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RESEARCH DESIGN AND IMPLEMENTATION:  
A PRELIMINARY REPORT

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## I. INTRODUCTION

The Vera Institute of Justice is currently conducting an evaluation of the Court Employment Project (CEP) in New York City. Established in 1968, CEP is one of the earliest pretrial intervention (diversion) programs in the United States. The purpose of the evaluation is to examine in detail the impact of the diversion process and to link this with a description of the CEP program, its intake and service activities and the context within which it operates.

The basic research design is an experimental (pre-post test, control group) evaluation of the consequences of diversion for a selected group of defendants who became CEP participants in contrast to the consequences of normal court processing for a concurrently selected control group. The design and implementation of the controlled research are described in this report with particular emphasis on how the experimental and control groups were identified and selected and the problems engendered by the assignment of defendants with outstanding court cases to a control group. The report includes a preliminary evaluation of the assignment method designed to select defendants for the study groups. It suggests that the unique, quasi-random procedure required by the policial and legal constraints of the research setting appears to have successfully approximated an equal probability (random) method. The report also describes the design of the research data base, the types of data being collected and the methods of collection.

In addition to the basic controlled design, the overall research includes an examination of three collateral aspects of the CEP diversion process that are important to a comprehensive evaluation. The designs of these studies are discussed in this report. First, in order to understand how defendants (including those in the controlled study) are selected for diversion and to obtain a sense for the systemic impact of CEP, the process by which defendants are identified as eligible, screened and rejected or accepted for diversion to CEP is being studied. Second, as a framework for interpreting the program's impact as measured by the controlled research, CEP's process of servicing defendants is described qualitatively in terms of the types of services offered by the program, how they are delivered, and the philosophy behind the program's service orientation. To provide an ever broader context within which to examine this CEP evaluation, the overall research design includes an examination of the diversion process in jurisdictions outside New York City. Finally, as one way of estimating the economic impact of the CEP program, the cost of diversion to CEP relative to normal court processing will be analyzed at the conclusion of the research.

The report is organized into three major sections: (1) the design of the controlled study (including discussion of the assignment procedure, a preliminary evaluation of its success, and a description of the major research data base); (2) the design of the collateral studies (selection, services, cost-benefit assessment); and (3) the plan of analysis for the basic evaluation

data (including discussions of methodological issues, measures of program outcome or "success," and basic research hypotheses).

## II. DESIGN OF THE CONTROLLED STUDY

The experimental study of defendants eligible and approved for diversion to CEP is the core of the evaluation design. A pretest, post-test control group design was chosen in order to have research groups of sufficient similarity to permit valid and reasonably precise measures of the impact of diversion. The consequences of diversion to CEP are compared with those of normal court processing by examining the behavior of simultaneously selected groups of eligible defendants for up to one year after their arrest and intake into the research. While the procedure used to assign eligible defendants to the two research groups is quasi-random, it was designed to approximate an equal probability method.

Data collection procedures for the controlled study provide multiple measures of the program's effects. The outcome of diversion and those of normal court processing are examined for all members of the research population including those in the experimental group unsuccessfully terminated from CEP. Pre- and post-intake behavioral measures include the disposition of their court cases, their recidivism during the one year follow-up period, and the stability of their life situations during that period, including employment, education and economic dependency. The data base also includes program variables (such as the intensity of experimental clients' contact with CEP, the specific types of services they receive, and whether their participation was successful or

unsuccessful). Cost benefit variables (such as detention, or probation time, number of rearrests and court appearances) are also collected.

In section A which follows, the procedures designed and implemented by the research to select and assign the 666 members of the research population are detailed. Subsequent sections describe the design of the data base being compiled, sources of data, and the procedures set up to collect them (Section B), and present preliminary data on the success of the assignment procedures in duplicating an equal probability method (Section C).

#### A. Selection of the Research Population Intake and Assignment Design

1. Summary. The research population is composed of 666 defendants selected primarily from the criminal courts of Manhattan and Brooklyn during the period January 19, 1977 through October 31, 1977. Research subjects were selected from among those defendants eligible and approved by the prosecutor for diversion to CEP. Of the total 666 defendants in the research population, 410 (61%) were assigned to be diverted to CEP for services and constitute the experimental group; 256 (38%) were assigned to a control group, not to be diverted but subject to normal court processing. Assignment of eligible defendants to either the experimental or control group was carried out in conjunction with the normal court screening and intake procedures of CEP, but it was controlled by the research. The assignment method, while not strictly an equal probability (random) selection procedure, was designed to approximate such a design while at the same time assuring defendants were not denied services solely because of the research. To accomplish this, CEP identified more eligible cases than it could service thus producing an "overflow" of eligible defendants; the research designed a quota system by which CEP diverted the first cases approved for diversion during any given time period (experimentals); remaining ("overflow") defendants approved during any time period were assigned to the control group. Use of variable length time periods assured that CEP screeners and research personnel could not influence assignment decisions. While exceptions to the assignment procedures occurred, they were kept to a minimum (12% overall, 15% of those assigned as exper-



imentals and 6% of those assigned as controls). Similar data collection and follow-up procedures (discussed in Section B) are being carried out for all 666 defendants assigned to the research population.

2. Selection procedures. The design of the procedures for selecting defendants into the research population relied heavily upon close cooperation between Vera research and CEP screening staff. To establish adequate experimental and control groups, the assignment of defendants to the research population had to take place as late as possible in the CEP eligibility screening process and after the prosecutor (a key decision-maker in the diversion process) had agreed to diversion. CEP obtained clients by both actively soliciting cases before arraignment and by evaluating the eligibility of cases referred to it after arraignment by judges or defense counsel. The research wanted to consider assigning to the research group only defendants who had passed through the full CEP screening process successfully and who had been approved for diversion by the prosecutor (an Assistant District Attorney designated as the CEP-liaison). Judicial review of the diversion decision was the only step in the diversion process that could not be taken before the case was assigned by the research.\*

To implement this research intake procedure, it was necessary to obtain the cooperation of the Legal Aid Society, whose attorneys represented many of the eligible defendants, and that of the District Attorneys of three separate New York City counties.\*\*

\*The court's consent to a four-month adjournment was required for formal diversion to CEP to take place.

\*\* CEP diverted cases in four jurisdictions within New York City, the criminal courts of New York County (Manhattan), Kings County (Brooklyn), the Bronx and Queens. All but Queens were originally included in the research design.

The District Attorneys readily agreed to the research intake procedures sketched above and to the research's use of an equal probability assignment method\* for determining which defendants would be assigned by the research to the experimental (diverted) and control (non-diverted) groups. The Legal Aid Society, however, was concerned that the research intake procedures would deny some defendants diversion services in order to create a control group. They were also afraid that an equal probability assignment procedure would be "arbitrary" and thus deny defendants equal protection and due process.\*\*

As a consequence, the final procedures implemented by the research were designed so that no defendant was denied diversion services solely because of the need for a control group. In addition, the specific assignment device developed, while approximating an equal probability method, did not subject individual defendants directly to a randomizing procedure.

3. "Overflow." The goal of not denying defendants diversion because of the need for a simultaneously selected control group was achieved by securing CEP's agreement to generate an excess of defendants eligible and approved for diversion. Since CEP could provide services only to a limited number of defendants (given its level of funding), a pool of defendants existed in the Criminal Courts who were eligible for diversion but who were not being screened by the program. Therefore, during the ten months needed

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\*That is, assignment via the toss of a coin or its equivalent which assures every subject (defendant) has the same probability as every other subject of being placed in the experimental (diverted) or control (normal court processing) group.

\*\*LEAA's General Counsel expressed similar concerns with the original assignment design.

to complete research intake, CEP screeners worked to identify more eligible defendants than the program could realistically divert and to secure the prosecutor's approval for their diversion. CEP diverted only some of these approved defendants; these are in the research experimental group. The remainder, not diverted, constitute an "overflow" and are the research control group.

4. Assignment method. The mechanism for determining which of these eligible defendants were diverted (experimentals) and which were "overflow" (controls) was developed and administered entirely by the research. It was designed to approximate an equal probability assignment by assuring concurrent intake into the two groups and by preventing either CEP or research staff from influencing individual decisions. A description of this procedure, acceptable to both the Legal Aid Society and LEAA's General Counsel, follows. Its major characteristics are the construction of a CEP quota and the use of variable length time periods.

Because CEP secured approval to divert more defendants than it could reasonably give services, it was appropriate to develop a quota system in order to select those cases CEP would divert during a given time period (e.g., a month, week, or day). When added together over the long run, these quotas should equal the total number of cases CEP had funds to divert. Once a quota was filled, the remaining cases screened and approved during any time period would constitute an overflow of eligible defendants to be processed normally by the court. These cases could then be assigned to a control group for purposes of research.

The Vera research controlled the mechanism for establishing the CEP quota and thus determined the assignment decisions. That is, Vera researchers informed CEP screening staff whether a particular defendant was part of CEP's quota or the overflow. The assignment procedures had the following three important characteristics: first, assignment to the two research groups was concurrent; second, no assignment decision could be influenced by CEP or research staff; and third, the CEP quota was filled on an approximately "first come, first served" basis (to overcome objections concerning arbitrariness).

The central aspect of the assignment procedure was the division of the total research intake period into multiple assignment periods of varying length. During each period, CEP received a quota of cases approved for diversion; the remainder were "overflow" cases and were processed normally by the court. To meet the requirement that CEP's quota not be filled arbitrarily, the research always designated the first cases approved by the ADA during any time period as part of CEP's quota. The use of variable length time periods meant that the shift from cases assigned to the CEP quota to those assigned to the overflow occurred at different times during the week and at various times of the day. Consequently the screening staff could not predict what would happen to any particular case. To accomplish this, hours (not days) had to be used as the unit of time in constructing the length of the assignment periods. If days had been used, CEP screening staff would have known immediately after the assignment

of the day's first approved case, that all remaining defendants screened that day would be either part of CEP's quota or the overflow. This knowledge undoubtedly would have altered their behavior in selecting cases. Variable length periods also meant the size of the CEP quota was never the same (i.e., a long period would have a larger quota than a short period). Since no one but the Vera researcher monitoring the assignments knew the length of the time period or the size of the quota, CEP screening staff could not predict when a case was assigned to their quota (or the overflow) whether it would be the only one that day or whether several would be assigned before their quota was filled (or a new quota begun).

The periods used in the research assignment were determined as follows. First, the total number of work days in six months of research intake\* was multiplied by eight, the number of hours worked per day by CEP screeners. The figure that resulted represented the total number of screening hours the research had to divide into variable length assignment periods. The length of each time period was selected on logical grounds. If the periods were too short, the assignment procedure would soon approximate a toss of a coin model and could be challenged as "arbitrary"; if they were too long, the selection of experimental and controls would not be concurrent. Consequently, time periods varying in length from 11 to 21 hours were chosen. Under this system, new assignment periods would begin approximately every one to three days and

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\* To secure a sufficient number of cases, research intake was continued for an additional four months. At the end of the first six months, the research continued to generate time periods using the method described above.

the shift would occur at different times during the 9 A.M. to 5 P.M. screening day. The total number of screening hours during the first six months of intake was divided, therefore, into an equal number of 11, 13, 15, 17, 19 and 21 hour periods. The order in which these periods were used was randomly determined before the start of the research.

To establish the size of CEP's quota for each time period, the research estimated the number of cases for which CEP was likely to secure approval during the next time period (e.g., the next 13 hours). This estimate was based on the average number of cases approved during all preceding time periods (calculated as the mean number per hour). If this figure was, for example, 0.3 cases per hour, the expected number of cases during the next 13 hour assignment period would be 4; that is,  $0.3 \times 13 = 3.9$ . It was originally assumed that CEP would be able to generate approximately twice the cases it could divert. The CEP quota, therefore, was set at 50 percent of the expected number of cases approved during any time period (or 2 in the example begun above).

Because CEP's quota was always filled first and because its screening performance was highly irregular\*, it was necessary to build an adjustment factor into the CEP quota to assure CEP diverted approximately 50 percent of the cases over the long run. In each time period this factor was based upon 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,

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\*Given the erratic flow of cases through the criminal courts, CEP might get many cases approved one day and none the next.

then half the expected number of cases in the next assignment period was assigned to CEP's quota. If the figure was less than 50 percent, the proportion in the experimental group was lagging and CEP's quota was increased in the next period to bring the proportions closer to 50-50. If the figure was above 50 percent, the CEP quota was reduced.\*

These procedures for identifying and assigning defendants to the research population were carried out without major alteration. During the ten-month intake period, 666 defendants were assigned by the research (see Table 1, Appendix). However, two adjustments in the original design were made in response to CEP's organizational needs. The first involved the percent of cases in CEP's quota and the second the assignment of co-defendants.

It must be recalled that the entire research intake design rested upon CEP screeners' ability to identify and secure approval for more eligible cases than CEP had resources to serve. There had to be in fact an "overflow." Only then, given the constraints under which the research operated, was a CEP quota appropriate. 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 to generate an overflow of equal size. As a result, the research agreed to increase CEP's quota to 65 percent (rather than 50%) of all expected cases; the overflow, therefore, would be 35 percent. These new assignment percentages remained during the second three months of the research intake period, after which time CEP agreed to return to 50 percent.

