from 0.2 to four months) in jail during the six months prior to intake into the research. Similarly, twelve experimentals and four controls spent time (ranging from 0.1 to 6.0 months) in jail during the first six months after intake, and seven defendants (four experimentals and three controls were in jail (for one to six months) during the second six months after intake. Because the number of members of the research population who spent any time in jail is so small, it is highly unlikely that incarceration could have an effect on the analyses of other activities.

EFFECTS ON EMPLOYMENT, EDUCATION AND LEVEL OF VOCATIONAL ACTIVITY

The concept of "life-style" is a broad one, referring here to employment behavior, school enrollment and attendance, and being generally vocationally active as contrasted with "hanging out." In the next section, we will discuss other dimensions including use of services, living conditions, and social activities. The amount of information and method of analysis varies among these facets of "life-style," and results obtained for each are discussed in separate sections below.

Employment

Employment is a crucial variable in the evaluation of CEP because it is consonant with CEP's own goal to place clients "in a job or appropriate educational/vocational setting..."(Project Brochure, December, 1977). Furthermore, the underlying rationale of diversion with services is to intervene in clients' lives to increase their stability, with employment one definition of life stability. In the current study, employment was measured using seven criterion variables,[1] each of which was measured for the three time periods described above. Because of the erratic nature of employment for this population, more variables were needed to provide a complete picture of their employment than would be necessary for a more stable population. Data were collected on the number of jobs each respondent held, as well as their starting and termination dates, number of hours worked, and salary. While increase in salary might be a sufficient measure of progress in employment for a middle-class, fully-employed population, it is likely not to be sufficient for this type of population. By using the seven measures described below, the most complete 18-month occupational pattern could be obtained and analyzed.

[1]The seven variables used to measure employment are not independent; that is, monthly salary is correlated with hours worked per month, as is each of the other criterion variables with the others. One possible treatment of this problem would be to use multivariate statistics such as canonical correlation; however, in the present research, techniques were chosen for their simplicity and familiarity. The design of the research is complex in its own right, and multivariate statistics would add to the complexity. A problem with the chosen alternative is that it is possible to make incorrect substantive conclusions from results of multiple regression analyses (or multiple analyses of covariance); this possibility arises when one of the criterion (dependent variables (Y_i) is merely an effect (or epiphenomenon) of another (Y_i). In such a case the relationships of the predictors to the dependent variable (Y_i) are spurious in that they would not hold if the effect of the other criterion variable (Y_i) were partialled first. Nevertheless, interest

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Four of the variables measured employment characteristics during the six month *periods* (hereafter referred to as intake, 6-month, and 12-month); these are number of jobs, average hours worked per month, average monthly salary, and number of months employed. The other three variables were measured at a *point* in time (i.e., on the date of intake, six months after the intake date, and twelve months after the intake date).[2]

Results were computed for each of the seven employment variables using repeated measures analysis of covariance (Winer, 1962). This technique allows one to measure change on a variable such as employment over time (i.e., from six months after intake to 12 months after intake) while controlling for differences in that variable during the six month period prior to intake. At the same time one can assess the degree of difference between the experimental and control groups and differences between the groups on rate of change over time. Each analysis has the same format: one between-subjects factor (research assignment, i.e., experimental or control), one within-subjects repeated measures factor (values of the dependent variable such as number of months employed for the six month period following intake and for the

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in each of the seven variables prompted the decision to conduct seven separate analyses; the intercorrelations are considered in interpreting the results of the analyses. (For a more complete discussion, see Cohen, J. and Cohen, P., 1975.)

[2]The number of individuals included in the analyses varies as a result of differing amounts of missing data. If a question was either not applicable to a given individual or the person did not know the answer, it was coded as blank; in such cases the individual was excluded from the analysis on that variable. For example, a person might be able to answer a question about whether he/she was working during any given month, but not remember the salary.

period six-to-twelve months after intake), and one covariate (the same dependent variable -- e.g., number of months employed -- for the intake period).

Because we wanted to know whether CEP had any impact on employment as measured by both number of jobs and length of employment, the starting and termination dates of each job held by a respondent were recorded on the timeline. This information enabled the research to count the number of jobs held during each six month period and to calculate how long each was held. Both measures represent crucial dimensions of service to CEP and are evidence for the courts of life stability. It can be seen from Table V-1 that the mean number of jobs was less than one (with the exception of the experimental group during the 12-month period); this reflects the fact that many people held *no* job at all during one or more time periods, thus deflating the mean.[3]

Table V-1

MEANS AND STANDARD DEVIATIONS
FOR NUMBER OF JOBS

Research Assignment**		Six Month Period* Prior to Intake	Intake to 6 months	6 to 12 months
Experimental (N=252)	Mean	0.417	0.556	0.853
	S.D.	0.601	0.667	0.909
Control (N=146)	Mean	0.390	0.555	0.767
	S.D.	0.612	0.641	0.860

*$F(1,396)=36.183$; $p<.001$ **$F(1,396)=0.387$; $p<.500$

[3]Although analysis of covariance was used to test for differences, because there was no initial difference between the experimental and control groups, the adjusted means do not differ from the unadjusted means. That is, controlling for number of months worked during the intake period does not affect the means for number of months worked during the six-month or twelve-month period.

A repeated measures analysis of covariance was computed (as described above) on the number of jobs held by experimental and control group members during the intake, six month, and twelve month periods.[4] The results of this analysis indicated that while the number of jobs held increased significantly from the six month period after intake to the twelve month period [5] there was no significant difference between the mean for experimentals and that for controls during the intake, six, or twelve month period. The means for each period are presented in Table V-1 above. While respondents increased slightly the number of jobs they held during each successive period, there is no difference between the experimental groups and control group in either the average number of jobs held or their rate of increase. One possible reason for the increase

[4]As noted above, the repeated measures analyses of covariance were computed on those cases for whom data were available for all three periods; i.e., those cases for whom the full twelve months of follow-up data were not available were excluded from the analysis. For this reason the Ns were reduced for these analyses; therefore, a series of analyses of partial variance was conducted on those cases for whom at least six months of follow-up data was available. The results of these analyses did not differ from those obtained from the analyses using the full twelve months of data; i.e., there were no significant differences between experimentals and controls in the magnitude of (regressed) change from the six months prior to intake to the following six months on any of the employment variables.

[5]For each of the analyses presented in this section, statistically significant differences will be indicated and the results of the F-test provided. The format of the presentation is as follows: the degrees of freedom are provided in the inner set of parentheses, followed by the value of the F-test and its significance level. Those tests that do not yield significant Fs will be discussed as nonsignificant differences. Those tests for which the F approaches significance, i.e., $.05>p<.10$ will be discussed as trends. It is important that we make the distinction between findings that are "statistically significant" and those that are "substantively significant" or meaningful. It is possible with large samples to attain statistical significance with very small absolute differences. Because statistical significance tests tell us whether the differences

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over time in the number of jobs is that the population matured. At intake, the median age of the research population was 18. With half the population under 18, many of the participants in the research had not yet entered the labor market, so that over the next twelve months many participants probably entered the labor market for the first time.

A second measure of employment, which might be more sensitive to change, is the total number of months employed during a six month period, regardless of how many different jobs were held. For each respondent the total number of months of employment for a given period was calculated from the timeline, and these data were entered into a repeated measures analysis of covariance. It can be seen from Table V-2 that the average member of the sample worked less than two months during any six-month period; it is, therefore, clear that the absolute amount of employment for this population was small. There

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of the observed magnitude is likely to be a result of chance rather than a real difference between the two groups, the larger the sample the easier it is to find statistically significant differences. Therefore, when interpreting the results of tests on large samples, it is important to consider not only the level of statistical significance, but also the *magnitude* of the difference or *strength* of a relationshp. For example, with a sample of 400 cases, a correlation of .10 is statistically significant (at the .05 level), but this correlation explains only one percent of the difference (or variation) in the dependent variable. Thus while it is statistically significantly different from zero, it would be of little value in, for example, using the independent variable to predict values of the dependent variable.

was no significant difference from six to twelve months in the number of months employed ($F(1,367)=2.403$; $p=.122$), nor was there a significant difference between experimentals and controls ($F(1,367)=0.27; p>.500$).

Table V-2

MEANS AND STANDARD DEVIATIONS FOR MONTHS EMPLOYED

Research Assignment **		SIX MONTH PERIOD * Prior to intake	Intake to 6 months	6 to 12 months
Experimental (N=235)	Mean	1.062	1.294	1.653
	S.D.	1.935	1.861	2.054
Control (N=134)	Mean	0.739	1.407	1.414
	S.D.	1.661	2.067	1.974

* $F(1,367)=2.403$; $p=.122$ ** $F(1,367)=0.27$; $p>.500$

For the members of this population, number of months employed during a specific period is correlated with the number of jobs held during the same period, especially since for those persons for whom the number of jobs held during a period is zero, the number of months employed is also zero.

The third measure of employment activity was average number of hours worked per month during a six month period. The mean number of hours a respondent worked each month during a six-month period was calculated by multiplying the number of hours worked per month (on a particular job) by the number of months the job was held, summing

over all jobs, and dividing the total number of hours by six. The means for each period are presented in Table V-3; the relatively low means indicate little employment activity for the population as a whole.

Table V-3

MEANS AND STANDARD DEVIATIONS FOR HOURS WORKED PER MONTH

Research Assignment**		SIX MONTH PERIOD* Prior to Intake	Intake to 6 months	6 to 12 months
Experimental (N=148)	Mean	35.860	41.806	51.193
	S.D.	57.961	51.160	59.676
Control (N=82)	Mean	21.251	39.897	52.631
	S.D.	44.473	55.495	61.322

*$F(1,226)=6.378$; $p=.01$ **$F(1,226)=.756$; $p=.386$

It can be seen from the table that both experimentals and controls showed an increase over time in the average hours they worked per month, but there is no significant difference between experimentals and controls. Again, the increase over time is probably a function of maturity.

Monthly salary is, of course, closely related to number of hours worked per month, and average monthly salary was computed using the same algorithm as was used for mean hours worked per month. It can be seen from Table V-4 that the average monthly salaries for the

members of this population are quite low, reflecting that the jobs they held were poor, that their employment patterns were irregular, etc. Because of the relationship between hours worked and salary, it is not surprising that there were no significant differences between the means for the experimental and control groups during either the six month or the twelve month period. There was, however, a significant increase in salary over time for both groups (see Table V-4).

Table V-4

MEANS AND STANDARD DEVIATION FOR MONTHLY SALARY

Research Assignment**		SIX MONTH PERIOD* Prior to Intake	Intake to 6 months	6 to 12 months
Experimental (N=100)	Mean	$154.99	$186.73	$258.38
	S.D.	209.438	186.85	316.317
Control (N=52)	Mean	$127.78	$229.50	$318.70
	S.D.	220.955	320.993	399.381

* $F(1,147)=7.277$; $p=.008$ ** $F(1,147)=1.545$; $p=.216$

Three single-item measures of employment at a point in time were computed: (1) dichotomous employment (yes/no), (2) hours worked per week, and (3) weekly salary. They were computed for each respondent on his/her intake date, six months after that date, and twelve months after the intake date. Measures that are computed for

a single point in time are likely to be less reliable measures than those computed using a number of data points.[6] That is, an individual who was not employed on the one date examined for the point in time variable, would be coded as "unemployed" even though he/she might have been employed for all the remaining days in the six month period. The variables discussed above, which were computed using a number of items of information for each period, are less likely to be affected by arbitrary choices of dates.

The first "point in time" measure was whether the respondent was employed at intake, six months,and twelve months later; although a repeated measures analysis of covariance was computed on these data, they are better understood as percent of each group employed. The percentages are presented in Table V-5 below where one can see clearly the increase in employment over the eighteen months of research. There was no significant difference between experimentals and controls, but there was a significant increase over time in the proportion employed in each group. As was the case with previously discussed variables, this result is probably due to the process of maturing which affects members of both groups at the same rate.

[6]We included these single-item measures in the analysis because they are useful for descriptive purposes; in addition, CEP records its service data in terms of Vocational Status at intake and exit from the program. The use of single-item measures of employment is a real problem in many studies of diversion that use unemployment at intake and employment at exit as evidence of program impact; however, in the context of other, continuous measures these variables serve to provide a richer description of the population's employment patterns.

Table V-5

PERCENT EMPLOYED

Research Assignment**	Point*		
	Intake	6 months	12 months
Experimental (N=252)	18%	23%	31%
Control (N=146)	16%	25%	32%

* $F(1,396)=7.867$; $p=.006$ ** $F(1,396)=.328$; $p<.500$

The second variable measured at a point in time was hours worked per week at intake, six months later and twelve months later. There was no significant difference between experimentals and controls; however, the mean number of hours worked increased for both groups over the twelve months. The means are presented in Table V-6. It is likely that the increase in hours worked per week is a function of the increase over time in the number of persons employed.

Table V-6

MEANS AND STANDARD DEVIATIONS
FOR HOURS WORKED PER WEEK

Research Assignment**		POINT* Intake	6 Months	12 Months
Experimental (N=252)	Mean	5.980	8.238	11.202
	S.D.	14.126	16.062	17.836
Control (N=146)	Mean	5.178	8.192	11.500
	S.D.	12.867	14.969	17.209

*$F(1,396)=9.982$; $p=.002$ **$F(1,396)=0.008$; $p<.500$

Similarly, there was no difference between experimentals and controls on weekly salary, but there was a small increase over time in weekly salary for both groups (see Table V-7). Again, the salary increase is probably the result of the increased number of individuals who were working (and therefore earning something).

Table V-7

MEANS AND STANDARD DEVIATIONS FOR WEEKLY SALARY

Research Assignment**		POINT* Intake	6 Months	12 Months
Experimental (N=252)	Mean	$19.75	$29.48	$41.76
	S.D.	47.540	57.888	64.098
Control (N=146)	Mean	$16.17	$32.61	$47.12
	S.D.	42.117	71.619	85.795

*$F(1,396)=11.059$; $p=.001$ **$F(1,396)=0.524$; $p=.470$

While the results of the analyses on the seven employment variables appear to show consistently that there are no differences between experimentals and controls in the rate of change over time, one should be cautious in interpreting the strength of these findings. Clearly these variables are not independent -- the number of hours worked per month during a six month period should be related to the monthly salary during the same period, which in turn is likely to be related to the number of jobs held during that time, etc. Therefore, one would expect each of these variables to yield the same results, and it might be more logical to think of the consistency of results as a demonstration of the reliability of the various employment measures rather than as seven separate analyses confirming the results.

Recognizing that the seven employment variables are somewhat redundent, the results are important. The increase over time in the percent of each group employed probably reflects maturation; that is, over the twelve months of the research the members of the population (both experimentals and controls) increased in age, and as might be expected, more of them entered the labor market. Despite this increase, the overall employment pattern remained erratic. In addition to the analyses presented above, correlations were computed between employment variables at intake and six months. There was a weak relationship between total number of months employed during the intake period and the number of months employed during the six month period ($r(340)=.24$; $p<.001$). While in much research on employment, knowing how many months a person worked during one period is a good predictor

of how much he/she worked during a subsequent period, this is not the case for this population. Even more striking is the extremely weak correlation between whether one is employed at intake and whether he/she is employed six months later ($r(334)=.15$; $p<.01$). Some caution should be exercised, however, in interpreting these correlations: the relationships between variables measured in the intake interview and those measured six months later may be small because of measurement errors in the sample, or the "true" correlation in the population may be small. Possible sources of measurement error include inaccuracy of reports by respondents and problems due to changes in marginal frequencies (if different numbers of respondents provide data in the two interviews, a ceiling of less than 1.0 is placed on the correlation coefficient). On the other hand, the correlations may be small because the employment pattern for this population remains erratic despite overall increases in vocational activity. It is likely that each of these factors contributes to the magnitude of the correlations among employment variables.

Nonetheless, these correlations cast doubt on the appropriateness of using such behavior as indicators of the success or failure of long range rehabilitation. Yet, during the period of the CEP evaluation, improvement in employment was used by the program to demonstrate to the courts that a defendant had shown increased stability and should therefore have his/her case dismissed. The correlations between employment variables at intake and six months indicate that any program impact is likely to be short run; that is, CEP did not seem to improve employment to a greater extent than do

whatever outside factors affected the control group. The program did not reduce the erratic employment pattern, did not "stabilize" and finally, if a divertee was employed on the day he/she left the program it is difficult to predict whether he/she would still be employed one, two, or three months later. Thus, the programmatic use of such short term changes as indicators of increased life stability is ill-advised.[7]

As an *employment* or *manpower* service program, the Court Employment Project confronted a major problem in the relative youth of the population it serviced. Approximately half the population was sixteen and seventeen years old; for most people, regardless of their life circumstances, this stage in life is one of transition between childhood and adulthood. As might be expected, therefore, many CEP participants seemed unsure of their goals vis-a-vis entering the labor market. While 49 percent of them stated that one of their goals at entry to CEP was employment, 45 percent had employment as a future goal, and 35 percent said that their present goal was education.[8] Therefore, a program that attempts to find employment for a population whose members are not quite sure that they want to work is handicapped. Such persons

[7]It may be somewhat of an overstatement to assert that all prosecutors or judges expected increased "life stability" from youthful defendants after such a short period of services. One prosecutor, for example, said that while they do look for something concrete, such a participation in a training program or job, for a CEP client to have the charges dismissed, "He's got to be doing something besides breathing; most of them are just breathing when they come in here [arraignment]." This suggests that evidence of increased vocational activity in *any* form was the central concern, not specifically school or a job. However, the analysis of general levels of vocational activity among the research population discussed below challenges the program's level of success even as defined by this much less optimistic criterion.

[8]The data referring to the present and future goals of CEP participants were collected from the program's client folders, and were part of CEP's intake interview.

are likely to be unstable, to take jobs and then decide that they don't want to work. In addition, many of the CEP participants had unrealistic expectations for employment; they entered the program expecting to be placed in a good job, while the CEP counselors evaluated their needs as prevocational (e.g., preparation for the world of work). These differences between program and client in expectations may help to explain the failure of the progam to have an impact on employment.

Finally, CEP's failure to have an impact on employment is partially a result of the perennial problems the program has had with job development. As was discussed in Chapter II, there are several reasons for the failure of the job development component: (1) the overall job market was poor; (2) they had difficulty finding low level and entry slots in which to place clients; (3) the available jobs did not meet client's expectations; (4) the Counseling Unit was slow in referring clients to the Services Unit for referral and placement; (5) the Services Unit was understaffed for job development; and (6) the Services Unit staff members were not uniformly well-trained specialists.

Educational Activity

In addition to employment, educational activity is an important factor in evaluating the success of CEP. Education is especially important because of the youth of the CEP service population. Since most had also not completed high school (the median educational level was tenth grade), helping some clients finish their education was an important goal for CEP. (CEP's stated goals included providing

"vocational training and assistance in educational and job placement..." Both CEP and middle-class society assume there to be a direct relationship between education and long range employment; that is, the more education one has, the more likely he/she will be to obtain a job or the better the job he/she can get. While it is questionable whether this assumption is realistic for the population being studied, it is a conception which has influenced CEP's attempt to help clients by having an impact on their educational achievement.

Three measures of educational activity were included in the time-line analyses: (1) enrollment at a point in time, (2) average weekly attendance at a point in time, and (3) number of months enrolled during a six month period. Repeated measures analyses of covariance yielded no significant effects on any of the three variables -- not only were there no differences between experimentals and controls, but there were also no significant changes over time. At intake 25 percent of the interviewed members of the research population (N=511) were enrolled in school; six months later 22 percent of the interviewed members of the population (N=445) were enrolled in school; and twelve months after intake 21 percent of the interviewed persons (N=399) were enrolled in school. Similarly, the average number of months enrolled during a period did not change significantly -- from 1.6 months during the six months prior to intake to a mean of 1.3 months during the second six months after intake.

The lack of significant results on education variables is further evidence of the inability of CEP's services to effect measurable changes in the population serviced by the program. As discussed

above, many of their clients expressed an interest in going back to school *as well as* a desire to obtain employment. It is likely that many are unsure of what they want to do, and may want school one day, a job the next, and be dissatisfied with the outcome of both. While 35 percent of CEP clients stated education -- either getting back into school or changing schools -- was a present goal and 20 percent expected help from CEP in attaining these goals, there was no change in the twelve months after intake in the proportion of either experimentals or controls enrolled in or attending school. In addition, the clients' perceptions of the usefulness of education in helping them secure the kind of jobs they wanted probably affected the amount of influence a program such as CEP can have. When asked (in the follow-up research interview), "Thinking about the education you have had so far: How useful do you think it has been to you for getting the type of job you want?", 14 percent of the respondents said that none of it had been useful, 20 percent responded that very little of it had been useful, 36 percent some of it had been useful, 19 percent that most of it had been useful, and nine percent that all of it had been useful. Once a 16-, 17-, or 18-year old person decides that school will not help him/her get the kind of job he/she wants, there is no reason to continue going to school.[9] Yet the pervasive

[9]There is research evidence that suggests that the perceptions of the members of the research population about the utility of education may be accurate. Although we assume that education and occupational opportunity are directly correlated, data suggest that the link between education and jobs is not so strong; the returns to human capital of educational investment for this population are low. Thus, having a high school diploma does not necessarily provide access to the primary labor market for lower class minority youths. See Robert Flanagan, "Labor Force Experience, Job Turnover and Racial Wage Differentials, "*Review of Economics & Statistics* (November 1974) and Barnet Harrison, *Education, Training & the Urban Ghetto* (Baltimore: John Hopkins University Press, 1972).

norms of society stress education as *the* route to success and thus many of these youths are ambivalent about what they *say* they want with regard to education. They may toy with the idea of returning to school in the hopes of getting that good job that they hear will be waiting for them once they have a high school diploma, but because their experience tells them that school will not help them get a job, they don't really apply themselves to the task of getting back into school. Furthermore, in the environment in which most of the members of the population live, the most successful role models are not enrolled in school or working in the legitimate economy; they are pimps, numbers runners, etc. Counselors at CEP or any other program would be hard-pressed to show these individuals (in one session per week) that they would benefit by going back to high school.

Level of Vocational Activity

Since one of CEP's goals was to demonstrate to the courts that their client population was motivated to change, to become more active and possibly more vocationally stable, a variable was created from the timeline data to measure whether (and for how long) an individual was engaged in any socially approved activity. That is, it may not matter which of school, work, job seeking, or childcare a person is engaged in, as long as he/she is not "hanging out," particularly if there is an assumption (correct or not) that those who are hanging out are with other idle people and likely to become involved in some illegal activity. A composite activity variable was created that measured

the number of months (in half-month increments) during which an individual was engaged in any of the following generally "vocational" activities: (1) attending school; (2) working; (3) in a job training program; (4) actively looking for a job; (5) in military services; or (6) taking care of children. It is important to note the algorithm used to compute this variable involves summing the number of months the person was engaged in *each* activity; therefore, it is possible for an individual to be active for *more* than six months during a six month period. For example, if a person were both working and going to school during one month, it would be scored as two months of activity. This is not a problem for the analysis, nor is it a conceptual problem if the units are considered as a continuous measure of involvement rather than as discrete months.

A repeated measures analysis of covariance on the amount of time active during a six month period again revealed no significant difference between experimentals and controls. There was, however, a significant effect for time; members of both the experimental and the control group tended to become more active across the 18 months beginning with the six months prior to intake through twelve months after intake. The means are presented in Table V-8. It can be seen from the table that the means for experimentals and controls are virtually identical, and that each increases over time. The increase over time may be partially attributed to the increase in employment over time (see Table V-2), since the number of months employed is part of this variable. The lack of difference between experimentals and controls, however, certainly would not lead one to infer that CEP had an overall impact of influencing its clients to become more involved in socially accepted activities.

Table V-8

MEANS AND STANDARD DEVIATIONS FOR LEVEL OF VOCATIONAL ACTIVITY

Research Assignment**		SIX MONTH PERIOD* Prior to Intake	Intake to 6 months	6 to 12 months
Experimental (N=237)	Mean	3.880	4.435	5.070
	S.D.	3.226	2.584	3.107
Control (N=142)	Mean	3.690	4.518	5.074
	S.D.	3.387	2.530	3.285

*$F(1,377)=9.317$; $p=.003$ **$F(1,366)=.034$; $p>.500$

A final variable (which was also included as part of the activity measure) was computed from the timeline: total months spent actively looking for a job during a six month period. This variable was included in the analyses because it is possible that while CEP might not directly affect the amount of employment (through increased number of jobs, hours worked, or salary) it might indirectly affect employment by increasing the motivation of the participant to look for work. This motivational increase might be detected by measuring the amount of time respondents' reported actively looking for jobs. The repeated measures analysis of covariance revealed no significant differences between experimentals and controls on this variable; ($F(1,393)=0$); nor was there an increase in the amount of this activity over time ($F(1,302)>.005$; $p<.500$). (One would have to exercise caution in interpreting

results from this variable; it was calculated from respondents' self-reports and relies on their interpretation of what it means to be *actively* looking for work.)

DIFFERENTIAL EFFECTS OF CEP BY AGE GROUPS

The lack of any differences between experimentals and controls on the variables computed from the timeline led us to consider the possibility that CEP might affect various groups differently. The majority of CEP's service population are young; however, some of the defendants are older (15 percent are between the ages of 20 and 25; 14 percent are over 25). Looking only at overall effects (i.e., effects on the population as a whole) might mask specific effects on particular groups. Age seemed a crucial variable for this type of analysis because of its relationship to employment and education, as well as its assumed relationship to crime. Therefore, a series of analyses were conducted on the variables discussed above to determine whether CEP affected young defendants differently than it did older defendants. The analyses were of necessity quite complex; therefore, a summary of the findings on age effects is presented below and a more complete technical presentation may be found in Appendix D.

Of the seven employment variables discussed above, three showed no evidence of differential impact of CEP on young and old defendants; these were employment (yes or no) at a point six months after intake, hours worked per week on the date six months after intake, and number

of jobs held during the period six months subsequent to intake. The remaining employment variables did show some age effects; these are summarized below.

The most general measure of employment was the change from six months prior to intake to the following six months in the number of months employed. Age was weakly related to employment during both periods: the older defendants were likely to be employed for longer durations than were younger defendants. Furthermore, the longer one worked during the six months prior to intake, the longer he/she was likely to work during the following six month period. Both of these relationships were quite small, however, and were likely to have little impact on reality; that is, although there were statistically significant effects, one would be unlikely to see any real differences. Furthermore, CEP did not appear to affect young defendants differently than older defendants; young experimentals showed no greater (or lesser) improvement than older defendants in number of months worked. Thus, for this variable, the results of the analysis for the population as a whole did not differ from that with age included.

The effect of age on average monthly salary during a six month period was stronger but similar to that on number of months employed. As was discussed previously, the higher a defendant's average monthly salary was during the six months prior to intake, the higher it was likely to be during the following six months. In addition, while age was not related to monthly salary prior to intake, the older respondents tended to have higher monthly salaries than young respondents during the six months after intake. So, average monthly salary in-

creased from intake to follow-up (as shown in Table V-4), and older members of the population earned more than did younger respondents. However, even with age included, there was no difference in the size of the increase for experimentals and controls. Thus, if CEP has any impact on monthly salary, that effect is not hidden by age. (A second salary variable was included in this series of analyses, total weekly salary at a point in time. The results of this analysis and the conclusions drawn from it were virtually identical to those for monthly salarly.)

The most complicated effect involving age was found for average number of hours worked per month during the first follow-up period; the effect of age on hours worked per month was different for experimentals than it was for controls. While for control group members the increase from intake to follow-up in hours worked per month was constant across age groups, older experimentals tended to increase their hours more from intake to follow-up than did younger experimentals. For the population as a whole, the more hours one was employed during the intake period, the more hours he/she was likely to be employed during the follow-up period, and for both experimentals and controls there was a slight increase from intake to follow-up in the number of hours worked (see Table V-3). In addition, the older the individual was at intake the more hours he/she was likely to have worked during the previous six months and the more hours he/she was likely to work during the subsequent six months. These results provided some evidence for different impact of CEP on defendants of different ages; it appears that older members of

the population made greater improvements in employment, as measured by hours worked per month, than did younger defendants. However, the absolute magnitude of these effects is quite small, and one should not be overly zealous in interpreting these results as evidence for CEP's impact.

