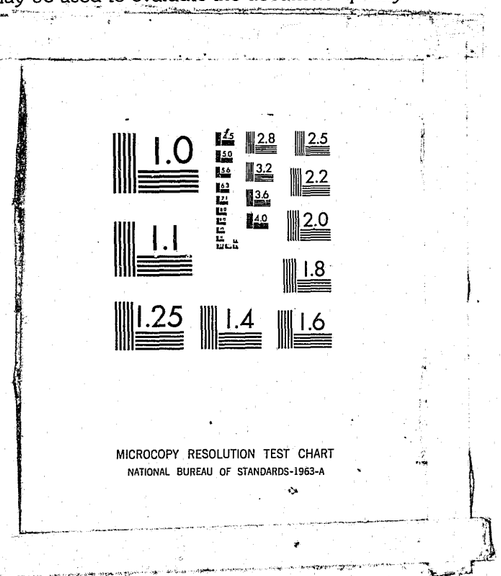

VOLUME

4

OF THE FINAL REPORT
ON THE
SUPPORTED WORK EVALUATION

THE IMPACT OF SUPPORTED WORK ON EX-ADDICTS

NATIONAL SUPPORTED WORK DEMONSTRATION

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MDRC

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The Impact of Supported Work On Ex-Addicts

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PREFACE

For the past five years, the Manpower Demonstration Research Corporation (MDRC), a private nonprofit corporation, has been engaged in managing the operations and research of the National Supported Work Demonstration, a multi-site program designed to test the effects of a structured work experience on four groups of individuals with severe employment disabilities: long-term female AFDC beneficiaries, ex-addicts, ex-offenders, and young school dropouts, many with criminal records. Supported work is primarily distinguished from other employment and training programs by its emphasis on three programmatic techniques designed to make participants comfortable with the world of work: peer group support, graduated stress, and close supervision. By supplying 12 to 18 months of stable employment and income to these individuals, and by gradually increasing performance and productivity standards on the job, supported work offers many participants their first real opportunity to develop two assets that should assist them in entering the regular labor market: good work habits and a history of stable employment.

Research into the demonstration's impact, benefits, and costs was conducted by Mathematica Policy Research, Inc. and the Institute for Research on Poverty at the University of Wisconsin. To obtain reliable answers, the design called for an experimental approach, in which eligible program applicants at 10 sites across the country were assigned at random to either an experimental or a control group. Those assigned to the experimental group were offered a supported work job, and individuals in both groups were interviewed regularly at nine-month intervals,

starting at their enrollment into the research sample and continuing for up to 36 months.

By comparing the behavior of individuals in the two groups, the research could determine whether participation in supported work resulted in any short- or long-term changes in employment, welfare dependency, criminal activities, or other measured activities. The research also included a careful assessment of the accuracy of the interview data and supplemental studies of work projects and program fiscal records to provide comprehensive information on the demonstration's benefits and costs.

This report is the fourth in a series of final reports from a comprehensive evaluation of all target groups. A summary version of the complete findings and experiences of the demonstration was issued early in 1980 by the Board of Directors of MDRC. Also available and listed at the conclusion of this document are numerous other reports on the demonstration's impact and on its implementation at the local sites.

Supported work was not uniformly effective with the four target populations. It proved surprisingly successful with the AFDC women -- leading to a long-term improvement in employment and earnings, and overall social benefits substantially in excess of costs. The results were less positive for ex-offenders and youths for whom measured benefits -- primarily limited to the in-program period -- fell short of costs. This report presents the findings for the ex-addict group: primarily males (83 percent), black or Hispanic (86 percent), with limited prior employment, a history of heroin use (94 percent) and arrest (90 percent), and an average of 2.5 years in prison. The long-term employment impacts

are ambiguous: ex-addicts enrolling during the demonstration's first year were favorably affected; later enrollees probably were not. Unfortunately, the detailed analysis presented in this report does not provide a simple explanation for this difference that could be used to retarget or redesign future programs to increase the probability of positive effects. Nevertheless, despite the lack of a consistent employment impact, the program did result in a sustained and substantial reduction in criminal activity, particularly in robbery and drug-related crimes. The companion benefit-cost study demonstrates how this persistent reduction, combined with other benefits, more than offsets program cost.

Based on the findings that supported work represents an effective program and an efficient investment of public funds, the MDRC Board of Directors, in its summary report on the demonstration, recommended immediate action to launch new and expanded supported work programs for ex-addicts, as well as AFDC women.

In a period when this country is simultaneously reassessing the utility of government-sponsored employment projects and voicing increased concern about crime in the streets, the ex-addict findings are particularly noteworthy. They suggest that transitional employment can be a successful tool for reducing crime, even though the nature of the relationship between the two remains unclear. Given the dearth of other success stories, these data from supported work deserve careful consideration.

Judith M. Gueron
Executive Vice President
MDRC

SUPPORTED WORK SITES

Location	Sponsoring Agency
Atlanta, Georgia	Atlanta Urban League-PREP
Chicago, Illinois	Options, Inc.
Cincinnati, Ohio **	Cincinnati Institute of Justice
Detroit, Michigan *	Supported Work Corporation
Hartford, Connecticut	The Maverick Corporation
Massachusetts (Boston area)	Transitional Employment Enterprises
New Jersey	
Atlantic City **	Atlantic County Vocational Services Center
Hackensack **	Bergen Supported Work Corporation
Jersey City	Community Help Corporation
Newark	Newark Service Corporation
Trenton **	Trenton Office of Employment and Training
New York, New York	Wildcat Service Corporation
Oakland (Alameda County), California	Peralta Service Corporation
Philadelphia, Pennsylvania	Impact Services Corporation
St. Louis, Missouri	St. Louis Housing Authority
San Francisco, California *	The San Francisco Phoenix Corporation
Washington State *	Pioneer Cooperative Affiliation
West Virginia (5 counties in northwest area of state)	Human Resource Development Foundation
Wisconsin	
Fond du Lac & Winnebago Counties	Advocap, Inc.
Ladysmith **	Indianhead Community Action Commission
Madison **	Community Action Commission for the County of Dane and the City of Madison, Inc.
Milwaukee **	Community Relations-Social Development Commission
Westby **	Coulee Region Community Action Agency
Whitehall **	Western Dairyland Economic Opportunity Council, Inc.

* Discontinued sites.
** New sites after fall 1978.

EXECUTIVE SUMMARY

Supported Work is a special work experience program intended to help groups of people with well-established employment difficulties obtain and keep a regular job. In addition to this major goal, other important objectives include reducing welfare dependence, drug use, and criminal activity.

Supported Work is specifically designed to be a temporary program. It provides individuals with employment for a limited time, after which they must leave the program, whether or not they have found jobs elsewhere. While they are enrolled, participants earn relatively low wages, but are given some opportunity to increase their earnings through bonuses and promotions for good performance and attendance. Support is provided through work assignments in crews of peers, and also through close supervision by technically qualified people who understand the work histories and personal backgrounds of their crew members and who will enforce gradually increased standards of attendance and performance until they resemble those of regular jobs.

The national Supported Work demonstration and its evaluation have been undertaken to assess the effectiveness of Supported Work in achieving its objectives. The four target groups that provide the focus for the demonstration are women who have been receiving welfare payments under the Aid to Families with Dependent Children (AFDC) program for substantial periods of time; ex-addicts who have recently been in drug-treatment programs; ex-offenders who have recently been released from prison or jail; and young school dropouts, many of whom have records of delinquency.

This report analyzes the impact of Supported Work on the ex-addict target group. It is based on a sample of 1433 ex-addicts in four sites--Chicago, Jersey City, Oakland, and Philadelphia. As these individuals applied for the program, approximately one-half were randomly assigned to an experimental group and were offered the opportunity to work in Supported Work. The remainder were assigned to a control group.

All those who went through this random assignment process were scheduled to be interviewed at the time the assignment took place and at subsequent 9-month intervals for up to three years. Those who enrolled earliest in the program were scheduled to be interviewed for 36 months following enrollment, but later enrollees were scheduled for only 27 months or 18 months of follow-up. Thus, conclusions concerning longer-term impacts of Supported Work are based on a relatively small sample and, thus, are not necessarily generalizable to the full sample.

A. EMPLOYMENT EFFECTS OF SUPPORTED WORK

During the first few months following enrollment, employment gains of experimentals were large because of their program jobs; however, these gains decreased sharply as experimentals left Supported Work. By months 16 to 18, when less than 10 percent of the experimentals were still in Supported Work, there was essentially no difference in the overall employment levels of the two groups. This similarity in experimentals' and controls' employment persisted over another 9-month period, after which those experimentals who received a 36-month interview exhibited a small but steady increase in their employment relative to that of their control-group counterparts. By the last 3-month period, 49 percent of these experimentals compared to 32 percent of the controls were employed, a difference which is statistically significant.

An important question is whether the upturn in results during the last half of the third year is representative of the results we would have observed had the full sample been followed for as long as 36 months. The analysis indicates that the program was more effective for the earliest enrollees in the 10 to 18 month period, in part because controls in that group suffered greater employment difficulties than did other controls. This is consistent with a pattern found for other subgroups: the program tended to be most effective for those with the fewest employment opportunities. It seems likely, therefore, that any post-program effects for the remainder of the sample would be smaller than those observed for the earliest enrollees, and perhaps non-existent.

Our overall conclusion is that under conditions such as those experienced by the earliest enrollees, Supported Work can be expected to have long-term employment impacts. However, these effects will tend to be much smaller if the programs operate during times when, or enroll individuals among whom, employment in the absence of the program experience will be relatively high.

B. EFFECTS OF THE PROGRAM ON DRUG USE

Supported Work did not have any significant influence on the ex-addicts' use of drugs. For this sample, the most important drug to consider is heroin--about 20 percent of both experimentals and controls reported having used it in the first nine months. The second most widely used drug (other than marijuana or alcohol) was cocaine--about 16 to 18 percent of both groups reported having used it. Use of marijuana was widespread among both experimentals and controls and persisted at high levels throughout the period of the study.

We also investigated the interrelationship between the experimental-control differences in drug use and employment, and found no significant differences. Based on these investigations, no clear relationship between employment and drug use was found, nor was there any indication that Supported Work would be more effective than other types of employment in reducing drug use among ex-addicts.