\* In practice, therefore, CEP's quota equalled the percent of cases previously assigned to the overflow (e.g., if the experimental group had 45 percent of the cases, the overflow had 55 percent, CEP's next quota would be 55 percent of the expected number of cases).

The major consequence of this deviation from the original design is that the size of the experimental and control groups are not equal; 410 defendants (61% of the total assigned) are in the experimental group and 256 (38%) in the control group.\* (See Table 1, Appendix.)

The second adjustment involved the assignment of co-defendants. Eligible defendants who were also co-defendants were always given the same research assignment: both were either part of CEP's quota or the overflow. The reason for this was practical: CEP could not get the prosecutor's approval to divert a defendant if his co-defendant was not also diverted. The prosecution was concerned 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.

5. Process of screening and assignment. Figure 1 on the following page contains a flow chart of the daily CEP screening and Vera research assignment processes as they were coordinated in the criminal courts. They are described step-by-step below not only to clarify the details of the process, but also to indicate how exceptions to the research assignment of defendants occurred.\*\* In all research conducted outside the laboratory, exceptions are inevitable because the research cannot fully control the actors in the system. Research control is particularly difficult when the system is as complex as are the courts and when

\*Another factor affecting the unequal size of the two groups was the necessity to always assign cases to the experimental group first. When intake was slow for a long duration (several assignment periods), the number assigned to the experimental group increased faster than those assigned to the control group. Because the number of cases involved was so small (2-4 each time period), the adjustment factor used could not fully correct the balance.

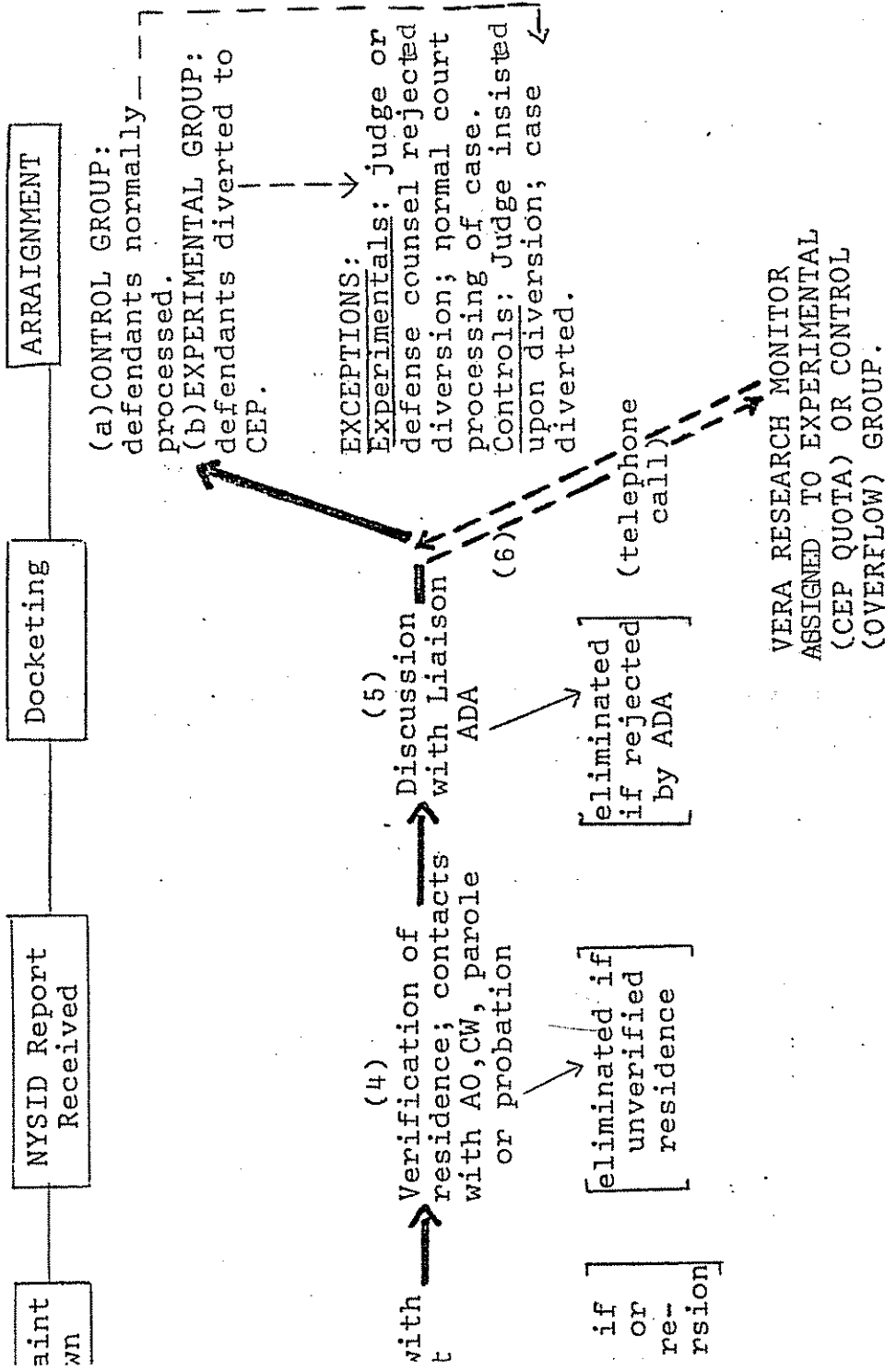
\*\*That is, how some defendants assigned to CEP's quota ended up not being diverted and how the reverse also occurred (controls who were diverted); See Table 1, Appendix.



FIGURE 1

ING AND RESEARCH ASSIGNMENT PROCEDURE\*

(7)



ment from Defense Attorneys or judges were screened as above by  
 After CEP secures ADA approval for diversion, Vera Monitor was  
 er diverted (if assigned to CEP quota) or not (if assigned to

research subjects have criminal charges pending against them. While the exceptions were not as frequent as might be expected given these difficult circumstances, they did occur and must be considered in relation to the design of the research.

During the period of research intake, the first step (1)\* in CEP's screening for diversion was carried out by New York City's pre-trial release agency (CJA Criminal Justice Agency, formerly Pretrial Services Agency). Prior to arraignment, CJA interviewing supervisors identified defendants formally eligible for diversion using CEP's written criteria. They provided CEP court screeners with copies of the ROR (Release on Recognizance) interview for eligible defendants. CEP screeners reviewed the sheets to assure eligibility (2) and then interviewed the defendant in the detention pens (3). They explained the program and asked if the defendant was interested in diversion services. Early in the interview, the CEP screener emphasized that the defendant might not be diverted even if he/she was eligible, wanted services, and the A.D.A. approved the case. They explained that CEP interviewed more defendants than it could take into the program and that it was possible there would be no place for the defendant in the program when his case came to court. Both CEP and the Vera research wanted to assure that from the beginning defendants were fully aware their eligibility and interest did not mean they would be diverted (that is, that there was an "overflow" of eligible defendants).

After examining the eligibility and interest of the defendant, the CEP screener attempted to locate the arresting officer (A0)

\* Numbers in ( ) represent the steps indicated in Figure 1, the Flow Chart of Screening and Research Assignment.

and complaining witness (CW) in the court to inform them that the defendant was being considered for diversion (4). While their consent was not required, program personnel believed they should be informed and any objections noted. The screener then proceeded to the Assistant District Attorney (ADA) liaison with the program (5). The screener discussed the possibility of the defendant being diverted with the ADA, engaged in advocacy on the defendant's behalf if necessary, and sought prosecutorial permission to divert.

All cases approved by the ADA liaison were then telephoned to the Vera Research Monitor by the screener in the court. The Monitor, located in Vera's research offices, recorded the names and identifying numbers of all defendants approved by the ADA. Using the assignment method described above, the Monitor informed the CEP screener whether the defendant was part of CEP's quota (to be diverted) or in the overflow (not to be diverted). The screener then returned the prosecutor's case file to the ADA on duty in the Arraignment Part. If the case had been approved by the liaison ADA and was part of CEP's quota, the screener would ask the defendant's Legal Aid attorney to divert the case (if this had not occurred before the screener went to the prosecutor). The judge would then be asked for a four month continuation for CEP to give service to the defendant. If the case had not been approved by the ADA liaison or was not part of the CEP quota (that is, it was part of the overflow), the CEP screener would simply return the papers to the prosecutor. If asked, the screener would tell the ADA or Legal Aid attorney that the case had been turned down as ineligible,

not approved, or that the agency did not have a place in the program and could not service the defendant.\* No more detailed explanation was given.

In addition to directly screening cases before arraignment, CEP also evaluated cases referred to it after arraignment by defense attorneys, judges, and even ADA. The process of screening and research assignment, however, was much the same as that already described. CEP would review the defendant's case for eligibility, interview the defendant and seek approval from the ADA liaison. If approved, the case was then called to the Vera Monitor for assignment to the CEP quota or the overflow. At the defendant's next court appearance, the case was either diverted or not depending upon the outcome of the CEP screening and Vera research assignment process.

During the ten months of research intake, CEP shifted\*\* from a predominately court screening (solicitation) method of intake to a predominately referral method. This did not affect the process of research assignment; from the standpoint of the research design, however, it provided an unexpected opportunity to examine differences (if any) between the type of defendant diverted under the two types of selection systems (See III Section A below).\*\*\*

6. Exceptions to the research assignment. While CEP and Vera research were quite successful explaining the overflow sit-

\* The research attempted to establish a way not to let the ADA or LA in the Arraignment Part know a control case had been considered and approved for diversion. This was to avoid any possibility of contaminating their further handling of the case. However, this was not practicable. Sometimes they knew it was an overflow (approved for diversion but no room in the program); other times they did not know the case had even been considered for diversion by CEP.

\*\* CEP changed its system in Manhattan after four months of screening and in Brooklyn after six months.

\*\*\* Fifty-six percent (187) of the Manhattan intake cases and 89 percent (264) of the Brooklyn cases were selected before the change to referral.

uation to individual defendants, defense attorneys , and judges,\* exceptions to the research assignment occurred. These fell into three categories. The two most important to the controlled design are defendants assigned as part of CEP's quota who were subsequently not diverted and defendants assigned as overflows (controls) who were diverted. These exceptions represent 12 percent of all the defendants assigned to the research. In addition, 66 defendants from Manhattan and Brooklyn were diverted to CEP but were not called into the Vera Monitor for assignment to the research. The explanation of those exceptions follows.

Whereas the research assignment was made as close to the end of the screening process as possible, there was no way to avoid having judicial review of the diversion decision occur after defendants had been assigned. Even if the prosecutor approved diversion and the defendant was assigned as part of CEP's quota, the defendant still had to appear before a judge (at arraignment or subsequent appearance) where the ADA, defense counsel, and CEP screener would jointly request a four-month adjournment for the defendant to be diverted to CEP. Judges in New York City generally assert the prerogative of judicial review and would occasionally refuse to divert the case. For example, a judge might feel the

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\*Considering that the New York City Criminal Courts arraign over 100,000 felony cases each year, the number of judges, legal aid attorneys and ADAs who came into contact with CEP screeners during the period of research intake was extremely large. Although CEP and Vera discussed the joint intake procedure with senior District Attorneys, judges and Legal Aid Society lawyers before beginning the research, the details did not always filter down to every person ultimately involved. Consequently, on many occasions during the ten months of research intake, CEP and when necessary Vera researchers, had to explain the existence of an overflow group to individual lawyers or judges.

defendant should receive a more lenient or a harsher disposition than diversion if he took a plea or was convicted. In addition, a defense counsel might reject diversion at the last moment if the ADA liaison had attached a condition to that diversion which was unacceptable to the attorney or if he felt he could get a better disposition by negotiating a plea. Sixty-three cases (15%) assigned by the research to the CEP quota were rejected in one of these two ways; these members of the experimental group were not diverted to or serviced by CEP.

Less frequently, a defendant assigned to the control group was diverted to CEP. This occurred, for example, when a judge insisted a particular defendant (already assigned as an overflow case) be diverted to CEP. It is important to note that CEP encouraged judges to support the agency's decision about whom to divert. Screeners and their supervisors resisted attempts to divert cases they had rejected (for whatever reason). However, the agency operates on the basis of informal agreements with the court and maintaining a good relationship with judges is essential to its continued operations.\* Therefore, in some cases when a judge insisted a case be diverted despite its overflow status, CEP would feel obliged to take the case. This happened with 16 overflow cases (6% of those assigned to the control group).

From the standpoint of the research design, once a defendant has been assigned to a research group, the person becomes a permanent part of the research population. They are therefore,

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\* As in many jurisdictions, diversion in New York City is not mandated by a court rule or state law. CEP is an independent organization funded by the city but having no official relationship to criminal justice agencies. As such, it diverts on the basis of prosecutorial discretion, but judges may assert judicial discretion with regard to sentencing and refuse to approve the continuance for diversion. CEP therefore, must have good relations with the court and prosecutors as well as defense counsel.

included in the follow-up and data-collection processes described below. This includes those assigned to the experimental group who are not diverted and those in the control group who are.\*

Finally, some defendants screened and diverted from Manhattan and Brooklyn are also not included in the research design. These cases were excepted from the research assignment for one of two reasons: either a judge diverted the case to CEP without the agency having screened it\*\* or a judge demanded that a defendant be diverted and CEP's director of court operations or the program director believed rejection of the case as an overflow would jeopardize the agency's informal relations with the court. These 66 defendants (one out of ten defendants screened in these two boroughs) are not included in the research.