Two education variables were included in the analyses -- average number of days of school attendance per week at a point six months after intake and number of months enrolled in school during the follow-up period. Younger members of the research population were more likely than older respondents to attend school, both as measured in number od days per week and as months enrolled. This was true for experimentals and controls at both intake and follow-up. The greater an individual's attendance was during the intake period, the more often (and the more months) he/she was likely to be in school at follow-up. However, experimentals did not show any greater improvement in school attendance than did controls; nor did younger members of the sample show any greater improvement than did older members. Thus, one can conclude that the consideration of a defendant's age does not reveal any hidden effects of CEP; in other words, with respect to education, CEP does not affect younger defendants differently than older defendants.

Similar analyses on general level of vocational activity did not yield any significant age effects. Thus the analysis of age effects on employment, education, and vocational activity failed to produce any strong effects. Only for hours worked per month was there any interaction between age and research assignment; that is, there was only one instance in which CEP appeared to have different impacts on participants of different ages.

EFFECTS ON GENERAL LIFESTYLE

Employment and education are the most tangible elements of lifestyle change that might be expected from participation in a program like CEP. There are, however, other positive changes in lifestyle that could accrue as benefits of a social service program. Using responses to the research interviews, comparisons were made over time between experimentals and controls on a number of measures of "quality of life." These include use of services (e.g., doctors), living conditions (e.g., rooms per person, problems with residence), drug use, alcohol use, and participation in group social activities. The results of these analyses are presented and discussed below.

Use of Services

One of CEP's goals was to provide a wide variety of other services to its client population. Most of these were provided through referrals to other agencies (because CEP's contract with HRA was for information and referral as opposed to direct provision of such services). CEP tried to provide a wide range of services and referrals both to fulfill its contract with HRA and because it believed that the population with whom it was dealing was needy and underserviced.

The data collected in the research interviews indicate that HRA's, CEP's and the criminal justice system's perceptions of this population as underserviced was accurate. The interviewed population as a whole did not report many contacts with other formal programs, nor did they receive help frequently from social service personnel. When asked whether they had been in any rehabilitation programs other than CEP,

only 13 respondents reported being in some court- or police-related program, either prior to or after entry into the research population, and only 30 persons reported being involved in any alcohol or drug program. Furthermore, respondents were asked in the research interview who had helped them with problems they might be having; very few mentioned social service workers (outside of CEP). For example, of those who reported having housing difficulties, only nine individuals had received help solving those difficulties from any social service worker. Four had received such help in obtaining their current jobs, and seven in obtaining their most recent (non-current) jobs. In addition, nine individuals reported getting help from a social service worker or counselor in enrolling in school, and five had been helped in obtaining welfare assistance. Although these numbers represent only those interviewed members of the population who reported needing help in a particular area, they are clearly quite small. While at the time of the research intake interview, 46 percent of the interviewed population reported that they were receiving welfare (including those on an AFDC budget) and were thus eligible for counseling and/or services through the Department of Social Services, the research findings suggest few received them.

Therefore, the research attempted to measure CEP's success in affecting the number of services received by its clients and concommitant changes in lifestyle. The variables used to measure change in receipt of services were constructed from respondents' answers to questions about how many times they had seen doctors, counselors, social workers, etc., during the period covered by the interview (either prior

to intake, from intake to six months later, or from six to twelve months after intake into the research). Respondents were also asked whether they had wanted more of these services and whether they had been healthy or ill during the period. Lifestyle was described using self-reported alcohol and drug use, participation in group social activities, and participation in illegal activities.

The results of comparisons between experimentals and controls on use of services showed no significant differences. Out of ten different types of services (visits to dentists, eye doctors, physical therapists, social workers, etc.), the average number of *types* they reported using during a twelve-month period was only one. The mean for experimentals did not differ significantly from that for controls, either in the year preceding or the year subsequent to intake into the research. While the average number of service types received did increase very slightly (from .89 to 1.28) over time ($F(1,463)=22.407$; $p<.001$), the difference is so slight as to be trivial. Another variable constructed from the same set of responses was how many services the respondent had actually received during a twelve month period (e.g., if an individual went to the dentist three times, to an eye doctor once, and saw a lawyer once during the twelve months subsequent to intake, his score on this variable would be 5). The mean number of services received did not differ for experimentals and controls, but again there was a significant increase over time ($F(1,435)=5.764$; $p=.017$). However, though the increase was statistically significant, it was quite small; on the average, members of the research population received 5.4 services during the twelve months prior to intake and 8.2 during the twelve months subsequent to intake into the research. The reasons for these increases are not clear, but likely to be attributable to maturation and recognition by the participants in the research of their need for such services. One can infer such recognition from the respondents' indication that they *wanted* more services twelve months after intake than they had wanted at intake ($F(1,461)=41.112$; $p<.001$). For each of the ten types of services the respondents were asked, "Did you want to see a (_____) more often than you actually did?" At intake the mean number of "yes" responses was .76, and 12 months later the mean was 1.44, a small but statistically significant increase. (In addition, experimentals reported wanting slightly but significantly more services than controls ($F(1,461)=4.013$; $p<.05$), both at intake (means of .88 and .64 respectively) and 12 months later (means of 1.57 and 1.30)).

Other Legal and Illegal Activities

Beyond the changes mentioned above, very little of the respondents' lives seemed to change during the research period. In each of the interviews, they were asked whether they had been "generally healthy or often ill"; there were no differences between the responses of experimentals and controls and no changes over time -- 94 percent of the research population reported being generally healthy at each interview. There was some decrease in self-reported use of alcohol

(χ^2_8=52.835; p<.001); at intake one out of four respondents said they drank daily, while six and twelve months later only 13 percent (or one out of eight) of the respondents said they drank daily. Their drug use remained fairly constant over this period; very few of the members of the population reported using drugs other than marijuana. However, 90 percent of the respondents (at each interview), reported using marijuana, 45 percent of them daily.

As a measure of involvement in legitimate activities, each interview contained the question, "...have you participated in any social, recreational, or group activities at a community center, settlement house, or church center?" Fewer than 25 percent of both experimentals and control group members indicated that they participated in any of these activities; of those who did participate, however, approximately 80 percent did so at least once per week. They were also asked whether their friends were involved in illegal activities or were "straight." While there were no differences between experimentals and controls, there was a slight increase for both groups in the proportion whose friends were "straight," from 59 percent at intake to 65 percent at six months and 70 percent at twelve months (χ^2_4=13.557; p=.01). Along with the increase in the proportion of respondents with straight friends was a small decrease in the proportion reporting any illegal

activity. The proportion of the research population reporting any participation in each of ten illegal activities is presented in Table V-9. The apparent (small) decline in illegal activities must be viewed with considerable caution, however; the low level of self-reported illegal activity is in contradiction with the percentage of the population arrested subsequent to intake (37 percent). Certainly not everyone participating in illegal activities would be caught, so these self-reports of low levels of illegal activity, if true, lead one to expect very few rearrests.

Table V-9

SELF-REPORTED ILLEGAL ACTIVITIES

Activity	INTERVIEW		
	Intake	6 Month Follow-up	12 Month Follow-up
Gambling	28%	10%	9%
Burglary	12	7	4
Boosting	13	5	5
Selling Drugs	21	12	11
Car Theft	5	3	2
Robbery	6	3	3
Fencing	14	7	6
Mugging	5	2	1
Con/Fraud	9	3	4
Pimping/ Prostitution	4	1	1

The lack of differences between experimentals and controls in the data discussed above indicates that CEP did not seem to play a measurable role in affecting the overall lifestyles of its clients. Whatever services CEP was able to provide its participants, members of the control group were able to obtain through other sources. While CEP's evaluation of this population as needy and underserviced seems accurate, the program was not successful in changing their basic life situations.

COMPARISON OF SUCCESSFUL AND UNSUCCESSFUL CEP PARTICIPANTS

Random assignment of persons to experimental and control conditions enables one to draw conclusions about effects on the participant that may be attributed to the program intervention (because the two groups are similar at intake). Comparing those people *within* one of these two groups, however, leads to less secure conclusions because there may be some self-selection processes occurring that mask effects of the intervention. Hence, earlier criticism of research that *only* compared successful and unsuccessful program participants are appropriate since such analyses heavily weight the evaluation in the direction of favorable program impact. It must be recognized, however, that among Court Employment Project participants are many (45 percent) who did *not* successfully complete the program (and received "terminations" or "administrative discharges"). Many of these clients received no services or counseling from the program because they never (or rarely) attended the program to receive these services. When the terminations are included with the successful

participants (or "dismissals") as members of the experimental group, effects CEP had on the lives of its successful clients may be hidden by the lack of effect on unsuccessful clients.

While the primary hypothesis testing analyses *must* compare an experimental group consisting of *both* successful and unsuccessful CEP participants with a control group, for exploratory purposes, we compared successful (dismissals) and unsuccessful (terminations and administrative discharges) CEP participants in the experimental group. Although such an analysis violates the experimental design, comparisons of lifestyle changes for the two groups may provide some information about the effects that CEP had on a subgroup of its clients.

A series of multiple regression analyses, similar to those involving age at intake, were conducted on the employment, education and vocational activity variables constructed from the timeline. These analyses were designed to determine whether change from intake to follow-up could be predicted from respondents' exit status from CEP -- that is, whether CEP had more impact on participants who stayed in the program for a full four months and had their changes successfully dismissed than on those who did not participate fully and were returned to court without a recommendation. The results of these analyses are presented below and their implications for the program are discussed. In general, they suggest that the program did not have impacts on employment or education, *even for those people who successfully completed the program.* Although there were some statistically significant effects, their size was so small that it is unlikely that they represent any real differences in the population.

Fifty-five percent of the members of the experimental group successfully completed the four month CEP program; the other 45 percent were terminated (40 percent) or administratively discharged (five percent). Because those persons who dropped out of the program before completing it were also more likely to drop out of the research than were successful CEP participants, 65 percent of those who were interviewed both at intake and six months later were successful CEP participants, and 35 percent were unsuccessful. The analyses of partial variance include only those members of the research experimental group for whom two interviews were conducted. Because the successful group is overrepresented in the analysis, the analyses are somewhat biased (though not seriously) in favor of the successful participants; because these analyses are exploratory and because the groups are clearly self-selected, this does not pose any serious problem. The "unsuccessful" CEP participants were, however, likely to be among those who were not interviewed. Their lack of success in the program and failure to receive the research follow-up interview were likely due to the same reason -- many had absconded and could not be located by either the program or the research. Therefore, the results of these analyses are related to the causes of success that remain *after* attrition.

The seven employment variables discussed previously were analyzed using CEP exit status as the independent variable; only one of them showed significant effects for exit status (number of jobs held during the six months after intake), supporting the results of the overall comparison between experimentals and controls. That is, CEP failed to

affect employment even for those people who successfully completed the program. The number of jobs a person held during the follow-up period was weakly related to number of jobs held during the intake period ($\underline{r}(189)=.16; p<.05$) and it was also related to CEP exit status ($\underline{r}(189)=-.18; p<.05$). Therefore, those who had held more jobs prior to the period were also likely to have more jobs during the follow-up period. This pattern was most prevalent among defendants who were successful CEP participants although the mean number of jobs held during the six months prior to intake was not different for the two groups. (*More* in this case generally means having one job as compared to no jobs; the mean number of jobs held during the six months subsequent to intake was .60 and during the six months prior to intake it was .44.) The results of the analysis of partial variance indicated that CEP exit status (successful or unsuccessful) accounted for a significant amount of the difference from intake to six months in number of jobs (the $\underline{R}^2$ increased from .03 to .06). Therefore, while this increment is satistically significant, the total variance accounted for by the two variables together (six percent) is extremely small; such a small effect cannot have much meaning for real change.

There was also a significant correlation between number of months spent searching for a job during the intake period (mean=1.16) and number of months spent searching for a job during the follow-up period (mean=1.45); there was a significant (but again very weak correlation) between CEP exit status and looking for a job during the six months after intake ($\underline{r}(195)=.17; p<.05$). *Unsuccessful* CEP participants spent slightly more time searching for a job than did CEP dismissals. From

these results one might conclude that those persons who attended the program received help finding a job (therefore, successful participants spent less time looking but held more jobs after intake) and those who did not participate successfully simply spent more time looking for work. However, both these results are *extremely weak* and thus may be unreliable; furthermore, other data from the research interviews do not support this conclusion. Employed respondents were asked in the research follow-up interview, "Did any of the following people help you find this job?" Included in the list were spouse, parents or other relatives, friends, counselor or other worker at CEP, other counselor, probation or parole officer, police officer, or lawyer. Of the respondents who were employed at the time of the interview (N=79), only six percent stated that someone at CEP had helped them find their current jobs. In contrast, 65 percent of them indicated that their spouse or girl friend/boy friend had helped. Similarly, 20 percent indicated that CEP had helped them find their most recent jobs (N=110), compared to 80 percent who received help from their spouses. There were also a number of employment variables for which CEP exit status did not predict change from intake to six months. These include number of months salary, number of hours worked per week and weekly salary at a point in time, and whether the person was employed at a point in time. If one considers the weakness of the significant results and the number of non-significant differences between successful and unsuccessful CEP participants, one is led to conclude that there are no real differences on employment variables between those members of the research population who completed the program and those who did not.

Two education variables were included in a similar analysis of the effect of CEP exit status: number of months enrolled in school during the six months after intake and average number of days per week of school attendance at a point six months after intake. As might be expected, the number of months in school during the six months prior to intake was moderately correlated ($\underline{r}(195)=.53$; $p<.001$) with the number of months in school during the six months after intake. School attendance six months after intake was also related to CEP exit status ($\underline{r}(189)=-.21$; $p<.01$); CEP dismissals were likely to attend school more days per week than were terminations. Over the six months from intake to follow-up, successful CEP participants tended to increase their weekly attendance, while CEP terminations tended to decrease school attendance (see Table V-10). The addition of CEP exit status as a predictor of regressed change in school attendance increased $\underline{R}^2$ from .31 to .34 (the regression coefficient was significant $\underline{t}=2.51$; $p=.03$). Thus, as is shown in Table V-10, when average weekly attendance at intake is controlled for (partialled), CEP dismissals are more likely to increase school attendance in the six months after intake, while terminations are more likely to decrease attendance.

Table V-10

MEANS AND STANDARD DEVIATIONS FOR AVERAGE NUMBER OF DAYS PER WEEK ATTENDANCE AT SCHOOL

CEP Exit Status		SIX MONTH PERIOD: Prior to Intake	SIX MONTH PERIOD: Intake to 6 months
Dismissal	Mean	1.55	1.70
	S.D.	2.28	2.28
Termination	Mean	1.08	0.78
	S.D.	1.97	1.72

It is clear from these results that while there are statistically significant differences between CEP dismissals and terminations on the magnitude and direction of change from intake to six months later in school participation, the size of the effect is so small that one can not expect to see real differences between the groups. The size of the effects on education variables is quite similar to those found for the employment variables; additionally, analyses on activity variables (combined employment, education, job search, military service, and childcare) yielded no significant effects. Taken as a whole, this series of analyses leads one to conclude that there are no sizeable differences between those participants who attended and successfully completed the program and those who did not attend and/or were terminated. This can be considered stronger support for the earlier evidence of lack of program effects based upon comparisons between the entire experimental and control groups. That is, if one assumes that those clients who completed the program are the "boy scouts and virgins" (to quote an earlier phrase), the person

most likely to be rehabilitated with or without CEP, then the lack of sizeable differences between them and the CEP failures has serious implications. The most important of these is that one cannot claim that the lack of sizeable differences between experimentals and controls is the result of combining "treated" experimentals with those who never participated in the program and thus were not treated. Rather, it appears that the program does not have measurable impacts on the people it services, or at least, it does not affect the particular lifestyle variables used in the evaluation.

It is, however, also possible that the failure to find an impact of the program on the lifestyles of its clients may be a result of measurement problems. For the variables used to measure employment, education, and vocational activity, the variation *within* the experimental or the control group was quite large. Large within group variation makes it necessary that there be very large differences *between* experimentals and controls if those measures are to detect significant differences between the groups, and one would expect the impact of a program such as CEP on the population it services to be small. Furthermore, since many members of the research population were unemployed during part or all of the research period, the sample size was considerably reduced in these analyses. This loss in sample size reduces the power of the statistical tests. Consequently, the measurement problems and small expected effect size make it very difficult to detect small effects that the program could be having on participants.

CHAPTER VI

PRETRIAL DIVERSION AS REHABILITATION
CEP'S IMPACT ON RECIDIVISM

INTRODUCTION

As discussed in Chapter I, a primary legitimating assumption for diversion has been that it is an intervention in criminal career development. Diversion with supervision and/or services is assumed to be a logical choice for prosecutors and judges because it reduces the recidivism of defendants. The rationale for this assumption is partially rooted in the theory that crime and employment are related, and that an increase in employment (or other related vocational attributes) for a population is likely to reduce its rate of crime. The rehabilitation rationale for pretrial services loses its legitimacy, however, if even without services, the population of diverted clients is a nonrecidivist population. Many of the criticisms of diversion growing out of previous research have been directed at this issue: diversion programs have not typically diverted "risky" (that is, recidivist) populations, and therefore, the process of diversion as a rehabilitative mechanism cannot be tested. The Court Employment Project evaluation, however, can address this issue. While CEP certainly diverts many defendants who do not get rearrested (63 percent within the 23 month research period), there are sufficient numbers who do recidivate (37 percent of the research population) to allow the research to investigate CEP's impact on subsequent criminal behavior.

The impact of CEP on recidivism is measured in a number of different ways in this chapter. First, proportions of experimentals and controls who were arrested subsequent to intake into the program are compared both with each other, and in relation to reports of other diversion programs. They are also compared on number of subsequent arrests; type and severity of the charges on which they were arrested, proportion convicted on subsequent arrests; and conviction charges. These comparisons are made for the period experimentals were in the program, that is, the first four months after intake into the research, for twelve months after intake and as of December 1, 1978 (the last time official records were checked by the research). In addition, similar comparisons are made that go outside the experimental design; some of the same variables are used to compare successful (i.e., dismissed) and unsuccessful (i.e., terminated) CEP participants. Within the control group, the effects of the disposition of the intake case and effects of prior arrest record on recidivism are investigated. Finally, an exploration of the factors, regardless of research assignment, that predict recidivism is reported.

COMPARISON OF RECIDIVISM RATES FOR CEP AND OTHER PROGRAMS

Recidivism data for the CEP evaluation were collected from the CJA files (and when necessary updated from Supreme Court records), and contain data on 659 defendants (or 99 percent of the research population). As of December 1, 1978, nearly two years after the start of the research, 37 percent of the research population (experimentals and controls combined) had been arrested on subsequent charges.

Although it is difficult to make comparisons of recidivism rates among diversion programs because the length of the follow-up periods and the research methods differ, the CEP population's rate of recidivism does seem higher than that reported by most other diversion programs. Of the four programs for which post-program recidivism data were reported by Rovner-Pieczenik (1974), the percent of recidivism one to two years after the program ranged from a low of 14.6 percent (participants in Dade County Pretrial Intervention) to a high of 34.0 percent (participants in the earlier evaluation of the Court Employment Project). Obviously the recidivism rates for these groups reflect *post*-diversion behavior; however even for non-participant comparison groups in these programs, the recidivism rates were lower, ranging from 21.5 percent (Project Crossroads) to 32.3 percent (Dade County). Both participant and non-participant groups in all these programs had lower recidivism rates than that found for the CEP research population as a whole. Similarly, Mullen (1974) reported cumulative rearrest rates over a 12 month period for the nine original DOL sites; for participants who favorably completed the program, 18.3 percent were rearrested within 12 months. When unsuccessful participants were added, the rearrest rate for this period was estimated at 32.5 percent (p. 106). In the Monroe County evaluation, Prior (1977) reported that within 12 months, 24.1 percent of the diverted group and 37.2 percent of the comparison group were rearrested.

While each of the diversion programs discussed above reports favorable comparisons between diverted samples and non-diverted

comparison groups[1], there was no significant difference between experimentals and controls in the CEP evaluation. As of December 1, 1978, 35 percent of the experimental and 39 percent of the control group had been rearrested (χ^2_1 =1.021;n.s.). The number of subsequent arrests ranged from zero to eight in the experimental group and zero to seven in the control group; there was no significant difference between the mean number of rearrests among experimentals (.728) and that for control group members (.772).

For each of the rearrested persons (N=240) the most serious arrest charge (across all subsequent arrests) was calculated; these charges ranged from A Felonies to Violations. Only four percent were arrested on charges of an A Felony (most serious), as compared to 15 percent on B Felony charges, 15 percent C Felony, 41 percent D Felony, 9 percent E Felony (least serious), 15 percent on Misdemeanor charges, and less than one percent on Violations. Experimentals and controls did not differ on the severity of rearrest charges.

IN-PROGRAM AND OUT OF PROGRAM RECIDIVISM RATES

It has been traditional to compare recidivism rates for experimentals and controls during the period that experimentals spend in the diversion program. The logic behind such comparisons is that during the program period, experimentals are receiving supervision and services, while members of the control group are "at risk." If

[1] Although the research on these programs reported favorable impact of diversion, as was discussed in Chapter I, they all have methodological problems, primarily, none used a true experimental design.

the program is rehabilitative in the short run, the in-program recidivism rate for experimentals should be lower than that for controls.

While some criticisms may be levelled at the evaluation methods used in other studies, it is useful to compare the in-program recidivism rates they report with the rates obtained in the CEP evaluation. Rovner-Pieczenik (1974) reported,

> Crossroads indicates that 8.5 percent of its participant sample and 21.5 percent of a non-participant sample is rearrested within three months of program intake; a differential of 13 percent exists. Dade County also indicates relatively low recidivism for its participants (8.8 percent) as compared to non-participants (20.6 percent). In both programs, the apparent success in reducing recidivism is attributable largely to favorably terminated participants. (p.76)

As Rovner-Pieczenik points out, in the above-mentioned studies, comparisons were between *successful* program participants and non-participants. In the CEP evaluation recidivism rates for all experimentals were compared to all control group members; during the four months of program participation, 19.8 percent of the experimental group was rearrested as compared with 16.5 percent of the control group (this difference is not statistically significant; χ^2_1 =0.904). Similarly, the mean number of rearrests during the first four months after intake for experimentals (.261) and controls (.213) did not differ (t=1.038, ns).

In the evaluation of the nine original DOL programs, rate of arrest per day was used to estimate rearrest during a standard 90-day period; this was done to facilitate comparisons among programs of different lengths (Mullen, 1974:94-95). It is also possible to estimate recidivism rates for a four-month (or 120 day) period using

the data provided by Mullen. Table VI-1 contains the estimated re-arrest rates for a standard four month period for the nine DOL sites and the actual rates for the CEP experimentals and controls. Although Table VI-1 shows substantial variation among recidivism rates, as Mullen (1974) states,

> Since sites differ both in the intake characteristics of their participants and in the overall frequency of re-arrest in the general population, as well as in the treatments provided during the pretrial period, *we cannot with any confidence conclude from these rates that any low rearrest rate program is more successful than any high rearrest rate program.* (p.96)

While Mullen's conclusion about using these figures to evaluate the success or failure of the various programs is correct, the table shows both the bias introduced by using only successful program participants in comparison with control groups, and the problem with comparing successful to unsuccessful program participants. A comparison of the estimated *total* in-program rearrest rates for the various programs and the recidivism rates found for the CEP experimentals and controls implies that CEP's client population is risker (i.e., more likely to recidivate) than those diverted by the DOL programs. (An additional assumption necessary to draw this conclusion is that there is little or no program effect on clients in these programs. While the data do not demonstrate the validity of this assumption, it is certainly plausible.) This supports the contention that the CEP population provides a better test of the rehabilitation rationale than did those previously studied. However, the lack of difference between recidivism rates for the experimental and control groups suggests that CEP failed to affect recidivism of its clients, even while they were in the program.

Table VI-1

ESTIMATED REARREST RATES FOR STANDARD 4-MONTH PERIOD

Site	Unfavorables	Favorables	Total
Atlanta	13.1%	1.3%	4.1%
Baltimore	16.1	5.3	8.7
Boston	20.3	8.8	12.7
Calif: San Jose	23.6	0.6	3.2
Calif: Santa Rosa	10.9	1.8	6.4
Calif: Hayward	39.6	1.5	8.2
Cleveland	13.2	1.8	3.8
Minneapolis	29.5	4.3	15.3
San Antonio	2.6	1.5	2.0
CEP (actual rate)	31.7	10.2	19.8
Control Group			16.5

Two other in-program recidivism measures were computed: most serious rearrest charge and severity of rearrest charge relative to the charge on the intake case. There was no significant difference between experimentals and controls on the severity of the rearrest charge; for descriptive purposes the most serious rearrest charges are presented in Table VI-2. Furthermore, for each member of the

Table VI-2

SEVERITY OF REARREST CHARGE
WITHIN 4 MONTHS AFTER INTAKE

Research Assignment	Charge Class				
	A/B Fel	C Fel	D Fel	E Fel	Misd/Viol
Experimental (N=81)	12%	16%	46%	12%	14%
Control (N=41)	24%	10%	44%	17%	5%

χ^2=9.382; n.s.; chi square was conducted on an expanded contingency table (with each charge category separate); the cells were collapsed for illustrative purposes.

research population who was rearrested, the severity of the rearrest charge (or the most severe if there were many) was compared to the severity of the intake arrest charge. There was no difference between experimentals and controls on this variable: 25 percent of each group were arrested on less serious charges than on the intake case; 31 percent on charges that were equally serious as those in the intake case; and 44 percent were rearrested on more serious charges.[2]

The results of these anlyses of recidivism data indicate that CEP had no significant impact on participants during the four months they spent in the program. None of the variables showed any

[2] A more powerful statistical test (Kolmogorov-Smirnov) on these data also produced nonsignificant results (χ_2=1.57).

significant differences between experimentals and controls. The data are of interest, however, because they demonstrate that in comparison to other diverted populations, CEP services a population of defendants who may be somewhat more likely to recidivate. In addition, the lack of differences found in comparisons between experimentals and controls, in contrast to the substantial differences between successful and unsuccessful CEP participants, illustrates the importance of controlled research in evaluation of diversion programs.

While there were no short-term (that is, in-program) effects of CEP on recidivism, it is possible that differences could appear over the longer run. If, for instance, it took the full four months of services and counseling for CEP to have an impact on participants' behavior or attitudes, then differences between experimentals and controls might not appear during the early months of the research. To test this possibility, the same set of analyses were computed on recidivism twelve months after intake into the research.

As was found in the analysis of short term data, there was no significant difference in the proportion of experimentals (30 percent) and controls (33 percent) who were arrested during the twelve months subsequent to intake. The percent of each group arrested, however, did increase substantially during this period; for experimentals, the percentage increased from 20 percent to 30 percent, and for controls, from 16 percent to 33 percent. Furthermore, there was no significant difference between the two groups in the mean number of arrests for the period; the mean number of arrests for experi-

mentals during the twelve months subsequent to intake was 0.517 and for control group members was 0.506 ($t<1$).[3] Experimental and control group members were arrested on charges of similar severity, and there was no significant difference between the two groups in the severity of the rearrest charges relative to the intake charges. Since there were no differences on any of the recidivism measures, one would not expect there to be differences between experimentals and controls on subsequent convictions. Nonetheless, an analysis was computed on subsequent conviction record, and as expected, it revealed no difference between experimentals and controls. Seventy-three percent of the population received no subsequent convictions, three percent received convictions on violations, 11 percent misdemeanor convictions, seven percent felony convictions, and six percent had open cases. Similarly, there was no difference between experimentals and controls on the number of subsequent convictions (of any type); the mean for experimentals was 0.332 and for controls 0.365.

The accumulated evidence consistently fails to show any CEP impact on recidivism.[4] The data presented in Chapter V suggested that

[3] It should be noted, however, that the relatively low recidivism rate for the control group (mean number of rearrests equals .506) makes it unlikely that one would find a significant impact of the program. The power of a t-test for a reduction in mean number of arrests from .5 to .4 (with alpha equal to .10 and standard deviation of 1.0) is .50. That is, under the conditions described above, one would have only a 50 percent chance of finding significant a difference between a control group with mean number of arrests equal to .5 and an experimental group with a mean of .4 arrests, where such a reduction represents a drop of 20 percent.