C. PROGRAM EFFECTS ON CRIME

Supported Work did have a strong effect on the criminal activity of the ex-addict group, as measured by contacts with the criminal justice system. In each 9-month follow-up period, smaller percentages of experimentals than controls were arrested, convicted, and incarcerated. These estimated differences were particularly large and statistically significant during the 10- to 18-month period when, only 13 percent of the experimentals as compared with 19 percent of the controls were arrested, and only 11 percent as compared with 16 percent were incarcerated. When the effects during each 9-month period are combined, the cumulative impact is even larger. Among those with at least 27 months of follow-up data, 43 percent of the controls compared with 32 percent of the experimentals reported having been arrested during the 27 months following their enrollment in the demonstration sample, and among the small sample with 36 months of follow-up data, 53 percent of the controls compared with only 35 percent of the experimentals reported having been arrested during the three years following enrollment. Furthermore, much of the change in arrest rates came from a reduction in arrests for robbery offenses, crimes that are typically associated with high costs to society.

CONCLUSION

Overall, it has been estimated that for the national Supported Work demonstration sample of ex-addicts, the program resulted in benefits to society that exceeded its costs by about $4,300 per participant.[1/] The size of this estimate depends in part on extrapolation of the post-program results, which varied among subgroups of the sample enrolled in the program at different times. Nonetheless, primarily because of the large benefits to society generated by reductions in ex-addicts' criminal behavior, we have concluded that, even under various alternative circumstances, Supported Work for ex-addicts is likely to be an efficient use of public resources.

[1/] See the companion report on the benefit-cost evaluation of the national Supported Work demonstration (Kemper, Long, and Thornton, forthcoming).

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CHAPTER I

THE SUPPORTED WORK DEMONSTRATION AND THE EX-ADDICT TARGET GROUP

Beginning in the mid-1970s, a national demonstration was undertaken to examine the success of Supported Work programs in helping groups with well-established employment difficulties obtain and keep a regular job. In addition, the demonstration was designed to test whether Supported Work was an effective means to reduce welfare dependence, drug use, and criminal activity. The four target groups that provide the focus for the demonstration are (1) women who have been receiving welfare payments under the Aid to Families with Dependent Children (AFDC) program for at least three years, (2) ex-addicts who have recently been in drug-treatment programs, (3) ex-offenders who have recently been released from prison or jail, and (4) young school dropouts, many of whom have records of delinquency.[1/] This report focuses on the effects of Supported Work on the ex-addict group.

A. THE DEMONSTRATION

The demonstration Supported Work programs provide individuals with employment in a supportive environment for a limited time (up to 12 or 18 months, depending on the site). Through this employment experience, Supported Work aims to prepare participants for transition to employment in the regular labor market. A supportive work environment is provided through work assignments in crews of peers and through close supervision

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[1/] The program also serves a small number of ex-alcoholics and former mental patients.

by technically qualified people who understand the work histories and personal backgrounds of their crew members. Standards of attendance and performance are gradually increased as the program proceeds, until they resemble those of regular jobs. While in the program, participants earn relatively low wages, but are given opportunities to increase their earnings through bonuses and promotions for good performance and attendance.

The work done by participants, most of it relatively unskilled, is varied. It includes clerical assignments, housing rehabilitation, building and ground maintenance, retail sales, and light manufacturing and is concentrated in the service and construction sectors. Goods and services are provided for a variety of customers, many of them in the public and private nonprofit sectors. In most of the projects, participants work under the close supervision of Supported Work program staff; however, some outside placements are made in which the day-to-day supervision is provided by the host agency.

The concept of Supported Work was implemented as a demonstration in 15 sites under the auspices of the Manpower Demonstration Research Corporation (MDRC). As part of the national demonstration, an experimental evaluation was conducted in ten sites, four of which enrolled ex-addict target-group members--Chicago, Jersey City, Oakland, and Philadelphia. In these evaluation sites, those who applied for the program between March 1975 and July 1977 and were found to be eligible were randomly assigned to either an experimental or control group. Those assigned to the experimental group were given the opportunity to participate in the program; those assigned to the control group were not. This opportunity

to participate is the only systematic difference between these two groups. The randomization process yielded a sample of 1,433 ex-addicts, roughly evenly divided between experimentals and controls.

The experimentals and controls were interviewed at the time they applied to the program and then reinterviewed at 9-month intervals for up to three years. All were scheduled to be interviewed 9 and 18 months following enrollment; those enrolled prior to January 1977 (a sample of 1,220) were also scheduled to be interviewed 27 months after their enrollment; and those enrolled prior to April 1976 (a sample of 472) were scheduled to be interviewed again 36 months following their enrollment.

This report compares the behavior of the experimentals and the controls based on data from these interviews. The first issue examined is whether experimentals are more likely to obtain employment and increase their earnings than are controls, and whether this additional income reduces welfare and other transfer payments. This report then examines whether experimentals are less likely to use drugs or to engage in criminal activity than are controls.

B. LOCAL LABOR MARKET CONDITIONS

Supported Work marks the first time an evaluation of a large-scale employment program with a randomly assigned control group has been conducted. The experimental design greatly facilitates evaluating the effects of the program. However, the demonstration is conducted within an uncontrolled environment.[1/] It is important that the reader be aware

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[1/] There is also uncontrolled assignment of participants to different program treatments, such as type of job or type of supervision. This report does not attempt to compare impacts for individuals assigned to various program treatments, due to problems of small cells and selectivity bias.

of these environmental factors and of their potential impact on the evaluation.

During the interviewing period of this study, national economic conditions improved. The national unemployment rate declined from 9.3 to 5.7 percent. Local unemployment rates for the various Supported Work sites also declined: in Chicago, from 7.0 to 5.1 percent; in Jersey City, from 13.0 to 12.1 percent; in Oakland, from 10.7 to 5.7 percent; and in Philadelphia, from 8.1 to 7.5 percent.[1/] It is not clear what effects these enhanced economic conditions may have had on experimental-control differentials since the possibility of finding a job in the regular labor market increased for both groups.

Another change in the environment that occurred during the course of the interviewing period was the introduction of the Special Unemployment Assistance (SUA) program. This short-term program (January 1975 through July 1978) extended unemployment compensation to individuals not covered by state Unemployment Insurance programs. As initially designed, participation in Supported Work would not have qualified workers for regular unemployment compensation benefits. However, depending on how local officials viewed Supported Work, workers could become eligible for the SUA benefits. As a result, many of the ex-addict participants (primarily in Jersey City) did receive this form of assistance immediately after leaving the program, thereby lowering their incentives to become re-employed quickly. If, however, experimentals used the period of SUA eligibility to search for better jobs, then the SUA program may have enhanced the long-run

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[1/] See various issues of the U.S. Department of Labor publication, Employment and Earnings. The data cited apply to the period between April 1975 and February 1979.

effects of Supported Work. Alternatively, if the experimentals remained out of the labor force for a prolonged period, the benefits from the Supported Work experience may have been lost.

Besides Supported Work, the sample members were potentially eligible for other programs. The Comprehensive Employment and Training Act (CETA) programs, in particular, were expanded during the evaluation. Thus, one might have expected a high percentage of controls to move into subsidized employment during the demonstration period. In fact, however, relatively few controls (fewer than 6 percent) reported having held CETA or WIN jobs during any of the 9-month periods for which data were collected; furthermore, there was no upward trend in such employment over time.[1/]

Thus, the estimated impact of Supported Work should be interpreted as taking place under these relatively poor but improving labor market conditions, with an inconsistent pattern across sites in unemployment compensation coverage, and a sizable public employment program (CETA). Because of the unplanned and uncontrolled nature of these variations in local factors that may affect the program's outcomes, it is not possible to estimate Supported Work's impacts under alternative situations. Nonetheless, subsequent discussions will relate program results to the prevailing conditions during a given time period or at a given site where a particular set of conditions prevailed, in order to provide as much insight as possible into the influence of those conditions on the observed results.

[1/] Even when other government jobs are added in, only 6 to 10 percent of controls reported having held such jobs in any of the 9-month observation periods and, again, there was no upward trend over time.

C. THE EX-ADDICT POPULATION

Over 200,000 persons in the United States were receiving treatment for drug abuse in April 1978.[1/] Such individuals generally suffer severe labor-market difficulties. The Drug Abuse Reporting Program (DARP) indicates that, in a representative sample of people entering federally funded treatment programs, over 40 percent had not worked at all in the preceding year.[2/] In addition to poor employment records and extensive involvement with drugs, over three-quarters of this population had spent some time in jail. Any of these factors may handicap the former addict in finding a job after leaving treatment.

Other studies of former drug users have found high recidivism rates for drug use and crime and persistent employment problems after the person leaves drug treatment. For example, of those in the DARP sample who were followed four to six years after entering treatment, 73 percent had returned to illicit drug use and 47 percent had been incarcerated at some time during that period. Forty-eight percent of the DARP sample was unemployed in the two months prior to the interview. Thus, treating addiction obviously is not sufficient to change a person's life-style.

Providing the former drug user with a job is frequently mentioned in the drug-use literature as an important step in reducing recidivism. Jaffe (1977) states, "The general consensus among clinicians is that legitimate work that provides reasonable income and gratification

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[1/] See U.S. Department of Health, Education and Welfare, National Institute on Drug Abuse (1978).

[2/] The Drug Abuse Reporting Program (DARP) sample is described in Simpson et al. (1976).

facilitates rehabilitation and reduces the likelihood of relapse." This widely held belief is apparently based on a comparison of those who are able to find work with those who are unwilling or unable to do so. However, such comparisons are subject to a potentially severe self-selectivity bias: those who are more committed to changing their life-style may be more likely to find employment. By virtue of the experimental design, Supported Work offers a unique opportunity to test the hypothesis that transitional employment can alter the ex-addict's subsequent employment, drug use, and criminal behavior.