7. Size of the research population. The original design called for selecting approximately 800 subjects (400 experimentals and 400 controls) within a six-month period. This was not accomplished. It required ten months of intake for the research to assign a total of 666 defendants to the two groups. This long period of research intake was necessary because CEP's intake and screening activities were much slower than they had originally anticipated. As a result of New York City's 1976 fiscal crisis, CEP was forced to close down for seven months; this occurred just before research intake was to have begun. CEP then had its budget considerably reduced and had to reorganize its screening before resuming operations. Therefore, while not an entirely new program within the criminal justice system, it was in some ways a different program

\* See the methodological implications of this, pp. 72 .

\*\* While this was never supposed to happen, on occasion it did.

and had to reestablish its place in the routine processing of cases. Nevertheless, 555 cases (83%) were approved and assigned to the research groups during the first six months of research intake.

There is an additional group of defendants who did not pass through the research assignment. They are not, however, technically exceptions since they were not intended to do so. These defendants are not considered part of the research design. This group of CEP participants includes all defendants screened by CEP in the borough of Queens (133 ) and the majority of those in the Bronx ( 61). Queens was not included in the original research design because traditionally the court had diverted very few cases. Queens defendants were also of a somewhat different socio-economic composition than those diverted in the other criminal courts.\* While the original design called for including the Bronx, the number of cases approved for diversion in the first eight weeks of research intake was very small (5 per week). Designating 35-50 percent of this small number of cases an "overflow" caused problems between CEP and legal aid attorneys who felt it was not worth their time to refer clients if so few were to be diverted.\*\* Research intake, therefore, was stopped in the Bronx. As a consequence, the research design primarily reflects CEP's diversion of defendants from the two largest of four criminal courts within which the program operates (Manhattan and Brooklyn cases represent 66% of CEP's total client population), although it also includes 36 cases from the Bronx.

\*Unexpectedly, the number of defendants diverted to CEP from Queens was substantial during the period of research intake. While in not taking cases from that borough, the research missed an opportunity to study diversion for a more working and middle class population, the steady flow of cases from Queens into CEP helped sustain the existence of an overflow in Manhattan and Brooklyn.

\*\* While the same attitude existed in the other boroughs, the larger number of cases overall mitigated its effects.



At this point, however, additional changes occurred which further affected the flow of cases into CEP and, therefore, into the research. First, the program ceased screening for an entire month; second, it further reorganized its screening procedures by moving to referral rather than relying on active solicitation; third, it began terminating a sizeable number of unsuccessful participants and returning their cases to court; fourth, it began some reorganization of its service unit and thus did not want high case loads during that period. Finally, under these conditions, the existence of an "overflow" of cases that the program would not service may have been a depressant to the number of cases referred by defense attorneys.

Research intake was suspended after ten months even though the goal of 800 cases had not been reached. A sufficient number of cases (666) had been obtained to carry out the required data analysis and intake would have had to continue a minimum of four more months, to secure enough additional cases to improve the statistical power of that analysis. This did not seem warranted.

## B. Design of the Data Base

1. Summary. The design of the controlled study calls for the collection of pre- and post-behavioral measures, both self-reported and officially recorded, beginning with intake into the research and continuing for a period of one year. All members of the research population (including those in the experimental group unsuccessfully terminated from the program) are followed for the full period. The data base is designed to permit examination of a variety of life areas including employment, education, living arrangements, dependency, criminal behavior and social service utilization in order to evaluate the consequences of diversion to CEP.

Three personal interviews (intake, 6 months, 12 months) are conducted to collect self-reported data with respect to education,

training, employment history (up to one year prior to intake); income and public assistance; criminal history and current illegal activities; personal background; current living arrangements, and utilization of social services. Where possible, official verification of self-reported school, employment and welfare status is obtained. For all members of the research population, official New York City record data are also collected on respondent's prior arrest history, current charge, disposition, rearrests; Social Security annual reported income data are also obtained. For diverted members of the experimental group, CEP service records are collected; these include measures of intensity of contact with the agency, types of services rendered and service outcome. For members of the research population who cannot be located or who refuse interviews, partially verified information is available at the time of their intake into the study. These include age, sex, ethnicity, address, living arrangements, and current employment, education, and welfare status. Procedures have been designed to assure the quality of the interview data and the confidentiality of all research data.

2. Interview data and follow-up design. The design of the data collection centers around three personal interviews with each defendant in the study population. This is administered by a Vera Research Interviewer. They take place at intake, six months, and twelve months after intake.\* A fourth interview with a sub-sample of defendants in the experimental group is also called for in the design, as are periodic telephone and mail contacts to keep in touch with all respondents. The timing of these interviews is indicated in Figure 2 on the following page.

Three interviews are sought with all research subjects, including those designated as exceptions to the assignment (e.g., experimentals who are not diverted) and those who are terminated unsuccessfully from CEP. Respondents are paid \$10.00 in cash for each personal interview and \$5.00 for a telephone interview

\*In practice, the timing of the interview may not be immediately after research assignment. Some defendants do not want to be interviewed after having spent one or more days in detention; appointments are made for a later date. In other cases, defendants must be located and/or persuaded to participate in the research. This may take several weeks or even several months. The possibility of such time lags is taken into consideration in the interviews by collecting continuous data on their employment, welfare or educational status month by month. The emphasis on behavioral rather than attitudinal measures also reduces the effect of this interview lag (See page 56 below).

FIGURE 2

TIME LINE FOR RESEARCH FOLLOW-UP

Month Number: 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22  
Date: 1/77 2/77 3/77 4/77 5/77 6/77 7/77 8/77 9/77 10/77 11/77 12/77 1/78 2/78 3/78 4/78 5/78 6/78 7/78 8/78 9/78 10/78

Initial Intake Interview  
C  
E  
(N= 666)

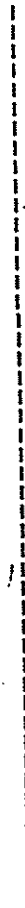
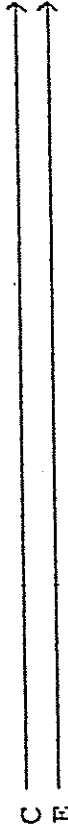
Mail and Phone contacts

First Follow-up Interview (6 months)

Mail and Phone contacts

Second Follow-up Interview (12 months)

Third Follow-up Interview (N=209) (16 months)



conducted by a research interviewer between personal interviews. Telephone interviews are for the purposes of maintaining contact with respondents and for up-dating their mailing address, living address, telephone number, employment, educational, welfare and rearrest status. A fourth interview (either in person or by telephone) will be carried out with those assigned to the experimental group during the first four months of research intake. The purpose of this additional interview is to collect data on diverted defendants for a full 12 months after they have completed the CEP program. Without these data there is a discrepancy in comparing controls and experimentals. During a 12 month follow-up period, members of the control group are "at risk" (that is, face problems of employment, education and training and pressure for criminal activity without special support) for all 12 months after intake; those in the experimental group, on the other hand, are at full risk for only those 8 months after they have completed the 4 month CEP program. In order to compare both groups during an equal period at risk, a small group of the experimental defendants will be interviewed 16 months after intake which is 12 months after they have completed the CEP program.

Interviews are conducted in Spanish or English, either at Vera's central research offices or those in the Criminal Courts. Alternatively, they are held in the respondent's home, neighborhood, or place of work. If members of the sample cannot be located or contact is lost, their names are periodically checked through the computerized records of New York City's pre-trial release agency (CJA). If they have been rearrested, research contact is again attempted.

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\* The research will, of course, make other comparisons between controls and experimentals; for example, differences will be compared during the 4 month period while experimentals are in CEP.

The information elicited in the intake interview is designed both to describe the basic social and economic characteristics of individuals approved for diversion and to provide baseline data for evaluating the relative impact of diversion and normal court processing on their behavior and lifestyle. Standard demographic and personal background questions solicit information about their age, ethnicity, place of birth, living arrangements during teenage years, education and occupation of parents. The educational history of the respondent is explored in some detail, including educational background, current educational status, attendance patterns, attitudes about schooling and future plans. Detailed information is collected concerning the respondent's current, most recent and longest regular and irregular job during the year prior to intake as well as more general information about their longer range employment histories. A comprehensive "activities chart" is filled out jointly by the interviewer and the respondent describing the latter's education, employment/unemployment, military, prison, training, home-making, or volunteer activities month-by-month for one year prior to intake. Further questions on job training experiences and job search behavior elicit data relevant to their job preparation and labor market experience before intake.

The initial questionnaire also obtains information on the respondent's current living arrangements and sources of income: with whom he/she lives, whether other members of the household work, whether the respondent is satisfied with these arrangements, and sources of income besides employment. The respondent is questioned concerning his/her use of medical, legal, child care and

other social services, and whether these were obtained publically or privately. They are also asked about services they felt they needed and why these needs were not met. Self-reported information is also obtained concerning their current drug and alcohol use, previous juvenile offenses (the official record of which is sealed in New York State) and their adult arrest and conviction history. In addition, respondents are asked about the type and frequency of current extra-legal activities and the income they derived from such activities.

Finally, respondents are questioned about the case that was pending when they entered the research population: what the police say the facts of the case were, what the case outcome was at arraignment and, if they were diverted, what they hoped to gain from participating in CEP.

Data collected in the second (six-month) interview parallel the baseline data collected at intake. To analyze changes since entry into the study, the questions in each substantive area solicit information from the respondent about behavior during the entire time period since intake. The respondent is also asked whether he/she received assistance from anyone in solving particular problems (e.g., obtaining new housing , job referral, etc.) For those in the experimental group, "CEP counselor" is one of the alternatives. All respondents are questioned further concerning their participation in social service, treatment or rehabilitation programs other than CEP. This is asked to provide a measure of whether "treatment" other than CEP is received by the experimental group and whether

the control group is receiving similar services to that of the diverted group. Additionally, the experimental group is asked about their experiences with and attitudes toward CEP and the services they received. If respondents did not attend or dropped out before completing the program, they are asked what CEP expected of them and why they did not attend or continue attending. Finally, all respondents are asked their perception of what happened (or what will happen) to the case pending against them at the time of their intake into the research.

The third interview (and fourth where called for) continues the collection of these data, tracking changes in the respondent's behavior and lifestyle over an additional six (to ten) months after the second interview. Data on respondents' participation in social service, treatment, or rehabilitation programs other than CEP and additional services from CEP are also included.

3. Verification of interview data. To improve the reliability of key variables, the research design calls for the verification (where possible) of employment, educational and welfare status as reported by respondents. Official verification of the criminal history they report (pre- and post-intake) is also being obtained; this will be discussed below in conjunction with the collection of other official records.

Respondents are asked to sign waivers permitting the research to verify their employment, educational and welfare status. Regular full- and part-time employers and school attendance officers are telephoned and/or written to secure verification of the in-

formation provided by the respondent. CEP also verifies such information for their clients who are members of the experimental group. In addition, the research verifies respondents' welfare status through the New York City Department of Social Services.

4. Official record data. The research design calls for the collection of various types of record data from official sources. These include criminal history data (from the New York City Police Department and the New York City Criminal Justice Agency); annual income data reported by the Social Security Administration; and, for experimentals, data on performance in the diversion program (from CEP).

The data collected on respondents' criminal histories includes prior adult arrests in New York State (charges and, in most instances, dispositions) subsequent arrests during the follow-up period (charges and dispositions) and information on the case pending when the defendant was selected for the research population (type of offense, days in detention, bail, number of court appearances, and disposition). Data on the pending case will be used to compare the legal consequences of diversion for the experimental group with the outcome of normal court processing for those in the control group.

Social Security annual reported income data are collected for each interviewed member of the research population. Respondents are asked to sign two Social Security Requests for Statement of Earnings. One is forwarded to SSA by Vera and returned to the



research after the intake interview and the other is sent out toward the end of the follow-up period. SSA earnings data include total reported earnings 1937 through 1950 and 1951 through 1972 or 1973; annual reported earnings for 1973 and/or 1974, 1975 and 1976, and earnings during the first quarter of 1977. These data provide verification of interview data on the respondent's income and an estimation of reported versus unreported income.

The research design calls for a description of the diversion services received by defendants in the experimental group from CEP. CEP record data include the following measures: length of time as program participant; the intensity of contact with the agency (i.e., number of visits and counseling sessions); the types of services given in-house and through outside referrals; special characteristics of the individual noted by counselors; and reasons given for their successful or unsuccessful termination.

In addition to these sources of official data, supplemental data are available from CJA for those members of the research population not interviewed in person. CJA interviews all defendants in the criminal court prior to arraignment and they attempt to verify the information they collect. The verified and unverified information relevant to the research includes: age, sex, ethnicity, living arrangements, employment, educational and welfare status at the time of research intake.

##### 5. Procedures for assuring quality of interview data.

Interviewers hired for this research were not professionals although some had had previous interviewing experience. All come from similar

backgrounds (social and cultural) to those of the research subjects. Before the field work began, the interviewers were trained for a two-week period. Besides being acquainted with the purposes of the study and the questionnaire, they were given extensive instruction about the need to maintain the confidentiality of the respondents' identities and answers.