[4] In drawing conclusions about the impact of CEP on recidivism, it is necessary to consider the power of the statistical tests used in the research. The power of a statistical test tells how much confidence should be placed in the results, that is, how likely the test is to yield statistically significant results; power depends on the sample size and the magnitude of the expected effect.

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CEP also failed to affect employment, education, or general vocational activity during the period of the research. If one assumes a relationship between employment (or, more generally, vocationally relevant activity) and crime, then it is not surprising that CEP's lack of impact on employment is accompanied by a lack of impact on recidivism. If the rationale for pretrial diversion is to intervene in clients' lives to increase their stability, then CEP has failed to affect life stability. It follows then, if reduced recidivism is a desirable by-product of life stabilization, that CEP would not have an effect on recidivism in either the short or the long run.

BEYOND THE EXPERIMENTAL DESIGN: THE RELATIONSHIP OF OTHER VARIABLES TO RECIDIVISM

The data presented so far lead one to conclude that CEP had no impact on the diverted defendants. This, in turn, leads to two questions: (1) what factors *do* affect recidivism; and (2) is *anyone* within the experimental group affected by CEP in such a way as to

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For example, suppose that the recidivism rate in the untreated group (control) was 30 percent; one might predict that CEP would lower that rate to 25 percent,20 percent, or less. In the present study, to detect a reduction from 30 percent to 24 percent recidivism (using chi square and alpha of .10), for power of .80 (or 80 percent chance of detecting such an effect), one would need a sample size of 1300. The CEP research sample of 666 would give one an 80 percent chance of detecting a reduction in recidivism from 30 percent to 21 percent, and a 90 percent chance of detecting a reduction from 30 percent to 20 percent. While a reduction in recidivism from 30 percent to 20 percent (i.e., recidivism in the control group of 30 percent as compared to 20 percent in the experimental group) appears to be a substantial effect for a program such as CEP, it is not larger than those reported by the DOL-funded programs (Mullen, 1970). It is also approximately the size effect designated as "small" by Cohen (1977). Consequently, whether one considers statistical power a problem for the CEP research design depends upon what effect size is anticipated.

make him/her less likely to recidivate? Because the answers to these questions require analyses that go beyond the experimental design, it is not possible to draw inferences about the impact of CEP from them; they are useful, however, in understanding the population that CEP serviced.

Knowing that there were no significant differences between experimentals and controls on the likelihood of rearrest, a multiple regression analysis was conducted to determine which factors predicted subsequent arrest.[5] That is, using demographic characteristics, prior criminal history, and educational and vocational activity during the twelve months prior to the start of the research, we tried to predict who, among all the research subjects, was most likely to recidivate within the research period. The result of exploratory analyses was a regression equation with five predictor variables: the sex of the defendant; how much time his/her family had spent on welfare while the defendant was an adolescent (10-16 years old); conviction record prior to intake into the research; attendance at CEP (if the defendant was a member of the experimental group); and his/her educational level twelve months after intake into the research. Using these five variables, we obtained a multiple correlation of R=.40; in other words, by knowing the defendant's

[5] See Appendix E for detailed discussion of the method used.

characteristics on these variables, we were able to explain 16 percent of the variability in whether a defendant was rearrested. While this is not an especially impressive amount of variation to explain, the results are interesting.[6] Defendants most likely to be arrested subsequent to intake into the research were males whose family had spent some time on welfare during the time when the defendant was between the ages of ten and sixteen. The defendant was likely to have a conviction record before intake into the sample. If he was in the experimental group, it was likely that he did not attend CEP very often; and his educational level twelve months after intake into the research was likely to be low (relative to the other members of the research population).[7]

Also included as predictors in the initial regression analyses on recidivism were ethnicity variables (dummy coded) and age. Ethnicity was unrelated to recidivism: the two variables used were black/not black (correlated with recidivism r=.065, n.s.) and

[6] While 16 percent of the variance is not a large amount to be explained, in research on complex social phenomena such as this, it is not unusual. Furthermore, this analysis was the last in a series that began with many more predictor variables, and even with additional variables there was no increase in the amount of variance explained.

[7] The variables were entered into the regression analysis in the order presented because it was thought that this was the most likely order of their occurrence in life. The particular variables used in the final regression equations were determined empirically for each dependent variable. The analysis was begun with the same set of predictors for each of the recidivism measures; only those that accounted for a meaningful percentage of the variance were retained in the final analysis.

hispanic/not hispanic (correlated with recidivism, r=-.055, n.s.); as a result, the ethnicity variables were dropped from subsequent analyses. Age is correlated slightly but significantly with recidivism; (r=.164; p<.001);however, because of the method used to analyze these data (see Appendix E), other variables that explained common variance with age (e.g., sex, welfare status during teen years) entered the equation first, and age was eliminated from future analyses. The correlation between age and recidivism suggests that younger members of the research population were more likely to be arrested subsequent to intake into the research.

As would be expected from the results of the previous analyses, research status (i.e., experimental or control) did not predict recidivism. Interestingly, however, of the five variables in the analysis, the one with the strongest relationship to likelihood of recidivism was lack of attendance at CEP (r=.29).While we cannot conclude that attending CEP prevented the defendant from further criminal activity, one can infer from these data that those people who attended CEP more often possessed characteristics (perhaps unmeasured) that were related to being less likely to recidivate. A related analysis was conducted in which successful CEP participants were compared to unsuccessful (terminations and administrative discharges) participants on the likihood of rearrest. Successful CEP participants (those who attended most often) were significantly less likely to be rearrested than were unsuccessful CEP participants (χ^2_1=21.98; p<.001). Twenty-four percent of CEP dismissals were arrested subsequent to intake, as compared to 48 percent of the terminations and administrative discharges.

In addition to knowing *who* is most likely to be rearrested, we wanted to be able to predict the *number* of rearrests; the variables predicting number of rearrests were quite similar to those predicting whether one would be rearrested at all. Number of subsequent arrests was predicted by age of defendant; whether the defendant was born in New York City; the family's welfare status while the defendant was between the ages of ten and sixteen; number of arrests prior to intake into the research; attendance at CEP; and educational level twelve months after intake into the research. Using these six variables, we obtained a multiple correlation of R=.42; thus we were able to explain 17 percent of the variance in number of subsequent arrests. In particular, the defendants with the greatest number of subsequent arrests were older defendants born outside of New York; their families spent some time on welfare; they had arrests prior to intake into the research; they did not attend CEP very often (if in the experimental group); and they had a low level of education relative to the other members of the research population. As in the previous analysis, attendance at CEP had the strongest relationship to number of rearrests. It is, therefore, not surprising that successful CEP participants were rearrested significantly less often than were CEP terminations; this was true within four months after intake (χ^2_4=28.637; p<.001), as well as for twelve months after intake (χ^2_6=36.294; p<.001), and for the total period of the research. The number of arrests for dismissals and terminations during the period of the research is presented in Table VI-3.

Table VI-3

NUMBER OF REARRESTS DURING THE RESEARCH PERIOD

CEP Exit Status	None	One	Two	Three	Four or More	(Mean)
Dismissal (N=190)	76%	15%	5%	3%	1%	(0.379)
Termination (N=161)	51%	22%	13%	7%	6%	(1.099)

$\chi^2_8 = 31.551$; $p<.001$; $\underline{t}\ (349) = 5.391$; $p<.001$

So also among members of the control group, defendants who received favorable dispositions on the intake case (dismissal or ACD) had significantly fewer rearrests than those who received unfavorable intake case dispositions (i.e., convictions or warrants); the mean number of rearrests was .56 for the former and .95 for the latter ($\underline{t}(240)=2.436$; $p<.02$). Although these results are interesting, they are quite weak -- the magnitude of the difference between these two subgroups of the control group is quite small.

Data on number of subsequent arrests imply that the "boy scouts" in both the experimental and control groups are most likely to remain boy scouts, and the "bad guys" stay bad. The control group members who received favorable dispositions on the intake case, and those members of the experimental group who attended CEP most often were the persons least likely to recidivate, even without services. Since the people most likely to be successful in CEP were those who attended most often and had the fewest number of prior arrests, we can conclude that those defendants who were most likely

to be rearrested (and to be arrested more often), and therefore most in need of rehabilitation were the ones least likely to attend CEP or receive its services.

With some idea of which members of the population were most likely to be arrested (and how many times) subsequent to intake into the research, we wanted to determine which factors were related to the severity of the rearrest charges. With nine predictors a multiple correlation of $\underline{R}=.58$ was obtained; in other words, we were able to explain approximately one-third of the variation in severity of rearrest. While we were able to explain a more sizeable proportion of variance in severity of rearrest than for the other recidivism variables reported above, the nature of the relationships between the predictors and the dependent variable remains unclear.

The predictor variables in this analysis included demographic characteristics (sex of defendant and family life during adolescence); age at first arrest; school and job behavior during the first six months after intake into the research; and life-style one year after intake (i.e., marital status, enrollment in school, educational level obtained). Members of the population arrested on the most serious charges were likely to be male, from families that were intact during their teens, and they were likely to have been relatively young at the time of their first adult arrest. Those with the most serious rearrest charges were likely to have been enrolled in school during the first six months after intake, and to have held few jobs during those six months. In addition, during the next six months, they were likely to be unmarried, enrolled in school, and have a relatively low level of education. (While these last three variables imply that

these are relatively young defendants, age (the first variable entered into the equation) was unrelated to severity of rearrest charges. It is difficult to determine the reasons for some of these relationships; consequently, this series of analyses must be regarded as exploratory. It would be unwise to place much confidence in the replicability of these results since a large number of significance tests were conducted and it is likely that some spurious results were obtained.

A final regression analysis was conducted on the number of *convictions* subsequent to intake into the research. The results were similar to those for the other recidivism variables: a multiple correlation of $\underline{R}$=.40 was obtained, explaining 16 percent of the variation. The predictor variables were also similar: sex of defendant, welfare status during adolescence, prior conviction record, attendance at CEP, enrollment in school twelve months after intake, and educational level. Male defendants were likely to have more subsequent convictions than were females; those with more convictions were likely to have come from families who spent some time on welfare, and to have a conviction record prior to intake into the sample. If in the experimental group they were likely to have attended CEP infrequently (if at all), and twelve months after intake, they were likely to have a relatively low level of education and were unlikely to be enrolled in school. It is important to remember that we cannot infer causation from these results; that is, one cannot validly assume that *because* these defendants were not in school and did not attend CEP they received more convictions. More likely, there are other external factors

that cause some defendants to drop out of CEP, to drop out of school, and to continue to be convicted for crimes subsequent to intake into the research. As would be expected, successful participants in CEP received fewer convictions and if convicted, were convicted on less serious charges than were unsuccessful CEP participants. (See Tables VI-4 and VI-5.) Similarly, among control group members, those defendants whose intake cases were favorably disposed were less likely to receive a subsequent conviction (χ^2_4=11.382;p<.023), and if convicted were convicted on less serious charges than were those who received unfavorable intake case dispositions.

Table VI-4

SUBSEQUENT CONVICTION RECORD
DURING THE RESEARCH PERIOD

CEP Exit Status	Type of Charge				
	None	Viol.	Misd.	Felony	Open Case
Dismissal (B=196)	86%	4%	4%	3%	3%
Termination (N=161)	60%	2%	16%	11%	11%

χ^2_4=42.260; p<.001

Table VI-5

NUMBER OF SUBSEQUENT CONVICTIONS
DURING THE RESEARCH PERIOD

CEP Exit Status	None*	One	Two	Three or More
Dismissal (N=196)	89%	7%	3%	1%
Termination (N=161)	71%	17%	9%	3%

$\chi^2_4 = 19.860$; $p < .001$

* Includes those with open cases.

The results of these two sets of analyses support the notion that the "good guys" remain good guys: successful CEP participants and control group members whose intake cases were dismissed, were less likely to receive subsequent convictions than were unsuccessful CEP participants and those members of the control group who received unfavorable intake case dispositions.

The relatively high multiple correlations obtained in the exploratory analyses on recidivism reported above suggested that if the good predictors of recidivism were statistically controlled, it might be possible to detect an effect for CEP. That is, after explaining the effects on recidivism of age, sex, family life during teens, etc., a difference between experimentals and controls might emerge. To test this hypothesis analyses of covariance were computed on each on the recidivism variables using age, sex, ethnicity, family welfare status, and number of prior arrests as covariates.

None of these analyses, however, produced significant differences between experimentals and controls. Additional analyses were computed to determine whether CEP had a differential effect for older compared to younger individuals, for members of different ethnic groups, or for those with different numbers of prior arrests. None of these interaction variables was significantly related to the recidivism variables.

The results of the recidivism analyses taken as a whole suggest that, despite the pretrial diversion rationales, those members of the research population who were diverted to CEP were not subsequently arrested for any fewer or less serious crimes than were control group members. The best predictors of recidivism seem to be aspects of individuals that predate their entrance into the program: age, sex, family life during adolescence, welfare history, prior criminal experience, etc; diversion to the Court Employment Project seems to have no additional impact.

There are several possible explanations for this lack of effect, ranging from methodological (i.e., lack of statistical power) to characteristics of the program or the population. If the research did not find significant effects because of a lack of statistical power, then future research will have to use larger samples or perhaps follow-up diversion participants for longer periods; both these tactics, however, are difficult to implement. It is also possible that CEP really did not have an effect on this population's rate of recidivism, employment, or lifestyle. This lack of effect could be due to the nature of the program or its setting:

four months may be too short a time to affect someone's life; the approach used by the counselors may be ineffective or inappropriate; its location in a nonpunitive criminal justice system may make it impossible to provide the clients with real alternatives. In addition, the lack of program effects may be a function of characteristics of the client population in relation to the nature of the program. The impact of program efforts confined to a few hours a week for four months may be miniscule in relation to the impact of sixteen or more years in a ghetto environment, adult role models who are far from "straight," and the lack of opportunity in the labor market. Which of these reasons (or interactions among them) are responsible for the lack of effects found in this research cannot be determined from these data.

CHAPTER VII

THE COURT EMPLOYMENT PROJECT: CONTINUITIES AND CHANGE

INTRODUCTION

On January 31, 1979, the Court Employment Project initiated a major change in the method by which it selects clients for social services. While maintaining its general structure as a social service agency and continuing its commitment to individuals in the criminal justice system, CEP announced it would no longer accept defendants referred under the traditional pretrial diversion model. This decision (the only exception to which was the Borough of Queens, discussed below) resulted from a long and still on-going dialogue among the agency's Board of Trustees, its management and staff. The findings of the Vera Institute evaluation were the initial focus of their discussion.

> We must respond in an intelligent and non-defensive manner to the Vera study. We've operated under certain premises; the study calls into question some of these premises and suggests a radical change. We understand the mandate of the Board is to move out of diversion and develop viable alternatives. (From a memorandum from the CEP Director to the CEP Board of Trustees, December 14, 1978.)

CEP continues to operate, as it has since 1971, under a basic contract with the Human Resources Administration (HRA) of the City of New York to provide information, referral and social services to clients from the city's criminal justice system. CEP is an ongoing agency that has utilized the difficult process of evaluation to en-

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courage an internal reassessment of its decade-old operating principles and to stimulate experimentation. The current direction of that effort seems less the search for new goals than the development of new ways to achieve traditional ones: namely, to provide social services to a population that has little access to such resources and, in so doing, to use service delivery as a mechanism to intervene in the way clients are treated by the criminal justice system. The Vera Institute evaluation helped the agency recognize that, within the context of the current New York City criminal justice system, it was not achieving these goals through pretrial diversion.

The purpose of the last chapter of this report is to describe the process of CEP's change --- the why and how of its decision to abandon pretrial diversion -- and to explore the direction these changes have taken. While recognizing that CEP will continue to change for some time, we attempt to examine where they are heading and to interpret the agency's changes within the context of the pretrial services movement of the last decade.

THE PROCESS OF CHANGE

The Evaluation

It is difficult to begin a discussion of CEP's decision to abandon pretrial diversion without describing the relationship between the Vera Institute research and the agency. In earlier chapters we indicated that the evaluation was conceived and implemented with the full cooperation of CEP's management. While no evaluation as lengthy

and large in scale as this could be without tensions, two factors facilitated the resolution of day-to-day problems. First, CEP's management was committed to pursuing the research design (uncompromised) to its completion. Second, the research staff was committed to adjusting data collection activities wherever possible (without damage to the basic design) to the agency's operational needs, and to provide information from the research on an on-going basis.

In one way, however, both the length and scale of the study facilitated CEP's desire to use its results. The preliminary findings unfolded slowly but cumulatively, giving agency staff the opportunity to see how one piece of data fit into the next, and how one data source verified another. The long time frame of the research also gave them time to think about and digest the results.

The Issues

By September 1978, the research data had been processed sufficiently to secure the basic findings of the study. The research staff presented these to CEP's Board of Trustees, and in light of the data, the agency's Board and management initiated a formal process of examining CEP's structure, objectives and rationles. For the most part, the discussion focused on the study's disposition data: despite statistically significant differences in the dispositions of the experimental (diverted) and control groups, the agency was not "diverting from prosecution" in a large proportion of the cases, it was not diverting from criminal convictions, and it was rarely "diverting from incarceration" or serious punish-

ment (see Chapter IV). Yet the agency had a long-standing commitment to provide social services and information and referral to defendants in the criminal courts, and to have those services affect case disposition. Whatever CEP's dispositional impact in earlier years, the Vera research data suggested that, by the mid-1970's, it was minimal. These results undermined a fundamental pretrial diversion rationale on which the agency had rested comfortably for a decade.

CEP's interpretation of the data was inescapable. Though the program was structurally independent of the prosecutor, real independence was illusory. So long as selection of cases depended upon deferred prosecution, CEP's diversion efforts were, in effect, an extension of the prosecution. The structural and ethical implications were distrubing to CEP. Because prosecutors wanted selection to take place soon after arrest, without the lengthy process disclosing the facts of the case, defendants and their counselor could never be truly informed as to the dispositional alternatives to diversion. In addition, despite the availability of other, less intrusive diversionary mechanisms, prosecutors were diverting defendants to CEP they would have treated leniently anyway, including those they would have diverted without supervision. Even CEP's attempts to control the dispositional use of the program by changing its intake criteria were unsuccessful (such as its shift to a felony-only policy in 1977), because CEP was not structurally able to control individual selection decisions. Consequently, while other important issues emerged from the research findings, CEP's central concern was focused on pretrial diversion as its method of client selection.

As reported in Chapter V, the Vera research also indicated that CEP's efforts to affect the vocational behavior of its clients were without measurable impact. Although the lives of both diverted and nondiverted research subjects improved over the one year follow-up period, the two groups improved equally. CEP's staff felt these findings were the result of several related problems faced by the agency. Paramount were the management and service delivery problems stemming from the agency's rapid expansion in the early 1970's, and its sudden contraction in 1976 because of the city's fiscal crisis (see Chapter II). However, most staff also felt that, by mid-1978, these problems had been successfully confronted by the agency's new management.[1] Consequently, many at CEP did not believe the research findings on service delivery reflected the service operations of the agency as of 1979.

The research findings did, however, generate discussion within the agency about the *constraints* on its service delivery system resulting from CEP's reliance on pretrial diversion for selecting clients. First, many believed the agency's deeply disadvantaged and youthful client population needed more extensive and intensive ser-

[1] In early 1977, CEP's Board appointed a new agency director to oversee the reopening of CEP. A lawyer, familiar with New York City's criminal justice system, he hired a new Social Services Director later that year to work with the service staff. She had considerable experience in employment and training programs and in the delivery of educational and other services to traditionally disadvantaged urban populations. She in turn became CEP's Director in August 1978. In both capacities she helped expand in-house services, streamline administration, and improve communication and supervision within the agency.

vices than the agency could generally provide under pretrial diversion. Since, as the research demonstrated, most diversion clients would not have received intrusive levels of criminal justice supervision in CEP's absence, CEP could not justify imposing longer and more intrusive service intervention for clients selected under pretrial diversion. Second, youths accepting diversion services because they wanted a dismissal of the charges against them may not have been motivated to utilize those services fully, particularly if they became aware that the court was not typically punitive in such cases. This does not necessarily suggest that the New York City criminal justice system is insufficiently punishing. Most of these youths were 16 and 17 years old, arrested as adults,but not always for activities warranting a felony arrest or prosecution (see Chapter IV). While most were not "Boy Scouts and Virgins," it is also not clear their arrests represented deepening criminal involvement: 60 percent had never been arrested before, and 70 percent were not arrested again within a year after diversion to CEP. For these youths at least, the requirement that they accept social services as a condition of charge dismissal may have been distasteful (despite their need for help with jobs, school and home), and may have exacerbated their already well developed alienation from formal institutions such as schools, employers, welfare, and "helping" organizations.

As a result of CEP's interpretation of the Vera research findings on its service impact, the agency's management and staff took the position that, despite the findings, the general types of ser-

vices CEP traditionally provided were appropriate for the criminal justice system clients it served. Therefore, the agency did not change the *nature* of its services in response to the Vera evaluation, although it did attempt to strengthen the *quality* of its overall service effort by improving supervision and by increasing the number and type of services offered. Consequently, the agency's management and board defined the central issue emerging from the research as client selection -- that is, alternatives to pretrial diversion.[2]

> Basically, going on the belief that the agency does provide service, it was felt that the Project's operation must be closely examined to determine where the agency has impact; to rethink the agency's goals and objectives; to update the rationales for the agency's program; to explore shifts in focus that might highlight where the agency is most effective. Specifically, it was mentioned that the agency might do well to draw away from a view of itself as a diversion program...(minutes, CEP Board of Trustees, 14 September 1978.)

The Decision To Change The Mechanics Of Client Selection

CEP's process of re-thinking pretrial diversion intake initiated a period of extensive informal discussion with various key actors in the criminal justice system, including defense counsel, prosecutors, and judges. In the course of their dialogue, it became apparent to CEP that most of the system actors with whom it typically dealt wanted the agency to continue "doing diversion," although their reasons varied considerably.

[2]Interestingly, the research data that suggested no impact on recidivism (Chapter VI) did not generate as much discussion in the agency as the other major findings. There were several reasons. First, there was general skepticism in the agency that short-term social services could have much, if any, impact on individuals' criminality, particularly for those in a "high risk" age category

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Despite the relatively small number of cases CEP had typically diverted from the criminal courts (see Appendix B, Table I), prosecutors with whom CEP worked wanted a formal pretrial diversion option. Since these prosecutors controlled the selection of cases, it was not surprising that they did not want to lose the flexibility pretrial diversion offered. Obviously, therefore, the research data showing a large proportion of dismissals and non-criminal convictions in the disposition of control group cases did not undermine *prosecutors'* beliefs that CEP diversion was an appropriate disposition for the cases they selected. The logic of this position was noted in Chapter I. Since resources are always limited, some defendants prosecutors "would like" to prosecute will be passed over in favor of other (possibly more serious, but for whatever reason, more pressing) cases. If resources expand (e.g., pretrial diversion), prosecutors are able to increase the number of cases over which they exercise some control. This does not, however, automatically mean greater leniency for defendants.

Under these conditions, one might assume defense attorneys would oppose pretrial diversion. While some did, many did not, and their rationales were complex. Some defense attorneys (largely Legal Aid lawyers) told CEP that they did not think prosecutors'

Continued...who live in environments where criminality is common. According to one person, "criminality is a little like the measles; it will pass." Second, despite the arrest rates for youths in New York City, the population of defendants prosecutors' referred to CEP had about a seven out of ten chance of not being rearrested within a year, according to the research. Consequently, many in the agency did not consider impact on recidivism to be a particularly important issue for clients selected by pretrial diversion.

"diversionary" approach to handling young criminals in New York City would last. They feared that, with increased public pressure, prosecutors would become more punitive and limit their use of non-supervised diversion and non-criminal sanctions. These defense lawyers wanted CEP's social service resources to remain available, specifically in the form of a formalized diversion program. Other defense attorneys took the position that since CEP had *some* impact on the dispositions of *some* cases (no matter how few), it was a useful added resource to help maximize the likelihood of dismissals. Finally, some felt the mere presence of CEP in the courts encouraged an "atmosphere" of leniency and that, without CEP, that atmosphere might be eroded.

There was considerable support for these defense positions within CEP especially because the staff had a specific commitment to using services to affect case dispositions. In addition, time and the city's fiscal deterioration meant CEP was virtually the only remaining agency giving direct services specifically to the court population. Consequently, few in the agency wanted to abandon CEP's traditionally linked goals: service delivery was considered a means to affect case disposition (through pretrial diversion), and affecting case dispositions was considered a means to attract individuals who needed social services. Yet the evaluation findings could not be ignored: CEP's pretrial diversion was controlled by prosecutors, and within New York City's expanded diversionary system, informed consent and *real* impact on case outcomes were difficult to assure. Under such conditions, the likelihood of diversion services having

a significant impact on dispositions was extremely limited. However, since the staff believed there were many potential clients within the criminal justice system who had both legal and social service needs, the agency began to seek new ways to combine its traditional interests.

Options were available. Because CEP was not funded through the criminal justice system and thus not financially dependent on its status as a "pretrial diversion" agency, it was free to experiment with alternatives. Furthermore, it enjoyed a generally good reputation within the criminal justice system for giving services and being "responsible," and it had more than ten years of exposure to the interests, goals, and needs of various parties in the court. Finally, it had recently expanded its service capacity and was prepared to provide services both more intensively and for longer periods of time. This flexible structure and "insider-outsider" position was conducive to experimentation. While discussions about continuing pretrial diversion went on within the agency, staff began testing alternative approaches for identifying criminal justice system clients.

During late 1978, CEP initiated several small scale, experimental programs. Some involved an attempt to use social services to affect case disposition without relying on pretrial diversion. Others focused on criminal justice system clients who had pressing social service needs but whose relationship to CEP would be fully "voluntary," that is, they would derive no legal (case-related) gain from participation.

To identify such "voluntary" clients, CEP began to develop relationships with the 41st Police Precinct, the Department of Probation, the State Division for Youth (which is responsible for incarcerated juveniles) and other agencies involved with youth for whom social services might be particularly timely (e.g., those being released from a state training school or the city's juvenile detention facility, or those nearing the end of a juvenile probationary period for whom the next arrest would be as an adult). While the CEP's experience with the criminal justice system indicated there were many such youths, its exploratory efforts to provide services led the staff to conclude that CEP could not base its operations *exclusively* on providing services to these clients.

Several things contributed to this perspective. Some involved the agency's funding sources and the service mandates connected with them, and others involved the agency's traditional commitment to intervention in disposition. As important, however, was the staff's growing belief that a completely "voluntary" client population was not organizationally feasible. The' experiences with these youngsters suggested that they were generally angry, wary of "helping" agencies (particularly if connected to "the courts"), and afraid of organizations located outside their narrowly defined but "safe" neighborhoods. Since CEP was not a local community-based service organization, the staff felt it would require extremely vigorous outreach to encourage these youths' initial and continued partic-

ipation in CEP. While the agency had always done considerable "outreach" with its court-related clients, its experiences during late 1978 and early 1979 suggested that the amount and cost of outreach needed to sustain even a small client caseload would be greater than could be justified. CEP concluded, therefore, that while such efforts should be a component of the agency's operations, they should be secondary to other activities.

In their exploration of alternative modes of operating, another pilot effort seemed far more promising as a main direction for the agency's future activities. CEP initiated a post-plea "alternatives to incarceration" program. With the cooperation of a Bronx Criminal Court judge, the agency began to offer a six-month period of social services as an alternative sentence to defendants selected by the judge (with the agreement of the defense attorney and defendant) who had taken misdemeanor pleas and were facing jail sentences of no less than 90 days. This program will be discussed in detail below because it has become an important part of CEP's recent activities.

After careful consideration of the Vera evaluation and the early results of its exploratory efforts at alternative intake methods, CEP's Board of Trustees met and voted on the issue.