D. ORGANIZATION OF THE REPORT

The next chapter describes the ex-addict sample and some of the characteristics of the Supported Work projects in which the experimentals participated. Chapter III reviews several mechanisms through which Supported Work is hypothesized to change participants' employment, drug use, and criminal behavior. Chapter IV discusses the effects of Supported Work on the earnings and employment of participants, and summarizes the program's effects on total income and on various income sources. Chapters V and VI discuss the program's effects on drug use and criminal activity, respectively. In the concluding chapter, the results are summarized and their implications discussed. Appendix A contains supplementary data; Appendix B presents details on the findings related to program impacts on total income and its sources, and other related impacts; Appendix C summarizes an analysis of the effects of nonresponse on estimates of program impacts; and Appendix D presents the results of an analysis of the effect of length of time spent in Supported Work on program impacts.

CHAPTER II

THE SAMPLE, DATA, AND PROGRAM EXPERIENCE OF PARTICIPANTS

A. THE SAMPLE

By design, the Supported Work sample of ex-addicts is not representative of all former drug users. The program was intended for those with severe labor-market difficulties; thus, eligibility standards were established to exclude those expected to have a reasonably good chance of finding employment on their own. The employment-history criteria, which were imposed on all target groups, specified that a person had to be currently unemployed (i.e., had worked less than 40 hours within the last four weeks) and had to have spent no more than three months in one regular job (a job of 20 hours or more a week and lasting as long as one month) during the preceding six months. In addition, to qualify for the ex-addict sample a person had to be 18 years of age or older and to have been in a drug-treatment program within the last six months. This latter requirement served as an objective indicator that the person had been addicted to drugs and that the involvement had been recent enough to create potential employment problems.

According to the answers provided to the baseline interview, 79 percent of the ex-addicts in the sample met all four eligibility requirements.[1/] Nearly 95 percent met each of the employment criteria, and all the sample members were at least 18 years of age. Although only 88 percent

[1/] Formal eligibility determination was the responsibility of Supported Work program operators. The assessment of eligibility reported above is based on responses to the baseline interview.

reported having been in drug treatment within the last six months, 95 percent reported having used drugs regularly.[1/] Thus, most of the ex-addict sample appears to have possessed characteristics that met the eligibility criteria.[2/]

Another factor influencing the nature of the sample was the referral process. There were very few walk-in applicants in the ex-addict sample; almost all ex-addicts were referred to Supported Work by official agencies, primarily drug-treatment agencies (75 percent), but also by manpower agencies (12 percent) and probation or parole officers (10 percent).[3/] However, only 15 percent of those enrolled in the demonstration reported that they had felt pressured to apply to Supported Work.[4/]

Tables II.1 and II.2 present selected characteristics of the Supported Work ex-addict sample.[5/] The sample is predominantly between the ages of 21 and 35, male, and black. Only 40 percent of the sample has

[1/] This figure excludes use of marijuana or alcohol. Regular use is defined as daily use for two months or longer.

[2/] For further analysis of compliance with eligibility requirements, see Jackson et al. (1978). We have investigated whether the program effects varied by whether the individual was eligible, and have concluded that they did not.

[3/] See MDRC (1978). These figures are based on program information rather than interview data and refer to ex-addicts in all sites rather than just in research sites.

[4/] Of those reporting pressure to apply to Supported Work, 50 percent reported being pressured by legal agencies and 25 percent by drug agencies. Even though the majority seems to have applied voluntarily, the effects of being pressured are examined later in the report.

[5/] In Appendix Table A.1, these characteristics are broken down by the duration of time between enrollment and the most recent follow-up interview completed.

TABLE II.1

PERCENTAGE DISTRIBUTION OF THE EX-ADDICT AND DARP SAMPLES
BY DEMOGRAPHIC, EMPLOYMENT, AND CRIMINAL CHARACTERISTICS

	Supported Work Ex-Addict Sample[a]	DARP Sample[b]
Years of Age		
Less than 21	7	30
21-25	39	33
26-35	42	16
More than 35	12	21
(Average Age)	(27.7)	n.a.
Sex		
Male	83	75
Female	17	25
Ethnicity		
Black (or other)	77[c]	46
Hispanic	9	18
White	14	35
Education		
High school diploma or equivalent	40	37
Years of education		
8 or less	14	n.a.
9-11	58	n.a.
12 or more	28	n.a.
(Average number of years)	(10.5)	n.a.
Marital Status		
Married	23	24
Other	77	76
At Least One Dependent	38	32
Employment Experience		
Worked in past year	51	42
(Average weeks worked in past year)	(10.1)	n.a.
Longest job ever held		
No job	5	10
1-12 months	40	45
More than 12 months	55	45
(Average dollar earnings in last 2 months for those who worked)	(589)	(612)
Received welfare last month[d]	40	n.a.
(Average dollar amount of welfare)	(78)	n.a.
Criminal History		
Number of arrests		
0	10	22
1	11	16
2-10	53	48
More than 10	26	14
(Average number of arrests)	(8.6)	n.a.
At least one conviction	76	55
(Average number of convictions)	(2.9)	n.a.
(Average number of weeks incarcerated)	(134)	n.a.
Number in Sample	1,154	27,460

NOTE: Unless otherwise specified, these data refer to the full sample. Numbers in parentheses are averages rather than percentages.

[a] The sample includes all individuals who are included in any of the analysis samples and the data come from enrollment interviews conducted by MPR staff.

[b] DARP is a representative sample of ex-addicts entering federally funded treatment programs. These data are from Simpson et al. (1976).

[c] Three sample members are from "other" ethnic/racial groups.

[d] Welfare includes AFDC, General Assistance, Supplemental Security Income and other welfare.

n.a. = not available.

TABLE II.2

PERCENTAGE DISTRIBUTION OF THE EX-ADDICT AND DARP SAMPLES
BY DRUG USE HISTORY

Characteristics	Supported Work Ex-Addict Sample[a]	DARP Sample[b]
Most Recent Treatment		
Methadone maintenance	53[c]	40
Drug free	25	21
Other	22	38
Number of Treatment Programs Ever Enrolled In		
None	6[d]	0
1	53	55
2	23	24
3 or more	18	21
(Average number of treatment programs)	(1.7)	n.a.
In Treatment at Time of Enrollment	76	n.a.
Types of Drugs Ever Used		
Heroin	94	n.a.
Other opiates	28	n.a.
Cocaine	67	n.a.
Barbiturates	37	n.a.
Amphetamines	32	n.a.
Psychedelics	26	n.a.
Marijuana	91	n.a.
Opiates only	20	n.a.
Other drugs only	3	n.a.
Both opiates and other drugs	75	n.a.
Length of Time Used Heroin		
Never used (or used less than a few times a month)	9	n.a.
Less than one year	11	n.a.
1-5 years	39	n.a.
More than 5 years	46	n.a.
(Average years of heroin use)	(6.3)	n.a.
Number of Times Previously Stopped Using Heroin (for those who ever used)		
None	11	n.a.
1-2	23	n.a.
3-4	20	n.a.
5 or more	46	n.a.
(Average for those who used)	(6)	n.a.
Number in Sample	1,154	27,460

NOTE: Unless otherwise specified, these data refer to the full sample. Numbers in parentheses are averages rather than percentages.

[a] The sample includes all individuals who are included in any of the analysis samples and the data come from enrollment interviews conducted by MPR staff.

[b] DARP is a representative sample of ex-addicts entering federally funded treatment programs. These data are from Simpson et al. (1976).

[c] The data for the Supported Work sample pertain to only those in treatment within the six months prior to enrolling in the demonstration. Twelve percent were not in treatment during this period.

[d] Prior enrollment in treatment was an eligibility requirement for ex-addicts. That some individuals reported never having been enrolled in drug treatment is due to some ineligible persons having been enrolled in the program or to reporting errors.

n.a. = not available.

the equivalent of a high school education. The typical Supported Work ex-addict exhibits a poor work history. Only 51 percent had worked at all in the year prior to enrollment and those who had worked, earned an average of $295 per month in the two months prior to enrollment. The ex-addicts also have a very extensive criminal record: 90 percent had at least one arrest; 76 percent had been convicted and, on average, sample members had spent over two-and-one-half years incarcerated.

The drug-use histories of the Supported Work ex-addicts are extensive (see Table II.2). Virtually all had had experience with opiates and nearly half had used heroin for more than five years. Three-quarters had used other types of drugs in addition to opiates. These data also show a pronounced pattern of recidivism to heroin use: 89 percent of those who had ever used heroin reported having stopped heroin use on at least one previous occasion; 46 percent of prior heroin users had stopped five or more times; and 41 percent had been in treatment more than once.

To examine how the Supported Work sample compares to a more general sample of ex-addicts, data from the Drug Abuse Reporting Program (DARP)--a representative sample of 27,500 people who were enrolled in federally funded drug-treatment programs between 1969 and 1972--are also presented in Tables II.1 and II.2.[1/] The Supported Work sample is similar to the DARP sample in terms of work history and educational background. The Supported Work ex-addict sample underrepresents both younger and older

[1/] Unfortunately, there is no information on lifetime drug use from the DARP sample to compare with that of the Supported Work sample. The DARP drug history pertains to the two months before treatment. Supported Work drug data are available on the month prior to assignment, but this period is after or during drug treatment.

treatment clients, as well as Hispanics, and whites. The Supported Work sample also exhibits more extensive criminal records than does the DARP sample.

It should be noted here that the Supported Work ex-addict sample shares many characteristics with the Supported Work ex-offender sample.[1/] The criminal history of the ex-addicts is nearly as extensive as that of the ex-offenders, although the ex-addicts have been incarcerated less recently than have ex-offenders. Similarly, over one-third of the ex-offenders have used drugs regularly, and approximately a quarter have been enrolled in a drug-treatment program at some time. However, these two groups came to Supported Work from quite different referral sources,[2/] and were enrolled at different sites. Also, as seen from comparing the results reported here with those reported for the ex-offender sample (see Piliavin and Gartner, 1981) the ex-addicts and ex-offenders responded differently to the Supported Work experience. Throughout this report, possible explanations for these target-group differences are noted.

B. RANDOM ASSIGNMENT AND THE DATA

1. Random Assignment

Determining the impact of Supported Work involves knowing what the behavior of participants would have been had they not participated in Supported Work. In most previous evaluations of employment and training

[1/] See Jackson et al. (1978) and Piliavin and Gartner (1981).