Interviewers were given written and oral materials on the basic rules of interviewing, how to cue respondents for the desired information and how to ask questions without overt or implied bias. They interviewed each other and other members of the staff extensively. In these sessions, problems in questionnaire flow and meaning of difficult questions were discussed in detail. Finally, some pretests were made in the courts before intake began.

As questionnaires come into the research office, they are reviewed by the field work supervisor for missing information and incorrect responses. Interviewers periodically go over questionnaires with their supervisor. Throughout the field work period, interviewers are gathered periodically for refresher training and for review of field work procedures.

6. Confidentiality of research data. The design of the research (especially the collection of sensitive interview data on unreported criminal behavior, welfare status, aliases, etc.) is predicated upon the ability of the research to maintain the complete confidentiality of the data. The privilege conferred on LEAA funded research by 42 U.S.C. §3771 protects these data from legal process and requires research staff to protect their confidentiality.

Consequently, the research has designed procedures to protect the confidentiality of the data:

1. All interviewers and other research staff sign pledges of confidentiality in which they agree that maintenance of confidentiality is a condition of employment. They are informed through these pledges of their obligation under federal law not to divulge confidential information obtained from research subjects except as authorized for research purposes. As indicated above, on-going training includes detailed discussions of interviewers' responsibilities in this regard and the procedures for assuring protection of the data.

2. As part of the procedures to obtain subjects' informed consent to participate in this research, respondents are informed of the voluntary nature of their participation and are advised that their answers to research questions will be kept in strict confidence, not to be revealed except for research purposes.

3. All identifying data (for example, names and addresses) are removed from the body of the questionnaire and kept in separate locked file cabinets.

4. All copies of interviews and other data in non-computerized form are kept in locked file cabinets. Access is limited to a small number of Vera personnel who require their use for research purposes.

5. Research data are rapidly converted to machine readable form and entered into data files containing no personal identifiers.

6. Access to the computer data files is limited to a few authorized personnel who need access to accomplish research objectives.

7. Tapes containing complete research data files on members of the sample will not contain identifiable information when they are turned over to NILECJ at the conclusion of the research.

C. The Experimental and Control Groups: A Preliminary Evaluation of the Assignment Procedure

1. Summary. This section compares the experimental and control groups on characteristics of the court case for which they were approved for diversion, on their criminal background characteristics, and on their demographic characteristics such as age and ethnicity. No statistically significant differences are found between the two groups, thus supporting the conclusion that the assignment procedure designed and implemented in this research produced results similar to equal probability (random) selection.

The experimental and control groups are also compared in terms of pre-intake employment and educational experiences and their use and need for social services. The two research groups are similar on these important dimensions. This not only further supports the adequacy of the assignment procedure, but also assures that the measurement of program effects begins with experimental and control groups having the same level of employment, education, and social service utilization.

This section describes the various social characteristics of the research population. This preliminary evaluation of the validity of the research assignment procedure includes all interviewed defendants assigned to the research population during the first six months of intake, 432 (65%) of the 666 defendants in the study. To assess the adequacy of the assignment design,

variables have been selected to compare the similarity of the experimental and control groups with respect to characteristics that are potentially relevant to measuring the impact of the program. Since research assignment did not rely upon a conventional equal probability procedure, comparison of the two groups is of particular concern.

In the tables discussed below (See Appendix for the tables), the experimental and control groups are compared on selected variables: characteristics of the arrest case, defendants' prior criminal background, demographic characteristics, employment and educational experience, and uses and needs for social services. All these variables are relevant to the selection of defendants for diversion and to the hypothesized outcomes of the diversion process. That is, the program and others involved in the diversion decision take many of these characteristics into account when considering whether or not to divert a defendant and they determine the kinds of services the defendant will receive while in the program. Our concern then, is to show whether those who entered the program (experimentals) are different from or similar to those who did not (controls). The preliminary data are encouraging. They indicate that the experimental and control groups are essentially similar in significant demographic characteristics, criminal, employment and educational background.

A note on presentation of the data. In order to assess the extent of bias in the assignment procedures, members of the research population must be compared according to whether the

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\*In Section III-A, the assignment procedure is described in detail.

research assigned them to the experimental or the control group rather than according to whether they were actually diverted or not at arraignment.\* Defendants diverted to the program without being subject to the research assignment are of course excluded from this analysis.

The majority of the tables refer to the first 432 cases assigned and interviewed by the research. In a few tables, however, data are based upon all 666 cases assigned during the full ten month intake period. These data are not from interviews but from the computerized criminal history records of CJA. When this data base is used, it is indicated on the tables by the label "Total Research Population."

2. Characterisitics of the Current Court Case. There is no difference in the types of cases for which controls and experimentals were arrested (See Table 2 part A).\*\* The majority of respondents were charged with one or more counts of property offenses (larceny, possession of stolen property), or combinations of property and other non-violent offenses (e.g., loitering). These two categories account for 76 percent of all arrests. Robbery and assault without robbery figure less prominently among arrest charges (8% and 10%, respectively). Other charges such as morals, drugs, obstructing justice occur infrequently and in combination with property or violence offenses. This overall pattern reflects the selection criteria of the diversion program.

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\* See pages 17 to 20 for a discussion of the "exceptions" that occurred in the research assignment.

\*\* Unless stated otherwise, all differences between the experimental and control groups in the tables are not statistically significant between the .01 and .001 levels.

For the first time this year, Court Employment restricted diversion to defendants charged with C,D, and E felonies.\* There is little difference between experimentals and controls on the severity of their charges either at their arrest or at arraignment (Table 2; parts B and C). Approximately one-half the defendants were charged by the arresting officer with D felonies and one-third with E felonies; only 14 percent were charged with the most serious felonies accepted by CEP (C felonies). While the pattern of charges changes somewhat when the prosecutor writes up the official complaint for the arraignment (Table 2C), there continue to be no differences between experimental and control group cases.

Experimentals and controls are also similar with regard to the type of victims involved in the alleged offense (Table 2D). Half of their arrests involve strangers, while another third involve corporations or agencies (e.g., department stores or schools). The rest are crimes against relatives or acquaintances of the respondents.

Over half (56%) of all respondents told research interviewers they were arrested with other persons, 55 percent of the experimentals and 58 percent of the controls. According to CEP records, however, 31 percent of experimentals and 29 percent of the controls had co-defendants. The difference between the percentages of co-arrestees and co-defendants probably reflects factors in the selection process, especially CEP's selection criteria and eligible defendants' own willingness to enter the program. The experimental and control research groups, however, are similar on both dimensions.

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\* The letters represent classifications of charge severity, A being the most serious felonies and E the least. An occasional misdemeanor case may be diverted to CEP, but these are exceptions.

3. Criminal Background. Similarities between controls and experimentals continue when official records of their prior experience with police and courts are compared and when their self-reported criminal activities are analyzed. According to CJA computerized summaries of defendants' official New York State records, 24 percent of the research population had previously been convicted of an offense (25% of the experimentals and 21% of the controls). In our interviews, over one-third of the respondents (37%) claimed to have been arrested before as an adult.\* Four-fifths said, however, that they had no juvenile record (See Table 3, A-C). Again, there are no significant differences between experimentals and controls.

Table 3D indicates the types of offenses reported by those in the research population who admitted a prior arrest. Three out of ten (29%) said they had prior arrests only for property crimes (theft). Another one out of ten reported committing a combination of property and other non-violent offenses. One-third reported arrests for crimes against persons (19 percent for robbery and 14 percent for assault without robbery). The final 15 percent reported prior arrests only for conduct offenses (e.g., loitering).\*\* Differences between controls and experimentals appear in these data. Controls report more arrests for loitering and conduct offenses and fewer for property and violent offenses than

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\* The official CJA records do not show arrests that did not lead to conviction, hence the difference between the 24 percent with prior convictions and 37 percent reporting prior arrests.

\*\*Those who reported prior arrests were asked for information only on the last three arrests. Since very few respondents reported more than two arrests, the categories of offenses on Table 3D should be very close to respondents' actual offense records.



experimentals.\* While this relationship is statistically significant, it does not seem analytically significant in the light of similarities between the two groups in other criminal background characteristics. As indicated before, similar percentages in both groups have no prior arrests. Table 3E also shows no relationship between assignment status and the severity of self-reported prior offenses.

In the research interview, respondents were given a list of illegal activities\*\* and asked about the frequency with which they had engaged in each activity during the past 12 months. Table 3F-G summarizes responses to those questions. Half the respondents reported they had not engaged in any type of illegal activity, one-fifth reported engaging in one type, while the remainder indicated engaging in two or more activities. There are no differences between experimentals and controls in this self-reported measure of involvement with crime.

4. Personal Background Characteristics. Table 4 shows the proportion of controls and experimentals with various personal characteristics. These characteristics are important because the impact of diversion may vary with them (e.g., age) and therefore they will be used in subsequent analyses to specify the effects indicated by the research.

Table 4A shows the research population to be young. Two-thirds (69%) are under 21. This population is only slightly older,

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\*Parenthetically, this supports the reliability of the self-reported arrest questions. We would expect those going into diversion to report fewer arrests and less serious offenses because of wanting to look good, but the tables show the opposite.

\*\* Seven types of violent and non-violent property offenses, in addition to selling of drugs and marijuana, gambling and pimping or prostitution, were included in the list. In subsequent analyses, we will cross-check responses to this question with background and other characteristics to determine the reliability of this question. In addition, the responses will be analyzed to determine what kinds of patterns (types and frequency) of illegal activities exists among respondents.

for example, than CEP's intake, in FY 1973-1974 when three-fourths were under 21 (data from CEP's Annual Report for that year).

When the age differences between controls and experimentals are examined, there are slightly more young people under age 17 and slightly fewer between 18 and 20 in the control group than in the experimental. Both groups, however, have similar percentages over 20. The slightly higher percentage in the under 17 category among controls is not significant.\*

In the data on other background characteristics presented in Table 4B-D, there are few differences between the two assignment groups. The majority of the respondents are male (9 out of 10). One out of ten respondents is married, while 8 out of 10 are single. Over half the respondents (56%) are black, ten percent white, and one-third are hispanic. No significant differences between the experimental and control groups are found on any of these dimensions.

5. Employment. In the remainder of this section, differences in the employment, education and social service use of experimentals and controls are examined. The concern with differences between the two groups on these variables not only stems from the necessity to evaluate the assignment procedures, but also to establish a baseline for the evaluation of CEP's impact. That is, are controls and experimentals similar at intake with respect to variables that the program itself considers important outcomes of its service

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\*The age data reported here reflect the entire research population whereas data on other personal characteristics in Table 4 are based on the interviewed group only. The age distribution of interviewed respondents indicated a significant difference between controls and experimentals in the proportion under 17. Therefore, the entire population was examined to see if that difference held up overall. It did not. The assignment procedure, therefore, selected equivalent defendants for the control and experimental groups with respect to their ages.

activities: employment, education, housing, etc.\*

Since one of the main purposes of the Court Employment Project is to provide job counseling and referral, it is not surprising that only 12 percent of both controls and experimentals were employed at a regular job\*\* at the time of intake (Table 5A). It is interesting however that one-half of the respondents (52%) had held a regular job sometime during the twelve months prior to intake (Table 5B). While a larger proportion of experimentals, relative to controls, had been employed during the last twelve months the difference is not significant. However, when differences in employment between the two groups are compared controlling for age (data not shown in the table), a larger proportion of experimentals in every age group were employed. Since these data do not yet reflect the entire research population, it is not clear whether these differences will constitute an analytic problem.

The research population worked an average of five months out of the year, and earned an average of \$105 a week before taxes. There are no significant differences between the two groups on these variables. As this salary level indicates, their jobs are mainly low-skilled and manual. As shown in Table 5D, 25 percent of the respondents had a white collar job during the year as compared with 41 percent in craft, operative or laboring employment; the remaining 31 percent had service occupations. This table shows few differences between controls and experimentals; moreover,

\*This also raises the issue of the adequacy of CEP's own selection process in relation to its service criteria, that is, to what extent do diverted defendants show employment and other characteristics fitting the services CEP offers. While data relevant to this issue are interesting, no conclusion can be reached at this time.

\*\* Defined in our questionnaire as 20 hours of weekly employment for more than one month's duration.

none are significant.

Few respondents have been in job training programs, although about three out of four in both assignment groups answered positively when asked if they had wanted job training in the last twelve months (Table 5E and F). About the same proportions wanted job referrals during that period.

These employment characteristics suggest some of the issues that will be involved in the evaluation of CEP. Clearly, CEP is selecting a population in need of job training, counseling, and referral. On the other hand, the program selects many people who have been employed within the recent past. It seems reasonable to assume that many of CEP's clients come into the program with some expectations about the kinds of jobs they will be offered. We suspect that these job expectations may be one of the program's problem areas.

6. Education. Besides job related services, CEP provides educational counseling and referral. Table 6 shows that there is a clear need for these services among the clients the program receives. The majority of respondents have not completed high school (Table 6A). Proportionately more controls than experimentals are presently enrolled in school, although both groups report the same degree of regularity of attendance (Table 6B and C). While the higher proportion of controls in school is not statistically significant, it is puzzling. Since there are more 16 and 17 year olds in the control group, age would seem a logical explanation, but it is not. In data not shown, there are still proportionately

more controls (85%) than experimentals (72%) in school\* among those 17 or under. For both controls and experimentals, however, the percentages in school drop equally precipitously after age 17.