> The Board determined to make its intention clear on the question of diversion. A motion was made and seconded and it is hereby
>
> RESOLVED that Court Employment Project, Inc. shall herewith not accept clients from the criminal justice system on a formal pretrial diversion basis as it has, historically, done. (Minutes, CEP Board of Trustees, 12 December 1978.)

In a January 1979 letter to prosecutors, judges and the Legal Aid Society, the agency formally announced that,

> CEP will accept referrals from any source in Criminal and Supreme Court (e.g., judges, defense attorneys, district attorneys, and other court personnel) at any point during the court process. Only with the agreement of the client will we issue reports to the referring source, and we will no longer work as a negotiator between specific parties as we historically have done in order to insure a formalized diversion process in court.

In effect, this policy initiated two major shifts in CEP's formal relationships with the criminal justice system. First, CEP shifted away from its previous relationship with the prosecution by encouraging closer direct ties with the *defense*. CEP felt that once defense attorneys identified clients likely to be convicted of crimes and possibly sentenced to jail, social services could be useful in encouraging leniency either by the prosecutor or judge. CEP's involvement is generally pretrial in these cases but occurs most often without the involvement of, or any prior agreement from, judges or prosecutors (CEP reports on defendants' program participation go directly to the defense attorney). The decision how to use CEP's services in the legal interests of the defendant is entirely at the discretion of the defense attorney, and CEP offers no recommendation as to case outcome. Second, to secure clients for whom services could be useful both directly and through their impact on *sentence*, CEP began to develop direct relationships with *judges* who agree to allow a defendant's participation in CEP services to affect sentencing.

Before discussing these shifts in greater detail, it is important to note CEP's temporary exemption of Queens from its decision to abandon pretrial diversion. The agency's experiences in Queens highlight the factors involved in CEP's recognition of the inherent difficulties with pretrial diversion in New York City.

Although CEP had diverted Queens' defendants since 1974, the number of cases had always been small, sufficiently so that the borough was excluded from the Vera experimental design. The number of cases from Queens began to expand in 1978 and CEP's records on them suggest that the historical process resulting in prosecutorial acceptance of non-supervised diversion (such as the ACD) *might not* be as widespread in Queens as in Manhattan, Brooklyn and the Bronx.[3] Because of an excellent relationship with a key assistant district attorney, CEP staff felt they were beginning to get "good" diversion cases from Queens (i.e., those that would not otherwise be dismissed or ACDd). However, in mid-1979, this assistant district attorney was

[3]The evidence is not strong but it is suggestive. The types of cases diverted and the outcome of cases rejected because the ADA felt they were "too serious" for diversion do not seem to differ in Queens as compared to the other boroughs in which CEP operated. However, there seems to be a difference in what happens to the cases of diverted defendants who were *unsuccessful* in the program. Somewhat more of the diverted defendants from Queens who were unsuccessful (i.e., "program failures") are subsequently convicted than were unsuccessful participants from the other boroughs (those in the Vera "experimental" group). While there are several possible explanations, the data cannot rule out that Queens prosecutors may view the *same* charges as *more* serious than do prosecutors in other boroughs, and thus deal with these defendants more harshly (i.e., prosecute them). This possibility temporarily dissuaded CEP from stopping diversion in Queens at the time it did so in the other boroughs; it has not yet made a final decision.

transferred, and CEP staff began to feel less sure about the quality of the cases being diverted. At the same time (as described below), CEP felt it was having considerable success in other boroughs influencing cases in which serious outcomes were more likely. As a consequence, CEP is now considering whether to conduct a small research effort in Queens (using the experimental approach of the Vera Institute evaluation) to determine the case outcomes for a control/overflow group, or whether to abandon pretrial diversion altogether.

THE MECHANICS OF CHANGE

While CEP is currently in flux, changes are occuring within the context of a relatively stable organization. In Fiscal Year 1978-79, CEP was budgeted by HRA for $1.1 million; in Fiscal 1979-80, it was again budgeted by HRA at the same annaul level. In addition, it has secured $900,000 from CETA and from several private sources to provide services not covered by the HRA contract (e.g., an in-house tutoring program). In Fiscal 1978-79, CEP serviced 3,000 clients overall; it is already servicing clients at the same rate in Fiscal 1979-80. In the first five months of Fiscal 1978-79 (July through November 1978), CEP accepted about 75 "court related" clients per month for services, virtually all of whom were pretrial diversion cases. In the last five months of Fiscal 1978-79 (February - June 1979), after ceasing diversion intake except in Queens, CEP was accepting about 78 "court related" clients per month for services; however, only 42 percent were diverted (all from Queens).

Selection and Intake

While multiple sources of criminal justice system clients are not new for CEP, until 1979 the majority of its clients were diverted pretrial. Since early 1979, however, CEP has expanded the ways in which it selects clients and substantially reduced the proportion of diverted cases. As shown in Table VII-1, CEP currently has two major categories of clients. *Court-related clients* are referred to CEP while their cases or sentences are pending in either Criminal (misdemeanor) or Supreme (felony) Court. CEP's goal is to provide social services to affect both their personal and their legal needs. Some of these clients are still diverted to CEP pretrial (42 percent) and CEP reports directly to the Queens prosecutor (line A1 in Table VII-1). Others are referred pretrial primarily by defense attorneys (37 percent); while some of these are referred by judges or other court officers, CEP reports only to the defense attorney (lines A2 and B1). Finally, 18 percent of its court related clients are referred post-plea, six percent by judges as part of a formal Alternatives to Incarceration Program (line A4) and 12 percent by defense attorneys when they believe a custodial sentence is likely (lines A3 and B2). In the former cases, the program reports directly to judges who have agreed to non-custodial sentences on condition that defendants participate successfully in CEP; in the latter cases, the program reports to either the defense counsel or the judge (depending upon the particular circumstances), but the judge has not always made a formal sentencing commitment. CEP also provides services to a few defendants who were diverted out of state but who live in New York City (two percent, line C).

TABLE VII-1

COURT EMPLOYMENT PROJECT CLIENTS BY TYPE OF INTAKE AND PRIMARY PERSON TO WHOM AGENCY REPORTS

February 1, 1979 - December 13, 1979

	Number	(%)	(%)
TOTAL NUMBER OF CLIENTS	2,869		(100%)
COURT RELATED CLIENTS (Person Reported To)	744	(100%)	(26)
A. Cases In Criminal Court-Misdemeanors	663	(89)	(23)
1. Pre-plea/Queens Diversion (Prosecutor)	313	(42)	(11)
2. Pre-plea (Defense Attorney)	261	(35)	(9)
3. Pre/Post Plea (Defense Attorney)	48	(6)	(2)
4. Post-plea/"Alternatives To Incarceration Program" (Judge)	41	(6)	(1)
B. Cases In Supreme Court - Felonies	65	(9)	(2)
1. Pre-plea (Defense Attorney)	18	(2)	(1)
2. Post-plea (Judge)	47	(6)	(2)
C. Out-Of-State Diversion Cases (Referring Agency)	16	(2)	(1)
OTHER CRIMINAL JUSTICE SYSTEM CLIENTS	2,125	(100%)	(74)
D. Information and Referral Only	1,475	(69)	(51)
E. Direct Services	650	(31)	(23)
1. Summer Youth Employment Program (CETA)	420	(20)	(15)
2. Youth Employment Training Program (CETA)	50	(2)	(2)
3. Friends & Families of Other Clients	105	(5)	(4)
4. Spofford Referrals	39	(2)	(1)
5. 41st Police Precinct Referrals	22	(1)	(1)
6. Division For Youth Referrals	14	(1)	(*)

*less than .5%

The second category of clients also involves *criminal justice related individuals* but they do not anticipate obtaining a *legal* advantage from their participation. CEP's efforts, therefore, are primarily social service intervention in these clients' immediate life circumstances. A majority (69 percent) are people to whom CEP staff give information and referral, but no direct services; that is, they do not typically see the agency's counselors or make multiple visits to CEP. These clients,often friends and relatives of defendants, are generally identified by CEP court staff in the Criminal Courts and most contacts with them are short-term.

The remaining 31 percent of the criminal justice related clients receive direct services from CEP's counseling staff. They are either referred by other CEP clients or by criminal justice agencies, such as the Division for Youth (which oversees juveniles and youth incarcerated in the state), Spofford (the City's secure juvenile detention facility), Probation, or the Police Department. Twenty percent are CETA eligible youths CEP places and supervises in temporary jobs as part of its annual Summer Youth Employment Project contract with the New York City Department of Employment; while not all are "criminal justice-related," most are, and all would be considered "high risk" youths. Finally CEP has 50 individuals (2 percent) in their in-house CETA sponsored Youth Employment Training Program.

Though all their clients are important, CEP *court-related clients* are of most interest. They receive the bulk of CEP's service effort,

and they are the heart of the agency's current attempts to be of use to and of influence on the criminal justice system. Referrals from other justice system agencies are interesting potential sources of clients.but, for the reasons noted above, they are small-scale efforts.

Social Services For The Defense: CEP's Relationship With Defense Counsel

Thirty-five percent of CEP's court-related clients come to the agency pretrial from the Criminal (misdemeanor) Court (Line A2 in Table VII-1). Generally, according to CEP, defense attorneys are seeking more lenient dispositions for their clients (e.g., a conditional discharge rather than an expected probation sentence, or an ACD rather than a conviction), but generally do not anticipate a custodial sentence even without CEP's intervention. In these cases, CEP reports on the defendant's progress in the program only to the defense counsel. Occasionally, CEP will report directly to a judge (with the agreement of defense counsel) if the judge has formally endorsed the court papers that a CEP report is expected and, if favorable, a particular disposition is appropriate.

Another six percent of CEP's court-related cases also come from the Criminal Court (and generally are referred by defense attorneys), but CEP is typically not involved until after a plea has been entered and the defendant awaits sentencing (line A3, Table VII-1). In these cases, defense attorneys tell CEP they are uncertain whether the judge will sentence the individual to jail, so they want to encourage a non-custodial sentence (e.g., probation) through CEP participation. Again, in most cases, CEP progress reports go directly to defense counsel, although on occasion a judge may be involved directly.

Finally, CEP obtains a few court-related cases (2 percent, line B1) pretrial in the Supreme Court. Whereas many are referred by defense counsel, they also may be referred by parole or probation officers, judges, or other court officers. Generally, as with post-plea cases from Criminal Court, defense attorneys tell CEP they are trying to encourage non-custodial sentences.

CEP's general policy is to accept *any* defendant referred for services if the defendant is willing to participate. The only exceptions are people the agency has traditionally excluded because its services are not appropriate (i.e., those who are emotionally disturbed or deeply involved with drugs or alcohol). After CEP's court liaisons have been contacted by defense attorneys, they tell defendants about the program's services, what is expected of clients, and elicit defendants' interest. While the liaisons generally find out about defendants' pending cases and the dispositional advantages attorneys hope to achieve through CEP participation, the information is obtained from the attorney; CEP's liaisons do not discuss either the case or its potential outcome directly with defendants.

These procedures represent a considerable departure from pre-trial diversion. Often the court and the prosecutor do not know of CEP's involvement with the defendant at the intake stage, which may be at any point in the adjudication process. Furthermore, CEP's procedures for reporting defendants' progress in the program to those involved with the court case is quite different from the pre-trial diversion process. After intake but before defendants' next

appearances, CEP provides defense counsel with written (or oral if time is short) reports if they want them (which is almost always). Defense counsel may, at their discretion, introduce the reports into the court record. CEP's court liaisons are also available to discuss the reports and the clients in court *if* requested to do so by the defense attorney. Such requests are frequent, and CEP liaisons take an active advocacy role on behalf of the client. Defense attorneys may use CEP's reports as a basis for requesting further adjournments for services, or in their negotiation over pleas and sentences. However, since CEP no longer makes formal recommendations to the court concerning dispositions it has no direct involvement in structuring the outcome of cases. While CEP is not *formally* involved in the plea or sentence negotiations, management and court staff report actively working with defense counsel to encourage referral of cases in which defendants face serious outcomes -- criminal misdemeanor or felony convictions and punitive sentences -- and in which social services are likely to be an effective tool of intervention.

It is particularly difficult to assess CEP's actual impact on the disposition of cases in which CEP is providing social services on behalf of the defense. According to the agency, these clients are as youthful as earlier diversion clients (half are 16, 17 and 18) and have similar social and personal characteristics. This suggests (though it does not prove) that they may not face particularly punitive treatment, since in recent years, the New York City criminal justice system has tended to "divert" youthful adult defendants from full prosecution and, if prosecuted, not to impose jail sentences,

unless the charges are particularly serious or their prior records long. While CEP's staff believes these recent non-diversion clients have longer prior records than did diversion clients, no systematic data are available. However, some of them (those referred either post-plea in Criminal Court or pre-plea in Supreme Court) are slightly older than those referred pre-plea in Criminal Court which may mean they have a longer prior record. Since those drawn from Supreme Court have also been indicated, the charges against them are likely to be more serious. With these two groups of cases, therefore, (eight percent of all the court-related cases and 21 percent of those where CEP's relationship is primarily with the defense attorney), CEP's clients may be facing more punitive outcomes. Nevertheless, there is no way to demonstrate satisfactorily that CEP has an impact on the final disposition.

While CEP actively encourages defense attorneys to use CEP's services primarily for "more serious" cases where lenient dispositions are in doubt, it is difficult for the agency to insure this occurs. So long as it maintains the stance that its *major* concern is to provide services and that services should be available to anyone referred from the courts, CEP cannot make *formal* screening decisions based upon its own legal assessment of cases.[4] While this

[4]It does, however, attempt to find alternative services in situations where CEP believes a case referred to it will be ACDd or dismissed without intervention.

means that the agency faces the same problem it did under pretrial diversion (it does not directly control intake), CEP's response to the problem is quite different under current operating procedures. Unlike pretrial diversion, CEP's formal relationship is with the defense, and it is the defense attorney (not the agency) who negotiates the case outcome. As important, its clients no longer waive any legal rights by participating in CEP, and the normal adversarial process proceeds despite CEP's intervention. Thus the agency does not feel as responsible as it did operating under diversion if the case outcome is not as favorable as the client or the attorney desired, or if the nature of the outcome suggests CEP's intervention was not necessary.

Alternatives To Incarceration: CEP's Relationship To Judges

A major early rationale for pretrial diversion was to help selected defendants avoid harsh sentences, particularly incarceration. Initially, some diversion agencies (including CEP) had a formal relationship with the judge rather than the prosecutor, though structural ties to the prosecutor are now more common for diversion programs. However, as suggested above, few youthful adult defendants in New York City receive such sentences unless their prior records are lengthy or the charges very serious, and few of those actually facing jail sentences were being diverted to CEP. The literature on other pretrial diversion programs suggests this is not uncommon. To use Joan Mullen's phrase, the early "dilemma of diversion" has been resolved most often by diverting

defendants not facing jail or serious sanction. Nevertheless, many pretrial diversion programs, and certainly CEP, continue to cherish the idea that social services should be a resource for defendants facing incarceration.

Faced with the challenge of designing alternatives to diversion, CEP wanted to develop an experimental program that would divert from an explicit jail sentence. Since the role of the judge would be crucial, CEP sought to establish a direct reporting relationship with Criminal Court judges who were willing to sentence selected individuals to six months of CEP participation in lieu of a misdemeanor jail sentence. Since the agreement of defendants and their counsel are obtained after plea negotiations (in which CEP plays no role) but before pleas are recorded, the role of the prosecutor is minimal.[5]

This experimental "Alternatives to Incarceration Program" began in late 1978 with a small number of defendants selected by a single judge in the Bronx. It expanded during 1979 to include Criminal Court judges in Manhattan and Brooklyn; Queens judges began participating in late 1979. In the first ten and a half months following CEP's decision to move away from diversion intake, 41 defendants were selected for the Alternatives program (six percent of CEP's court-related clients, Line A4 in Table VII-1).

[5]Unless the charge against the defendant is still a felony (in which case the prosecutor must agree to reduce the charge to a misdemeanor for a jail sentence that is less than a year), the sentence to CEP is entirely at the discretion of the judge.

According to CEP, defendants potentially eligible for the Alternatives Program are initially identified by the judge (two-thirds) or the defense attorneys (one-third), generally at the point in plea negotiations when the prosecutor, defense counsel and judge are discussing a plea including a custodial sentence. There are only two formal eligibility criteria: defendants may not be drug or alcohol abusers or emotionally distrubed, and they must face a sentence of no less than 90 days. CEP's court staff discuss the program's services and requirements with the defendant and, if acceptable, the defendant signs a general "contract" to participate. The conditions of participation are: attendance at scheduled service and counseling sessions; cooperation in the design of a mutually agreed upon vocational and educational program plan; fulfillment of that program plan; avoidance of substance abuse; no arrest and conviction; and permission for a counselor to make a home visit. The agreement also specifies the consequences of non-participation: termination from CEP and re-sentencing for a specified amount of jail time. Only after this agreement is signed does the defendant take a plea. The plea is accepted by the judge who enters an endorsement on the court papers indicating the amount of jail time to which the offender will be sentenced if he or she is disassociated from CEP. The case is then adjourned for four weeks.

After this initial period, CEP's court staff sends the judge and defense counsel a report based upon a CEP counselor's summary of the defendant's progress. If CEP considers participation satis-

factory, it requests the judge grant a five month additional adjournment for the defendant to continue CEP participation. At the end of this longer period, CEP again sends a report to the judge indicating that the defendant has successfully completed participation; the defendant is then unconditionally discharged. If the individual fails to cooperate at any time during the five months, CEP requests that the case be advanced on the calendar and the judge sentences the defendant to the agreed upon jail term.[6]

It is difficult to assess the dispositional impact of CEP's services to participants in the Alternatives to Incarceration Program, although it appears that those who succeed avoid jail terms. While the charges vary from robbery and assault to criminal possession of a controlled substance and even petit larceny, CEP reports that these defendants all have substantial prior arrest and conviction records which make custodial sentences likely;[7] they also tend to be older than CEP's other court-related clients (their median age is 20). Half had a three month jail alternative to CEP entered on the court papers by the judge at the time they took their plea.

[6]CEP has recently established this procedure. Previously, the judge gave the defendant a conditional discharge after the first CEP report, the condition being five more months of CEP participation. CEP did not like the earlier arrangement for several reasons. First, CEP sometimes had a difficult time getting court clerks to return a conditional discharge to the calendar when the condition had been violated. Second, the defendant had a right to request a court hearing on the violation; while this never happened, CEP wanted to avoid such hearings because they might discourage judges' use of the Alternatives Program. Third, judges indicated they wanted the opportunity to talk with defendants in court after they had successfully completed the program.

[7]Although CEP's records are not complete, all eight defendants who successfully completed the CEP Alternatives Program on August 29, 1979 had prior records: two had four prior convictions; two had three prior convictions; three had two prior convictions and one had two outstanding warrants.

CEP reports that 47 percent of the first 32 defendants in the Alternatives Program completed the six month period successfully; 53 percent were not-successful (generally because they failed to attend), and were returned to court. Of these 17 defendants, eight were sentenced to the promised jail time; one received a conditional discharge (which the judge later acknowledged to be an error on his part); two were transferred to residential drug treatment centers by CEP (because CEP could not provide appropriate services) but the judge maintained the same sentencing conditions; two are still pending; and four failed to appear in court for re-sentencing and warrants for their arrest were issued.[8]

In addition to this formal Alternatives to Incarceration Program in the Criminal Courts, CEP receives defense attorney (and some judge) referrals from the Supreme Court often after defendants have taken a plea (six percent of its court-related clients, line B2 in Table VII-1). If the judge and the defense attorney believe a custodial sentence is likely (for example, if the defendant has violated probation and the judge is faced with no alternative to a jail term), the judge may agree to a special adjournment for the defendant to participate in CEP. While in these cases the judge does not typically guarantee a non-custodial sentence if the CEP reports are favorable, he or she does agree to consider it. As with the more formal Alternatives Program in Criminal Court, CEP's relationship is with the judge to whom they report directly.

[8]CEP reports that the judges cooperating with the Alternatives Program are not disturbed by the flight of these four individuals (24 percent). Since pleas had been entered and jail sentences set, the judges will place them in custody if they are returned on the warrants or if they are arrested on new charges.

Since the alternative sentence is not sure, CEP's impact on sentences in these cases is difficult to assess. In eight cases in which the defendant received a satisfactory report from CEP, agency records indicate three received jail terms that were less than had been originally anticipated by defense counsel, four were placed on probation (with youthful offender status), and one case is still pending. In ten cases in which the defendant received an unsatisfactory progress report from CEP, agency records show three received custodial sentences but dispositions for the rest are unknown. In three other cases, defendants were discharged from the program (neither satisfactorily nor unsatisfactorily) because they were arrested on new charges. All three were sentenced to jail terms (possibly after the two sets of charges were combined).

CEP is expanding both these "alternative to incarceration" efforts by developing formal relationsips with more Criminal Court judges who will commit themselves to a non-custodial sentence in advance, and by exploring the willingness of judges in the Supreme Court to consider CEP participation when deciding upon a sentence. The agency has been moving slowly, however,(only 12 percent of its court-related clients have been selected in these ways), in order to insure the defendants selected are facing jail terms, and to see if their expanded services can hold a sufficient proportion of these clients to make the program viable.[9]

[9]CEP records indicate about half the Criminal Court Alternatives clients who leave the program unsuccessfully do so *before* CEP returns to court with its first progress report (that is, within about three weeks).

Services

Generally, CEP's services to all clients are similar to those it has traditionally offered. However, the agency has streamlined service delivery, and expanded the number of services available in order to develop a capacity to provide more intensive services. CEP now provides counseling, group and individual activities, tutoring, job referral, and other services that enable (and encourage) clients to come more than once a week. While some other clients come more frequently, the more intensive services are given primarily to Criminal Court Alternatives clients. The counseling and social service staff consider these clients to be more difficult than other clients the agency provides services because they are older, more experienced in criminal behavior, and it is claimed, more deeply involved in criminal life styles. In addition, the agency believes many of them have been involved previously with "counseling" and social service efforts (either in jail, through probation, or in other ways connected with earlier delinquent or criminal behavior) and thus they are skeptical of such efforts.[10] Alternatives clients are required to attend CEP at least three times a week (unless they are working full time, in which case other arrangements are made for counseling and services). A more experienced group of counselors work with them, and both the counselors and the clients are given more supervision than is typical.

[10]Of the eight successfully completing the Alternatives Program on August 20, 1979, two had served a jail sentence and two had been on probation (one twice and he had violated his probation both times).

Since early 1978, CEP's management has developed new financial resources to supplement CEP's basic HRA contract which, for example, cannot be used for client educational services. Primary among its new service efforts is an in-house tutoring program employing seven tutors (all ex-offenders under CETA Title VI) introduced in 1978, and a Youth Employment Training Program (also under CETA) added in 1979). The latter involves on-site training in office and clerical skills, classroom, and on-the-job training for which participants receive stipends. CEP has also added an in-house health assessment with the assistance of a local Nurse Practitioner program, and has begun to expand cultural and recreational activities. Finally, group work has been reintroudced into the agency. Some groups are designed to have a direct theraputic effect; others are designed to provide clients with information, life skills, and peer support (e.g., how to job hunt, write a resume, respond to a job interview; how to read a subway map, a telephone book, or newspaper want ads).

One important change in CEP's operations is the addition of a group orientation session which all clients must attend. It is conducted by counselors on a rotating basis, and is held on clients' first visit to CEP after referral and intake in the courthouse. At this session, clients are introduced to the services available at CEP and what is expected of them. The session leader emphasizes that immediate employment is not the only, nor even the primary focus. According to CEP management, the orientation was introduced because the Vera Institute evaluation drew attention to the fact that clients were often attracted by the agency's *name* and by court liaisons'

references to "jobs" during diversion screening and intake. Many clients, it seems, became disillusioned when CEP's counseling efforts were then directed toward a wider range of issues. The new orientation session was designed to make clients aware from the outset of the agency's *multi*-service approach.

To facilitate service delivery, the agency has also tried to increase the flow of communications among staff. While some barriers still exist, CEP's Deputy Director for Social Services believes they have been reduced by shifting away from a reliance on counseling "teams" (described in Chapter II). CEP's current case management approach makes one counselor responsible for providing direct services and for coordinating the specialized services delivered by other CEP staff (e.g., tutors, the community resource specialist, job developers, etc.). Counselors have also been reorganized into specialized units and supervision has been tightened. One unit now provides counseling and services to 16 and 17 year old clients, and the others service older clients.

It might be noted that, while CEP has hired new counseling staff in the last year, it has not abandoned its reliance on ex-offenders. Although CEP has a larger number and proportion of college graduates and individuals without criminal backgrounds than in the past, it continues to hire and promote non-college graduates and ex-offenders. This commitment stems from the belief that a mix of counselors having different skills, experiences and styles of interacting is good for the clients and for the staff. Counselors' assignments to units and their supervision is specifically directed toward encouraging their interaction.

Length of Service And Definitions Of "Success"

Under traditional pretrial diversion both the length of program participation and the definition of client success were standardized. This is no longer the case as CEP has shifted to other methods of intake.

Court-related Clients

The length of service for recent Queens diversion clients is unchanged -- four months, agreed to by the prosecutor, with attendance (required by CEP) usually once a week. "Success" is also defined as CEP has always done: regular attendance at counseling sessions, though the agency's report to the court contains a description of personal progress the individual has made during the period of service. CEP reports that 75 percent of recent Queens pretrial diversion cases have been "successful" and the agency has recommended a dismissal of the charges.

The period of services for Alternative to Incarceration clients referred by Criminal Court judges is longer as well as more intensive. Clients are expected by judges to participate in CEP for six months and to attend at least three times a week.[11] Clients' activities are reviewed weekly by counselors and their supervisors in order to identify and deal with problems. Their "success" in

[11]The average number of months attended by all Alternatives clients (successful and unsuccessful) who entered CEP before June 1979 was about five months as of November 1979; however, some of these are still attending.

the program depends heavily upon active participation as well as attendance, and Alternatives clients are also required to avoid rearrest and conviction. Unlike diversion clients, a rearrest and conviction automatically leads to termination; a rearrest alone results in termination only if the client is incarcerated. As noted above, CEP reports a preliminary success rate of about 47 percent.

Court-related clients for whom CEP is a defense service remain in the program for varying lengths of time, generally, according to CEP, until the adjournment at which their cases are disposed. Since the period of participation is not mandated by the judge or prosecutor, CEP is unsure how long it is on the average. "Success " is also harder to define for these clients because there is no official (court or prosecutorial) definition. If the individual attends, CEP's report to the defense attorney says so; if services have been given and responded to with life improvements, that too is included. While CEP may consider these "successful" social service efforts (as may the defendant and/or counsel), such "success" may or may not have an impact on a defendant's case. If participation has been long enough for the client to have completed his or her initial program plan, then this is reported to the attorney, along with the information that the individual is either leaving the program or continuing to work toward additional objectives. (Such a report may "nudge" the defense counsel to move the case toward disposition if this is not already happening.) Regardless of legal circumstances, CEP encourages all clients to remain in the program as long as its services are needed. Presumably

CEP would consider clients who extend their participation to be particularly "successful" as social service clients.

This ambiguity underlines the agency's *two* distinct yet related definitions of "success" for court-related clients. The first type of "success" involves the activities which satisfy a client's *legal* needs. This may be simply program attendance (as with many diversion clients), or it may involve minimal to substantial program participation (depending upon what the sentencing judge or referring defense attorney thinks is needed). The second type of "success" involves activities which satisfy the CEP staff that the clients' *social service* needs are being met. Counselors look for change in individuals' life situations that indicate their own, the agency's and their clients' "success." These two types of success may (and often do) vary independently.

Other Criminal Justice System Clients

The largest number of CEP's clients whose relationship to the agency does not have direct legal implications are those CEP provides only information and referral services. Most of their contacts with CEP are limited in duration and CEP does not do any follow-up to assess the success of its efforts. CETA eligible Summer Youth Employment Program Clients (15 percent of CEP's total client population) are screened, placed and supervised by CEP in public sector employment for seven weeks in the summer. Programmatic

"success" for CEP is to fill all the slots under their contract; clients are "successful" if they stay through the full program.