[2/] As previously noted, ex-addicts were referred primarily by drug-treatment programs. Ex-offenders were referred by criminal justice officials, manpower agencies, and came to the program on their own. (See MDRC, 1978.)

programs, this has been accomplished either by using a comparison group of nonparticipants who have characteristics similar to participants or by comparing the behavior of sample members before and after participating in the program. Both methods of assessing program impacts have serious shortcomings that can be overcome only by using a randomly selected control group. While not without risk and limitations, an experimental design was adopted for the national Supported Work demonstration, making it possible for researchers to estimate with a known degree of statistical confidence the impact of the Supported Work program.

Eligible applicants for the ex-addict target-group slots in four of the demonstration sites were randomly assigned to either an experimental or a control group. Members of the experimental group were offered a Supported Work job for up to 12 or 18 months, depending on the site; members of the control group were not. The random assignment process was successful in terms of its generating experimental and control groups with similar characteristics. At enrollment there were no significant differences between experimentals and controls in personal characteristics, employment histories, arrest histories or previous drug-use experience.[1/]

2. The Data

The data for this report are drawn primarily from responses of ex-addict sample members to in-person interviews that were administered at program entry (baseline interview) and at subsequent nine-month intervals, for up to 36 months after enrollment. Most of the interviews were

[1/] See Jackson et al. (1978) and Appendix Table A.19.

conducted in the MPR site office or the respondent's home; however, a few were conducted in prisons and over the telephone.[1/]

The length of follow-up of sample members was determined by the date of program entry. All members of both the experimental and control groups were scheduled to be interviewed by Mathematica Policy Research (MPR) staff at the time of their application for Supported Work to determine their demographic characteristics, their employment history, welfare dependence, drug use, and criminal justice experiences. They were then scheduled to be reinterviewed 9 and 18 months later to collect post-enrollment data on items such as employment, welfare dependence, drug use, and criminal activities. Because all interviewing was terminated in March 1979, only 85 percent of the sample (those enrolled prior to 1977) were scheduled to be interviewed again 27 months after their enrollment, and 33 percent (those enrolled prior to April 1976) were scheduled to be interviewed both 27 and 36 months after their enrollment.[2/] Appendix Table A.2 indicates the number of each type of interview assigned and completed.

Because of the differential length of follow-up among sample members and interview nonresponse, analysis of impacts for the various post-program periods have been based on different subgroups of enrollees: analysis of outcomes during the first 18 months following enrollment have

[1/] Prison and telephone interviews were abbreviated, omitting questions about current drug use and criminal activity. See Jackson et al. (1979) for further details of the field procedures.

[2/] This sampling strategy was undertaken to accommodate the gradual build-up of the program and to maximize statistical power of the analysis, within a fixed budget (see Ruth et al., 1980, or MDRC, 1980, for further discussion of the sample design). The enrollment of the sample by site and over calendar time is presented in Appendix Table A.22.

been based on those who completed an enrollment, a 9-month and an 18-month interview;[1] analysis of impacts for the 19- to 27-month period is based on data for those who completed an enrollment interview plus a 27-month interview, regardless of whether or not they completed the assigned 9- and 18-month interviews; and the analysis of 28- to 36-month outcomes relies on data for those who completed an enrollment and a 36-month interview (see Table II.3).[2]

In addition, particularly for analysis aimed at measuring program impacts on criminal recidivism, some analysis has been based on cumulative results over 18-, 27-, and 36-month periods. For these cumulative result analyses, the samples used consist of only those individuals who completed all scheduled interviews within the cumulative time periods: results for months 1 to 18 are based on those who completed a baseline, a 9-month, and an 18-month interview; those for the 1- to 27-month period are limited to the subset of the 1- to 18-month sample who also completed a 27-month interview, and results for the 1- to 36-month period are based on those sample members who completed the baseline and all four follow-up interviews. Thus, the samples used to estimate cumulative results over

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[1] Separate samples for the 1- to 9- and 10- to 18-month periods would have been slightly larger than that used. However, offsetting the advantages of larger samples were added complications of comparing results across time for somewhat different samples and higher computation costs.

[2] Analysis samples for the 19- to 27- and 28- to 36-month outcome measures were defined in this manner in order to maximize the number of usable observations, given the smaller sample sizes for the later follow-up periods.

TABLE II.3

MAXIMUM SAMPLE SIZES FOR THE ANALYSIS,
BY REFERENCE PERIOD OF THE OUTCOME MEASURE
EX-ADDICT SAMPLE

	a. Results for 9-month Periods[b]			b. Cumulative Results[c]		
	Months 1-18	Months 19-27	Months 28-36	Months 1-18	Months 1-27	Months 1-36
All Sample Members	974	885	311	974	729	240
Cohorts[a]						
18-month	230	n.a.	n.a.	230	n.a.	n.a.
27-month	490	596	n.a.	490	448	n.a.
36-month	254	289	311	254	241	240

NOTE: These figures include respondents to substitute interviews that were administered well after the scheduled interview to people who have not responded to a prior interview: 54 substitute 9-month interviews, 75 substitute 18-month interviews, and 16 substitute 27-month interviews are included in the totals. Actual sample sizes for analysis varied somewhat due to missing data for the outcome measures, but generally included 88 to 99 percent of the cases in these totals (see Appendix Table A.20). Most of the evaluation results are based on multivariate analysis that controls for preenrollment characteristics of experimental and control group members. Therefore, all analysis samples include only individuals who completed the enrollment (baseline) interview.

[a] Cohorts are defined by the latest completed follow-up interview. A few sample members were scheduled to receive subsequent interviews but failed to complete them. Also, recall that some individuals in the samples for analysis of 19- to 27- and 28- to 36-month outcomes did not complete a previously scheduled follow-up interview. Thus, row totals vary.

[b] Analysis of outcomes during the first 18 months after enrollment has been based only on individuals who, in addition to the enrollment interview, completed both the 9-month and 18-month follow-up interviews (referred to hereafter as the 18-month analysis sample). Analysis of outcomes during months 19-27 and months 28-36 are based on individuals who completed the 27- and 36-month interviews, respectively, regardless of what other follow-up interviews they completed. They are referred to hereafter as the 27-month analysis sample and the 36-month analysis sample.

[c] Analysis samples for cumulative results include only those individuals who completed interviews covering the full reference period. The 1- to 18-month sample is the same as that used for 9-month results; the 1- to 27-month sample includes only those who completed a 9-, an 18-, and a 27-month interview; and the 1- to 36-month sample includes only those who completed a 9-, an 18-, a 27-, and a 36-month interview.

n.a. means not applicable since a later follow-up interview was not assigned to this cohort.

the 1- to 27- and the 1- to 36-month periods are somewhat smaller than the samples used to analyze 9-month results for the 19- to 27- and the 28- to 36-month periods (see Table II.3).

An implication of the analysis samples is that those used for analysis of various post-enrollment periods are distinguished from one another by the date an individual enrolled in the program: only the earliest enrollees received the longer-term follow-up interviews. We refer to these subsamples followed for varying periods of time as the 18-, the 27- and the 36-month cohorts. As seen in Table II.3, results for the 1- to 18-month period reflect program results for members of all three sample cohorts; those for the 1- to 27- and the 19- to 27-month periods are based on data for only the 27- and 36-month cohorts; and finally, the results for the 1- to 36- and the 28- to 36-month periods are based on only the sample members in the 36-month cohort.

Thus, to the extent that individuals' characteristics, local labor-market conditions, and program characteristics varied across these enrollment periods, the estimates of longer-term results based on these particular subsamples may not be representative of those that actually occurred for the full sample. Because of this fact, care has been taken throughout the report to discuss the extent to which the results vary among the sample cohorts.

Another potentially serious problem for the analysis concerns the attrition of sample members scheduled to be given later interviews. There was a somewhat higher response rate to the 9-month interview for experimentals than controls (80 versus 75 percent). To the extent that respondents were not a randomly selected subset of the full ex-addict

sample, the inclusion of only post-baseline experiences of respondents may have biased the results of the evaluation. However, in a detailed analysis of the effects of nonresponse on selected outcome measures in various time periods, no significant biases were found (see Appendix C and Brown, 1979). We have therefore concluded that comparisons based on completed interviews generally yield unbiased estimates of the true effects for the full sample of ex-addicts enrolled in Supported Work.

The quality of interview data was assessed by comparing it with other data sources. A comparison of Social Security records of the ex-addicts with their self-reported earnings data shows that more earnings were reported in the interview than were reported to Social Security.[1/] This may be due either to the lack of complete coverage by the Social Security system[2/] or to errors in reporting. The Social Security records also showed a substantially smaller experimental-control difference than did the interview data, although the difference in the two estimates was not statistically significant. When data on arrests reported in the interviews were compared with official arrest records for a sample of respondents in California and Connecticut, there was some evidence of underreporting in the interviews but no significant experimental-control difference in the extent of underreporting was found.[3/] No official records data exist for drug use, but some comparisons of reported use for

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[1/] See Masters (1979).

[2/] The difference was not due to uncovered earnings in the public sector because the difference persisted even when these earnings were subtracted from the interview data.

[3/] See Schore et al. (1979).

identical periods across interviews were made, which turned up no evidence that reported use during any 9-month period was differentially reported by experimentals and controls.[1/]

C. PROGRAM EXPERIENCE OF THE EXPERIMENTALS

The process of implementing Supported Work and the nature of the programs in which the experimentals participated are described in detail in other reports.[2/] This section presents only a brief description of some of the more important features of the program experience.

The evaluation component of the demonstration focuses on 10 of the demonstration sites; four of these sites selected ex-addicts as one of their target groups. The program is implemented at the local level by independent agencies whose major function, in most cases, is running the Supported Work program. The local organizations receive some of their funding from Manpower Demonstration Research Corporation (MDRC), but they must also obtain revenue from local funding and from marketing their output. In implementing Supported Work, the local organizations must conform to national guidelines and are monitored by MDRC field staff. However, much local flexibility is allowed to meet local needs and circumstances and to conform with philosophies of the local program directors. As a result, the program experience of experimentals varied by

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[1/] See Dickinson (1979a). The comparisons did suggest that in the 9-month interview, experimentals were less candid about their drug use for periods when respondents had specific contact with the interviewers. This tendency was evident only in the 9-month interview, however, and affected the reported timing of use and not the percentage of the sample using drugs.