It is clear from the respondents' feelings about and desire for more education (Table 6 D and E), that CEP can provide a service in this area. Thirty-seven percent of respondents show some dissatisfaction with the relevance of their education for the jobs they want and over half have wanted help with enrollment in schools or educational programs.

7. Need for Social Services. As part of its counseling, CEP provides the clients with help and information about social services. The range of these services is broad: from getting teeth fixed to applying for welfare benefits for which they may be eligible. Table 7 shows some aspects of the economic situation of respondents. Of those respondents who knew their households' income\*\* about half earn under \$7,000 yearly, while one-fourth earn over \$10,000 yearly (Table 7A). Thus, for the most part, this is a low income group, with a minority in the low-to-middle range. As Table 7B indicates, half the respondents or some member of their households were receiving government assistance (mainly Aid to Families with Dependent Children). Ten percent of respondents had received some

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\* Since controls are more likely to be in school and less likely to have been employed, the puzzle might be solved by looking at the combination of schooling and employment for each group, controlling for age. This analysis will have to await entering data on the complete intake population on tape.

\*\*This is self-reported income from all sources. Note the low number of responses in the table. Many respondents, especially young ones, did not know their household's income, although they report their own earnings with relative ease.

sort of transfer payment, usually unemployment compensation and one-fourth have wanted information about welfare or other social services during the year before intake (Table 7C and D). There are few differences between experimentals and controls in their use of and need for social services.

Table 7E also shows some aspects of the respondents' housing situation. Two-fifths of respondents would like to move. The most common reasons given are living in a bad neighborhood and wanting to live alone.

Though the data are not shown in Table 7, respondents were asked extensive questions about medical and health services. Over two-thirds visited a doctor at least once during the last 12 months for emergency treatment or for treatment of illnesses. About half visited a doctor at least once for a check-up. Very few (21%) wanted to see a doctor during that period but were not able to. With respect to other medical services, dental services is the single area where many respondents expressed a need to receive services or receive them more often (35%). In other areas such as counseling or psychological care, respondents did not report much use of services, nor did they report wanting them. There were no differences between control and experimentals in their use or desire for these health services.

Finally, members of the research population diverted to CEP were also asked why they accepted diversion.\* In our evaluation we will be interested in how these reasons predict success in the program. Table 8 shows job opportunities as the most common reason

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\*For practical reasons, controls were not asked any questions having to do with CEP.

for accepting diversion, followed by wanting to have the case dismissed. Education and help in getting out of jail are next in importance, while help in dealing with personal problems is the least mentioned.\* These reasons may have much to do with what the program's screeners tell respondents while they await arraignment. After the follow-up interviews, we will be able to compare their reasons to the respondents' expectations and experiences as CEP clients.

8. Conclusion. This section has explored various social characteristics of the research population in order to determine the efficacy of the assignment procedure. In terms of characteristics of their court case, criminal background, and demographic status, the control and experimental groups are basically the same. The typical person accepted for CEP's diversion program has been arrested for a property-related felony, most likely for the first time, and is likely as not to be engaged in illegal activities as an on-going pattern of life. The typical person accepted by CEP is under 20, single, male, and black or hispanic.

Data have also been presented on differences between controls and experimentals in employment, education and their use of and need for social services. No differences between the two groups on any of these variables have been found. These data also establish the baseline for comparing changes in the lives of people in the two groups after intake into the research population. The typical person accepted for diversion is unemployed, although he or she is as likely as not to have had some part-time work in the

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\*Interviewers were instructed to record answers to these questions without probing for additional answers.

last twelve months . If there has been a job, most likely it has been a low-skilled manual or service job earning an average of \$105 a week. The typical person in the research population is not in school, comes from a family earning under \$7,000 yearly, and is as likely as not to come from a public assistance dependent household.

In general, the research population's need for services fits with the services CEP provides. Most want job training and referral; half want educational referrals; a smaller percentage are interested in other types of services such as welfare information or medical services. Most who enter the program mention help in finding jobs and getting charges dismissed as the reasons for their deciding to go into CEP. In the next twelve months it will be the task of the research to see how CEP's actual services meet these expressed needs, and what effects the program's services have on the population's future illegal behavior and their life situation in general.



### III. DESIGN OF THE COLLATERAL STUDIES

#### A. Selection for Diversion

1. Summary. To examine how defendants in the controlled study population were selected and approved for diversion and to obtain a sense for the impact of CEP diversion on the courts, the research has designed a collateral study of CEP's screening and intake operations. Four groups of defendants in the Manhattan and Brooklyn criminal courts during the first six months of research intake will be compared using data available from CEP and CJA records. To explore in greater depth the process by which formally eligible defendants are rejected for diversion, data are collected on who rejected the case, the reasons given for the rejection and the final disposition of the case. Finally, to examine whether different screening procedures affect the type of defendants approved for diversion, cases selected for the controlled study before and after CEP moved from an active (solicitation) to a more passive (referral) system will be compared.

Necessary to any evaluation of diversion is an examination of how defendants are selected and how they compare with groups of other criminal defendants. In order to understand how the defendants in the controlled study population were identified as eligible and selected out of all the defendants arraigned in the criminal courts, the research designed a separate, small-scale study of CEP's screening operations during the first six months of research intake. In addition, the research wanted to establish some indicators of CEP's impact on the processing of cases through the court. The design of this phase of the research centers on comparisons between four groups of defendants in the Manhattan and Brooklyn Criminal Courts: those arraigned on felony charges; those screened by CEP as eligible; those eligibles rejected for the program, and those approved for diversion. The goals of the comparisons are to assess the proportion of all arraigned defendants eligible for diversion, the proportion of eligible defendants

actually screened by CEP, and to analyze who rejects eligible defendants, the reasons for the rejections, and the case outcomes.

To carry out these comparisons, the research designed data collection procedures in cooperation with CEP court staff to count and classify all defendants identified by CEP as eligible for diversion and screened by them during the first six months of research intake. Each eligible case was classified according to the screening outcome: for example, identified as eligible but missed in court or never interviewed by CEP; transferred out of criminal court (to family court, mediation, etc.); rejected for diversion or approved for diversion. For each rejected case, CEP screeners also coded the person who rejected the defendant (e.g., the defendant himself, CEP, the ADA, the defense attorney, judge, or probation officer) and the reason for the rejection (e.g., defendant did not want CEP services, the ADA thought the charges or the prior record too serious, the defense attorney would not accept a condition set by the ADA).

Using CEP's administrative summaries of these detailed records ( their "Weekly Screening Reports") the research is able to enumerate the number of cases actually screened by CEP and the results of that screening. To assess in greater depth the process of rejection and approval, the research randomly sampled 25 days during the first six months of research intake. All 599 cases rejected in Manhattan and Brooklyn on those days constitute a "reject sample." Using the original screening records, the research will examine the process of their rejection in greater detail. A

profile of the group will also be compared with the profile of those approved for diversion during the same period. To relate these groups of defendants to the larger population from which they were selected (that is, the total population of defendants arraigned on felony charges) the research is attempting to establish a method of drawing a 20 percent random sample of defendants arraigned on the same 25 days using the computerized files of CJA (N= about 600 cases). If this proves impossible, CJA aggregate data will be used to estimate the relationship of the diverted population to all arraigned felony cases.

CJA computerized records constitute a uniform data base for comparing all these groups with respect to age, sex, current charges, prior convictions, open cases, and case disposition. Somewhat more detailed data are available to compare the sample of rejected defendants with those approved for diversion; these include: ethnicity, employment and educational status at arrest, living arrangements, children and prior arrest status.

Finally, CEP's decision to move from an active screening procedure (solicitation by screeners in the court) to a more passive procedure (referral from judges and defense attorneys) provides the research with an opportunity to examine an additional aspect of the selection process: whether or not different types of cases are approved for diversion under different selection systems. Cases approved for diversion and taken into the controlled study population will be compared before and after the change in screening procedures.

B. Description of CEP and Selected Other Diversion Programs

1. Summary. Descriptive materials on CEP's screening and service activities are called for in the overall research design as a necessary framework for interpreting findings of the controlled research and the study of selection. In addition, six site visits of other diversion programs were made to examine the structure and context of diversion in different jurisdictions. Materials collected on each program focus on: where the program is located in the criminal justice system; its eligibility criteria; selection and intake procedures; characteristics of diverted defendants; services offered by the program; and exit procedures for successful and unsuccessful participants. Where possible, recent changes in the programs are noted.

The overall research design includes a description of CEP's screening, intake and service delivery system. These descriptive materials are to provide the background for a more comprehensive interpretation of the findings in the controlled research and the study of selection. Without such descriptive data, an evaluation cannot clarify the context within which the program operates, the processes that affect its client population and the choice of its service orientation.

To supplement the descriptive materials on CEP, the research design includes site visits to selected other diversion programs. These materials are designed to provide the research with a broader perspective from which to examine the diversion process in New York City and the issues raised by its evaluation.

Besides CEP, the research identified six programs to visit: the Hudson County Pretrial Intervention Project (Jersey City, New Jersey); the Bergen County Pretrial Intervention Project (Hackensack, New Jersey); Operation Midway (Mineola, New York); the Court

Resources Program (Boston , Massachusetts); the Dade County Pretrial Intervention Project (Miami, Florida); and Operation de Novo (Minneapolis, Minnesota.).

Selection of these programs was not designed to be representative of all diversion efforts in the United States. Since the Vera evaluation examines a well-established program, one in existence ten years and locally funded, it was considered appropriate to include among the other programs visited some that have also been in operation since the early 1970's and that have good reputations in the pretrial area. It was not accidental, therefore, that three of the programs selected by the Vera research were described by Mullen in 1973-1974\* and four were included in the profiles compiled by the National Pretrial Intervention Service Center in 1974.\*\* These earlier documents reflect the period when diversion programs were new and funded by U.S. Department of Labor or LEAA demonstration money.

In addition, several other important structural dimensions were considered in selecting the programs to visit. Regional diversity was important (although resource limitations affected the geographical distribution and number of sites selected) as was their location in an urban though not necessarily central-city setting. The remaining factors included: the basis of their authority to

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\* Joan Mullen, The Dilemma of Diversion: Resource Materials on Adult on Adult Pre-Trial Intervention Programs, U.S. Department of Justice, Law Enforcement Assistance Administration, National Institute of Law Enforcement and Criminal Justice, 1974.

\*\*Descriptive Profiles on Selected Pretrial Criminal Justice Intervention Programs, American Bar Association, Commission of Correctional Facilities and Services, National Pretrial Intervention Service Center, April, 1974.

divert; the organizational auspices under which they operate; at what point in the legal process they divert; their eligibility criteria with regard to pending charge and the defendant's juvenile or adult status; their service philosophy or orientation; and whether the outcome of successful participation is a dismissal of the charges or charge reduction.

Site visits were structured to take three to four days and to be conducted by one or more Vera researchers. In the case of two programs, the visits were only one day in length and, as might be expected, the research staff visited CEP far more frequently. In addition to talking with program directors and key administrative staff, Vera researchers talked with court screeners, counselors, and service specialists in the programs. The project directors also arranged for researchers to observe the programs in operation and to interview prosecutors, judges and defense lawyers. While the materials collected largely reflect the programs "frozen in time," researchers attempted to identify recent changes in the programs.

The collection of interview and observational data was designed to explore the nexus between the location of the program in the criminal justice system, the basis of its authority to divert, its eligibility criteria, who makes the decision to divert, the composition of the client population, and the services rendered. An evaluation of these relationships with regard to outcomes (either for the defendant or the criminal justice system) was not attempted. More elaborate research techniques than qualitative data collection are necessary to evaluate program impact. This is particularly true because most programs do not give priority to collecting even the most basic outcome data.

The qualitative field data collected on the seven programs (including CEP) follow the same framework though there are differences in the amount of information available in each area and in the degree of emphasis. First, where is the program located in the criminal justice system? Data were collected on the legal authority under which the program diverts (e.g., court rule, state legislation, prosecutorial discretion); the auspices under which it operates (e.g., a line criminal justice agency, informal agreements); and from whom the agency receives funding. Second, what are its eligibility criteria? Data was sought on the ways in which the program's place in the criminal justice system and its service philosophy influence formal and informal selection criteria. Third, what are the program's selection and intake procedures? Descriptions were sought on how the program selects defendants for diversion from among those identified as eligible and who the major decision-maker(s) is (are) in the process. Fourth, what are the demographic and social characteristics of diverted defendants? Researchers attempted to understand the composition of the client population served by the program and how the program counselors perceive these clients, their life situation and reasons for their criminal behavior. Fifth, what services, therefore, does the program offer? Researchers tried to elicit the service philosophy and aims of the program as well as to describe the types of concrete services it offers and how it delivers those services. Sixth, how does the diverted defendant exit from the program? Data were collected on how the program defines client "success" and "failure"

and how this is related to the requirements that it report back to the court or prosecutor. Procedures for handling "unsuccessful" terminations were also considered. Seventh, and finally, what are the special features that make the program distinctive? Unique or unusual aspects of the program were identified in the field visits and described.