The other sub-categories of clients are very small. They are individuals with a wide variety of social service needs and relationships to the criminal justice system. Therefore, their length of participation and measures of their success are determined on a case-by-case basis, using counselor's subjective judgments.

THE NATURE OF THE CHANGE

CEP is in transition. It has substantially reduced the number of clients it diverts pretrial and is considering abandoning this procedure altogether. It is still, however, bifurcated: it is a court related agency that attempts to affect clients' personal lives. The Vera evaluation indicated that, partly because the criminal justice system in New York City had become so "diversionary" and partly because the agency was so dependent upon prosecutors, CEP was not meeting its dispositional goals as a pretrial diversion program. The research also suggested that reliance on diversion for client selection may have had negative implications for CEP's social service goals. Rather than abandon its dispositional goals entirely, CEP has developed alternative mechanisms to obtain criminal justice clients for who both types of intervention are suitable. Therefore, while CEP remains within the traditional framework of the pretrial services movement, it weaves these goals together without pretrial diversion.

Conceptually, from the standpoint of the defense, most pretrial and post-plea services are based upon the rather old assumption that defendants benefit from demonstrating they are of good character, already undergoing rehabilitation, or otherwise *worthy of leniency* from the prosecutor or court. The pretrial services movement of the last decade is also based upon the recognition that the ability to demonstrate such "worthiness" is unequally distributed. Poverty, a lifetime of disadvantage, and culturally different lifestyles make such a demonstration particularly difficult for many criminal defendants. The pretrial movement assumes (though rarely demonstrates) that, if such opportunities are available, particularly early in the adjudication process, the system will take into account defendants' willingness to seek "help," as demonstrated by their submission to counseling, services, or supervision.[12]

Formally organized and publicly-supported efforts to provide such resources to disadvantaged (and other non-elite) defendants

[12]Initial pretrial reforms in the area of bail focused upon those poor and disadvantaged defendants who already had sufficiently stable lifestyles to be able to demonstrate "worthiness," although they did not have money to make bail. It was clearly an important achievement to encourage judges to take such factors into consideration. It is not surprising, therefore, that the next effort at pretrial reform (pretrial diversion programs) grew out of the further recognition that there were defendants who might be able to demonstrate that they could *become* "worthy," despite unstable lifestyles, *if* they were given the resources to do so. Furthermore, diversion took this idea another step; such a demonstration might be used not only to get the defendant out of jail pretrial, but also to encourage a decision not to prosecute. CEP first offered job referral to these defendants. It expanded its services as it became evident that mere lack of knowledge or help in finding a job was not the only assistance typical defendants in the New York City courts needed to become stable enough to demonstrate "worthiness" and, thus, to warrant leniency.

during the last decade have been made largely through the mechanism of pretrial diversion. That is, they have been located within prosecutor-based decision-making systems. Despite some exceptions, few defendant services have been available to this population without pretrial diversion funding. Some private defense attorneys have a social service capability within their law offices or know where to find it in the private or public (but non-criminal justice) sectors, but often only at the expense of the defendant. Some public defender systems have also generated funds for the delivery of social services (for example, the Offender Rehabilitation Project in Washington, D.C. and the Special Defender Services Division of the Legal Aid Society of New York). Although it is difficult to know how widely available such services are, they may be expanding. Senna (1975) found that half the public defender offices he surveyed nationally either had or said they were planning to have social workers on staff. Such a figure does not, however, clarify the number of defendants for whom such services are available or what those services may be.

These non-diversion services have not been easy to fund with *public* dollars. Jacobson and Marshall (1975) report that the 1971 Subcommittee on Legal Representation of the Indigent of the Association of the Bar of the City of New York (the Carter Commission) recommended that the Appellate Divisions require Legal Aid Societies to include social as well as legal services for defendants. However, according

to these authors, the New York City Legal Aid Society (the largest in the state) could not get funding for a full scale program from the city-based agency responsible for awarding LEAA moneys because, conceptually, the funders had accepted the idea that deferred prosecution was the appropriate model for offering such services. Given this perspective, the only proper loci were either prosecutors' offices or third party agencies (such as CEP) which operate through deferred prosecution.[13]

The logic behind locating services designed to help the defendant in the office of the prosecutor or its surrogate was to encourage the use of services *early* in the pretrial process. It was assumed this would (1) avoid the maximum amount of damage to the defendant from criminal prosecution; (2) save public/prosecutorial resources; and (3) reduce stigma by securing a dismissal of the charges. The problem is, of course, that the system is adversarial; despite the attempt of such services to retain their "independence," they *de facto* become associated with the prosecution if not in decisions about the specific services to be given, then in decisions about who should have them. Therefore, the decision concerning which defendants will be given public resources to demonstrate their "worthiness" is, in effect, left to the prosecutor. The Vera Institute research and

[13]It might be noted that the New York City LAS program was probably not duplicative of pretrial diversion efforts. That is, its selection of cases was (and still is) based upon defense attorneys' judgements that the case in question was likely to receive jail time; many of these defendants were in pretrial detention. Hence, the LAS effort was more a last resort than was diversion, since those included had been (or were likely to be) excluded from other programs (such as CEP).

the earlier PTD/I literature suggest that prosecutors make these decisions in favor of defendants they are *already* inclined to treat leniently. It is possible, however, that in New York City and perhaps elsewhere, the diversion process has encouraged prosecutors to expand the numbers and types of cases they treat leniently even if they do not receive diversion services. But without longitudinal research, it is difficult to know how much of a contribution pretrial diversion programs have made over the last decade in New York City, or what impact they may be having elsewhere.

While CEP's shift is away from pretrial diversion and a formal relationship with the prosecutor, it remains at least partially within the traditional pretrial services framework of providing social services (with public funds) to intervene in both people's lives and their court cases. Insofar as its efforts occur pretrial and its formal relationship is with the defense, CEP's present direction might be more accurately characterized as an alternative form of defense than as an "alternative form of prosecution" (to use Nimmer's phrase for diversion (1974)).[14]

There are advantages in CEP's shift toward the defense in its pretrial intervention activities: informed consent is more likely because the case continues to progress toward disposition while service participation occurs; the defendant does not have to waive

[14]CEP's post-plea activities and its formal relationship with the judge is not as clearly within the recent pretrial reform framework. While its goal of reducing the number incarcerated is similar, it does not share the other *pretrial* service rationales.

any legal rights; and the defendant can withdraw from services without affecting the normal course of the adjudication process. There is also maximum flexibility with respect to both services and case outcomes. Services can be provided to any defendant who needs them or whose case can potentially benefit, regardless of how serious the charges or the prior record.[15] The attorney may consider such services an aid in affecting disposition, or simply as a way to help the defendant prepare for a difficult outcome, such as incarceration.[16]

There are those who argue that such pretrial services belong under the full control of the defense (that is, within the defender's office) because they are an intrinsic part of the defense strategy (Jacobson and Marshall, 1975; Frazzini, 1976). However, structural independence from the defense (such as that enjoyed by CEP) also suggests certain advantages.

First, services can continue even after the case has been disposed, or when they are no longer considered relevant to case disposition. This is probably more difficult in a defense office because scarce resources are likely to be allocated on the basis of *legal* needs rather than individuals' *personal* needs. Although

[15]While this may be viewed as an antidote to pretrial diversion's emphasis on less serious cases, the system is also flexible enough to help first offenders. For example, if the New York City system becomes more punitive toward youths, CEP can take the same type of defendants it previously diverted without changing its operating structure.

[16]For example, CEP reports recently helping a client for whom a jail term was mandatory. While CEP believes its services helped shorten the jail term (by justifying a reduction in the charge), the agency was also able to help the defendant (an elderly first offender) prepare for the considerable ordeal of a jail term.

the defense may exercise control over which defendants come to the attention of the independent service agency, it does not completely control the allocation of its service resources.

Second, multiple sources of money (public and private) may be more readily attracted to the independent service agency, including but not limited to court-related dollars. Not only are a larger number of financial sources likely (e.g., CEP's CETA money, Title XX, foundation grants, etc.), but the retention of such funds is not typically dependent upon demonstrating difficult to achieve results such as a reduction in recidivism, reduced court costs, or even impact on case disposition. If the agency can demonstrate that good quality social services are being given a population that needs them, the moneys will probably be thought well-spent regardless of their impact on the criminal justice system.

Third, according to Frazzini (1976) and others, conflict between professional social service workers and defense lawyers is endemic. Social service workers see their activities as intrinsically worthy, not just for defense or legal purposes. Sometimes this can lead to serious case-related conflicts and to service workers' feelings of being "subsidiary" to legal needs (Frazzini, 1976: 68-70). With the independence of the social service agency, this conflict may be reduced, although the social service worker may experience personal tensions concerning clients' service versus their legal needs. To some extent even this can be dealt with if, as in CEP's situation, direct relations between the agency and the defense are conducted through specially trained liaisons (supervised, perhaps, by a lawyer)

who are in close contact with counselors but who do not do counseling. If defense counsel do not think the services being provided will help their clients' cases, they can so inform defendants, or they can seek other resources. In addition, the defense can choose not to use the information provided by the agency. However, if (as in CEP's case) individuals within the service agency are highly experienced with clients who face court cases, it may also develop a joint expertise (what courts are looking for/what clients personally need). This is less likely in a social service agency with little or no court or criminal justice experience.

Fourth, defense attorneys apparently often fear their clients will resent the intrusion of social services and will think their attorneys assume they are guilty or will take a plea. Consequently, formal separation of the services from the defense may allow attorneys some distance from the social service "intrusion."

Fifth, defense attorneys do not have to personally assume the "social worker" role. It is reported that this "social worker" self image is an extremely difficult one for many lawyers, personally as well as in relation to their clients. The availability of separate services might increase defense utilization.[17]

[17]It is reported that defense attorneys often don't use social services that are available (Frazzini, 1976; personal communications from individuals in several agencies providing such services). This is said to stem from their dislike of "becoming social workers;" however, it must be noted that it may also result from (possibly appropriate) skepticism about the *efficacy* of such services in case disposition.

Finally, prosecutors and judges might be less likely to develop the image that defense-oriented social services are excessively biased and thus unreliable if they are provided by an independent agency, with a good reputation, strong professional standards, and an organizational need to maintain such a reputation.

Why should such social services not be totally independent of the criminal justice system, that is, located in agencies that have little or no involvement with the system and that take clients from a variety of sources? Obviously such agencies exist and do help defendants when approached by the defense or when an existing client becomes a defendant. However, as we have already suggested, many people in the system report that defense attorneys do not know how to seek out such services, do not take the time to do so, or feel uncomfortable in that role. This is probably true in a busy urban court in which defense attorneys are hard pressed even to meet scheduled court appearances. But it may also be true in any jurisdiction where criminal defendants cannot readily afford such services. As important, general social service agencies may not understand the particular problems of a client facing legal process, the trauma of a criminal conviction or a jail sentence. They may lack the expertise to assist the client deal with the court experience and the threat of punishment, to help the client fully utilize the social services for his defense, and to prepare pertinent materials (written or oral) that will be effective in court. This takes experience many agencies with different constituencies and clients may not have. Finally, an agency involved in the criminal justice system is

likely to become a respository of experience in the use of social services in case disposition. Such an agency, therefore, may become a valuable *guide* to defense attorneys in creative uses of their services.

This discussion has tacitly assumed there are cases within the courts deserving leniency that is not yet forthcoming; that social services are a potentially useful resource for encouraging such leniency; and that such activity deserves some priority in the distribution of social services dollars. None of these assumptions may be appropriate for a particular jurisdiction. While the first and third are value issues, the second is an empirical question and is the keystone of the entire conception. The Vera Institute evaluation of CEP suggests that social services may *not* affect disposition in a meaningful way, at least in New York City when those services are delivered in the context of pretrial diversion. However, so long as most pretrial agencies devote their resources to cases that would *otherwise* be treated leniently, it is difficult to test the underlying assumption. And, so long as they do not provide careful, probably controlled, experimental assessments of their impact, tests which *are* carried out will be inconclusive.

While CEP appears to have passed fairly smoothly through the initial phase of its transition from pretrial diversion (though certainly not without stress or uncertainty), it is by no means fully settled on its future course of action. During this phase, it has maintained its traditional dual goals by shifting toward providing pretrial social services for the defense and post-plea diversion from

a custodial sentence. In addition, it carries out much needed information and referral in the courts, and provides a flexible social service resource to help clients referred from other criminal justice agencies. The major question is which focus the agency will emphasize in the long run. It may decide that its current experiments with case intervention are successful and should be expanded. However, it may also decide that, because the system in New York City is so "diversionary" overall, there are more pressing criminal justice arenas within which it should be operating without affecting dispostion. If so, the direction of such efforts is not yet clear.

Finally, CEP has not really confronted the issue of under what conditions their current (or alternative) ***services*** are likely to influence their youthful clients' *lives* (outside their legal circumstances). It may be that changing procedures for selecting clients -- eliminating pretrial diversion -- by itself addresses that problem; but it probably does not. CEP's client population, however selected from the criminal justice system, is multiply disadvantaged and difficult to provide services that have demonstrable effect. It is in this area that creative program planning and closer research attention are clearly needed in New York City as elsewhere.

from

THE COURT EMPLOYMENT PROJECT EVALUATION

Final Report

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APPENDIX A

COLLECTING INTERVIEW DATA

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Over the course of 23 months during 1977 and 1978, Vera's interviewing staff conducted more than 1,500 interviews with 533 individuals in a research population drawn from the criminal courts of Brooklyn, Manhattan, and the Bronx in New York City. The number of interviews[1] was influenced by a variety of factors making the data collection a difficult process. We will discuss those factors in relation to two aspects of the data collection: the task of initially contracting respondents and securing an intake interview, and the efforts required to locate and recontact respondents for follow-up interviews. We will also describe the methods employed - both successful and unsuccessful - to deal with these tasks. Our purpose in including this appendix is to make detailed material on our field contacts and interviewing efforts available to other researchers undertaking similar work. All too often these experiences are lost, known only to the scattered members of a particular research

[1] A total of 666 defendants were selected into the research population over a ten month period (410 randomly selected experimentals and 256 controls). The research interviewing staff was able initially to locate and secure intake interviews with 80 percent of this population. Of these 533 respondents, the staff was able to find and re-interview 87 percent after approximately six months (that is, 70 percent of the total research population). Annual interviews (after 12 months) were obtained with 376 members of the original population (66 percent).

team. The valuable accumulation of knowledge about field and interviewing strategies generally does not occur, particularly when the research population is difficult to study or the data collection circumstances are unusual.

OBTAINING THE INTAKE INTERVIEW

Initial Intake and Contact in the Court

Our research population consisted of people arraigned in criminal court who were eligible and selected for participation in the Court Employment Project. Intake took place over a ten-month period, January through October, 1977. During this period we had a Vera research interviewer stationed in the court buildings in Manhattan and Brooklyn where CEP was screening for diversion and, for short periods, in the Bronx and in the Brooklyn night court. The Vera interviewers were informed of each new research intake by the CEP screening staff; they then attempted to contact and interview these respondents in the court. Two additional Vera interviewers worked out of the central research office at Vera. The CEP screening staff supplied them with the addresses and phone numbers of all members of the research population missed in court by the Vera staff, so that they could be contacted at home. As additional means of contact we had access to respondents' future court appearance dates and incarceration information.

These procedures were originally designed to allow the research to contact and interview most subjects selected for the research on or shortly after the day they were selected. In actuality, less than a third of the respondents were initially interviewed so promptly because research interviewers experienced considerable difficulty making contact with many respondents while they were in court. The most significant reasons involved the complex and hectic structure of activities in a busy urban court and the nature of CEP's own screening process.[2]

[2]This research effort differed from Vera's earlier longitudinal research on a supported work experiment with a drug addicted population (Friedman, 1978), insofar as it did not have a "captive" research population at the point of intake. Because defendants were being selected for program and research participation during the very early stages of the court process (that is, prior even to arraignment), agreement to participate in the research could not be made a condition of program participation. The Vera supported work research (and also such studies as Kenneth Lenihan's work with men released from prison (1976)) were able to secure such agreement and thus not only obtain intake interviews on all members of the research population, but also secure detailed follow-up information for each respondent. In addition, the CEP program/research population had important social and demographic differences from, for example, the supported work population which made them somewhat more difficult to track. They were considerably younger and, therefore, did not tend to have stable relations with spouses, parents or relatives. While many "lived" at home with relations or parents, they were rarely in residence with them and had few alternative or regular contact places known to their parents. In addition, because of their youth and lack of involvement in formal organizations (e.g., schools, social programs, probation, drug or other treatment programs), there were few, if any, official agencies through which they could be located. Finally, all these characteristics are associated with their considerable geographic mobility,both within the New York Metropolitan area and beyond (particularly the southern U.S. and Puerto Rico). Consequently, the extensive field efforts necessary to locate a single respondent often ranged over many city neighborhoods and, less frequently, several states.

Our first contact with a respondent assigned as an experimental (i.e., a program participant) was supposed to occur immediately after the CEP screener (who called the case in to Vera's Central research office to obtain its assignment to the experimental or control group) had brought the individual to CEP's court screening office to complete the program's own intake procedures. Our interviewer, having an adjacent office in each court, was to introduce himself and explain our research study to the respondent.. The first contact with a defendant assigned to the control group (a non-program participant) was supposed to take place in the court immediately following arraignment. When our interviewer was informed of a control group intake by the CEP screener, he would also be given the courtroom location so he could locate and contact the respondent. It was very difficult, however, to actually implement these procedures.

A major problem was that the flow of cases called in by the CEP screeners was extremely erratic and unpredictable. During the heaviest months there were days with no intake at all, occasionally followed by a day in which a half dozen cases were called in within an hour of each other. It should be noted that CEP normally took all co-defendants in a case, so that a screener frequently called in several cases simultaneously. There were no particular days of the week or times of the day when intake was consistently heavy or light. It was, in practice, impossible for an interviewer to assure that he would be in his office (i.e., not elsewhere in the court building trying to contact respondents) when a participant intake was called in and brought to the screening office. Similarly, an interviewer often would be

conducting an interview or out of his office when a control group intake was called in, and could not get to the court parts before the respondent's arraignment was finished.

Another problem was that an experimental subject could be rejected for CEP by the judge at arraignment, or change his mind about participating in CEP. Such non-participant experimentals constituted about 10 percent of our sample. The interviewer, expecting a participant to be brought up to the CEP screening office after arraignment, would not know until too late that he had to contact the respondent in the arraignment part.

A third difficulty in making initial contact involved the mechanism of informing the court interviewers of new intakes. A CEP screener was supposed to notify the interviewer as soon as he called a new intake into the Vera office for research assignments, but this was not a reliable procedure. CEP screeners were under tremendous time pressure because they had to complete work on an intake before the defendant's arraignment. Since this involved gathering all the relevant papers and the prosecutor's case files, interviewing the defendant, and getting approval from both the defense attorney and DA's office before arraignment, the screeners spent most of their time literally running around the court building against a deadline.[3] Once having called in

[3]All the Criminal Court Buildings within which the interviewers and screeners operated are large, rambling buildings containing dozens of court parts, several different detention locations, and decentralized case information systems.

a case to Vera, they sometimes simply forgot to call the court interviewer, or had a chance to do so only after hunting down an unoccupied pay phone or racing back up to the CEP screening office. There were also many times when the Vera interviewer was unreachable because he was rushing between arraignment parts looking for respondents. These delays in notification contributed to the number of respondents (especially controls) missed in court. Eventually the problem of notifying court interviewers of new intake was dealt with somewhat more successfully by having the research assignment monitor at Vera also telephone the court interviewer after the CEP screener had called in a case.

While several aspects of the CEP screening process created problems for the research interviewers, the working relationship between them and the CEP screeners on a personal level was very collaborative. They came to regard each other as members of the same staff. One benefit was that, as time went on, CEP screeners made efforts to help the Vera interviewers contact respondents above and beyond their basic responsibility to tell a respondent that "somebody here would like to interview you." When an interviewer could not be reached, it was not uncommon for a CEP screener to try to persuade a participant to wait in the CEP screening office until the interviewer returned, or to persuade a control to come to the CEP office after his arraignment. Another benefit was that our interviewers gained valuable familiarity with court procedures and personnel because they spent time side-by-side with the highly experienced CEP court screeners. Two interviewers, for example, were thus able to establish rapport with various personnel

in the court clerk's office; this reduced the amount of time they spent locating respondents in the court. The Manhattan Court interviewer, through the CEP screening staff, met and developed an informal relationship with various CEP counselors (whose offices were a few blocks from the court), which helped facilitate contacts with participant respondents when they went to counseling appointments. Though many intakes were missed in court, there would certainly have been many more without this collaboration.

Another difficulty with working in the courts was the high rate of refusals: more than 11 percent of the sample initially refused the research interview when contacted in court. The most important reason for such referrals was the negative influence of the criminal court setting on respondents and their families, and the extent to which they identified the research staff and the research effort with the criminal justice system.

The negative physical and psychological impact of arrest, detention, and arraignment was substantial even for respondents who had experienced criminal court arraignment previously (40 percent of the sample). By the time of arraignment, a respondent had been held in detention from a few hours to a few days; he had been questioned by police officers, Pre-Trial Services Agency (ROR interviewers), attorneys, and a judge; and he had been crowded together impersonally with people accused of crimes ranging from shoplifting to murder. Research interviewers, therefore, in explaining the study to a respondent, tried to make it clear that the interviews were voluntary and that Vera had no connection with the court or the police. Very often, however, only the

voluntary part of this was comprehended or accepted. Respondents often refused to be interviewed because they did not want anything more to do with the court and they perceived the interviewers and the research as part of the degrading, upsetting process they had just experienced. The distinction between that process and a "research interview" which might eventually benefit others in a similar situation was often too abstract, particularly in the face of their desire to "put it all behind me," or "get away from the whole scene here," or "go home and get a bath and a meal." In addition, since 42 percent of the sample was only 16 and 17 years old, a respondent's family was often present in court and frequently felt this way even more strongly than did the respondent.

An additional problem was that some controls and non-participant experimentals felt a particular hostility towards our interviewers because they had been turned down for a program that they thought might have helped them get their cases dismissed. When first talking to defendants, CEP screeners were extremely careful to explain that there might not be room in the program even if all the necessary approvals were obtained. When defendants were subsequently assigned as "overflows" (that is controls), the screener would tell them that there was, in fact, no room. Often, however, the CEP screener would not have time to locate and speak to "overflows" at all after calling the cases in to Vera, so these individuals would find out only at arraignment that they were not in the program. Since at first the research interviewers were considered to be CEP by respondents, they sometimes got an embittered reaction: "They (You) wouldn't help me - why should I go along with you?"

Another adverse effect of the court environment was that respondents who were willing to be interviewed often would not do so right away; they wanted to go home first. As a general rule, interviewers pushed for an immediate interview only if they could not arrange a later appointment or get the contact information necessary to contact a respondent later. As it turned out, however, many respondents did not show up for later appointments or were difficult to contact later, so their frequent desire to postpone the intake interview became a serious problem for the research.

Since the court interviewers had to operate within a setting that was negative for respondents, various countermeasures were used. The most obvious was paying each respondent for his time, coupled with a promise to also pay him for several additional interviews over the course of the following year. The $10 cash payment did induce some respondents to agree to be interviewed, who would otherwise have refused or postponed. It is impossible to determine the exact number for whom money was central, but there were enough instances of respondents being "on the verge of walking out" until the money was mentioned to suggest it was an important factor. In addition, more than a few respondents told court interviewers that they were completely broke, and that the research money was their only way of getting home.

Aside from the cash payment, the general style and approach used by the interviewers was an important part of the research staff's effort to deal with the negative reactions of respondents. They introduced themselves on a casual, first-name basis and attempted to

establish an informal rapport which would be a positive contrast to most of the other contacts the respondents had had in the courts. All the interviewers, moreover, had themselves spent a good deal of time "on the streets" in New York City, and several had had personal experiences with the courts. When they attempted to persuade respondents to be interviewed, they used "street language" and geared the way they presented the research to the perspective of the respondent. This personalized approach will be discussed in more detail below because it was also important when interviewers worked in the field and in conducting the formal interview itself. Although we cannot systematically compare this approach to others, we had one interviewer who tried using a more formal, "professional" approach when he first began working in the courts; after encountering a variety of refusals and put-offs, he changed his style to what has been described above and immediately got better results.

The most successful method for overcoming the court refusals was the "last resort" of recontacting respondents later. In most cases, this was not done until several months after a refusal. As a matter of policy, we did not recontact anybody who had refused vehemently or who had asked us not to get in touch with him again; but when refusals in court were less firm, later results were good. Well over half the "refusals" we were able to recontact agreed to be interviewed, and overall the number of refusals was reduced by almost 25 percent.

Three different strategies were employed in this effort. If the initial refusal had been lukewarm ("I don't really think I'd be interested"), the field interviewers attempted to telephone and speak personally with the respondent after a few weeks. This approach proved to be ineffective and was abandoned. During the last month of research intake,a letter was sent to all refusals, explaining that we still wanted to interview them and would pay $20 in cash. While we received only a few direct "second refusals," there was only about a 10 percent response to the mailing. Our third approach was to mount a fullscale *field* effort from May to October 1978 to recontact refusals in person. Roughly three-quarters of the refusals we were able to contact in person agreed to the interview.

Details of how this was done (including the offer of larger payments) will be discussed below, but one aspect of the process should be noted. The *more* time that passed between a refusal in court and successfully recontacting the respondent, the more successful we were in obtaining his cooperation. This was largely because a respondent no longer assumed a direct link between the research and the courts.

Field Operations

When a respondent was not interviewed in court, the court interviewer continued his contact efforts by sending a letter, telephoning, or attempting another personal contact if the defendant had another court appearance or a CEP counseling appointment scheduled. If these efforts failed, the Vera field staff took over. Although there was some overlap between the responsibilities of field and court interviewers, at least 300 of the intake interviews (over 60 percent of those completed) required at least some "field contact." *More than 15 months of various kinds of field activity were necessary to secure interviews with 80 percent of the original research population.*

A number of factors made the field activity particularly difficult. Primarily, the only contact information research interviewers were able to obtain in court was of poor quality. It was often incomplete, inaccurate, or unreliable. This was especially true of the information contained on the PTSA/ROR interview reports -- the main source of home addresses and phone numbers when we had had no personal contact with a respondent in court. Names were frequently recorded incorrectly; addresses were unclear or inaccurate; and information was sometimes indecipherable because of poor handwriting or indistinct photocopies. As we learned in talking with respondents ourselves, these problems were not entirely the fault of the PTSA personnel; both deliberate and unintentional misinformation was provided by respondents. Some respondents did not use their real names when arrested and aliases were only apparent if a respondent had been arrested previously and used his or her real name. Some respondents did not tell PTSA interviewers the name they normally used, although the name they gave was real. For example, someone's full name might be "Juan Julio Torres Rodriquez," When asked for his first and last name, he would give "Juan Rodriquez." His family, friends, landlord, school, and/or employer, however, might know him as "Julio Torres." Finally, having two last names (that of both parents) is not uncommon among Hispanic families in New York City because of the Spanish tradition of placing the father's name last but using the mother's name generally. To further confuse the issue, we had more than one respondent who genuinely could not tell us how to spell his name correctly.

Instances of respondents being uncertain of their precise street addresses or home telephone numbers were also common. Finally, some addresses provided by respondents were clearly imaginary or false.

Some of the information recorded by the PTSA interviewers was not available to research staff until considerably after the person came into the research sample. This problem arose because we received these reports through the central CEP office. CEP screeners were supplied by PTSA staff with photocopies of the interview report for all defendants CEP screened; the screeners would turn this copy over to our court interviewer if we assigned the case to the control group. But the main CEP office had to forward all the reports for *participants* to our office at Vera. Quite a few PTSA reports got "lost in the shuffle" either during the screening process or at CEP. Some were only received after several weeks or more; some we never received and had to obtain directly from PTSA.