[2/] See, for example, MDRC (1978), Ball (1977), and MDRC (1980).

site. Table II.4 presents some key characteristics of the programs in which ex-addicts were enrolled.

The initial wage rates paid to ex-addicts in Supported Work were set equal to approximately 78 percent of the wage rate that participants might be expected to earn on a regular job.[1/] This reference wage was estimated from poverty-area wage data from the 1970 census and from Bureau of Labor Statistics data on wage changes over time.[2/] Longevity and cost-of-living adjustments were made, although the extent of adjustments varied by site. The intent was to limit such increases so that by the time the participant left Supported Work, he or she would be earning slightly less than the reference wage rate.

Most programs attempted to create jobs for participants that were labor-intensive and relatively low-skilled.[3/] Over half the work done by ex-addicts involved construction jobs. In Chicago and Oakland the participants painted residences in low-income neighborhoods, and in Jersey City and Philadelphia they did extensive restoration of such buildings. Very little time was spent in higher-skilled jobs such as carpentry or masonry. Work in service industries (such as a food service and delivery project in Jersey City) accounted for approximately 20 percent of the work performed by the ex-addicts. Most of the work was for public or private nonprofit agencies, but there were some exceptions. The Chicago and

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[1/] For ex-addicts these wage rates always exceeded the minimum wage.

[2/] See Hollister et al. (1974) for a detailed description of how the wage rates were determined.

[3/] See MDRC (1978).

TABLE II.4

SELECTED CHARACTERISTICS OF PROGRAMS ENROLLING EX-ADDICTS

	Program Location			
	Chicago	Jersey City	Oakland	Philadelphia
Program Hourly Wage Rate as of January 1977[a]	2.60-3.00	2.68-2.83	2.63-3.02	2.30-2.65
Other Main Target Groups Served	AFDC, Ex-offender	Ex-offender, Youth	AFDC, Ex-offender	Ex-offender, Youth
Program Size[b] (number of slots)	184	303	109	99
Average Crew Size[c]	4.9	6.7	4.2	7.2
Percentage of Ex-Addicts in Various Types of Jobs[d]				
Construction	63%	45%	57%	62%
Business services	27	24	8	19
Other	10	31	35	19
Maximum Length of Program Participation	12 months	12 months	12 months	18 months

[a]/Source: MDRC (1978), Table VI-11. Program wages were always above the prevailing minimum wage.

[b]/Source: MDRC (1978), Table II-1. The program size figures are for June 1977.

[c]/Source: MDRC (1978), Table IV-2.

[d]/These averages are for the second contract year. They represent the percentage of project hours in these industries. Source: Special MIS Report, July 1977.

Oakland programs, for example, arranged with private firms to subcontract some manufacturing, packaging, and assembly operations.

Peer support was generally implemented by placing workers in small crews. All sites served target groups other than ex-addicts, and it was common to mix the target groups within the crews. Thus, the concept of peer group was defined broadly as those with poor work histories rather than narrowly as those with drug-use problems.[1/]

The implementation of graduated stress varied across sites.[2/] The intention was to establish modest performance standards when the person began participation in the program and then to increase them until, at graduation, they were similar to market standards. Attendance rates and work loads were major components of these standards. In the Jersey City housing rehabilitation projects, new enrollees were assigned to general clean-up crews and were then moved up to more demanding tasks as their work behavior improved. In Chicago, workers who were successful in the early phases were moved out of the crew environment and placed into jobs where they worked alone. Often, graduated stress was implemented less formally and took the form of supervisors simply increasing work standards for a participant within the same job. Each site also made provisions to reward good performance through some type of bonus system. Both the Chicago and Jersey City programs also had transition bonus policies, whereby individuals who were successful (either on their own or with Supported

[1/]Many of the ex-offenders and a few of the youth also had extensive drug-use histories. However, almost none of the AFDC group reported any prior drug use.

[2/]See MDRC (1978).

Work's assistance) in finding a nonprogram job after leaving Supported Work received a bonus.

Although the demonstration's guidelines allowed programs to provide a limited amount of formal training, job-search counseling, and other ancillary services, relatively little time was spent on these activities, but the workers did receive some on-the-job training in job-specific skills. In the Jersey City construction jobs, for example, crews were rotated among the various jobs required by the projects.

Participants were required to leave Supported Work after a fixed period. In all sites except Philadelphia, the maximum length of program participation was 12 months, and in Philadelphia, it was 18 months. In fact, as can be seen from Table II.5, the ex-addicts enrolled in the four research sites stayed in Supported Work for substantially shorter periods of time than allowed under program guidelines. The average length of stay was only 6.7 months, which is over one month less than has been estimated for participation in CETA public service employment (see MDRC, 1978). It should be noted, however, that the ex-addict sample tended to be considerably more disadvantaged than the average CETA enrollee.

Overall, 14 percent of the participants in the analysis sample reported having left Supported Work to take another job, and 42 percent reported terminating for negative reasons such as firing, incarceration, and quitting because of dissatisfaction with the program.[1/] The

[1/] These figures on types of terminations, which were generated from interview data, show a lower percentage of both positive and negative terminations and a higher percentage of neutral terminations than those reported in the Supported Work demonstration's Management Information System (MDRC, 1978 and MDRC, 1980). Explanations for these discrepancies include differences in the time periods and samples covered, as well as

TABLE II.5

AVERAGE LENGTH OF STAY IN SUPPORTED WORK AND REASONS FOR TERMINATION
EX-ADDICT EXPERIMENTAL SAMPLE

	A. Site				
	Chicago	Jersey City	Oakland	Philadelphia	Total
Average Number of Months in Supported Work	6.8	8.3	5.0	5.0	6.7
Percentage Who Left Supported Work:					
Because they exhausted allowable time in program[a/]	13.7	33.1	3.2	1.2	17.6
To take another job or to enroll in school or training	21.9	10.3	22.6	10.8	14.2
Because of poor performance[b/]	37.0	31.6	48.4	61.4	42.1
For other reasons[c/]	27.4	25.0	25.8	26.5	26.0

	B. Amount of Follow-up Data[d/]			
	18 Months	27 Months	36 Months	Total
Average Number of Months in Supported Work	6.2	6.4	7.8	6.7
Percentage Who Left Supported Work:				
Because they exhausted allowable time in program[a/]	8.4	20.1	24.4	17.6
To take another job or to enroll in school or training	18.1	14.1	7.3	14.2
Because of poor performance[b/]	43.4	42.7	36.6	42.1
For other reasons[c/]	30.1	23.1	31.7	26.0

NOTE: For definition of samples see Table II.3.

[a/] This includes individuals not leaving Supported Work to take another job, to enroll in school or job training, or because of poor performance, but who either spent the maximum number of months in the program or exceeded the maximum calendar time for participation.

[b/] This category includes those terminated because of conflicts with the boss or crew members, use of drugs or alcohol, illegal activities or incarceration, absenteeism, poor punctuality, or low productivity.

[c/] This includes reasons such as low pay and health, and child-care or transportation problems.

[d/] The samples for the various follow-up categories include individuals with the designated interview and all prior interviews.

probability of a negative termination was highest during the first two months of enrollment in the program. Although it is clear that some participants had trouble working in the program, it should be noted that 45 percent of the sample had never before worked on a job that lasted longer than a year.

Within the Supported Work ex-addict sample, the length of stay in Supported Work and the reasons for terminations varied considerably according to both site and the date individuals enrolled. Those enrolled in Jersey City stayed in Supported Work considerably longer than average (8.3 months), and those in Oakland and Philadelphia stayed substantially less time than average (only 5 months). Over one-fifth of those enrolled in Chicago and Oakland left to work at another job, while only about 10 percent of those in Jersey City and Philadelphia left for such a positive reason.

The length of stay in the program tended to decrease with calendar time, as evidenced by the fact that those enrolled prior to April 1976 (the 36-month cohort) stayed 7.8 months, on average; those enrolled between April 1976 and January 1977 (the 27-month cohort) stayed 6.4 months, on average; and those enrolled in 1977 (the 18-month cohort) stayed only 6.2 months, on average. Furthermore, the reasons for termination varied considerably by cohort. Over one-fifth of those in the 27- and 36-month cohorts exhausted their allowable time in the program and, particularly

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unavoidable differences in the actual definition of categories. Furthermore, the MIS data are based upon program operators' classifications of reasons as opposed to those of participants, and these two groups may have different interpretations of the reasons for a departure.

among the 36-month cohort, positive terminations were lower than average (7.3 percent).[1/]

Three factors are associated with the duration of time spent in Supported Work: being white, having recent employment experience, and receiving welfare at enrollment are associated with relatively early termination from the program. A common characteristic of short-term participants is that they are likely to have better employment alternatives or have more sources of non-labor income than other ex-addicts.[2/]

An analysis of the effect of length of stay on subsequent labor market performance is presented in Appendix D. The results indicate that increasing the duration of participation in Supported Work, per se, will not affect Supported Work's effects on post-program employment.

D. PARTICIPANTS' ASSESSMENTS OF SUPPORTED WORK

Although our impact results are based on experimental-control differences in behavior as reflected in the interview data, we also asked the participants themselves to evaluate how Supported Work affected them. Table II.6 presents participants' subjective responses to several questions about program experiences--in particular, whether they felt Supported Work was different from other jobs, whether it had prepared them for regular employment, what the most important aspects of the Supported Work experience had been, and what complaints they had about the program.

[1/] In part this was due to the programs' having not fully developed and implemented their job-placement components in the earlier period.

[2/] These results are based on the analysis of the correlates of and consequences of varying the length of stay in Supported Work, presented in Appendix D.