### C. Preliminary Design of the Cost-Benefit Study

The last element of the overall research design is an assessment of CEP's cost-effectiveness with respect to the criminal justice system and its longer range impact where measurable in monetary terms.

The first aspect of this study is a per-case\* comparison of the cost of processing defendants normally through the courts as opposed to diverting them. Appearance history data collected in the course of the controlled study will be used to provide the basis for comparing relative court processing costs for non-diverted and diverted cases (including those unsuccessfully terminated from the program and returned to court for prosecution). Data will include the number of court appearances to disposition, detention days pretrial , probation and prison time served. To the costs for processing experimental cases through the courts will be added the per case costs of CEP services.

The second and more speculative aspect of this assessment will be to examine the potential economic costs and benefits of

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\* A per-case approach is probably more appropriate for this analysis than one focusing on the over-all impact of the entire agency. This is because the controlled research population does not include all types of defendants diverted by CEP; cases from the Bronx and Queens are not reflected in the measurement of diversion outcome.



diversion over the somewhat longer run. Criminal history as well as personal data are being collected in the course of the controlled study to assess differences between controls and experimentals over the 12 month follow-up period with respect to recidivism (further court processing); economic dependency (including earnings, welfare, unemployment insurance, food stamps, etc.); and the use of public services (including health, day care, counseling). These behaviors may be translated into monetary terms and the cost or benefits to the community assessed. The analysis of CEP's economic impact at this level, however, must be cautious. Because the follow-up design covers only a twelve month period, and because CEP's client population is so youthful, immediate "costs" could still be longer range "savings." For example, while helping a young diverted defendant get on welfare (or into a public training program) is a "cost" over the follow-up year, in the longer run it could result in a more stable, productive member of the community.

## IV. DESIGN OF THE ANALYSIS

Summary. This section discusses methodological issues related to the analysis of the controlled study of the CEP diversion program. Three major groups of issues are discussed.

1. Problems of internal and external validity to be taken into account when analyzing the data;

2. Problems of reliability in before and after measures, that is, in the pre-program situation of the research population and in post-program outcomes;

3. A plan of analysis, that is, a discussion of outcomes that constitute program success, a definition of important subgroups for whom program outcomes will be measured, and a listing of intervening variables that potentially affect program outcomes.

A. Problems of validity in the analysis

The use of pre-post test control group design adequately assures that many sources of invalidity in experimental research will not constitute problems in this study. Thus, the design itself eliminates the confounding effects of history, maturation, and statistical regression. Because of the unusual construction of the control and experimental groups in this study, however, other sources of invalidity merit discussion.

Possible invalidity from differential selection of the experimental and control groups was the thorniest problem in this study. In Section II C above, preliminary data were presented which indicate that the assignment procedure has produced basically similar control and experimental groups. Further analysis of a similar nature will be carried out as the analysis proceeds. The remaining sources of invalidity--testing, instrumentation,

experimental mortality, and the interactive effects of these factors--are less problematic than that of selection, but they need to be briefly discussed.

With respect to testing, the experimental literature notes that pre-test observations may produce changes in the post-test behavior of subjects. This is especially the case with measures of attitudes and personal dispositions. While these types of measures have been used in the questionnaires administered to our research population, the majority of the central observations in this study relate to behavior. For the most part, the intent is to measure changes in employment, education, income and its sources, use of services, and criminal behavior. The tone of these questions is neutral. Thus, there is no reason to suspect that the act of recording data concerning these behaviors will influence respondents to change them.

A related problem is the time gap between being assigned to the experimental or the control group and being administered the intake ("pre-test") questionnaire. On the average, experimental subjects were interviewed within 9 days of assignment. In practice, this means that the majority were interviewed before their CEP counselors learned the particulars of their cases. Even for those who began to be counseled in depth before the research interview, the research's emphasis on behavioral measures for the most part precludes contamination of the questionnaire by the respondent's having had some experience with the program.

The average control was interviewed within two weeks of assignment. This longer time gap reflects the interviewers' greater difficulty in locating controls before their release at arraignment.\* At this point it is difficult to determine whether the minority of respondents interviewed after a greater-than-average time gap provided less reliable data. A priori, there is no reason to expect behavioral measures to be less reliable, particularly because most such data are collected on a month-by-month basis (See page 26 above). Attitude and personal disposition data may be more suspect. Therefore, a preliminary part of the analysis will be to compare the answers of respondents with short interview time gaps with those of respondents with longer time gaps.

The questionnaire's emphasis on behavior also minimizes the problem of instrumentation, that is, differential measurement of subjects because of their status as experimentals or controls, or because the observations are pre- or post-test. Interviewers are required to make few judgments about the "meaning" of the questions, since they primarily check pre-recorded answers.

Mortality constitutes a more serious (but manageable) problem. Table 9 in the Appendix shows the number of subjects interviewed and not interviewed for each outcome of the assignment procedure; it includes the reason the interview was not carried out. It may be seen that experimentals had a higher response rate (81%) than controls (71%), while subjects who were exceptions to the exper-

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\* Subjects who were exceptions to the experimental and control assignment were more difficult to locate and to persuade to participate in the study: on the average, it took slightly less than one month from date of assignment to interview them.

imental assignment had the lowest non-response rate (56%). Most of these inter-group differences in response rates are significant\*. Therefore it is important to know if there is any interaction between selection and non-response, that is, whether non-interviewed respondents are different from those who agreed to be interviewed. In addition, it is important to know if there are any experimental-control differences between interviewees and non-interviewees.

Since all defendants in New York City are interviewed by the Criminal Justice Agency to determine their eligibility for release-on-recognizance, partially verified data from those interviews are available for the entire research population. Regardless of interview status, therefore, some background characteristics, as well as future rearrests and case dispositions are available for the entire research population. CJA data (Table 10), for example, show that interviewees are slightly younger than non-interviewees. It would appear that older people were less likely to agree to an interview (or to be easy to locate). But the age differences between interviewed and non-interviewed are not sizeable enough to create analytical problems. In addition, there are no significant age differences between controls and experimentals, controlling for interview status. Among interviewees, controls are slightly younger than experimentals, but the difference is not significant. Among non-interviewees, there are no age differences between experimentals and controls.

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\* For example, the difference between controls and experimentals is significant at less than the .001 level.

Invalidity from differential post-test mortality among experimentals and controls is a major concern. However, at this moment, the follow-up phase has not progressed enough for an evaluation to be made about the extent of differential mortality.

Another set of validity problems concerns the representativeness of the research population. What types of generalizations can be made from the research findings and to what groups outside the research population? These questions are particularly pertinent with respect to two groups: generalizations to the current CEP client population, and to a more loosely defined group of potential divertees to CEP or other programs. The latter group encompasses defendants fitting all current criteria for diversion to CEP but not brought to the attention of decision-makers in the court setting, future defendants that could be diverted to CEP, and diverted defendants in other pretrial intervention programs. For the sake of precision, it must be noted that the research findings cannot be generalized directly to those who are rejected for CEP or themselves reject the program before arraignment, and those who do not fit the formal criteria for eligibility.

The research population constitutes 57 percent of all defendants diverted by CEP between mid-January and October 31, 1977. The non-research CEP client population can be divided into the following groups:\*

-66 participants from Manhattan, Bronx and Brooklyn Criminal Courts assigned directly by judges into the program, i.e., not subjected to the research's assignment procedure;

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\* Pages 17 to 22 above explain the decisions behind which courts and defendants were excluded and included in the controlled study.

-61 participants diverted into the program in Bronx Criminal Court after the controlled study was discontinued in that court;

-133 participants diverted from Queens Criminal Court which was not part of the controlled study.

The findings are probably generalizable to other Bronx defendants diverted after the controlled study was discontinued in that court. With respect to Manhattan and Brooklyn participants not subject to the research's assignment procedure and Queens divertees, it is not yet known how similar they are to the research population. Characteristics of these groups will be compared with the research populations by means of CJA and CEP records.

The broad research findings should be generalizable to defendants who are potential divertees but who are not now being brought to the attention of CEP, and to any future CEP client population, so long as the selection criteria for the client population remain the same (or shift in the direction of one of the subgroups now being served), and so long as the program maintains its current service orientation. Since the analysis will look at program outcomes for various subgroups (by age, offense, employment status, etc.), CEP will be able to estimate the consequences of shifting its service emphasis towards one or another of these subgroups.

More difficult to resolve is the problem of generalizing from the research population to other diversion programs. Programs vary in the kinds of people they divert and the services they offer. While CEP was one of the first diversion programs, it is not typical of most diversion programs in the kinds of people it

serves (young, inner-city, lower class, minority status clients) nor in its manpower-oriented services.\* However, many programs do have some defendants who are similar to the typical CEP participant among their clientele. Moreover, the typical CEP client is precisely the type of defendant many programs claim are the most difficult for them to counsel. Therefore, this study's findings will be helpful and possibly generalizable to other programs, so long as it is made clear to which subgroups of defendants and to which type of diversion services the findings apply.

#### B. Problems of reliability

Since the research stresses behavioral measures of program outcome (employment, schooling, criminality) and at the same time depends on self-reports for these measures, the problem of reliable measures needs consideration.

Some of the self-reported data will be verified with official records. Schooling, employment, public assistance and earnings are being verified by contacting the relevant agencies or employers. All respondents will be checked for their use of public assistance whether or not they acknowledge its receipt in the interview. Only reported employment or schooling, however, can be verified since unlike arrests or welfare, there is no single source to contact with respect to employment or education.

Respondents' self-reports of prior and future arrests will be checked against their official court and police records. Some

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\* See the study's descriptive profiles on six diversion programs outside New York City visited by the research staff.



preliminary cross-checking of self-reported and CJA data (from the questionnaires now on tape) has been conducted. Only 7 percent of respondents report fewer previous arrests than the total felony and misdemeanor convictions plus open cases shown in their CJA records. Only 7 percent of respondents report no previous arrest while CJA records show one or more previous convictions and open cases.\* Only 3 percent of respondents report fewer convictions than the total number of felony and misdemeanor convictions shown in the CJA interview reports, while only 3 percent reporting no previous convictions are shown by the CJA data to have been convicted at least once before. Thus, the first check on self-reported arrest and conviction behavior seems to indicate that it is a fairly reliable measure. As a further check, self-reported arrests will be checked against police records of previous and future arrests.

Self-reported illegal activities will be checked for consistency with court and police records as well as other variables (for example, age and interviewer ratings of respondents' cooperation with the questionnaire). Similarly, experimentals' attitudes and reports on what happened to them while in CEP will be checked with counseling records kept by the program.

Even though official records will be used as reliability checks, their own reliability needs evaluation. For example, before merging court case information with the questionnaire information, a 25 percent sample of the research population's CJA interview reports will be matched with their police arrest records. This

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\*The CJA interview reports do not show arrests leading to dismissals before or after being brought to court. Thus, many respondents report more arrests than are shown in CJA interview reports.

will indicate how many and what kinds of arrests are missed by relying solely on CJA's court processing information.

The degree of discrepancy between self-reported and verified information will be taken into account in the evaluation analysis. If the discrepancy is great, the verified information will be used instead of the self-reported information. In some cases, the verified information will be used as a control, that is, to compare those whose self-reports are verified with those revealing contradictory information.

### C. Plan of analysis

The remainder of this methodological section discusses the measures that have been included to determine the success of program outcomes, and estimates the power of the design with respect to measuring these outcomes. The discussion specifies the sub-groups within the research population for whom success will be measured, and intervening variables to be taken into account in analyzing program success.

1. Measures of program success. CEP, like many social service agencies, provides a variety of services. In addition, CEP stands between the criminal justice and the social service systems in New York City and is answerable to both. It is important to keep in mind prior to any evaluation that CEP's effects may be mixed. For example, CEP may divert some people from the criminal justice process, but not be as successful in improving their lives; or vice-versa. The program may also be successful with some types

of defendants, but not be able to effect other types (age is an obvious example). Finally, program success--with CEP as with most social service programs--is not an either-or proposition. CEP may not be able to stop defendants from engaging in illegal activity but its seriousness or frequency may be reduced. Similarly, the program may not be able to increase its clients' employment, but it may facilitate an increase in their skills or job-readiness.

Because of the broad nature of what may be considered success, various types of behaviors and situational changes are listed below as measures of program outcome. As a guide to future analysis and for convenience of exposition, program outcomes are listed as propositions to be tested in the form of differences between control and experimental group means.\*

Case outcomes. Besides the services that a pretrial intervention program offers defendants, diversion is supposed to spare them many of the negative consequences of normal court processing:

1. Experimentals will be more likely to have charges dismissed than controls.

Since this is one of CEP's main selling points with defendants, it may seem a self-evident proposition. However, diversion is not an outright guarantee of dismissal of charges and, alternatively, defendants who are not diverted may also receive dismissals. Thus, it is necessary to evaluate the benefits that

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\*Actual analysis of the research data will also include differences in proportions, correlations, and other measures of statistical relationship.

defendants receive in terms of their case outcomes by participating in CEP.

2. Convicted experimentals will be more likely to be conditionally released (ACD, conditional discharge) and will receive more lenient sentences (smaller fines, less time) than controls.\*

3. After arraignment, experimentals will make fewer court appearances on their case, will be less likely to be returned to court on a warrant than controls.