The lack of good contact information was critical because *other* means of contacting respondents were unreliable. Many respondents who made appointments with a court interviewer for a later date did not show up, necessitating further contact efforts. Although we would know the date and courtroom location of a future court appearance, that knowledge was only minimally helpful. Even with access to the court calendar, no one could determine at what time of day the case would be called and it was rarely possible for an interviewer to spend the entire day waiting in court. Frequently, respondents would not appear in court at all. Contacting participants through their CEP counselors was not always successful either, since broken appoint-

ments and "no-shows" were common at CEP. (About 45 percent of the experimentals failed to complete the program because of poor attendance.)

When we asked respondents, either systematically or informally, why they did not go to appointments, the answers varied. For CEP counseling appointments and research interviews, the most common response was lack of interest (e.g., "I didn't feel like bothering"). Sometimes respondents also indicated that unwillingness, inability, or fear of travelling to an office located outside their neighborhood was important. They lacked subway fare, got lost, or simply were afraid to make the attempt. Court appearances were sometimes skipped because the individual forgot the date or location, never received a mailed reminder, and/or thought his case was finished. Despite cash payments for interviews, the problem of "no-shows" plagued the interviewing staff throughout the efforts to conduct follow-up interviews.

One method used to counteract the unreliability of contact information was to obtain updated addresses, further court appearance dates, and incarceration information from CJA's computerized files. This was an important source of information, since over one-third of the sample (36 percent) was arrested at least once during the follow-up period. Although the problems of inaccurate addresses and phone number and failure to appear continued, this material gave us one additional opportunity to contact respondents who were otherwise "lost" or difficult to locate. The same was true if a respondent was detained or incarcerated: despite enormous difficulties finding and

interviewing a detainee, at least we knew where he might be for some length of time. (In New York City, the Department of Corrections has had grave problems with updating information on inmates and the difficulty of locating detainees is a systemic problem.)

In sum, to establish a first contact with research subjects, our interviewing staff had to do more field contact work than was initially expected, and they had to carry it out under serious handicaps. Most of the respondents not talked with in court required weeks or months of repeated efforts before even a contact was made and often longer before an interview was secured. Interviewers often did not know the contact information they had obtained from the court was in error until, for example, the respondent skipped an appointment and a letter sent to his home address was returned by the Post Office. By then, several weeks would have passed and the number of new intakes from the court requiring field work continued to increase. Consequently, field interviewers had an ongoing backlog of uninterviewed cases needing field contact, which was continuously increased by new intakes missed in court. We could not spare court interviewers to work in the field because the flow of new intakes in the court was so erratic.

Almost 80 percent of the sample had one or more telephone numbers listed on the PTSA interview report, either a home phone or that of a relative, friend, etc. However, interviews often found one or more of the numbers disconnected within a few weeks of intake; any time lag before full field contact efforts began for a respondent increased the likelihood that his household's phone would be turned off.

While we often learned later that this was due to the non-payment of the bill, we could not be sure at first whether the family had moved. Whether permanent or temporary, such disconnections reduced the efficacy of our phone efforts. Furthermore, even if a number was correct and in service, our interviewers typically had to make many calls over an extended period of time in order to reach a respondent himself. The phone sometimes belonged to a friend, neighbor, relative, etc., who did not see the respondent regularly, and messages would get garbled, forgotten, or ignored. Respondents were difficult to catch at home, and often other family members were "in and out" also. This would mean multiple calls would be made before reaching anyone, much less the respondent. Respondents also frequently ignored our messages asking them to call us back, a problem also encountered when letters were sent out.

There were two other problems in contacting respondents by mail. Letters sometimes did not reach a respondent because the mailboxes in his building were vandalized or non-existent. In such cases the Postal Carrier either left the letter by the door (where it was often lost) or returned it to us as undeliverable. The former situation would result in our staff sending out several letters, waiting for a response, and then, after considerable delay, making a field visit. At first when the Post Office returned a letter (generally stamped "no such party at address"), we would assume the address was incorrect and cease trying to contact the respondent there. It was only when we did finally contact a respondent by other means and verified his home address that we found this had been a wrong assumption. Some

respondents, in fact, told us (or we saw for ourselves when making a field visit) that there was a properly labelled and operable mailbox although the Post Office had returned letters as undeliverable. In short, interviewers were not only uncertain of the accuracy of the addresses and phone numbers but were unable to rely on the normal means of home contact, the phone and the mail.

Because of our lack of experience with this population and the heavy field workload, the interviewing staff initially attempted to use one means of contact, waiting to see if that was successful, while going on to other cases, and then trying another means. Intensive simultaneous techniques such as sending a letter, making numerous phone calls at all hours of the day and evening for several days, and then automatically beginning field visits were only applied later on. Such efforts, moreover, are extremely time consuming and demand intensive concentration on a fairly small group of cases. Our staff did not have the time to develop and employ such routines consistently so long as they were heavily involved in keeping up with the influx of new cases.

Our initial expectation that we had enough "contact points" (addresses and phone numbers, court appearances and CEP appointments) to contact most respondents outside the courts was, therefore, wrong. Given the difficulty of securing interviews in the courts, and the mail and phone contact problems, interviewers had to spend increasing amounts of time in the field in order to locate and interview respondents. Unfortunately, since each court had to be covered by a research interviewer all day, every day, they were not available for field work. The

necessity to prolong intake four additional months (from the six initially expected to ten) to secure a sufficiently large research population also reduced the number of interviewers available for field work.

Time, in fact, was a constant negative factor in the field work. As mentioned above, efforts to obtain the intake interviews spanned 15 months. At a very rough estimate, more than 3,000 person-hours were involved outside the courts during those months. Aside from the extraordinary delays described above in our mail and telephone efforts, each field visit that was made required a great deal of time. Interviewers frequently had to travel one or two hours each way on public transportation to visit a respondent's home. Travelling between addresses in the same general area was also very time consuming, and making as many as eight visits constituted an unusually full day of field work from the standpoint of travelling time alone. (It was not possible for our staff to have access to cars, except sporadically, until we were into the follow-up phase.) This was especially true in the larger and geographically more spread-out boroughs of Brooklyn, Queens and the Bronx.

Aside from travelling time, interviewers typically had different objectives for each "stop," and a single stop could require a substantial amount of time. There might be an appointment for a home interview, which involved waiting for the respondent to arrive and conducting the long interview. "No-shows" (though less common when an appointment had been made at home than they were at our office or in the courts) were regular; an interviewer might waste up to half a day between travelling and waiting. A visit might also be made because we were

told that a respondent "probably" would be home around a certain hour, in which case the interviewer would try to stay in the area and come back several times if the respondent was not home at first. To reiterate, many respondents in our sample did not consistently stay at home even after returning from school or a job; they were "in and out." When our interviewers made "blind" visits (i.e., with no advance indication a respondent might be at home), they usually spent only the time necessary to leave a message if the respondent wasn't there. Since a day's field work typically combined "blind" visits with "possible contacts" and appointments for interviews, relatively few addresses could be reached, even if the entire day was spent in the field.

Efforts to reduce the amount of time spent on field visits, and to increase their efficiency had mixed results. Geographically grouping addresses to be visited only had an impact in the smaller, more densely populated borough of Manhattan, where it was sometimes possible to find several addresses within walking distance of one another or within a quick bus or subway connection. In the other boroughs, the public transit system simply did not allow for easy connections, and the physical distances were greater. Calling ahead to confirm appointments helped cut down on lost time and no-shows, but many respondents did not have telephones or were not going to be home until the scheduled time, so confirmation was not always possible. Enlisting the cooperation of respondents' relatives and friends, making blind visits at night or on weekends, and spending time "hanging around" local neighborhoods to find

respondents were all strategies that could not be fully utilized during the intake phase. The major factors, discussed below, included (1) our concern for preserving the respondent's privacy; (2) suspicion and lack of cooperation on the part of respondents and their families; (3) the dangerous neighborhoods in which many of the respondents lived; and (4) the difficulty and complexity of staff coordination and case management so long as new intakes into the sample continued.

From the outset, research staff felt a particular need to be cautious about explaining the nature of the interview to people other than the respondent. We could not tell a respondent's family that we had tried to contact him in criminal court and wanted to interview him about his experiences. On the one hand, the family might not be aware he had been arrested; on the other, they might assume he had gotten into further trouble. We had not only the normal research responsibility of maintaining the confidentiality of the information provided us by the respondents, but also one of not disrupting the normal lives of the (mostly teenage) respondents in the course of our contacts with their families or friends. Research interviewers, thus, used a low-key and non-professional approach when leaving messages (e.g., "Would you tell Willie that Pete came by, and ask him to give a call tomorrow? Here's my number"). This problem underlines the importance of our having failed to initially contact respondents in court. It meant a field interviewer had no obvious guidelines as to "how far he could go" in identifying himself. At the very least, if an initial court contact had been established, the interviewer could say that he had spoken with the respondent before and was "supposed to get back in touch with him."

At best, the family might have been in court and know about the interview; in such a case we could ask for their help in contacting and/or arranging an appointment with the respondent.

We were never certain, however, of having any cooperation from respondents or their families. Respondents themselves were often indifferent towards the interview, so our messages were often ignored. Our own deliberate effort to be low-keyed when leaving messages often exacerbated this problem. Sometimes the $10 payment was considered "not worth the trouble," and sometimes respondents were just not very conscientious about returning calls or responding to messages. Family members and friends also frequently viewed the research interviewers with suspicion. In the experience of many of these families, unknown people were often police, welfare "inspectors" or bill collectors. Here again, our concern for confidentiality had an adverse -- and generally unavoidable -- effect. Nevertheless, even when our interviewers were able to be more specific about the reason we were trying to reach the respondent, there remained the difficulty of making clear what "this research interview business" was all about before we could get the cooperation of relatives or friends. In sum, the most significant effect on our field efforts was that interviewers frequently were not able to obtain reliable information about how and when they could contact a respondent. They had to play it by ear with each contact, and use their own judgment both on how to act and how much to believe what they were told.

The possibility of physical danger to interviewers in the field was a concern throughout our field efforts. Each interviewer automatically followed his or her own "street instincts" to avoid danger -- e.g., spending as little time as possible on the street if the neighborhood seemed particularly dangerous or hostile. More importantly, the interviewers were justifiably reluctant to make evening visits or canvass a neighborhood in such situations. While the research staff had been aware all along that safety factors had to be taken into account when working in the field (since this involved going into some of the worst slums and high-crime areas of the city), there was little that could be done to avoid having this affect the efficiency of the field efforts. Working in pairs and assigning visits in certain areas to interviewers who knew that particular section better (or were more comfortable there) helped somewhat.

Ethnicity was a major determinant in these field efforts. Two of our interviewers were black, two Hispanic, and one (the only woman) black-Hispanic. In primarily black neighborhoods, the Hispanic interviewers received less cooperation from everybody, got "hassled" more frequently on the street, and generally felt less at ease. The same was true for the black interviewers in Hispanic neighborhoods. Another important determinant was familiarity with "the turf." When an interviewer went into an area he or she knew very well, it was both easier and more natural to informally "ask around" for a respondent -- whether this meant talking to the neighbors or stopping into a local hangout. Since we had respondents living in dozens of different neighborhoods, however, all the interviewers combined were familiar with only a certain number of the areas in which we conducted field work. It was only during our follow-up efforts (and after an interviewer nearly got mugged in the field) that available methods could be used to approach the problem more systematically. One reason for this delay was a lack of manpower; with two or three of the five interviewers needed full time in court buildings, pairing and/or special assignments were difficult to coordinate.

There were also conflicting demands on the time and attentions of the field interviewers which prevented full and consistent coordinations of their early field efforts. The need -- on an unpredictable basis -- to rush to one of the court buildings to help contact and interview new intakes, together with the need to conduct unscheduled interviews with respondents who "walked in," meant that field work had to be arranged on a day-to-day basis. Planning field work in advance, concentrating intensive field efforts on cases that were hard to reach, or assigning field work cases to specific interviewers for maximum efficiency was not consistently possible because our plans might have to be dropped suddenly to cope with one of the situations described above. Another time consuming job was the clerical task of recording in detail the work which had been done on cases and of what needed to be done (i.e., "case management"). Each of the (normally two) field interviewers had a load of thirty or more cases which required field work. As respondents were interviewed (or refused) and new intakes missed in court came in, the individuals who needed to be contacted constantly changed. Since the interviewers' time was fragmented and their workload was large, it was sometimes difficult and time consuming

for them, or anyone on the staff, to record the detailed history and contact efforts for each case. The result was that the early field work on some cases was not done as soon as it might have been.

Time had other effects; the longer it took to get in contact with a respondent, the greater the chances that he would become impossible to contact. He might go to live with a relative outside New York, get a job in another state, go away to school, or be incarcerated outside the city. It was impractical for the interviewing staff to make more than an occasional home visit outside metropolitan New York City, due to budgetary and time constraints and the lack of access to car transport.

In addition, some respondents completely dropped out of sight -- either moving with no forwarding address or leaving their homes without telling anyone where they were going or when/if they might return. This was a more common occurrence than any of the previously mentioned "moves" and particularly difficult to deal with because of the uncooperative attitude of relatives or friends. By the time research intake was nearing completion (the beginning of September 1978), more than 40 respondents were uncontactable. There also were approximately 35 who were "possible to contact," ranging from very recent intakes missed in court to cases that the field interviewers had been working on for up to four months. These two uninterviewed groups represented about 11 percent of the total sample, and there was an additional 11 percent that had refused to be interviewed in court.

In an effort to secure as many intake interviews as possible from the still contactable group and the refusals, a decision was made to delay the start of the first (six-month) follow-up interviews until November and concentrate staff efforts on locating intake cases. In addition, the research staff decided to offer $20 instead of $10 for the interview. These efforts met with only minimal success. As described above, there was very little response to the letters sent to refusals, and the research staff decided not to expend field effort on refusal cases in order to concentrate on the others. This was done so that there would be more manpower available with fewer scheduling problems (court intake had, by September, slowed to a trickle and would end in late October) and a concentrated field effort on a relatively small number of cases could be mounted with maximum efficiency. An improved system of case management, scheduling, and assignment was set up. This still did not overcome all the problems. When a court interviewer was sick or on vacation (both situations occured several times during this period), one of the three field interviewers had to work full time in the court, even if there were only a few intakes. This was also the first time that an intensive field effort had been initiated, so the normal problems of beginning a new set of procedures took place.

A final attempt to obtain intake interviews with difficult and "lost" respondents was made between May and October 1978. There were 159 uninterviewed intake cases, slightly more of whom were uncontactable ("lost") than had refused. The method employed in this

push was an all-out, intensive field effort. While a specific description of these techniques is included in Section II below which deals with our other follow-up efforts, several features unique to the intake interview efforts during this period are worth discussing here.

First, there was the problem of identification: making sure that the person we finally contacted and interviewed was, in fact, the same individual who was assigned to our sample. This was, of course, a concern in all our efforts, but in follow-up cases we were able to verify a respondent's signature against the signature we had obtained on the first interview, and often a respondent would be recognized by the interviewer who had conducted the previous interview. With *intake* cases, however, we had no signatures to compare, and often none of the interviewers would have seen the person before. Since we were offering larger amounts of money for these interviews (up to $50), and since we had already identified a few imposters during our early follow-up work, we had to take particular care. Whenever an intake was being interviewed, the interviewer asked for a picture I.D. card with a signature on it, or a Social Security number and date of birth. In addition, items would be checked from the initial PTSA interview report and/or CEP screening sheet (e.g., previous addresses, arrest charges, AKAs, names of relatives, etc.). These checks were performed whether or not the interviewer recognized the respondent, to protect against mistaken identifications occurring because of the passage of up to 20 months since prior contact.

OBTAINING FOLLOW-UP INTERVIEWS

The follow-up stages of the data collection involved personal interviews conducted in two waves (six months and 12 months after intake) *with all respondents for whom we had intake interviews.* In this section we will primarily discuss those features of the data collection process (and the constraints encountered) that were specific to these interviews. It must be kept in mind, however, that the conditions and difficulties described in the preceding section concerning our mail, telephone, and field contact efforts continued into the follow-up stages. One major difference in obtaining the follow-up interviews was that interviewers were no longer required in the courts, and the staff's workload was no longer dependent upon an erratic and unpredictable intake. The staff knew how many cases needed to be dealt with and when new cases were coming due, and we had the flexibility to utilize all the interviewers in the field. On the other hand, the staff was constantly faced with a backlog of follow-up cases at the same time additional cases were coming due for another follow-up interview. This meant that the staff still had to cope with the same contact and interview problems encountered during the intake stage, and there was a larger overall workload.

Personal Follow-Up Interviews

The follow-up interviewing began in November 1977 and continued through the following November, the end of the research data collection period. Over roughly twelve months, 907 interviews (466 six-month and 441 twelve-month) were obtained from a total of 533 respondents interviewed at intake.

Obviously, however, the scope and complexity of this data collection was not only/or primarily a function of the number of cases; there were many other factors. Because of difficulties contacting respondents, follow-up interviews could not necessarily be conducted on or soon after the date they were due. (As an illustration of the extreme cases, almost fifteen percent (65) of the six-month interviews were obtained more than six months *after* they came due.)[4] A further complication of the follow-up stages was that, as discussed previously,a portion of the interviewing staff's time between May and October 1978 was devoted to a final effort to obtain intake interviews. For most of this 12-month period, therefore, the staff was conducting two or three *different* interviews involving the same group of respondents (intake, and/or six-month, and/or 12-month) simultaneously. As a consequence, the follow-up interviewing was more time-consuming and more difficult than the intake phase. It was extremely labor-intensive. Not only was the entire interviewing staff (five interviewers and a supervisor) involved, including substantial overtime, but additional staff were hired at different points to do both field contact work and office interviewing, and two other members of the permanent research staff filled in at times.

[4]To handle this from the standpoint of data analysis, all the respondent's activities were constructed as continuous variables, so that, for example, a respondent's work history was coded in two week intervals over the entire time period from one-year prior to research assignment to one-year after, regardless of when interviews were obtained.

Initial Follow-Up Efforts

The research staff had delayed starting the follow-ups in order to extend sample selection through the end of October 1977, and to concentrate interviewing efforts on intake cases. During August and September a limited amount of work was done with follow-up cases, partly to permit a pretest of the six-month questionnaire instrument and partly to assess how difficult it would be to recontact respondents. With very minimal effort (sending a letter and, in a few cases, making a couple of phone calls) about half the respondents due for interviews in those months were contacted and successfully scheduled for interviews in the office. This (unfortunately illusory) good response rate led to the decision not to assign any of the interviewing staff to work on six-month cases until the intake interviewing was ended, and furthermore to primarily utilize phone and mail efforts, rather than field work, for contacting the six-month cases.

This strategy resulted in about 300 cases being due for a six-month interview by early November, with 100 cases added by January. The initial caseload assigned to each member of the interviewing staff, therefore, when follow-up began in earnest, was over 60 cases -- approximately double the caseload for field interviewers during the intake phase. In order to contact and interview such a large number of respondents as expeditiously as possible, the staff tried to schedule interview appointments in the office (rather than at respondents' homes), and relied almost exclusively on letters and phone calls to contact respondents. The target for completing these

400 interviews was just over two months, since after mid-January the first 12-month cases would come due for interviews. At the outset this goal seemed possible because phone numbers and addresses had been obtained directly from respondents, rather than from official (PTSA) sources, and because of the apparent effectiveness of office-based work during the exploratory follow-up period.

However, the difficulty obtaining follow-up interviews was underestimated for numerous reasons. First, the scheduling of interviews was more complex than anticipated. When respondents phoned in to the office in response to a message left by an interviewer, they typically asked for the interviewer by name. The receptionist was provided with a list of all respondents and trained to pass them to another interviewer if the interviewer asked for was not available. Having the receptionist simply take a message was inadequate because many respondents would not call again, and because they often called from a phone booth, we could not return their call.

Further problems were created by no-shows, respondents coming in hours late for an appointment, and those simply who walked in without an appointment. Here again, we had to follow a policy of conducting an interview whenever a respondent showed up, no matter how inconvenient, because of our past problems with rescheduling interviews. The net effect was analogous to our problem with our court operations during the intake phase: it was essential that several interviewers *always* be available in the office whether or not anything was scheduled. Some appointments necessarily had to be set-up at respondents' homes, a

court office, a place of employment, or a detention facility, so all five interviewers seldom could be in the office all day. A rotating schedule was designed to ensure that two or three people at a minimum, were available to conduct interviews at all times. Even so the supervisor and other members of the research staff frequently had to conduct interviews.

The daily volume of interviews fluctuated enormously during these two months, ranging from two or three to 15 or 16, which added difficulty in scheduling and tracking the caseloads of each interviewer. Often an interviewer had to schedule an office interview at a time when he or she did not expect to be free, so that the schedules of the interviewers had to be constantly changed. A central posting of all scheduled interviews was used, but there was inevitable changes and scheduling mismatches. For example, an interviewer might set up an appointment with a respondent, while another interviewer was setting up another interview for the same time. Given the unreliability of many of the respondents and the resulting "catch them when you can get them" approach we had to use, there had to be an almost constant monitoring of the interviewing schedule in order to cover all the interviews.

Likewise, all the interviewers -- whether physically in the office or not -- had to be kept informed of *who* had been interviewed or scheduled, to avoid duplication of effort. It was not uncommon for one interviewer to send the initial letter to a respondent, then have the

appointment (or appointments) subsequently made by another interviewer who took the phone call, and to have the actual interview conducted by still a third at an unscheduled time.

In this hectic setting, several other factors further complicated the staff's work. There were some respondents with the same or similar names in our sample, and many (as described in the previous section) who used more than one name. Aside from the obvious necessity of knowing *which* respondent we were talking to, we had to be sure the proper questionnaire was administered. Experimentals and controls, for example, were asked very different questions in the six month instrument. Furthermore, many questions in the six-month instrument were keyed to events that had taken place between each respondent's specific date of intake and the present. During the first few weeks of this period, several interviews had to be redone because one respondent's name was mistaken for another. For example "Julio Torres," a hypothetical experimental selected into the sample in June, might have been administered a *control* questionnaire based on a *February* intake date because he gave his name as "Joe Torres," another research subject. While there were only a few such mistakes, the necessary attention to such details was time consuming.

The size of the workload was also harder to deal with because one interviewer was hospitalized just as the full follow-up effort began. A temporary replacement had to be hired, and the normal difficulties of training and orientati n were magnified by the pressured environment.

By mid-January, over 125 six-month cases remained uninterviewed, a majority of which were several months overdue for the interview. There were three main reasons for this backlog. First, we found that the contact information obtained in the intake interviews was not as useful or reliable as we had expected. A major shortcoming was an insufficiency of alternative addresses and phone numbers, since we continued to have letters returned and found phones disconnected. While overall, the home addresses and phone numbers given us by respondents were more reliable than the initial PTSA contact information, they were not as efficient a means of contact as anticipated. Simply asking, as we did in many of the intake interviews, if there was somewhere else to contact the respondent had not yielded sufficient information. We found that it was necessary to *insist* on having at least one home or other phone number given us by the respondent and at least one alternate person to contact in order to have the maximum likelihood of contacting the respondent later. Unfortunately, even these contacts were often not reliable because of the unstable lives not only of the members of the research population, but of their friends and families.

A second reason for the backlog was that we had not, albeit deliberately, gone in to the *field* to contact respondents unless it was necessary to secure an appointment. Given the heavy workload in the office, the time-consuming nature of field work, and the size of our staff, we tried to limit the field work. In retrospect, however, it seems clear that this exacerbated the backlog problem. While there does not seem to be any consistent way to predict the best means of

contacting respondents such as those in our sample, the increasing difficulty of making contact as time passed is a key issue. Since many respondents proved difficult to reach by phone or mail, the best way to minimize lag time in contacting respondents ultimately moved to be the inclusion of some field work *from the outset*. In our experience, avoiding early field work was false efficiency based on an illusory amount of success in the pretest efforts. Respondents were eventually *extremely* difficult to contact using only the mails and telephone.

The third major factor in the creation of this backlog was, in our opinion, the most crucial, particularly in terms of the practical lessons that can be of use to future research data collection efforts. During the first months there was not enough monitoring done to review the progress of specific cases. The interviewers were, in the classic situation, faced by social workers with large caseloads. They were always busy with situations requiring immediate attention: making appointments on the phone, conducting interviews, and making initial contact efforts with respondents who were new additions to their caseload. They did not have the leisure to do more than a cursory review of every "open" case, so it was easy to let time slip by with few contact efforts on cases that did not initially appear to be problems (e.g., cases in which a respondent had called for appointments a couple of times and not shown up, but seemed easy to contact). They also tended not to expend the *extraordinary* efforts for cases that required a great deal of frustrating and fruitless work before any results were achieved (respondents, for example, whose families kept taking messages, saying that calls and/or visits would only be of value if the respondent was actually at home).

In addition, the supervisor did not have enough time to regularly discuss the action that should be taken on as many as 30 or more cases with each of five interviewers. Setting out in detail a comprehensive strategy for contacting or inducing each hard-to-pin-down respondent to be interviewed was not consistently done, both because not enough time was allocated to doing so, and because it seemed more important to aim supervision at the *majority* of an individual's cases rather than at discussing each one. In retrospect, the importance of such detailed monitoring by both interviewers and supervisor is underscored by the fact that closer monitoring would have revealed much earlier our error in avoiding regular fieldwork.

There also were cases in the backlog which could have been interviewed during those first couple of months if greater attention had been paid to systematically following through on telephone and letter contacts. Not enough emphasis, for example, was placed on spending *large* amounts of time *repeatedly* calling respondents with working phone numbers, or on setting up a regular system for making such calls outside the daily office environment. Some cases were too readily considered lost or uncontactable except in the field. Later experience revealed that *repeated* mail and phone efforts, continued over a long period of time, could result in a successful contact and interview even if the initial results were discouraging. This was particularly true of respondents whom we were told had moved or whose whereabouts were unknown. It might take months, but it was not uncommon to suddenly find a heretofore lost respondent coming to the phone or responding to a letter, having just stopped by or moved back.

In addition, cases would for a time simply slip through the cracks. As described earlier, most of the interviewing staff was not initially experienced and thus not accustomed to the extremely detailed tracking, logging and follow-up techniques necessary to ensure that all possible bases had been touched with each one of a large number of cases. Some cases, therefore, remained untouched for too long after an initial contact effort, while the interviewer was concentrating on other cases, and by the time work on those cases was resumed, some respondents were uncontactable. Without a rigorous monitoring on a regular basis of all cases, some respondents dropped out of sight.

Full-Scale Field Efforts

Both the substance and the character of the follow-up field work changed significantly after the initial months. In January the interviewing staff began regularly working in the field, in addition to the office scheduling and interviewing. Twelve-month interviews also began at that time, and for the remainder of the data collection period, we were dealing with both six-month and 12-month interviews. In addition, the final effort to secure intake interviews (described in the previous section), began in May and continued through October. Finally, the whole tenor of the interviewing staff's work changed; intensive work on smaller numbers of cases replaced the previous pressured efforts to deal with a massive caseload.

In a number of ways, the final phase was the most difficult and demanding period of the entire data collection effort. The interviewing staff frequently spent 60 to 70 percent or more of each week in the field, and overtime work in the field during evenings and weekends became necessary. (This assumes a greater significance when one considers that the first few months of this field work period was at the height of a very rough winter which included two of the heaviest blizzards of the past few decades.)

Aside from weather conditions, moreover, this continual field work was as training on the staff. Simple frustration commonly affected the amount and quality of the interviewing staff's work; they had to spend a great deal of time over an extended period (up to ten months) working on cases that did not readily yeild results. Aside from cases that had come due for a particular interview each month, each member of the interviewing staff regularly had as many as 20 cases which had been worked on unsuccessfully for several months or more. Such cases, as will be described below, usually required repeated contact efforts which consumed enormous amounts of time and produced few results. Normal contact methods failed in so many cases that it became necessary to try strategies we knew were unlikely to succeed, just on the off chance of getting results. Under such circumstances high staff motivation was essential but often very difficult to maintain.

The basic problems encountered in conducting the field work effort have been described in the previous section, so here we limit discussion to the particular methods employed during the final follow-up period. During the first few months - January through March - field efforts

were similar to those of the last part of the intake phase; by April, however, it became apparent that increased field efforts would be necessary. We had started regular field work in January with a backlog of over 125 six-month cases, and at the end of April we had a combined backlog (six-month and 12-month) of almost 160 cases. These difficult cases had to be given special attention, since they only became more difficult as time went by. The research staff, therefore, instituted an all out "blitz" effort on the backlog cases while continuing work on the current cases.