TABLE II.6

PERCENTAGE OF EXPERIMENTALS REPORTING VARIOUS ASSESSMENTS OF SUPPORTED WORK
EX-ADDICT SAMPLE

	Months in Supported Work			
	Less than 3	3 to 11	12 or more	Total
Supported Work Different From Other Jobs	81.2	74.8	67.4	74.1
Supported Work Prepared Him/Her to Obtain Regular Job	14.8	43.9	53.7	41.8
Prepared Him/Her by:[a/b]				
Teaching job skills, trade	55.6	48.5	60.8	52.8
Improving habits and attitudes	44.4	44.4	25.5	38.4
Other	0.0	28.7	21.6	25.0
Most Important Result from Working in Supported Work[a]				
Learning job skills, trade	5.7	20.2	25.3	19.4
Developing better work habits and attitudes	15.1	14.9	17.9	15.7
Having a steady job and income	15.1	17.2	17.9	17.1
Developing self-confidence, self-esteem	13.2	16.2	13.7	15.2
Staying out of trouble and/or off drugs	5.7	11.0	4.2	8.5
Other things	24.5	24.1	25.3	24.5
Nothing	39.6	27.6	22.1	27.9
There Were Things He/She Did Not Like About Supported Work	65.0	59.0	46.3	56.9
Did Not Like:[a/c]				
How program was run	27.3	35.4	25.0	31.9
Low pay	36.4	31.5	36.4	33.3
Other complaints	51.3	44.4	45.5	45.8
Number in Sample	61	246	95	402

NOTE: The sample includes those experimentals who completed at least a baseline, a 9-month, and an 18-month interview and who left Supported Work prior to completing their last follow-up interview. Except where noted, percentages are based on the total sample.

[a] Percentages may sum to more than 100 because multiple responses were allowed.

[b] Figures include only those who said Supported Work prepared them to obtain a regular job.

[c] Figures include only those who said they did not like Supported Work.

About three-quarters of the ex-addict sample felt Supported Work was different from prior jobs primarily because the work itself was different. Over half the sample was willing to air complaints about the program. About one-third of those thought the program was poorly run, and another third thought the pay was too low. Not surprisingly, the longer a person stayed in the program, the more likely he or she was to say that it prepared him or her for regular employment, usually because the program had taught new job skills.

When the respondents were asked what they felt was the most important thing that happened to them as a result of working in Supported Work, a substantial number could not report any important result. However, 20 percent reported that learning skills was the most important result of program participation. Some of the answers reflected the unique features that Supported Work was designed to provide: 16 percent reported that participation in the program helped them develop better work habits, and 15 percent reported that it increased their self-confidence and self-esteem.

Before we present the results of the evaluation of Supported Work, we discuss in the next chapter several hypotheses concerning the effects of the program on the employment and earnings, drug use, and criminal activity of ex-addicts.

CHAPTER III

RESEARCH HYPOTHESES AND ANALYTIC METHODOLOGIES

The primary purpose of Supported Work is to improve the earnings and employment of participants. It is also hoped that by increasing their employment options, participation in Supported Work will make it easier for ex-addicts to resist pressures to return to drug use. If the program does increase employability and/or reduce drug use, then it may also reduce participants' involvement in criminal activity. In general, the purpose of the program is to facilitate the transition from a life-style of drug abuse and detachment from the labor force to a life-style in which regular employment is important. This chapter first discusses various theories of employability, drug use, and criminal activity, and suggests specific hypotheses about the effects of Supported Work. It then reviews the previous empirical studies related to these hypotheses. Finally, it describes the methods used to assess the impacts of Supported Work.

A. EMPLOYMENT AND EARNINGS HYPOTHESES

One mechanism by which Supported Work is hypothesized to increase participants' wage rates is by increasing the number of hours they work. According to human capital theory, workers are paid the value of their marginal product, so that the cause of low wage rates is low productivity.[1/] Supported Work is hypothesized to increase participants' productivity, and thus their wage rates, by developing good basic work

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[1/] See Becker (1964) and Mincer (1974) for discussions of various aspects of human capital theory.

habits such as regular attendance, the ability to work in groups, and the ability to take directions from supervisors.

If Supported Work does improve these skills, it may be particularly appropriate for ex-addicts. Not only do ex-addicts tend to have low levels of education and training, their extensive involvement with drugs may have fostered the poor work habits that Supported Work is designed to improve. The work attendance of those addicted to drugs may have been erratic, and thus former drug users may not have acquired the necessary "human capital" during their previous work.[1/]

An alternative view of the labor market, provided by the segmented market theory, suggests that some workers have low wage rates because employers are reluctant to hire them into the better-paying jobs.[2/] Employers take risks when hiring a new employee into these jobs. They must often invest in training the new worker, and they must integrate the new worker into the existing work force. According to this theory, employers try to reduce these risks by looking for signals that the person is trainable and can work with others.

The members of the ex-addict target group generally possess poor credentials. They have poor work histories, extensive criminal records, and histories of drug abuse, all of which may signal to potential employers that they are poor risks. Supported Work can aid such people by providing credentials to counteract the effects of their pre-enrollment records.

[1/] For example, if a person needed to use drugs during work hours, he or she may have had to leave work to do so; if drugs were temporarily unavailable, the person may have needed to spend time obtaining them and may not have shown up for work.

[2/] See Doeringer and Piore (1971) and Thurow (1975).

Thus, by certifying that participants have been able to work in crews and to maintain acceptable work patterns, Supported Work is hypothesized to increase their access to better-paying jobs.

Another mechanism by which Supported Work may increase participants' earnings is that of increasing their hours of work. In the year prior to enrollment, ex-addicts in the sample worked an average of less than ten weeks. It is possible that they may have chosen to work so few hours because of their wage rates, alternative sources of income, or the value placed on alternative uses of their time. Many of the ex-addicts have extensive criminal records and could, possibly, obtain more money through illegal activities than through legitimate work. Nearly 40 percent received welfare in the month prior to enrollment, which provided them with an alternative source of income and reduced their net wage rate from earned income. While they were using drugs, these individuals may also have placed a relatively high value on leisure time.

For ex-addicts who were voluntarily unemployed, Supported Work is hypothesized to have increased their work hours by increasing their wage rate. Such an increase could lead people to shift from illegal work and reduce their leisure time. Because the welfare system lowers the net wage rate, the positive effect on hours worked is hypothesized to be smaller for those receiving welfare.

The segmented market theory also indicates that low-paying jobs are inherently unstable, so that the people in the so-called secondary (unskilled) segment of the labor market frequently find themselves out of work involuntarily. If participation in Supported Work provides signals to potential employers that participants can work in permanent jobs, then the program can increase participants' access to more stable employment.

In summary, to the extent that Supported Work increases the productivity of participants, it should lead to increases in participants' wage rates and, in turn, the number of hours they work. To the extent that Supported Work provides credentials to counteract the effects of participants' previous work records, it may increase participants' access to better-paying and more stable jobs.

B. DRUG-USE HYPOTHESES

Theories of drug use have been developed in several disciplines, including psychology, sociology, and economics. Psychological learning theories of drug use emphasize both the positive and the negative reinforcement characteristics of opiate use. Those that emphasize positive reinforcement explain the recidivism to opiate use in terms of the pleasure of drugs.[1/] After prolonged use, the "drive" for opiates becomes a permanent alteration in the addict's preferences and is as strong as the natural drive for food. Extreme versions of these positive reinforcement theories would lead us to predict that Supported Work would not be effective in reducing drug use.

Theories that emphasize negative reinforcement stress the role of withdrawal symptoms in conditioning the addict's behavior.[2/] Several effects are noted: (1) the activity of obtaining drugs is repeatedly reinforced by the reduction of pain; (2) after the addict has repeatedly experienced the withdrawal distress in a specific environment, that environment itself begins to elicit withdrawal symptoms, even after

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[1/] See Ausubel (1964) and Bejerot (1972).

[2/] See Lindesmith (1947), and Akers, Burgess, and Johnson (1968).

physical dependence is gone; and (3) the avoidance response may generalize, leading the addict to use drugs as an escape from other types of distress, especially distress associated with deprivation.

These negative-reinforcement theories suggest several mechanisms by which Supported Work may reduce opiate use. By placing participants in a work environment, Supported Work reduces the participant's contact with his or her previous environment. It provides reinforcement for the activity of working to counteract the past reinforcement for the activity of hustling for drugs. Further, by providing additional income, Supported Work may lessen the deprivation distress that may lead to drug use.

There are several sociological theories which attempt to explain drug use.[1/] The subculture-difference theory argues that access to legitimate means of satisfaction is distributed unequally within society so that, for some segments of society, the probability of obtaining satisfaction through work is lower than the probability of obtaining satisfaction through deviant activity such as drug use. This differential probability leads the subculture to devalue work relative to drug use. The social control theory suggests that the expected payoff from deviant behavior is almost always greater than that expected from conforming behavior. According to this theory, it is the cultural commitment to conformity that is distributed unequally. In contrast, the labeling and self-concept theories of deviance postulate that belonging to a deviant subculture is not sufficient to induce drug use. Having a self-concept as a person who uses drugs is also needed. A person's self-concept is formed

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[1/] See Harris (1977) for a review of the sociological theories of deviance.

through interaction with others, particularly those who are in a position of authority. If a person has been labeled by authorities as a drug user and if this self-concept is reinforced by peers, it is likely the person will return to drug use even after having been treated for addiction.

Supported Work can change several parameters of these sociological models. It can change the expected payoff from work by increasing a person's wage rate and by increasing the probability of obtaining a job. Through the process of peer support for work, Supported Work can change the person's perception of the value of work. And by giving the participant the opportunity to be viewed as employed by those in authority, it may change the person's self-concept.

The economic theory of drug use assumes that a person's preferences for work and drug use are given and concentrates on how changes in the person's opportunity structure affect his or her behavior. As was discussed above, Supported Work may increase participants' wage rates, thus making time-consuming activities relatively more "expensive."[1/] Opiate use can be very time-consuming, since opiates are sedatives which may make a person drowsy and lethargic for several hours. If the person is addicted, the euphoric effects may not be present after each injection, in which case the person must use the drug at more frequent intervals.[2/] Since the drug is illegal, its use must be concealed, and efforts to conceal it may interrupt other activities such as employment. Since

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[1/] Becker (1965) discusses the implications of time-intensive commodities.

[2/] Lindesmith (1947) argues that the euphoric effects cannot be attained at all after the person has become addicted.

opiate use is time-intensive, increasing a person's wage rate is hypothesized to lead him or her to reduce the use of drugs. This should be possible for those who are not currently addicted, but would be less possible for those who are currently addicted to opiates.