Recidivism. When considering recidivism and other important program outcomes, the notion of a "period at risk" needs to be introduced. Since a client's behavior is to some extent monitored by CEP while he/she participates in the program, but controls are not subject to any monitoring, the proper time frame for comparing experimentals and controls over the long run is equivalent time periods after discounting the treatment period. In terms of this study, therefore, controls will be followed up for one year after the date of assignment while experimentals will be followed up for 16 months (on the average, clients participate in the program for 4 months).\*\*

4. Experimentals will be re-arrested less often than controls while in the program and after completion of the program.

5. Experimentals will be re-arrested for less serious offenses than controls.

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\* If this is not true, it may be assumed that unsuccessful participation in diversion has either a negative or neutral effect on the defendant's case.

\*\* Section II B (pp.22-31) above describes the follow-up schedule for the research population in greater detail.

6. Experimentals will be arrested for fewer violent crimes than controls.

The indicator of seriousness will be the penal code rating of the offense (for example, C felony, B misdemeanor, etc.). The measures for the above outcomes will be derived from CJA and police re-arrest records, as well as self-reported information.

7. Experimentals will report a lower frequency of illegal activities than controls while in the program and after completion.

8. Experimentals will report less serious illegal activities than controls.

9. Experimentals will report less income obtained from illegal activities than controls.

10. Experimentals will report shorter time periods in which they are engaged in making money illegally.

Propositions 7 to 10 will be derived from self-reported information in the questionnaires. Although information about non-economic or violent crimes is not included in the self-reported items, it is possible to categorize self-reported offenses according to their gross categories such as larcenies, robberies, drug offenses etc. One problem of validity with propositions 7 to 10 is the possible effects of the program in producing distorted responses to these questions on the part of program participants. Actual re-arrests will have to be used as a control for self-reported crime, although it is an imperfect control variable, because it will not detect cases where unreported illegal activity has not led to re-arrest.

11. Experimentals will be more likely than controls to report reducing association with others engaged in illegal activities while in the program and after completion.

In the initial questionnaire, respondents are asked whether or not their friends engage in illegal activities. In the follow-up questionnaires, respondents are asked the same question and, in addition, are asked if they are continuing to associate with the same friends they had before the research assignment date. In the follow-up questionnaires, respondents are also asked open-ended questions about changes in their life since the research assignment date. Many respondents report dropping old associates as a significant change in their lives.

Employment and job training. Respondents are asked about the characteristics of jobs they had (occupation, duration, salary) during the 12 months prior to the research assignment date. The questions are repeated during the follow-up phase. In addition, respondents are asked how they look for jobs.

12. More experimentals will have jobs than controls.

13. Employed experimentals will hold jobs longer than employed controls.

14. Employed experimentals will have higher earnings than employed controls.

15. Employed experimentals will have more skilled jobs than employed controls.

16. Unemployed experimentals will seek jobs more actively than unemployed controls.

17. Experimentals will use a greater number of job search methods than controls.

18. Unemployed experimentals will get jobs more quickly than unemployed controls. .

19. Experimentals will spend more time actively looking for jobs than controls; they will also spend less time making money illegally or hanging around than controls.

20. Experimentals will receive more job training than controls.

All these propositions about employment entail straightforward measures. The only exception is job skill level. CEP assesses its clients' job skill level, but no equivalent comparison is available for controls. In coding reported occupations, the research is using a modified version of the Census Bureau's list of occupations. With this measure one cannot determine different skill levels within an occupational category (for example, the difference between a private guard and a short-order cook), but it does yield grosser measures of occupational upgrading, for example, changing from a service to a factory operative occupation. Interviewers probe for the actual work a respondent does, so that occupations may be more accurately classified.

Education. Besides job counseling and referral, CEP provides various services related to education. School dropouts are encouraged to resume schooling. If a client presently in school is having academic or other kinds of difficulties, counselors try to help him or her to resolve these, or to place the participant in alternative schools, including GED programs. In

addition, CEP will soon have its own tutorial program for participants in need of remedial education. In the questionnaires, respondents are asked about past and present schooling, including reasons for leaving school, future educational plans, and satisfaction with their education.

21. Experimentals will increase their amount of schooling more than will controls.

22. Experimental dropouts will be more likely than controls to resume their schooling.

23. Experimentals in school will report better attendance than controls.

24. Experimentals will be more likely than controls to complete their present level of schooling.

25. Experimentals will aspire to higher educational levels than controls.

26. Experimentals will express more satisfaction than controls with the education they have had.

Use of health and social services.

Because CEP aims to improve clients' access to and use of various social services, respondents are asked about their health, visits to medical and other service professionals, use of day care, and perceived needs for these services. For all of these services, information on frequency of use and costs are obtained.

27. Experimentals will receive more health and social services than controls (both in quantity and different types of services).



28. Experimentals will pay less than controls for these services.

29. Experimentals will report more satisfaction than controls with the level of services received.

Respondents are asked about their health, visits to medical and other service professionals, use of day care, and Welfare and other public benefits. Since CEP may help clients apply for those public benefit programs to which they are entitled, respondents are asked about their use of welfare, including medicaid, as well as receipt of unemployment compensation and other types of public benefits.\*

30. Experimentals will be more likely than controls to receive public assistance, including medicaid.

31. Experimentals will receive more non-wage income than controls.

From the standpoint of a social service agency, CEP's success in the short run may be judged by its ability to help clients receive needed short-term financial, housing, or medical attention even if it is at public expense. However, in the longer run, these services should encourage increased economic independence. Consequently, hypotheses 30 and 31 should more correctly read: " in the short run." Additional hypotheses should also be included concerning the longer range likelihood that experimentals are less likely to be on public assistance or receive non-wage income (public) income (particularly if their employment rises). However, it is not clear that a 12 month follow-up period is sufficient for the research to measure these longer-range outcomes.

Life-style.

32. Experimentals will be more likely than controls to move to better living quarters.

33. Experimentals will be more likely than controls to participate in community social activities.

34. Experimentals will be more likely than controls to reduce alcohol and drug consumption.

In the research, better living quarters are defined as an increase in the number of rooms lived in per occupant in the household, and the quality of housing (for example, walls in good condition).

The above propositions constitute some program outcomes to be evaluated. The list is not exhaustive, especially with respect to the quality of personal counseling services, where comparison of experimentals and controls is more difficult. However, the follow-up questionnaires include questions on participants' perceptions of CEP services received, their interaction with counselors and perceived changes in their lives. These items will be important in evaluating the program's services.

2. Statistical criteria for determining program success.

In order to determine what constitutes program success, it is necessary to establish what percentage difference between experimental and control outcomes will be considered significant. The usual starting point is to establish the statistically significant difference between group means. In this case, any difference in proportions between experimentals and controls less than 10% would be suspect.\*

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\* For example, if the experimental re-arrest rate were 20 percent and the control rate were 30 percent, the 95 percent confidence interval for the former (N=347) would be 16-24 percent, while for the latter (N=240) it would be 24-36 percent.

This criterion could be relaxed somewhat if it were assumed that this is a study of a research population rather than a sample. From this perspective, any difference between experimental and control group means would be significant. While the actual research population includes virtually all eligible defendants screened during a 10 month period in two counties, it is one of an infinite number of populations that could have been selected (using different time periods), each with different experimental and control subjects. Secondly, the experimental group constitutes 57 percent of the total CEP client population (from four counties) during the assignment period. Insofar as the experimental group can be shown to be similar to the non-research client population, some generalization of findings is possible. Hence, a confidence interval would have to be established in order to generalize to the latter population.

Besides a statistical criterion for determining success, it is also necessary to determine what difference in proportions between experimental and control group outcomes is programmatically significant. Many factors have to be taken into account in order to do this. Some of these factors are hard to quantify, for example, property losses incurred through re-arrest. From the point of view of programmatically significant differences, then, a statistically significant difference of 10 percent may not indicate program success, although it would be difficult to establish a priori what percentage difference would constitute success.

Another analytical problem to be considered is the treatment of exceptions to the research's assignment procedure. Contained within the experimental group are some defendants not diverted to CEP (not subject to "treatment"). In the control group are some who were diverted. The chart below presents three alternative ways of treating these exceptions in the analysis of program success:

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Alternative (1)	$E_a - C_a$
Alternative (2)	$(E_a + E_r) - (C_a + C_r)$
Alternative (3)	$(E_a + C_r) - (C_a + E_r)$

Where:  $E_a$  = Assigned experimentals accepted into CEP.  
 $E_r$  = Assigned experimentals rejected for CEP.  
 $C_a$  = Assigned controls rejected for CEP.  
 $C_r$  = Assigned controls accepted into CEP.

Alternative (2) would be the most methodologically defensible, since it would keep the research assignment procedure intact. It also subjects the program to a more stringent test, since it would include success outcomes of people who never received treatment. By the same token, it would count among controls some people who did receive treatment. By the same logic, alternatives (1) and (2) would provide weaker tests of the program's success.

In practice, the relatively small number of exceptions in the research population is likely to make the results of all three alternatives similar. For example, measuring differences in recidivism between experimentals and controls under each alternative would produce basically similar confidence intervals.

Given this, the most cautious approach in the analysis will be to measure program outcomes using each alternative in turn, to see if any consistent differences in outcomes appear among the alternatives.

3. Subgroup Analysis. While in an evaluation it is necessary to estimate the overall success of a program, a more interesting and probably more useful procedure is to specify for whom and to what extent success occurs. Thus, when looking at recidivism and other measures of program outcomes, differences in these will be specified for various subgroups of the research population.

Below are listed most of the subgroups that seem important to analyze from a policy point of view. The list indicates which groups are excluded from analysis, and the reasons for excluding them.

Characteristics of CEP's screening process.

1. Arraignment court (Brooklyn vs. Manhattan). Bronx defendants will be excluded when controlling for court, since a very small number of respondents were assigned from this court.

2. Type of screening procedure. CEP changed from an active solicitation procedure to a more passive referral procedure at the midpoint of the research assignment period. It will be interesting to see if there are any differences in program outcomes among defendants selected under each type of screening.

Characteristics of the arrest case.

1. Type of offense. Theft, robbery and assault are the most typical of arrest offenses found in the research population.

2. Severity of offense, that is, whether the arrest charge was a C, D, or E felony.

Criminal background.

1. First offenders vs. prior offenders.

2. Among prior offenders, type and severity of prior offenses.

Thus, three subgroups are possible: first offenders, "light" offenders, and "heavy" offenders.

Demographic characteristics.

1. Age groups. Most of the research population is under 21 (see Table 4 in the Appendix). These could be divided into older teenagers (17 or under) and young adults (18 to 20). Older respondents could be further divided into those under 25 and those over 25.

2. Ethnicity. Most respondents are either black or hispanic. Whites are only 9 percent of the total population, but it may be possible to make some comparisons with whites as one of the ethnic categories.

Females and married respondents each constitute 10 percent of the research population, so it will be difficult, though not impossible to make comparisons by sex or marital status.

Education.

1. Years of education. The population can be divided into those with some elementary education, those with some high school, and those with at least a high school diploma.

2. In school vs. out of school at the date of research assignment. The latter could be further classified into recent vs. less recent dropouts.

Employment.

1. Work history. Those never employed vs. those who have

been employed off and on vs. those who have been employed steadily for a number of years.

The size of the research population also allows for more refined classifications, that is, combinations of two subgroup characteristics. Examples would be age and ethnicity, education, and employment, or court and type of offense. Which of these refined classifications are used will depend on the findings from the first exploratory analyses of the data.

4. Factors affecting program outcomes. An additional aim of the research is to specify how program outcomes come about, that is, what factors constrain or facilitate success among program participants. Analytically, these factors can be seen as intervening variables, that is, they are intermediary between subgroups and program outcomes. Two broad categories of intervening factors may be considered: characteristics of the diversion process itself and factors outside the control of the program. For each category, various variables are listed below, and their anticipated effect on recidivism hypothesized. While it is also possible to hypothesize the effects of these factors on other program outcomes, we have not done this here in order to avoid clouding our exposition of the factors themselves.

Program variables.

1. Client expectations about CEP. In the initial and first follow-up questionnaires, respondents are asked the reasons they agreed to participate in CEP and what they expected to get out of the program. Thus, it will be possible to determine whether the client was interested in a specific outcome, such as having his/her charges dismissed or getting a job, or in more compre-

hensive services such as personal counseling. The more comprehensive the services expected, the more likely that the client would be inclined to "invest" his/her time in the program; therefore the less likely to recidivate.

2. Clients perceptions of program services. Experimentals are asked what conditions they had to fulfill in order to finish the program successfully. Does the client see CEP as a monitoring program or as a service program? It is expected that the former perception will be positively related to recidivism, while the latter will be negatively related.

3. Program status. Was the client terminated or did he/she successfully complete the program? The latter will be negatively related to recidivism.

4. Attendance. The more frequent the client's attendance, the less the likelihood of recidivism.

5. Program services. Clients may receive only one service or many (for example, job referral, vocational counseling, health related referrals). Experimentals are asked about various services they may have received and their perceptions of the service's usefulness. The greater the number of services received, the less likely the client will recidivate. The more positive the client's perception of these services, the less likely the client is to recidivate.