This effort primarily involved an intensification of techniques used before. For example, a very tightly controlled, centralized system of assignments, schedules, and case reviews was instituted. Backlog cases were assigned on a more individualized basis, taking into consideration the appropriateness of a given interviewer (in terms of language, ethnicity and overall familiarity with the geographical area). Prior familiarity with the particular respondent, as well as any favorable or hostile reactions towards that interviewer in prior contacts with the respondent and his friends or family, were also considered. Geographic grouping of cases was done more rigorously than before, taking into account which of the interviewers had a car (three of the five staff members did). The subjective judgments of interviewers who had worked on a case in the past were examined to identify approaches that might be more successful, such as concentrating on contacting a respondent's friend rather than family. Each month, as new cases were assigned, some backlog cases were rotated to avoid an interviewer getting stale on particularly hard or tedious cases.

As part of this effort, every possible source of contact information available to us was reviewed for every case. A sweep of PTSA arraignment records was done, for example, to look for *old* addresses or aliases which might be tried. Copies of the full PTSA interview reports, giving past contacts were reviewed, as was information (such as past employment or leisure activities) from our own intake interviews that might provide a lead. For CEP participants, the agency's case folders were re-read to see if a counselor had found a way to contact a respondent or had information on a subsequent job or training program through which we could reach the respondent. As it turned out, the alternate contacts listed on the PTSA reports yielded the greatest amount of useful information, even if the addresses or phone numbers were a year old; this was the first time we had consistently tried to make use of *past* addresses, and it was unexpectedly useful. For example, we sometimes could contact a relative with whom a respondent had once lived and find out where that (otherwise lost) respondent might now be located.

The schedules of the interviewing staff were arranged to allow them the greatest amount of time in the field while providing for regular coverage of the office. One day a week, in rotation, was designated as an individual's office day. Unless there were special interviewing needs (e.g., a Spanish-speaking staff member, or several interviews scheduled around the same time), other interviewers spent the balance of the time in the field. Once or twice a day, all staff members in the field called the supervisor who would inform them of any office needs, bring them up to date on any office

contacts made with respondents assigned to them, and pass on any cases for whom new addresses had been found (e.g., from an arrest check) in the neighborhood in which they were working.

This activity -- contact between field and office and passing on immediately work that could be done in the field -- was extremely difficult to coordinate but worth the effort. Given the erratic contacts with respondents and the amount of time being spent in the field, it was very useful to quickly "field" a case over the phone rather than wait a day.

At least once a week, each interviewer reviewed his or her caseload with the supervisor. Each case, whether recent or backlog, was discussed individually to determine what had been achieved, what actions had beer taken, and what further actions were needed. Typical issues in these conferences were the relative efficacy of telephoning vs. field visits on an intensive basis; whether a different interviewer (a woman instead of a man, an interviewer not known to the respondent instead of the "same old face," black instead of Hispanic) might get better results; and whether a case was at a dead end. In addition to these regular case reviews, interviewers would give cases to the supervisor whenever they felt they had exhausted all means of contact. These cases would either be put aside as lost or, if further constructive action could be identified by the supervisor, returned to the interviewer or another one with specific recommendations.

One of the most important features of both the monitoring and assignment procedures was that not all the backlog cases were assigned all the time. Each interviewer would have as few as six (generally no

more than eight or nine) hard cases, and would work intensively on those. This enabled the supervisor to keep track more easily of what had been or might be done, particularly if an interviewer was running out of inspiration or patience on a case. It also concentrated the interviewers' full attention and effort on the full range of field work that could be tried on each case.

This system of case management was not put in operation all at one time. It was originally set up somewhat more loosely, and refined over the course of several months as various aspects assumed greater importance. It has been described as a totality because this more rigorous and comprehensive system ultimately achieved the best results in terms of both the number of completed interviews and smoothness of office operations. The same staff was handling two waves of interviews (three, when the final push for intake interviews was conducted between May and October, 1978) with cases that varied greatly in their level of difficulty, and had (with one exception) only minimal experience prior to this project. Under these circumstances, basic coordination and monitoring of efforts had a particular importance.

In order to implement such extensive coordination of field and office activities, however, two conditions had to exist. First a large amount of supervisory time and attention had to be available to concentrate *solely* on these aspects of the data collection effort. They were not emphasized as much during the first six months of intake because a staff member whose sole concern was the daily administration of the interviews was not available. Even during the initial follow-up phase, when there *was* such a supervisory staff member, this person's efforts were often diverted by -- among other

things -- the need to conduct many interviews personally. The second necessary condition was having enough personnel to permit staff flexibility. The preemptive need to have the court offices always staffed for interviews during the intake period, and later to have staff available for conducting interviews in the office, precluded the optimum coordination of the interviewing staff which was possible later.

With respect to the character of the actual field activities of the interviewing staff, the difficulties and extended time demands were similar to those described in the previous section. What was different was the deliberately *intensive* approach taken during this period. Instead of trying to *minimize* time spent in the field by the interviewing staff, emphasis was placed on *increasing* the time spent on individual cases in the field regardless of efficiency. The research staff realized that extraordinary efforts were needed to locate and contact respondents in backlog cases, and that labor-efficient methods did not work.

Typical of the difficult cases were respondents with no telephones, for whom letters sent to home addresses had either been returned by the post office or not responded to, and in which multiple field visits had been made only to find no one home or to be told that "we haven't seen him in a while." Based on the street knowledge of our interviewing staff and the experiences of other research efforts with a similar population, we could surmise that many such respondents *actually* were somewhere in the area. The

difficulty was in getting face-to-face with the respondent, who might be spending most of his time hanging out somewhere in the neighborhood rather than at his home, or might have moved a couple of times but still be in the area without having left any formal forwarding addresses.

To overcome these obstacles, we assigned cases to interviewers who knew, or were comfortable with the neighborhoods of particular respondents. These interviewers spent up to several days in a neighborhood simply circulating: talking to local people in the playgrounds, on building stoops, in local hangouts, and in neighboring buildings and apartments. They would ask about a respondent informally, and try to find out where he could be found. For example, whenever possible, interviewers asked for a respondent by his nickname (which had been asked for in our intake interviews) rather than the full name (e.g., "Hey man, do you know where Chico hangs out?"). When we did not have a current address, the interviewers sometimes went through apartment buildings and neighboring houses, asking if people knew where the family had moved. In this way, sometimes they would meet someone who was a friend of the respondent or his family, and could leave a message for the respondent to call us.

Another strategy was to visit addresses that seemed to be "good" (i.e., we had reason to believe that the respondent and/or his family did live there) at odd hours: early in the morning, late in the evening, or on the weekends. In such cases we either had

previously gotten no answer during working hours (either on field visits or telephone calls) or had been told that the respondent was simply difficult to catch at home. One difference between these efforts and earlier ones is that we often knew enough about individual respondents by this time to schedule visits at more likely times. Another difference, particularly when we did not have the advantage of previous contacts with a family member or a close friend, was that we made many more "blind" visits (without knowing anything about a respondent's schedule) at a variety of times. If daytime or weekend visits had previously yielded no results, we would try to make visits late at night. If weekends and evening visits had been unsuccessful, early morning visits were tried.

Making field visits at these unusual hours, given that many neighborhoods were dangerous and that travelling to more than one area might involve crossing unwritten community or ethnic boundaries, also meant that interviewers frequently had to work in pairs. (For example, if one interviewer who was black knew that he would be making some stops in a nearby, primarily Hispanic neighborhood, he would arrange to work together that day with a Hispanic staff member.) While this further reduced the efficiency of our field work, it was essential for getting the best results.

It is hard to assess the efficacy of each specific field effort. Many respondents successfully contacted and interviewed via these intensive efforts might not have required such measures had it been possible to gather more comprehensive and accurate contact data at an earlier stage, but we cannot be sure. We do know that the results of

the intensive efforts were visible only after a long period of seemingly fruitless work. It was common for interviewers to concentrate their major field time (3-4 days per week plus evening and weekend time) on only three or four difficult cases while still devoting some smaller portion of their time to calling or otherwise contacting more easily-reached cases. Despite very limited success with the difficult cases using less intensive field efforts, the extra work was ultimately productive. The total completion rate for six-month interviews only rose from 70 percent to about 76 percent during the first months of normal field activities. After utilizing the various intensive efforts described above, that rate rose to 86 percent.

The third major characteristic of the final period of the data collection was offering of larger amounts of money to respondents for the interviews. While hard-to-get intake cases were also offered more than $10.00, the procedures were different during the final stages. Primarily, the research staff began to use a system of flexible payments rather than setting a fixed amount for each interview. Up to a total of 50 dollars would be paid, with the specific amount varying according to how difficult a respondent had been to contact and interview. A backlog case, for instance, which had been unsuccessfully worked on in the field for some time would warrant an offer of 15 to 20 dollars in subsequent contacts with the respondent. That amount could be raised to 25 or 30 dollars for a case in which there had been no response from the individual for several weeks, but where the staff had reason to believe he was receiving the messages.

As an inducement for them to appear promptly,a "conditional bonus" was used with respondents who had made and broken appointments. In such situations, a respondent was told that he would be paid an additional five dollars (on top of whatever amount had been specified) *if* he made another appointment for example, within 10 days *and* kept it.

This method had been used from the start of the follow-up phase, but until the "blitz" effort began, the total amount paid for any interview -- including a bonus for keeping an appointment -- was limited to 30 dollars. As part of the strategy to interview the most difficult cases, the research staff decided to automatically offer 50 dollars to respondents with whom we had had no contact at all or no success obtaining an interview over a long period of time. In addition, for those respondents the staff had been unable to even locate, a "finder's fee" system was instituted. When an interviewer was scouting around a neighborhood and found somebody who knew a particular respondent, that person was told he would be paid 5 dollars either to bring the respondent into our office or to put us in direct touch with the respondent (provided that the contact resulted in an interview).

With the exception of the automatic 50 dollar payment for extremely difficult cases and uninterviewed intake cases, the actual amount of money offered was discretionary. There were no set guidelines for the point at which a higher payment was used; the interviewing staff and supervisor used their own judgment on a case-by-case basis. By the time the all-out field effort began, however, several rules of

thumb were fairly consistently applied. When working on a new case which had just come due, the interviewer used only the basic $10 payment unless that respondent had previously been a no-show (or otherwise difficult to pin down for an appointment). If that was the case, the $5 conditional bonus was offered right away. Increases in the amount were in increments of 5 or 10 dollars; more drastic raises were reserved for especially difficult cases and were discussed ahead of time with the supervisor. Finally, the highest ($40-$50) payments were used as a last resort when substantial work had been done on a case without success.

It was, however, very difficult to decide whether to offer more money to an individual respondent and, if so, how much. Even after using these higher, flexible amounts for several months, it was unclear to what extent "raising the ante" was an inducement to respondents. There were certainly some respondents who told interviewers they had come for the interview only because of a larger payment, but there was no consistency in this response. Because of the difficulties involved simply in *locating* many respondents, it was not uncommon for the staff to find that a respondent had never received any previous messages until the payment had been substantially raised. In most such cases, it was difficult to ascertain whether the respondent would have responded the same way if the offer had been, say $15 rather than $50.

It was also necessary to monitor and carefully record who had been offered what amount. The rules of thumb described above were developed primarily to ensure that there was some control on costs. Since the budget for these payments was limited, it was necessary to keep close

track of the amounts offered to each respondent and to justify substantial increases. Furthermore, it was also necessary to always know the *latest* amount offered to any respondent in case he walked in or called in when the assigned interviewer was not available. If a respondent told us that he was expecting a larger payment than the staff presently knew about, it was important to be able to verify that.[5] This was an added complication to the difficulties described above in monitoring and scheduling interviews.

Several other aspects of these variable payments should be noted. Hostility was encountered from the parents of some respondents when their son or daughter was offered a relatively large amount of money (over $20) just for being interviewed. The reaction was particularly strong if a respondent had been in and out of trouble, either at school or with the police, because parents felt that such payments reinforced an already-demonstrated tendency to "hustle" or "think that it's easier not to work or go to school." Perhaps the reason this reaction was not encountered more often was that, in general, those respondents who were offered the highest payments had been difficult to contact because they were *not* close to or in regular contact with their families. This is an issue, nevertheless, which should be of concern in any similar effort.

[5]As discussed below, some respondents apparently knew that acquaintances had been paid larger amounts. On a couple of occasions, respondents told us fallaciously that the amount offered to them had been larger than it actually was.

The interviewing staff also felt that some respondents did not respond or come for appointments because they were deliberately waiting for "the ante to rise." This may have occurred when a respondent had been paid an amount greater than $10 for a previous interview, and therefore thought we would probably raise the amount again. (Consequently, we generally reserved payments over $10 for very difficult cases only.) Less commonly, a few respondents knew other respondents who received a fairly substantial payment after the passage of much time, and so waited to respond until a similar amount was offered to *them*.

Overall, however, the use of higher payments at flexible levels seems to have had a positive effect on our follow-up efforts. It was one part of the multi-faceted effort which -- as illustrated by the completion rates -- eventually had the desired effect. Clearly, *no* single aspect of the "blitz" effort was sufficient for all those interviews that were successfully obtained; it was the *combination* of special efforts which succeeded, along with the continuation of those efforts over an extended period.

TELEPHONE FOLLOW-UP INTERVIEWS

These interviews, as originally designed, were to have several purposes. One was to gather "interim" data from respondents at a mid-point in time between the various personal interviews. Another purpose was to maintain contact with respondents, some of whom the research staff had anticipated from the start would be difficult to

relocate as time passed. In the latter consideration, we were building on previous research data collection experience with similar populations of respondents. It was expected that respondents in our sample would be easier to recontact for a subsequent interview if there was some direct communication in the interim. Furthermore, these interim contacts were intended to enable us to obtain necessary data and forms which were either unintentionally skipped over or unobtainable during an interview. Specific factual information (Social Security or Welfare numbers, for instance) and signatures on one of the various release forms were typical examples of missing items.

A separate staff member was brought in to work full-time on this effort, and a very brief questionnaire instrument (5-10 minutes to administer) was developed to obtain basic data that followed up on the respondent's school, employment, court involvement, and income status since the intake interview three months before. Respondents who had no telephone, or were difficult to reach by telephone, were sent a letter asking them to call the Vera research office for a short telephone interview -- for which they would be sent five dollars. Any forms that needed to be filled out or signed were appended to this letter with a stamped return envelope. A "tickler" card file was set up to inform the telephone interviewer which respondents were due to be contacted each week and to identify missing data to be obtained from them.

The three purposes of these phone interviews were only marginally served by this system. Earlier in this section the unreliability of the addresses and phone numbers taken from PTSA reports and our intake interviews was mentioned, and this was as much a problem for the telephone interviewing as it was in our follow-up efforts. Since it was beyond the resources of the staff to spend time in the field tracking down "telephone" cases in addition to the intake cases, no further work was done if telephone contacts and letters were unsuccessful. We also found that respondents from whom we needed forms, even if they *were* interviewed on the phone, commonly did not send back the forms. The phone contacts in this effort were further hampered by calling only during business hours. After several months, telephone interviews in this format were abandoned.

CONTINUED

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APPENDIX B

COLLATERAL STUDY: SELECTION FOR CEP DIVERSION

To examine how defendants in the controlled experimental design were selected and approved for diversion to CEP prior to the research assignment and to obtain a sense for the impact of CEP diversion on the courts, the research designed a collateral study of the screening and intake operations in Manhattan and Brooklyn. As is apparent from Table B-1 in this Appendix, three-quarters of all the formally eligible cases CEP actively screened during the first six months of the research selection period (January through June 1977) were explicitly rejected by one of six major decision-makers in the system, including the defendant him or herself. These cases, therefore, were not included in the research population. In order to examine the selection procedures more fully, researchers collected data on the cases which were eligible but not diverted to CEP. Twenty-one days were chosen at random during the six month period and 594 cases not selected for CEP on those days were studied. The source of the data was CEP records on all defendants screened; these were kept in order for the agency to identify who rejected each defendant and for what reason. The research purpose was the same so the data collection instruments used during that period were developed jointly. In addition, the research collected case outcomes on all those in the sample from CJA's computerized records. The data from this collateral study are presented below in tabular form; they are discussed throughout Chapter IV.

Table B-1

NUMBER AND PERCENT OF CASES ELIGIBLE, SCREENED, REJECTED AND APPROVED FOR DIVERSION TO CEP

(Mid February - End of June 1977)

	Manhattan Criminal Court	Brooklyn Criminal Court	TOTAL
* 1. Estimated Eligibles	7,925	7,326	15,251
**2. Total Referred by CJA	1,502	3,066	4,568
3. Total Screened by CEP	952	2,134	3,086
4. Cases Missed by CEP	126	192	318
5. Rejected Cases	614	1,724	2,338
6. Approved Cases	212	218	430
7. Cases Diverted	141	133	274
Referred as Eligible (2 ÷ 1)	19%	42%	30%
Screened as percent of Referred (3 ÷ 2)	63%	70%	68%
Missed as percent of Screened (4 ÷ 3)	13%	9%	10%
Rejected as percent of Screened (5 ÷ 3)	64%	81%	76%
Approved as percent of Screened (6 ÷ 3)	22%	10%	14%
Approved as percent of Eligibles (6 ÷ 1)	2.6%	2.9%	2.8%
Cases Diverted (CEP intake) as percent of Eligibles (7 ÷ 1)	1.8%	1.8%	1.8%
Cases Diverted (CEP intake) as percent of Screened (7 ÷ 2)	15%	6%	9%

* #1 is based upon Criminal Justice Agency data: estimated number of cases interviewed where arraignment charge was C,D, or E Felony and where there was no outstanding warrant. CEP's formal eligibility criteria also exclude defendants with more than one open misdemeanor case; these cases are *not* excluded in these data.

** #2-7 are based upon CEP's Weekly Screening Reports; cases labeled "rejected" include those transferred by ADAs to other courts and those where the Defendant him or herself rejected CEP.

Table B-2

REJECT SAMPLE: WHO REJECTED CASE

PERSON REJECTING CASE FOR CEP	Manhattan (N=170)	Brooklyn (N=424)	TOTAL (N=594)
CEP Screener	34%	26%	29%
Defendant	29	33	32
ADA Liaison	21	17	18
Defense Attorney	8	13	12
Other ADA [a]	3	5	5
Judge	5	5	5
TOTAL	100%	100%	100%

[a] Cases designated by ADA for transfer to other courts (eg., family court, mediation, etc.); therefore de facto rejected for diversion to CEP.

Table B-3

REJECT SAMPLE: REASON FOR REJECTION

CASE REJECTED BY:	REASON GIVEN	Manhattan (N=170)	Brooklyn (N=424)	Total (N=594)
CEP Screener	Alcohol or drug involvement	15%	10%	12%
	Defendant formally ineligible[a]	7	7	7
	Psychopathology	-	1	1
	Other	12	8	9
Defendant	Not interested in services	24	24	24
	Will plead or fight case	5	7	7
	Other	1	2	2
ADA Liaison	Case too serious	10	9	10
	Too many priors	2	5	4
	Priors too serious	4	1	2
	Case too minor	2	1	1
	Other	2	1	1
Defense Attorney	Thinks Defendant can get better deal	8	12	11
	Other	-	1	1
Other ADA	Case transferred	3	5	4
Judge	Case too minor	4	1	2
	Case too serious	-	2	1
	No more breaks for D	2	2	2

[a] e.g., non-resident of New York City; outstanding warrant; etc.

Table B-4

REJECT SAMPLE: DISPOSITION BY BOROUGH

DISPOSITION	Manhattan (N=170)	Brooklyn (N=424)	TOTAL (N=594)
Dismissed	14%	21%	19%
ACD	23	18	20
Unconditional Discharge	1	1	1
Conditional Discharge	12	18	16
Fine/Imprisonment	7	9	8
Probation	9	5	7
Imprisonment	11	9	10
Time Served	4	2	3
Trans.to Grand Jury	9	7	8
Trans.to Other Court	1	2	2
Warrant Ordered	4	5	5
Case Still Pending	2	*	2
Disposition Unknown	6	3	2
	100%	100%	100%

*less than .5%

TABLE B-5

REJECT SAMPLE: DISPOSITION BY WHO REJECTED CASES AND BY BOROUGH

A. MANHATTAN

Disposition	CEP	Defendant	ADA Liaison	Defense Atty	Other ADA[a]	Judge
Dismissed	21%	8%	6%	15%	20%	22%
ACD	17	30	14	54	-	22
Uncon. Discharge	-	-	-	8	-	-
Con. Discharge	12	14	9	15	20	-
Fine	3	14	-	8	-	11
Probation	3	10	20	-	20	11
Imprisonment	14	-	20	-	20	22
Time Served	3	8	-	-	-	-
Trans.To Grand Jury	5	8	23	-	-	-
Warrant Ordered	9	2	3	-	-	-
Trans.to Other Court	2	-	-	-	-	-
Case Still Pending	2	2	3	-	-	-
Disposition Unknown	9	4	3	-	20	11
TOTAL = (N=170)	100% (58)	100% (50)	100% (35)	100% (13)	100% (5)	100% (9)

B. BROOKLYN

Dismissed	21%	23%	18%	19%	18%	14%
ACD	16	21	4	32	5	38
Uncon. Discharge	-	1	3	-	5	-
Con.Discharge	15	17	22	19	5	33
Fine	5	16	7	12	-	-
Probation	3	5	13	2	5	10
Imprisonment	18	5	13	-	9	5
Time Served	3	2	3	4	-	-
Trans.To Grand Jury	6	4	11	5	36	-
Warrant Ordered	5	4	6	7	-	-
Trans.To Other Court	5	1	-	-	14	-
Case Still Pending	2	1	1	-	5	-
Disposition Unknown	1	-	-	-	5	-
TOTAL (N=424)	100% (110)	100% (142)	100% (72)	100% (57)	100% (22)	100% (21)

Table Continued.../

Table B-5
Continued

C. TOTAL (Manhattan & Brooklyn)

Disposition	CEP	REJECTED BY Defendant	ADA Liaison	Defense Atty	Other ADA[a]	Judge
Dismissed	21%	19%	14%	19%	19%	17%
ACD	16	23	7	36	4	33
Uncon.Discharge	-	1	2	3	4	-
Con.Discharge	14	16	18	17	7	23
Fine	4	15	5	11	-	10
Probation	3	6	15	1	7	7
Imprisonment	16	4	15	-	11	7
Time Served	3	4	2	2	-	-
Trans.To Grand Jury	6	5	15	4	30	-
Warrant Ordered	6	4	5	6	-	-
Trans.To Other Court	4	-	-	-	11	-
Case Still Pending	2	2	2	-	4	-
Disposition Unknown	4	1	1	-	4	3
TOTAL = (N=594)	100% (170)	100% (190)	100% (107)	100% (70)	100% (27)	100% (30)

a Generally cases transferred to other courts (e.g., mediation, juvenile court, etc.) rendering case de facto rejected for CEP, in some cases, however, the transfer may not have occurred and a disposition appears in the Criminal Court record.

* less than .5%

TABLE B-6

ADA LIAISON REJECTIONS BY BOROUGH, BY REASON FOR REJECTION AND BY DISPOSITION

A. Brooklyn

DISPOSITION	Reason for Rejection: Priors Too Serious	Case Too Serious	Case Too Minor	Other
Dismissed	20%	15%	33%	25%
ACD	-	5	-	25
Uncon.Discharge	-	3	33	-
Con.Discharge	16	25	-	50
Fine	16	3	-	-
Probation	12	15	-	-
Imprisonment	24	5	33	-
Time Served	8	-	-	-
Trans.To Grand Jury	4	18	-	-
Warrant Ordered	-	10	-	-
Case Still Pending	-	3	-	-
Disposition Unknown	-	-	-	-
TOTAL	100%	100%	100%	100%
(N=72)	(25)	(40)	(3)	(4)

B. Mahattan

DISPOSITION	Reason for Rejection: Priors or Case Too Serious	Case Too Minor	Other
Dismissed	7%	-	-
ACD	11	67	-
Uncon.Discharge	-	-	-
Con.Discharge	7	-	20
Fine	-	-	-
Probation	22	-	20
Imprisonment	15	-	60
Time Served	-	-	-
Trans.To Grand Jury	26	33	-
Warrant Ordered	4	-	-
Case Still Pending	4	-	-
Disposition Unknown	4	-	-
TOTAL	100%	100%	100%
(N=72)	(27)	(3)	(5)

C. TOTAL (Brooklyn & Manhattan)

DISPOSITION	Reason for Rejection: Priors or Case Too Serious	Case Too Minor	Other
Dismissed	14%	17%	11%
ACD	6	33	11
Uncon.Discharge	1	17	-
Con.Discharge	17	-	33
Fine	6	-	-
Probation	16	-	11
Imprisonment	13	17	33
Time Served	2	-	-
Trans.To Grand Jury	16	17	-
Warrant Ordered	6	-	-
Case Still Pending	2	-	-
Disposition Unknown	1	-	-
TOTAL	100%	100%	100%
(N=107)	(92)	(6)	(9)

Table B-7

DEFENDANT REJECTION OF DIVERSION BY BOROUGH BY REASON FOR REJECTION AND BY DISPOSITION

	BOROUGH/REASON FOR REJECTION					
	Manhattan			Brooklyn		
DISPOSITION	Plead or Fight	No Services Wanted	Other	Plead or Fight	No Services Wanted	Other
Dismissed	22%	5%	-	26%	20%	40%
ACD	33	28	100	29	21	-
Uncon.Discharge	-	-	-	-	2	-
Con. Discharge	22	13	-	10	20	10
Fine	-	17	-	16	16	10
Probation	-	12	-	6	3	20
Imprisonment	-	-	-	3	4	20
Time Served	-	10	-	-	3	-
Trans.To Grand Jury	11	8	-	-	6	-
Trans.To Other Court	-	-	-	-	1	-
Warrant Ordered	11	-	-	3	5	-
Case Still Pending	-	2	-	6	-	-
Disposition Unknown	-	5	-	-	-	-
TOTAL (N=192)	100% (9)	100% (40)	100% (1)	100% (31)	100% (101)	100% (10)

TABLE B-8

DEFENSE ATTORNEY REJECTIONS BY BOROUGH, BY REASON FOR REJECTION AND BY DISPOSITION

	BOROUGH/REASON FOR REJECTION		
	Manhattan	Brooklyn	
DISPOSITION	Thinks Defendant can get a better deal	Thinks Defendant can get a better deal	Other[a]
Dismissed	15%	21%	-
ACD	54	31	40
Uncon.Discharge	8	-	-
Con. Discharge	15	19	20
Fine	8	13	-
Probation	-	-	20
Imprisonment	-	-	-
Time Served	-	4	-
Trans. To Grand Jury	-	2	20
Trans. Other Court	-	2	-
Warrant Ordered	-	8	-
Case Still Pending	-	-	-
Disposition Unknown	-	-	-
TOTAL (N)	100% (13)	100% (52)	100% (5)

[a] E.G., rejects a condition imposed by ADA.