Supported Work participants are also hypothesized to have higher incomes than are controls. For the person currently addicted, it is possible that a substantial proportion of the extra income will be spent on drugs.[1/] For the person not currently addicted, the income effect on drug use is hypothesized to be smaller.[2/]

Thus, based on economic theories, the net effect of Supported Work is hypothesized to depend on whether the person is addicted to drugs at the time he or she applies to Supported Work. If someone is addicted to a drug, the substitution effect is likely to be weak and the income effect is likely to lead to increased drug use. If someone is not addicted currently, the direction of the response is ambiguous: higher wage rates may lead to a substitution away from drug use, but the increase in income may lead to larger drug purchases.

[1/] If one were not previously using enough heroin to prevent withdrawal symptoms entirely, then almost surely much of the increased income would be spent on drugs. If one were consuming enough to prevent withdrawal distress, one might find that the increased income would allow one to consume enough to attain the euphoric effects of heroin, although the ability of addicts to attain these effects is disputed. (McAuliffe and Gordon, 1972, review this dispute.)

[2/] For some, drugs may be "inferior" goods so that the amount consumed decreases as income increases. This is less likely for participants in the ex-addict group who have developed a strong preference for drugs than for those in the youth group who may find drug use an inferior means of obtaining satisfaction.

In summary, on the one hand, Supported Work is hypothesized to reduce drug use through several mechanisms. To the extent that work performance is affected by drug use, increasing the ex-addict's employment opportunities makes it more expensive to use drugs. By placing the ex-addict in a work environment, the program reduces the participant's contact with his or her past surroundings and may change the person's self-image from addict to worker. The peer support for work may counterbalance the influence of a subculture in which work is devalued. The additional income from the program may also reduce deprivation distress to which drugs may have become a generalized response. On the other hand, Supported Work may tend to increase drug use to the extent that participants may spend some of their additional income on drugs, particularly if they have become re-addicted.

C. CRIME HYPOTHESES

As noted, the ex-addicts generally have very extensive criminal records, almost as extensive as the ex-offenders in the Supported Work sample. The association between criminal activity and drug use is well established, but the causal link is greatly debated.[1/] The view that drug use leads to crime is based on economic considerations: drug users generally earn little through legal employment and thus may turn to crime as a means of economic support. The main alternative view is that there is no causal relationship between crime and drug use, but that both behaviors are manifestations of deviant life-styles.

[1/] See Greenberg and Adler (1974) for a review of the literature on the relationship between crime and drugs.

The economic theory of crime suggests that a person weighs the relative payoffs of legal and illegal work in deciding how to obtain income.[1/] The payoff to legal work is the person's wage rate; the payoff to illegal activity is the proceeds from the crime if the person does not get caught, counterbalanced by the expected punishment if the person does get caught. Whether the person engages in crime depends both on the person's legal wage rate relative to the criminal opportunities and on the person's willingness to take risks. Drug users may be more willing to take risks if they have insufficient earnings to purchase the quantity of drugs necessary to prevent withdrawal symptoms.

In this model, Supported Work is hypothesized to reduce criminal activity through several mechanisms. By increasing participants' employment opportunities and wage rates, individuals may switch from illegal activity to legal work. By increasing the income of participants, they may be less willing to take the risks associated with illegal activity. In addition, if the program leads to reduced drug use, then the economic necessity for crime and the person's willingness to take risks may be reduced.

Sociological theories of criminal activity are similar to the sociological theories of drug use discussed above. Many of these theories stress the importance of peer influence and subculture membership in the motivation for crime. Whether these theories suggest that Supported Work would reduce the criminal activity of ex-addicts depends on the nature of the ex-addict's peer-group associations. Cloward and Ohlin (1960)

[1/] See Becker (1968) and Block and Heineke (1975).

characterize drug addicts as "retreatists" from both conforming and deviant subcultures. If this is the case, the peer support for work provided by Supported Work might be expected to have little effect on the crime of ex-addicts.

This view has been challenged by Preble and Casey (1969), according to whom drug use is a symptom of substantial involvement in a deviant subculture. If this view is correct, then the peer support offered by Supported Work may offset the peer support for drug use and crime offered by the addict subculture.

In summary, Supported Work is hypothesized to reduce economically motivated criminal activity by increasing participants' wage rates and income. To the extent that the program also reduces drug use, it is hypothesized also to reduce crime. If ex-addicts have been heavily involved in a deviant subculture, as some theories suggest, then the peer support for work may be particularly effective in reducing crime among ex-addicts.

D. REVIEW OF EMPIRICAL EVIDENCE FOR HYPOTHESES

Although training programs and sheltered workshops are frequently recommended as useful tools in the rehabilitation of addicts, very little empirical work exists on the effects of such programs on former drug users.[1/] Several follow-up studies of people who were in drug treatment have found that those who are employed after treatment are less likely to

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[1/] See, for example, Jacks (1973), Lamb and Mackota (1973), and Danaceau (1973).

return to drug use or to engage in crime.[1/] However, the direction of causation is unclear in these studies. It may be that those who want to reduce drug use and crime are more likely to obtain employment.

Two experimental evaluations of employment programs for ex-addicts --Wildcat (the prototype for the national Supported Work demonstration) and TREAT--are worth reviewing briefly.

The Wildcat program included graduated stress and peer support for workers but differed from the national demonstration programs in that there was no mandatory graduation. The evaluation of Wildcat had an experimental design in that individuals were randomly assigned to an experimental or control group. However, experimentals who did not show up for the program and those experimentals who were ineligible according to the program criteria were excluded from the evaluation sample. Furthermore, as a result of there being no mandatory graduation policy, 23 percent of the eligible ex-addicts were still working in Wildcat jobs at the time of the third-year follow-up report on the program.

Based on a comparison of 197 experimentals and 207 controls in the third year of the program, experimentals worked an average of 26 weeks and earned an average of $3,596, while controls worked an average of 17 weeks and earned an average of $1,951.[2/] There was also a significant reduction in arrests among the experimentals in the first program year, but no significant difference between experimentals and controls by the third year. No significant effects on drug use were observed for any period.

[1/] See Platt and Labate (1976), Stephens and Cottrell (1972) and Duvall et al. (1963).

[2/] See Friedman (1978).

As mentioned above, these results must be qualified by the fact that many experimentals were still in the program and by the fact that no-shows were excluded from the analysis. The exclusion of no-shows would tend to overstate the effectiveness of the program to the extent that they were the most likely to return to drug use and/or to engage in crime.

The TREAT program provided employment and training for six to nine months for a sample of ex-addicts in Washington, D.C. The reported employment and earnings results, which include experience while in the program, indicate significant short-run gains in emloyment rates and hours of work, although, surprisingly, not in earnings. There were also significant reductions in drug use among participants but no significant effects on arrests. Longer term effects have not been assessed. Again, in interpreting these results, it should be noted that although a random control group was chosen, the experimentals who dropped out early were "replaced" and not included in the experimental-control comparisons.[1/] As in the Wildcat case, to the extent that early dropouts are more likely to return to drug use and/or to engage in crime, program effectiveness will be overstated by the results.

Several employment and training programs for ex-offenders have been evaluated, and the results have some relevance since most of the ex-addicts in our sample have criminal records. These programs have provided various combinations of vocational training, employment experience, and job placement. The estimated effects of these programs on either employment or crime have generally been small. Taggart (1972) concludes, "It does

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[1/] Seventeen percent of the experimentals were replaced (see Bass and Woodward, 1978).

not seem likely that the employment problems of ex-offenders can be significantly alleviated by manpower programs, or that these programs will have a noticeable impact on the rate of crime."

Another policy for ex-offenders that has been evaluated is the provision of income maintenance immediately after release from prison.[1/] An income maintenance program differs in many respects from Suported Work, but both programs are expected to increase the income of participants, which may, in turn, reduce crime. The LIFE program provided income support and job-placement services to a sample of ex-offenders who had a high risk of recidivism, but who had no drug-use history.[2/] Those who received financial support showed a significant decrease in arrests and a significant increase in employment. The job-placement efforts, however, had no observed effect. Two other demonstration programs based on the LIFE model--one in Texas and one in Georgia--offered various combinations of income support, job training, and job-search assistance to a broader group of ex-offenders, but they have not shown similarly favorable results.[3/]

In summary, the nonexperimental literature suggests that employment is associated with lower rates of recidivism to drugs and to crime, but such results are subject to potential self-selectivity biases. Two experimental evaluations of employment programs for ex-addicts report modest effects, but neither study maintained strict random assignment. Evaluations of employment and training programs for ex-offenders have not

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[1/] The effects of such a policy may be particularly relevant for ex-addicts since many of them are eligible to receive welfare.

[2/] Mallar and Thornton (1978).

[3/] See Stephens and Sanders (1978) and Smith et al. (1978).

found such programs to be successful: those income-support programs which did have significant effects on crime and employment specifically excluded ex-addicts.

Despite theoretical reasons to expect Supported Work to increase earnings and to reduce drug use and the criminal activity of the ex-addicts, the empirical evidence that exists does not provide strong support for other hypotheses. The evidence that pertains directly to ex-addicts is sparse. Furthermore, with the exception of Wildcat, the other programs have differed from Supported Work in important dimensions. The graduated stress and peer support offered by Supported Work may be particularly important to the members of the ex-addict target group.

E. ANALYTIC TECHNIQUES USED TO ASSESS THE PROGRAM'S IMPACT[1/]

Most of the formal evaluation of Supported Work impacts on participants has been conducted using multiple regression analysis.[2/] Since random assignment to the experimental and control groups was strictly adhered to,[3/] comparison of experimental and control-group means will provide unbiased estimates of program effects.[4/] Regression analysis has two advantages over direct comparisons of means. First, to the extent

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[1/] Discussions of the various analytic techniques and statistical tests described here can be found in Hanushek and Jackson (1977) and in other econometric textbooks.