These are some of the program variables that are expected to have important effects on the success of the program's outcomes. Others may suggest themselves after the clients' coun-



seling folders are examined in greater detail.

Factors having effects independent of CEP. Both experimentals and controls may have experiences outside of CEP effecting the program's degree of success. In the case of experimentals, these may strengthen or weaken the effect of program services. In the case of controls, they may serve as effective alternative mechanisms to produce equally successful outcomes. Insofar as these mechanisms operate, they will tend to reduce differences between experimentals and controls on the outcome measures discussed above.

1. Services provided by other agencies. In the follow-up questionnaires, respondents (experimentals and controls) are asked about their participation in rehabilitation, counseling and other programs. (Experimentals are also asked about CEP's role in their participating in outside programs.) The greater the number of services received, the less likely a person (control or experimental) is to recidivate.

2. Help from others. When asked about employment, education, and other experiences, all respondents are asked if they received help from relatives, friends, CEP workers (in the case of experimentals), and other professionals outside CEP. Thus, it will be possible to weigh the relative effects of various people in helping CEP clients, as well as the effects of other people in helping controls. The greater the number of others helping respondents to get employment, education, and other services, the less likely the individual is to recidivate.

Respondents are also asked how each of the persons identified was helpful with a particular problem (CEP or others). The answers will be coded as to degrees of involvement, that is, did the help consist of information or encouragement, or was there more active participation in helping the respondent to solve the problem. The greater the involvement, the less likely the individual is to recidivate.

3. Situational changes. Respondents may have experiences that have either positive or negative effects on successful outcomes independently of their involvement with CEP. Changes in marital status, changes in relations with relatives and associates, and changes in schooling, employment, housing and other situations may change a person's outlook on life and his/her future behavior. Respondents are asked questions about who they live with, the people they associate with, and the extent to which they engage in social and recreational activities. In addition, respondents are asked open-ended questions about important things that have occurred in their lives between the date of research assignment and the follow-up date. Answers to these questions are coded as to area of change (personal relations, involvement with the law, etc.), and the direction of change (positive or negative). Those who have had positive changes will be less likely to recidivate than those who have had negative changes.

These are some of the effects that intervening variables are expected to have on program outcomes. It is expected that the variables will operate in a more complex manner than out-

lined above. For example, for experimentals, some of the situational factors that have been discussed above (for example, finding a job) may be successful program outcomes that lead to other successful program outcomes. For both experimentals and controls, some intervening variables may have an effect on other intervening variables rather than having a direct effect on recidivism or some other outcome. Each variable may have different effects on different subgroups. Some variables may also have positive effects on some outcomes and negative effects on other outcomes. As the analysis progresses, these will be further elucidated (possibly by path analysis or multiple regressions) and additional intervening variables will undoubtedly be discovered.

V. APPENDIX

TABLE 1

Research Assignment and CEP Screening Outcome by Borough:  
 Total Research Population (1/19/77-10/31/77)

Research Assignment Status	Actual Screening Outcome	Borough			Total
		Manhattan	Brooklyn	Bronx	
Experimental	Diverted	175	154	18	347
	Not Diverted*	27	34	2	63
Control	Not Diverted	123	103	5	240
	Diverted*	9	5	2	16
TOTAL Research Population		334	296	36	666
Not Subject to Research Assignment*		47	18	1	66

\*Exceptions to the research assignment

TABLE 2

CHARACTERISTICS OF THE CURRENT COURT CASE  
BY RESEARCH ASSIGNMENT STATUS

Court Case Characteristics	Research Assignment Status		TOTAL
	Experimental	Control	
<b>A. <u>Type of arrest charges</u></b>			
Theft only	51%	53%	52%
Theft and other non-violent offenses	26	21	24
Robbery	7	11	8
Assault w/o robbery	11	9	10
Other	5	6	6
TOTAL (=100%)*	(225)	(141)	(366)
<b>B. <u>Highest severity among arrest charges</u></b>			
Felony C	14%	13%	14%
Felony D	55	47	52
Felony E	29	38	32
Misdemeanor A/B	2	2	2
TOTAL (=100%)*	(224)	(138)	(362)
<b>C. <u>Highest severity among arraignment charges (complete research population)</u></b>			
Felony C + B	10%	8%	9%
Felony D	51	46	49
Felony E	36	43	39
Misdemeanor A/B	3	3	3
TOTAL (=100%)*	(390)	(243)	(633)

\* Missing data or no answers excluded from total.

(Table 2 continued...)

TABLE 2 (continued)

Court Case Characteristics	Research Assignment Status		TOTAL
	Experimental	Control	
<u>D. Relation to victim</u>			
Stranger	46%	53%	50%
Relative or Acquaintance	21	17	20
Agency or corporation	33	30	30
TOTAL (=100)*	(289)	(135)	(374)
<u>E. Percentage arrested with others for the same incident</u>			
	55%	58%	56%
TOTAL (=100)*	(250)	(144)	(394)
<u>F. Percentage with co- defendants in arrest case (complete research population)</u>			
	31%	27%	29%
TOTAL (=100%)*	(396)	(250)	(646)

\*Missing data or no answers excluded from total.

TABLE 3

CRIMINAL BACKGROUND CHARACTERISTICS BY  
RESPONDENT ASSIGNMENT STATUS

Characteristics	Assignment Status		
	Experimental	Control	TOTAL
A. <u>Official Prior Adult Conviction Record</u>	25%	21%	24%
TOTAL (=100%)*	(223)	(132)	(355)
B. <u>Prior Adult Arrest (self-reported)</u>	39%	33%	37%
TOTAL (=100%)*	(250)	(144)	(394)
C. <u>No Juvenile (pre-16) Arrests (self-reported)</u>	80%	84%	81%
TOTAL (=100%)*	(251)	(144)	(395)
D. <u>Types of Offenses Among those Reporting Prior Arrests**</u>			
Theft only	30%	25%	29%
Theft and other non-violent offenses	13	3	10
Robbery	21	16	19
Assault without robbery	16	9	14
Drugs only	2	11	5
Conduct	10	27	15
Other	8	9	8
TOTAL (=100%)*	(92)	(44)	(136)

\* Missing data or no answers excluded from total.

\*\* Significant between .01 and .001 level.

(Table 3 continued...)



TABLE 3 (continued)

Characteristics	Assignment Status		TOTAL
	Experimental	Control	
<u>E. Severity of Offenses</u>			
<u>Among those Reporting</u>			
<u>Prior Arrests</u>			
Two or more felonies	23%	17%	21%
Two or more combinations of felonies or misdemeanors	30	25	29
One felony	33	33	34
Two or more misdemeanors or violations	14	25	16
TOTAL (=100%)*	(86)	(36)	(122)
<u>F. Number of Types of</u>			
<u>Illegal Activities</u>			
<u>Committed in the Last</u>			
<u>12 Months (self-reported)</u>			
None	51%	57%	54%
One	20	17	19
Two or more	29	24	27
Mean Number of Types of Illegal Activities (self-reported)	1.3	1.2	1.3
N= *	(255)	(148)	(403)

\*Missing data or no answers excluded from total.

TABLE 4

PERSONAL BACKGROUND CHARACTERISTICS BY RESEARCH  
ASSIGNMENT STATUS

Background	<u>Assignment Status</u>		
	Experimental	Control	Total
<hr/>			
A. <u>Age</u> (Total research Population)			
17 or less	40%	46%	42%
18-20	28	25	27
21-25	17	16	16
26 or more	15	13	15
TOTAL (=100%)*	(393)	(250)	(643)
<hr/>			
B. Percent male	89%	91%	90%
TOTAL (=100%)*	(244)	(171)	(415)
<hr/>			
C. <u>Marital Status</u>			
Married	11%	10%	11%
Single	81	84	82
Other	8	6	7
TOTAL (=100%)*	(249)	(144)	(393)
<hr/>			
D. <u>Ethnicity</u>			
Black	53%	61%	56%
White	9	10	9
Hispanic	37	29	34
Other	1	--	1
TOTAL (=100%)*	(249)	(143)	(392)

\*Missing data or no answers excluded from total.

TABLE 5  
EMPLOYMENT CHARACTERISTICS AND ASSIGNMENT STATUS

Employment Characteristics	Assignment Status		TOTAL
	Experimental	Control	
A. <u>Held job of 20 hours or more per week at date of intake</u>	12%	12%	12%
TOTAL (=100%)*	(251)	(146)	(397)
B. <u>Held job of 20 hours or more per week in the last 12 months</u>	56%	46%	52%
TOTAL (=100%)*	(248)	(146)	(394)
C. <u>Among those employed in jobs of 20 hours/week or more in the last 12 months:</u>			
Mean number of weeks worked	21	23	22
Mean weekly salary before taxes	\$100	\$114	\$105
Percentage reporting hours and salary	90%	91%	90%
D. <u>Occupations of those who held jobs in the last 12 months</u>			
Professional, technical, managerial	5	10	7
Clerical	21	13	18
Crafts	3	11	6
Operatives	18	19	18
Laborers	19	13	17
Service	34	34	34
TOTAL (=100%)*			

\* Missing data or no answers excluded from total.

(Table 5 continued...)

TABLE 5 (continued)

Employment Characteristics	Assignment Status		TOTAL
	Experimental	Control	
E. <u>Percent having completed a job training program</u>	12%	8%	11%
TOTAL (=100%)*	(251)	(146)	(397)
F. <u>Percent wanting job services during last 12 months</u>			
Job training	77	70	74
Job referral	72	74	72
TOTAL (=100%)*	(251)	(144)	(395)

\* Missing data or no answers excluded from total.

TABLE 6

EDUCATIONAL CHARACTERISTICS BY RESPONDENT  
ASSIGNMENT STATUS

Education Characteristics	Assignment Status		TOTAL
	Experimental	Control	
<b>A. <u>Years of school completed</u></b>			
8 or less	21%	16%	19%
9-11	65	71	67
12 or more	14	13	14
TOTAL (=100%)*	(243)	(143)	(386)
<b>B. <u>Presently in school</u></b>			
	33%	43%	37%
TOTAL (=100%)*	(246)	(143)	(389)
<b>C. <u>Among those presently enrolled: attending almost every day</u></b>			
	70%	70%	70%
TOTAL (=100%)*	(82)	(64)	(146)
<b>D. <u>Percent considering none or very little of their education useful for the jobs they want</u></b>			
	38%	36%	37%
TOTAL (=100%)*	(250)	(145)	(395)
<b>E. <u>Have wanted school or educational program referral within the last 12 months</u></b>			
	59%	52%	56%
TOTAL (=100%)*	(251)	(144)	(395)

\* Missing data or no answers excluded from total.

TABLE 7

SOCIAL SERVICES CHARACTERISTICS BY  
RESPONDENT ASSIGNMENT STATUS

Social Service Characteristics	Assignment Status		TOTAL
	Experimental	Control	
<b>A. <u>Family yearly income</u></b>			
\$4,999 or less	37%	38%	37%
\$5,000 - 6,999	11	20	15
\$7,000- 9,999	26	17	23
\$10,000+	16	25	25
TOTAL (=100%)*	(115)	(71)	(186)
<b>B. <u>Respondent or household on government assistance</u></b>			
	50%	40%	47%
TOTAL (=100%)*	(247)	(144)	(391)
<b>C. <u>Receiving some transfer payment, e.g., unemployment compensation</u></b>			
	12%	9%	11%
TOTAL (=100%)*	(249)	(145)	(394)
<b>D. <u>Have wanted information about welfare or other social services during the last 12 months</u></b>			
	25%	20%	24%
TOTAL (=100%)*	(251)	(146)	(397)
<b>E. <u>Would like to move</u></b>			
	45%	39%	43%
TOTAL (=100%)*	(251)	(145)	(396)

\* Missing data or no answers excluded from total.

TABLE 8  
EXPERIMENTALS' REASONS FOR ACCEPTING  
DECISION TO BE DIVERTED INTO CEP

<u>Reason Given</u>	<u>Percent Giving Reason</u>
Help in getting out of jail	33%
Help in finding a job or better employment opportunities	63
Help in getting more education	35
Help in dealing with personal problems	21
Help in getting charges dismissed	52
N=*	(219)

---

\* Missing data or no answers excluded from total.

TABLE 9

Interview Status, with Reason for Failure to  
Interview Respondent, by Assignment Outcome

Interview Status	Experimental		Control		Total	
	Diverted	Not Diverted	Not Diverted	Diverted		
Interviewed	83%	56%	71%	82%	76%	
Not Interviewed:						
Refused	7	20	15	6	11	
Unsuccessful attempts to schedule an interview	5	11	6	6	6	
Unable to locate respondent	5	13	8	6	7	
TOTAL= 100%	(N)	(347)	(63)	(240)	(16)	(666)



TABLE 10

Distribution of Age Groups by Research Assignment  
Status, According to Interview Status, Complete  
Research Population

Age Group	Interview Status				Total Research Population*
	Interviewed		Not Interviewed		
	Experimental	Control	Experimental	Control	
17 or under	41%	49%	36%	39%	42%
18-20	29	24	24	28	27
21 or over	30	27	40	33	31
TOTAL (=100%)*(308)		(174)	(85)	(76)	(643)

\* Missing data excluded.