Table B-9

JUDGE REJECTIONS BY REASON FOR REJECTION/BY DISPOSITION

	BOROUGH/REASON FOR REJECTION						
	MANHATTAN			BROOKLYN			
DISPOSITION	Case Too Serious	Case Too Minor	No More Breaks	Case Too Serious	Case Too Minor	No More Breaks	Other
Dismissed	-	33%	-	-	20%	-	33%
ACD	-	33	-	57	60	-	17
Uncon.Discharge	-	-	-	-	-	-	-
Con. Discharge	-	-	-	29	20	67	33
Fine	-	17	-	-	-	-	-
Probation	-	17	-	14	-	33	-
Imprisonment	-	-	100	-	-	-	17
Time Served	-	-	-	-	-	-	-
Trans.To Grand Jury	-	-	-	-	-	-	-
Trans. Other Court	-	-	-	-	-	-	-
Warrant Ordered	-	-	-	-	-	-	-
Case Still Pending	-	-	-	-	-	-	-
Disposition Unknown	100	-	-	-	-	-	-
TOTAL (N=)	100% (1)	100% (6)	100% (2)	100% (7)	100% (5)	100% (3)	100% (6)

Table B-10

REJECT SAMPLE: CRIMINAL HISTORY CHARACTERISTICS

CHARACTERISTIC	Manhattan (N=170)	Brooklyn (N=424)	TOTAL (N=594)
CURRENT CASE-SEVERITY:			
A-B Felony	2%	2%	2%
C Felony	10	15	13
D Felony	44	53	50
E Felony	22	24	24
Misdemeanor	13	4	7
Violation	-	*	*
N.A.	9	1	4
CURRENT CASE-TYPE OF CRIME:			
Theft (ex. robbery)	48%	48%	48%
Assault (w/o robbery)	12	24	21
Robbery	9	11	11
Weapons	7	6	6
Forgery	7	3	4
Conduct	5	3	3
Drugs	2	2	2
Morals	-	2	1
Obstructing Justice	-	*	*
N.A.	10	1	4
FIRST ARREST (SELF-REPORTED):			
No	57%	55%	55%
Yes	37	43	41
N.A.	6	2	4
PRIOR FELONY CONVICTIONS:			
None	89%	89%	89%
1	7	7	7
2	2	2	2
3	1	1	1
3+	1	*	*
N.A.	1	1	1
PRIOR MISDEMEANOR CONVICTIONS:			
None	72%	74%	73%
1	9	13	12
2	5	3	3
3	1	4	3
3+	11	5	7
N.A.	2	1	2
CURRENT OPEN CASES:			
None	59%	57%	57%
1	21	23	22
2	8	9	9
3	8	4	5
3+	2	6	5
N.A.	1	2	2

Table B-11

REJECT SAMPLE: SOCIAL AND PERSONAL CHARACTERISTICS

CHARACTERISTIC		Manhattan (N=170)	Brooklyn (N=424)	TOTAL (N=594)
SEX:	Male	92%	92%	93%
	Female	7	8	7
AGE:	15	-	1%	*
	16-17	12	19	17
	18-20	22	23	23
	21-25	37	20	20
	26+	25	36	39
	N/A	4	1	1
ETHNICITY:				
	Black	48%	52%	51%
	Hispanic	31	29	29
	White	14	18	16
	N.A.	7	1	3
LIVING ARRANGEMENTS:				
	With Parent/Guardian	38%	46%	43%
	With Spouse	15	21	19
	Alone	16	12	13
	With Friend	17	8	10
	With Other Relative	11	10	10
	With Children	-	2	2
	N.A.	3	1	2
HAVE ANY CHILDREN:				
	No	78%	65%	68%
	Yes	16	24	22
	N.A.	6	11	10
EMPLOYMENT:				
	Full-time	24%	30%	28%
	Part-time	10	7	8
	In Training Program	-	1	1
	Unemployed	61	57	58
	Disabled	1	3	2
	N.A.	4	2	3
IN SCHOOL:				
	No	79%	80%	79%
	Yes	15	18	17
	N..	6	2	4

APPENDIX C

MULTIPLE REGRESSION: SUCCESS IN CEP

Having analyzed the data within the experimental design, and having found no effect for CEP, an analysis was conducted in an attempt to determine what type of defendant was most likely to succeed in CEP. This analysis was a multiple regression that necessarily involved only members of the experimental group. In addition, since one of the conditions for successfully completing the four-month program was attendance, a second regression analysis was computed to predict which members of the experimental group were likely to attend most often.

Both analyses began with the same list of 51 predictors, including demographic characteristics of the participants, criminal history (both juvenile and adult), characteristics at intake into the program (e.g., work status, marital status, living arrangements), and data related to CEP (e.g., expectations for the program, counselor's evaluations of client needs). After the initial, exploratory analysis was computed, those variables that explained a significant amount of variance were retained for the final analysis. The initial regression was computed using a combination of hierarchical and stepwise techniques; sets of variables were entered hierarchically (based on the order of their occurrence in time), and variables within sets were entered stepwise.

Success in CEP was defined by the participant's exit status from the program -- successful participants were those who completed the four month program and received recommendations for case dismissals, and unsuccessful participants were CEP's terminations and administrative discharges (returned to the court with no recommendation). Six predictor variables were included in the final regression equation (and are presented in the order in which they entered the equation): average monthly salary during the six months prior to intake, months employed during the six months prior to intake, number of prior arrests, attendance at CEP (self-reported; no or yes), evaluation by CEP counselor as having court-related needs, and evaluation by CEP counselor as needing preparation for the world of work. The regression results are presented in Table C-1.

Table C-1

REGRESSION RESULTS FOR SUCCESS IN CEP

Variable	R	R^2	Partial r[a]	Beta[b]	F[c]
Salary	-.14	.02	-.14	-.38	7.050[d]
Mo. Employed	.22	.05	.17	.26	11.764[d]
Priors	.29	.08	.19	.12	15.550[d]
Attended CEP	.52	.27	-.45	-.42	98.245[d]
Court Needs	.56	.31	.25	.20	25.411[d]
Prep. Needs	.57	.32	.10	.09	4.276[d]

[a] Partial correlation with dependent variable at step prior to that at which variable entered the equation; zero-order correlation is presented for first variable to enter.

[b] Beta is that for final step.

[c] Significance test of Beta weight at step in which variable entered the equation.

[d] $p<.01$.

The multiple correlation coefficient obtained using these six variables as predictors was $R=.57$ ($F(6,193)= 26.232$; $p<.01$). It is clear from the results presented in Table C-1 that the strongest predictor of success in CEP was attendance. But there are other variables of interest included in the equation. Those participants who successfully completed the program were most likely to have had a high salary (relative to other CEP participants) during the six months *prior* to entering the program, to have worked more months and to have had few prior arrests (perhaps no priors). Then, in addition to attending CEP, they were likely to have been evaluated by their CEP counselors as having court-related needs and to need preparation for the world of work. Thus, we can see that vocational activity prior to entering CEP has some predictive power, while demographic characteristics such as age, sex, and ethnicity are unrelated to success in CEP. Furthermore, while number of prior arrests explains some of the variance associated with success in CEP, other criminal history variables do not. Since the experimental data provided no evidence for effects of CEP on lifestyle or recidivism, and the regression analysis does not provide definitive information as to who is the best candidate for CEP, further research is necessary to answer these questions.

Since success in CEP is clearly determined by attendance, a regression analysis was computed to determine which participants attended CEP most often. This analysis contained five predictors and produced a multiple correlation of $R=.78$ ($F(5,194)=121.898$; $p<.01$).

The variables used to predict CEP attendance (self-reported; never/once or twice/three or four times/more often) were number of arrests prior to intake, vocational status at intake (employed/unemployed), belief that a condition for successful completion of the program was attendance (no/yes), statement of other conditions (eg., employment, school, staying out of trouble) for successful completion of the program, and belief about the fairness of these conditions (no/mixed/yes). The variables were entered in the order presented, except that the last three variables (beliefs about CEP) were entered simultaneously (on the third step). The regression results are presented in Table C-2. Clearly the strongest predictor of attendance is the belief that attending the program is a condition for successful completion. Those participants who believe that they have to attend CEP to get their charges dismissed are the defendants who attend most often.

Table C-2

REGRESSION RESULTS FOR CEP ATTENDANCE

Variable	R	R^2	Partial r[a]	Beta[b]	F[c]
Priors	.14	.02	-.14	.02	7.991[d]
Voc. Status	.29	.08	.25	.22	26.780[d]
Cond:Attend	.78	.61	.72	.70	450.214[d]
Cond:Other	.78	.61	.29	.20	41.216[d]
Fair	.78	.61	-.02	.12	12.785[d]

[a] Partial correlation with dependent variable at step prior to that at which variable entered the equation; zero-order correlation is presented for first variable to enter.

[b] Beta is that in final step.

[c] Significance test of Beta weight at step in which variable entered the equation.

[d] p.<.01.

APPENDIX D

AGE EFFECTS ON LIFE STABILITY VARIABLES

The lack of any differences between experimentals and controls on the variables computed from the timeline (and discussed in Chapter V) led us to consider the possibility that CEP might affect various groups within its service population differently. That is, if CEP had no impact on some subgroup, positive impact on another, and negative impact on a third, an analysis on the aggregate might well mask its effects. Age was considered an important variable for an investigation of this possibility. Therefore, analyses of partial variance[1] were conducted on each of the measures discussed in Chapter V to determine whether there were any changes on any of them from before intake to the six month follow-up period as a function of age and research assignment.

[1] Analysis of partial variance is a special case of hierarchical multiple regression analysis. A set of independent variables (A), covariates, that is believed to potentially distort the impact in the variance of a dependent variable (Y) is partialled from Y. Then, another set of independent variables (B), representing the research focus, is entered into the equation. In this manner conclusions can be drawn about set B, with set A statistically controlled or held constant (Cohen & Cohen, 1975:p. 364). In the present research Y is any one of the timeline variables, for example, number of months employed during the six months after intake; A is composed of months employed during the six months prior to intake and age at intake; and B is research assignment, experimental or control. Using analysis of partial variance, we are able to test for differences between experimentals and controls in change over time in number of months employed, after removing age effects. Thus, any possible distortion due to the relationship between age at intake and employment would be removed before research assignment was entered into the regression equation.

The seven employment variables discussed above were analyzed using this method. Three of them, dichotomous employment at a point six months after intake, hours worked per week on the date six months after intake, and number of jobs held during the period six months after intake, showed no age effects. That is, age at intake was uncorrelated with any of these variables. The remaining employment variables did show some age effects and are discussed below.

The most general measure of employment was the change in the number of months spent working from six months prior to intake to the following six months. Age at intake was significantly but weakly related to both number of months worked during the intake period ($\underline{r}(328)=.14;p<.05$)and number of months worked during the six month follow-up period ($\underline{r}(328)=.15;p<.01$. Number of months worked prior to intake was an extremely weak predictor of months worked subsequent to intake ($\underline{r}$ = .05;$\underline{t}$ (328)=4.15; p<.001), and although the increment in R^2 for age was significant ($R^2=.06;\underline{t}(327)=2.21$; p=.03), it is so small that it is not substantively meaningful. That is, knowing both number of months worked during the intake period and age at intake, we can only explain 6.4 percent of the variance in number of months worked during the follow-up period. Furthermore, there was no

effect for research status; specifically, regressed change[2] from intake to follow-up in months employed cannot be predicted from research status, even when age is controlled for.

The age effect on average monthly salary during a six month period is stronger. As we discussed in Chapter V, monthly salary during the intake period was related to average monthly salary during the first follow-up period ($\underline{r}(140)=.23;p=.005$). In addition, while age was not related to monthly salary prior to intake ($\underline{r}(140)=.16$), it was related to salary during the subsequent six month period ($\underline{r}(140)=.30$; p<.001). The addition of age at intake to the regression equation significantly increased ability to predict a respondent's salary six months after intake ($R^2=.13;\underline{t}(139)=3.37$; p=.001). The more money an individual was earning during the intake period, the more he/she was likely to be earning during the follow-up period; furthermore, while average monthly salary increased from intake to follow-up, the older members of the population earned more than younger members. The addition of research assignment to the equation did not produce a significant increment in $\underline{R}$. Thus, if one wanted to explain the variation in salary between members of the

[2] Analysis of partial variance is used to produce regressed change scores. There is a distinction between an analysis of change over time that used partial correlations and that which used difference scores. To use difference scores, for each member of the population one would subtract the number of months worked during the follow-up period; then the unit of analysis would be the result of that subtraction. On the other hand, in analysis of partial variance the number of months worked during the intake period is *not* subtracted from but rather held statistically constant from the number of months worked during the follow-up period. The result is a "regressed change score"; that is, we are left with the number of months worked during the follow-up period after *controlling for* number of months worked during the intake period. (See Cohen & Cohen, 1975.)

research population, he/she could explain about 13 percent of the variance in salary during the six months and age at intake, but knowing whether the individual was an experimental or a control would not provide additional information about salary. Age was included in the analyses in an attempt to "unmask" CEP effects that might be hidden by age; the results indicate that if CEP has an impact on monthly salary the effect is not obscured by age.

Analysis of partial variance was computed on a second salary variable measured on the timeline. This variable is total weekly salary (at a point in time) six months after intake and is likely to be less reliable than average salary over a six month period (see discussion in Chapter V); however, this variable contained more cases with complete information, and therefore a larger N. The results, however, are quite similar to those generated by the analysis on average monthly salary. Salary at intake was correlated with salary six months later ($\underline{r}(332)=.29$; $p<.001$); age was correlated both with salary at intake ($\underline{r}(332)=.22$; $p<.001$) and with salary six months later ($\underline{r}(332)=.27$; $p<.001$). The increment in R^2 for age at intake was significant ($R^2=.13$; $\underline{t}(331)=4.09$; $p<.001$), and there was no effect for research assignment. The results of this analysis are virtually identical to those for monthly salary, and the conclusions one can draw are also the same.

The most complicated relationship involving age at intake was with average number of hours worked per month during the first follow-up period. Average hours worked per month during the six months prior to intake was significantly correlated with average hours worked per month during the follow-up period ($\underline{r}(208)=.30$; $p<.001$),

and for both experimentals and controls there was a slight increase from intake to follow-up in the number of hours worked. (This increase is discussed in Chapter V.) Age at intake is also significantly correlated with hours worked prior to intake ($\underline{r}(208)=.32$; $p<.001$) and with hours worked during the follow-up period ($\underline{r}(208)=.23$; $p<.001$). The older the person was when he/she entered the research population, the more hours per month he/she was likely to have worked during the previous six months and the more hours per month he/she was likely to work during the subsequent six months. The analysis of partial variance indicated that the change in hours worked per month was also significantly related to age ($\underline{R}=.33$; $\underline{F}(2,207)=12.83$; $p<.001$); older members of the population tended to show a larger increase from intake to follow-up in hours worked per month. The addition of research assignment did not significantly increment the multiple correlation; however, the interaction (as measured by the partial correlation = -.25) between age and research assignment on hours worked per month was significant ($\underline{R}=.41$; $\underline{F}(4,205)=10.30$; $p<.001$). The effect of age on hours worked per month was different for experimentals than it was for controls; older experimentals tended to increase their hours more from intake to follow-up than did younger experimentals. In contrast, the increase in hours worked from intake to follow-up was constant across age groups for controls. For illustrative purposes sample predictions using the regression equation produced by this analysis are presented in Table D-1.

Table D-1

PREDICTIONS OF HOURS WORKED
FOR EXPERIMENTALS AND CONTROLS

$\hat{Y} = .2232X_1 +$

Age	Experimental	Control
16	22.5	31.4
18	30.4	31.0
20	38.4	30.6
22	46.3	30.2

Where $\hat{Y}$ = Monthly hours worked during follow-up and X_1 = Monthly hours worked during intake.

Table D-1 is designed to illustrate the interaction between age and research assignment; the column of figures for the experimental group shows that the older the individual the larger the figure one should add to the product of .2232 and X_1 (monthly hours during intake period). In contrast, if the individual was a control, the best prediction of hours worked per month during the follow-up period is obtained by multiplying the hours worked during intake by .2232 and then adding 31.

The implication of these data is that CEP may have had different effects for participants of different ages, that older members of the population made larger gains in employment as compared to control group members, while younger CEP clients made smaller employment gains than control group members. A note of caution is in order, however; while the effects discussed above

are both statistically significant and interesting, they are small.[3] This is clear from an examination of the magnitude of the multiple correlation at each step; the squared multiple correlation of hours worked per month at follow-up with intake hours worked per month and research assignment is R^2=.10 ($F(2,212)$=11.28; $p<.001$); when age and the interaction between age at intake and research status are added the R^2=.17 ($F(4,205)$=10.30; $p<.001$). In other words, 10 percent of the variance in hours worked per month at follow-up is explained by research assignment and hours worked per month during the intake period; and 17 percent of the variance is explained by the combination of hours worked during the intake period, research assignment, age at intake, and the interaction of age and research assignment. Even with these four variables, 83 percent of the variance in hours worked per month remains unexplained.

Two education variables were included in the analysis -- average number of days of school attendance per week (at a point) six months after intake and number of months enrolled in school during the follow-up period. Age at intake was related to school attendance at intake ($r(332)$=-.27; $p<.001$) and to school attendance six months later ($r(332)$=-.31; $p<.001$); the older a respondent was at the time he/she entered the research population, the fewer days per week he/she was

[3]The other caution was discussed previously; that is, when a large number of significance tests are conducted the experiment-wide error rate is much larger than the alpha level for the particular test under discussion, so that the confidence in the reliability of any one significant result is reduced.

likely to attend school. In the present research this can most likely be translated as, the older the respondent the less likely it is that he/she attended school at all. The first prediction variable in the multiple regression equation was weekly attendance at intake; age accounted for a significant increment in explaining the variance in attendance six months after intake ($\underline{t}(331)=-4.14$; $p<.001$). The squared correlation between days of attendance at intake and days of attendance six months later was $\underline{R}^2=.19$; with the addition of age at intake the multiple correlation squared was $\underline{R}^2=.23$. The more days per week an individual was attending school at intake, the more days he/she was likely to be attending six months later, and younger respondents were likely to be attending more days per week than were older members of the research population. There was no significant difference between experimentals and controls in the amount of change in attendance.

The second education variable was number of months enrolled in school during the six months after intake (notice that this variable includes data from an entire six month period rather than from one point in time). Age at intake was significantly correlated with number of months the person was enrolled in school during the six months prior to intake ($\underline{r}(332)=-.29$; $p<.001$), and was also correlated with the number of months enrolled in school during the six months after intake ($\underline{r}(332)=-.31$; $p<.001$). These correlations are virtually identical to those found for weekly attendance, supporting the theory that older respondents did not attend school at all, i.e., were not enrolled. As with weekly attendance, some of the variance in months enrolled in school during the follow-up

period can be predicted from months in school prior to intake ($\underline{r}=.21$; $\underline{F}(1,332)=86.73$; $p<.001$), and age significantly increased the predictive power ($\underline{r}=.24$; $\underline{F}(2,331)=53.34$; $p<.001$). The more months one was enrolled in school during the intake period the more months he/she was likely to be in school during the follow-up period, and in addition, younger respondents tended to spend more months in school during the follow-up period than did older members of the population. However, there was no significant increment for research assignment; after controlling for months enrolled during the intake period and age at intake, research assignment did not provide additional information. These data lead to the conclusion that partialling age from the education variables does not reveal any CEP effects.

As was discussed previously, general level of vocational activity was measured by summing the total months active at employment, school, job search, military service, and childcare. A second vocational activity variable indicated whether the individual was active at a point in time six months after intake. Analyses of partial variance were conducted on both variables; neither yielded any significant age effects.

Overall, then, the analysis of age effects on employment, education, and activity failed to produce any strong effects. While age was related to employment variables (older members of the population tended to be employed more) and to education variables (younger respondents tended to spend more time in school), only for one variable was there any additional effect involving research

assignment. That is, older experimentals tended to work more hours during the follow-up period than did younger experimentals, and there was no age difference for control group members. As was discussed earlier, this is very weak evidence for any program effect, and one must be extremely cautious about relying on this one significant effect.

APPENDIX E

MULTIPLE REGRESSION ANALYSES: RECIDIVISM

After analyzing recidivism data within the experimental design (i.e., comparing experimentals and controls), a series of exploratory multiple regression analyses were computed in an attempt to shed some light on recidivism in the research population. These analyses were conducted as hypothesis generating rather than hypothesis testing and were used for descriptive purposes in Chapter VI.

Four criterion variables were used: dichotomous rearrest (yes/no), number of subsequent arrests, severity of rearrest charge (the most serious if there were more than one), and number of subsequent convictions. For each of these criterion variables the same procedure was followed. An initial regression was computed using 31 predictor variables, selected because they made theoretical sense. These included demographic characteristics (such as, whether the defendant was born in New York City, amount of time his/her family spent on welfare during the defendant's adolescence, ethnicity, sex, marital status at intake into the research); criminal history (age at first juvenile arrest, number of juvenile arrests, age at first adult arrest, number of prior arrests, prior conviction records, etc.), and lifestyle during the period of the research (employment, school activity, alcohol use, drug use, attendance at CEP, marital status one year after intake, etc.). The initial regression equation combined hierarchical and stepwise procedures for entering predictors;

that is, sets of variables were entered hierarchically depending upon when they occured in time (characteristics of the defendant's childhood occurred before entry into CEP, and were therefore entered into the equation first). Within sets, however, the variables were entered stepwise -- those variables with the highest partial correlation with the criterion variable enter first. This method was used because we had no hypothetical model on which to rely, and because the analysis was exploratory, we were willing to accept the disadvantages inherent in this approach. This process yielded four regressions, each with 31 predictors, some of which accounted for no significant variance. Those variables not accounting for a significant amount of variance were eliminated, and separate regression analyses were computed for each of the recidivism variables. The results of these analyses are presented below.

Five predictors were retained for the final regression on dichotomous rearrest; they were (entered in the order presented below) sex of defendant, amount of time the defendant's family spent on welfare while he/she was an adolescent, prior conviction record (none/violation/misdemeanor/felony), attendance at CEP (if in the experimental group), and educational level attained within twelve months after intake into the research. These five variables produced a multiple correlation of R=.40 ($F(5,394)$=14.96; $p<.01$). A summary of the results of the regression is presented in Table E-1. The results imply that defendants most likely to be arrested subsequent to intake into the research were males whose families spent some time on welfare during the defendants' adolescence. The defendant was likely

Table E-1

REGRESSION RESULTS FOR DICHOTOMOUS REARREST

Variable	R	R^2	Partial r[a]	Beta	F[b]
Sex	.14	.02	.14	.10	8.396[c]
Welfare	.23	.05	-.18	-.14	13.423[c]
Convict. Rec.	.27	.07	-.14	-.11	8.259[c]
CEP Attendance	.38	.14	.29	.25	33.308[c]
Educ. Level	.40	.16	.13	.13	7.193[c]

[a] The partial correlation with the criterion variable on the step before entering the equation; for the first variable to enter the equation, the zero-order r is presented.

[b] The F value of the significance test for Beta in the step in which the predictor was entered.

[c] $p<.01$

to have a prior conviction record, to attend CEP infrequently (if at all), and to have a low level of education relative to the rest of the research population. While the relationship of each of these variables with the rearrest variable was small (as indicated by the partial correlations in Table E-1), they were the best predictors among those tested.

The second recidivism variable was number of rearrests within the research period (ranging from zero to eight). For this variable there were six predictors: age, whether the defendant was born in New York City (yes/no), family welfare status during defendant's

adolescence, number of prior arrests, attendance at CEP, and educational level twelve months after intake. These six variables produced a multiple correlation of $R=.42$ ($F(6,393)=13.651$; $p < .01$); a summary of the regression results is presented in Table E-2. The results imply that the older defendants, born outside of New York, whose families spent some time on welfare were likely to be arrested most often. In addition, those defendants with prior arrest records who attended CEP infrequently or not at all, and had a low level of education relative to the other members of the research population were likely to be rearrested more often than those without prior arrest records or those who attended CEP often.

Table E-2

REGRESSION RESULTS FOR NUMBER OF REARRESTS

Variable	R	R^2	Partial r[a]	Beta	F[b]
Age	.19	.04	.19	-.14	14.000[c]
NY Born	.22	.05	-.11	-.11	4.525[d]
Welfare	.26	.06	.14	.11	8.018[c]
Priors	.30	.09	.16	.10	10.121[c]
CEP Attend.	.40	.16	-.28	-.25	33.748[c]
Educ. Level	.42	.17	-.12	-.12	5.610[d]

[a]The partial correlation with the criterion variable on the step before entering the quation; for the first variable to enter the equation, the zero-order r is presented.

[b]The F value of the significance test for Beta in the step in which the variable was entered.

[c]$p < .01$

[d]$p < .05$

The third measure of recidivism was severity of rearrest charges, quantified as A Felony, B Felony, etc., down to Violation. If the defendant had more than one arrest subsequent to intake into the research, the most serious charge was used. There were ten predictor variables used in this analysis; although each of the included variables had Beta weights that were significantly different from zero, the directions of some of the relationships are counterintuitive, as will be discussed below. The variables, in the order in which they entered the quation, were sex of defendant, family life during adolescence (i.e., intact family or broken home), age at first adult arrest, enrolled in school six months after intake into the research (no or yes), average monthly salary during the six months following intake, number of jobs held during the six months following intake, number of months enrolled in school during the six months after intake, marital status twelve months after intake (married or single), enrolled in school twelve months after intake (no or yes), and educational level twelve months after intake into the research. The multiple correlation obtained using these ten predictors was $R=.58$ ($F(10,349)=17.548$; $p<.01$); the results are summarized in Table E-3. Males were likely to be arrested on more serious charges than were females. Defendants from *intact* families were likely to be arrested on more serious charges than those from broken homes (the reason for this is unclear). Those arrested on the most serious charges were also likely to have been arrested for the first time when they were fairly young, to be enrolled in school

Table E-3

REGRESSION RESULTS FOR SEVERITY OF REARREST

Variable	R	R^2	Partial r[a]	Beta[c]	F[b]
Sex	.14	.02	.14	.17	7.422[d]
Family Life	.24	.06	.20	.17	15.765[d]
Age 1st Arr.	.28	.08	.16	.12	10.717[d]
Enroll 6 Mo.	.32	.10	-.15	-.25	8.509[d]
Salary 6 Mo.	.36	.13	-.17	-.60	11.819[d]
# Jobs 6 Mo.	.40	.16	.20	.39	15.774[d]
Mos. School	.42	.18	.14	.23	8.118[d]
Marital Stat.	.51	.26	-.31	-.30	40.796[d]
Enroll 12 Mo.	.55	.31	-.25	-.25	26.699[d]
Ed. Level	.58	.33	.20	.18	16.943[d]

[a]The partial correlation with the criterion variable on the step before entering the equation; for the first variable to enter the equation, the zero-order r is presented.

[b]The F value of the significance test for Beta in the step in which the variable was entered.

[c]Beta at the last step.

[d]P<.01

six months after intake, to be earning a high salary (relative to the rest of the population), and (with the previous variables held constant) to have held few jobs during the six months after intake into the research. (It is not immediately obvious why those with

the most serious rearrest charges would be enrolled in school, earning relatively high salaries, and hold few jobs during the six months following intake into the research. Before spending a great deal of time attempting to interpret these data, however, it would be wise to replicate these findings. It is likely that other samples drawn from the same population would yield different results. Additionally, those arrested on the most serious charges were likely to have spent *few* months in school during the six months after intake, to be unmarried, to be enrolled in school twelve months after intake, and to have achieved a low level of education relative to the rest of the research population. Clearly further research is needed to identify the factors that determine severity of rearrest charges.

The final recidivism variable was number of convictions on charges received subsequent to intake into the research. The six predictors used in this regression were sex, family welfare status during the defendant's adolescence, prior conviction record, attendance at CEP, enrollment in school twelve months after intake into the research, and educational level attained. Using these predictors a multiple correlation of R=.40 was obtained. A summary of the results is presented in Table E-4. They imply that defendants most likely to receive subsequent convictions (during the period of the research) were male, whose families spent some time on welfare during the defendants' adolescence. These defendants were likely to have prior conviction records, to have attended CEP infrequently, not to be enrolled in school 12 months after intake, and to have attained a low level of education relative to the rest of the population. A discussion of the implications of these results is contained in Chapter VI.

Table E-4

REGRESSION RESULTS FOR NUMBER OF CONVICTIONS

Variable	R	R^2	Partial r[a]	Beta[b]	F[c]
Sex	.13	.02	.13	-.08	6.369[d]
Welfare	.21	.05	.17	.13	12.203[e]
Prior Con.	.25	.06	.14	.10	7.932[e]
Attend CEP	.37	.14	-.27	-.25	34.274[e]
Enroll 12 Mo.	.39	.15	-.12	-.11	5.706[e]
Educ. Level	.40	.16	-.11	-.11	5.309[e]

[a] The partial correlation with the criterion variable on the step before entering the equation; for the first variable to enter the equation, the zero-order r is presented.

[b] Beta at the last step.

[c] The F value of the significance test for Beta in the step in which the variable was entered.

[d] $p<.05$

[e] $p<.01$

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