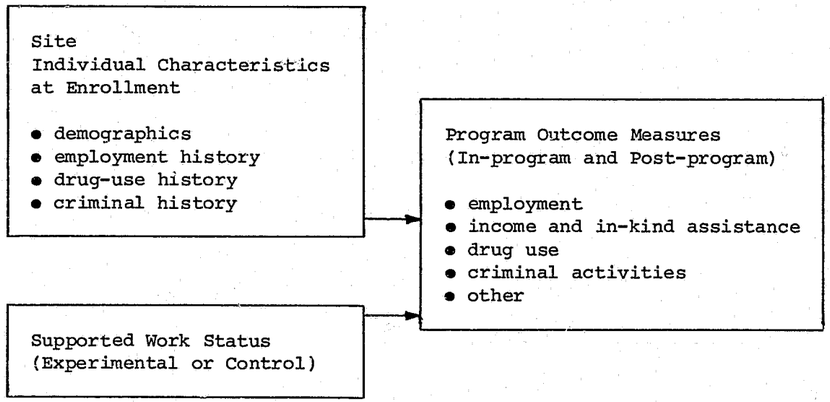
[2/] Means and standard deviations of control variables used in the regressions are tabulated in Table A.11.

[3/] For evidence of the success of the random assignment procedures, see Jackson et al. (1978).

[4/] By unbiased we mean that, on average, the estimate neither overstates nor understates the true effect.

that measurable factors exogenous to the program treatment itself influence the outcome measures, regression analysis permits us to obtain estimates of program effects that have a higher degree of precision than those obtained through a simple comparison of means approach.[1/] Second, regression analysis permits us to investigate easily whether program effects vary significantly among subgoups of the sample or among ex-addicts enrolled in different sites.

The most general model to estimate overall program effects can be depicted as follows:



Formally, the impact of program participation is estimated through regression models of the form:

[1/] The precision of the estimates is a measure of the likelihood that true program effects will not go undetected.

$$Y = a_0 + a_1X_1 + a_2X_2 + \cdots + a_MX_M + bS + u$$

where Y is the observed outcome measure; X_m ($m = 1, \ldots, M$) is a set of variables indicating the Supported Work site and the characteristics of the individual; S is a binary variable indicating whether the individual was assigned to the experimental group; and u is a random error term. The symbol a_m measures the impact of X_m on Y; and b is a measure of the overall impact of the program whose statistical significance level is measured by a t-test. (Appendix Table A.11 identifies the control variables used in the analysis and their means and standard deviations, and Table A.12 presents estimated coefficients on these control variables from selected regression equations used in the analysis.)

The extension of this basic model to estimate effects for subgroups of the sample is quite straightforward. The types of models estimated can be expressed formally as:

$$Y = a_0 + a_1X_1 + a_2X_2 + \cdots + a_MX_M + b_0S + b_1SX_1 + \cdots + b_KSX_K + u$$

where X_k ($k = 1, \ldots, K$) is a subset of X_m. In this model, the program effect for a particular subgroup is measured by a linear combination of the b's; for example, if X is a set of binary variables to designate all but one of the Supported Work sites, then b_0 is the program effect for the omitted site and $b_0 + b_k$ is the program effect at site k. The statistical significance of the various subgroup effects can be measured by an F-test, as can tests of whether program

effects vary among the subgroups (i.e., $b_1 = b_2 = \cdots = b_k = 0$).[1/]

These simple linear regression models may not, however, yield estimates of program effects with desirable statistical properties in cases where the outcome measure is truncated (for example, hours of work) or in cases where it is dichotomous (for example, employed or not). Maximum likelihood techniques have been developed to account for these properties of the outcome measures, but are prohibitively costly for routine use in a project of this magnitude. Thus, since the standard regression techniques have repeatedly been shown to yield quite accurate estimates in most applications, we have tended to rely on this procedure and to selectively re-estimate a number of the results using the maximum likelihood techniques Probit (for dichotomous outcomes) and Tobit (for bounded outcome measures) to ensure that the basic conclusions are indeed insensitive to this analytic constraint. In addition, some of the results presented in the subsequent chapters are based on simple comparisons of means. We have noted throughout the report both the results of maximum likelihood re-estimates of the program impacts and those places where simple comparisons of means have been used.

Regardless of the analytic technique employed (linear regression, maximum likelihood, or comparison of means), the discussion in subsequent chapters focuses on experimental-control differences in the various outcome

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[1/] In subsequent tables, statistical significance of experimental-control differences both for total samples and for sample subgroups are denoted by asterisks. Statistically significant differences in the magnitude of program impacts among subgroups (that is, whether the hypotheses that the program impacts are similar for all subgroups can be rejected) are denoted by the pound symbol (#).

measures. Since these differences are based on estimates of sample means, which are subject to sampling variability, we must consider the likelihood that the estimated difference between experimentals and controls is due to a true program effect as opposed to the random sampling variability. The statistical concepts that relate to this likelihood are the confidence interval around and the statistical significance of the estimated differentials.[1/] In this report, we have adopted the standard procedures of indicating those estimated program effects that are significant at the 5 percent level on a two-tailed test--which means that there is less than a 2.5 percent chance that there was no program effect, given the estimated differential. We also designate estimates of program effects that are significant at the 10 percent level, meaning that there is less than a 5 percent chance that the true effect is zero.

While we have adopted these standards for denoting "significant effects" in this report, there are two counterbalancing considerations that we also take into account in interpreting the results. The first is the small probability that a difference as large as that which is significant would have been observed if the true effect were, in fact, zero. This means that one must expect the occurrence of occasional significant

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[1/] The confidence interval, which is uniquely defined at various levels (the most common being the 95 percent level), is the range of values with a 95 percent probability of containing the true value. That is, if repeated samples were drawn, and estimates and confidence intervals constructed for each, 95 percent of these intervals would contain the true value of the impact. If both ends of the confidence interval are greater or less than zero, the experimental-control differential is referred to as statistically significant (at the designated confidence level). For example, if we observe a differential whose 95 percent confidence interval is between $100 and $400 per month, there is only a .05 probability that the true differential is less than $100 or greater than $400.

differentials, even in the absence of real program effects. The second is that failure to observe significant experimental-control differences does not necessarily mean that they do not exist. It may simply mean that there is so much sampling variability relative to the true effect that a reliable estimate of the true effect cannot be obtained.[1/] Given these considerations, in addition to adopting the standard criteria for denoting statistical significance, we have exercised some judgment in deciding which results or patterns of results are particularly worth noting in the discussion and interpretation of the findings.[2/]

[1/] Increasing the sample size, of course, reduces sampling variability and, consequently, the likelihood that such true effects will go undetected. This concept of the likelihood that true effects will, in fact, be recognized as such in the analysis is commonly referred to as statistical power.

[2/] Yet another consideration in interpreting the results is that, in some cases, estimated program effects may meet the criteria of statistical significance but may be so small in magnitude that they are of little policy relevance or, in other cases, results that do not meet standard criteria of statistical significance may be so large that a policymaker may want to act on the basis of the findings.

CHAPTER IV

PROGRAM EFFECTS ON EMPLOYMENT, EARNINGS AND INCOME

A. INTRODUCTION

Supported Work is designed to increase the employment and earnings of those who participate. To the extent that the experimentals stay in the program, it is expected that, for the periods in which they participate, their employment and earnings will be higher than those of controls. To the extent that the program increases either the productivity of the experimentals or their access to better jobs, their employment and earnings are expected to be higher in the post-program period as well. In this chapter, we examine the employment experiences and earnings of the experimentals and controls for up to three years after enrollment in the demonstration sample.

In the first section, we compare the experimentals and controls on three measures of labor market performance: the percentage who were employed in each 3-month period, the average number of hours worked, and average earnings. The second section presents the overall experimental-control differences in these measures, examines the extent to which these overall impacts are similar to those which would have been estimated had the full sample been followed for three years, and explores possible explanations for the observed pattern of effects over time. The third section considers the extent to which the effectiveness of the program varied for different subgroups of the sample. The fourth section explores possible mechanisms through which Supported Work may have affected the employment and earnings of experimentals, including their post-program

job experiences and participation in other educational or training programs. The final section examines the extent to which the program effects on earnings were reflected in higher total incomes for participants and lower public-assistance costs for taxpayers.

B. OVERALL EFFECTS ON EMPLOYMENT AND EARNINGS

1. Experimental-Control Differences

Table IV.1 presents differences between experimentals and controls in the percentage employed in each of twelve 3-month periods, beginning with the time that experimentals enrolled in Supported Work. As an indication of the extent to which these differences are due to experimentals working in the program, we also present the percentage of experimentals who worked in Supported Work jobs at some time during each period and the percentage who worked only in Supported Work jobs.

The experimental-control difference was the largest in the first 3-month period, when 86 percent of the experimentals participated in the program: only 30.9 percent of the controls worked at all during that period compared to 91.7 percent of the experimentals. The differential narrowed over time until it became essentially zero in months 16 to 18, when only 5 percent of the experimentals participated at all in the program: in this period 40 percent of the controls and 39 percent of the experimentals were employed. This equality in employment rates between experimentals and controls continued for another nine months, through month 24. However, beginning in the third year, experimentals tended to increase their employment rates, while controls reduced theirs, resulting in a small but steady increase in the employment of experimentals relative to controls. By the last 3-month period, 49 percent of the experimentals

TABLE IV.1

PERCENTAGE EMPLOYED
EX-ADDICT SAMPLE

Months	Experimental Group Mean	Control Group Mean	Experimental-Control Differential	Percentage of Experimentals with:[a] Any Supported Work Job	Only Supported Work Jobs
1 - 3	91.7	30.9	60.8**	86.1[b]	80.8
4 - 6	76.8	39.0	37.8**	66.3	61.4
7 - 9	67.2	37.1	30.1**	52.7	47.5
10 - 12	54.5	36.4	18.1**	33.9	29.8
13 - 15	50.6	40.4	10.2**	20.3	15.5
16 - 18	39.4	40.0	-0.6	5.3	3.3
19 - 21	40.0	40.4	-0.4	1.7	1.5
22 - 24	43.0	43.4	-0.4	0.9	0.7
25 - 27	45.4	43.0	2.4	0.2	0.2
28 - 30	42.0	37.8	4.2	0.0	0.0
31 - 33	47.2	38.7	8.5	0.0	0.0
34 - 36	48.8	31.6	17.2**	0.0	0.0

NOTE: Except where noted, all data are regression-adjusted. Control variables used in the regressions are listed in Appendix Table A.11. The samples used are defined in Table II.3.

[a] These data are not regression-adjusted. No experimentals should have been in Supported Work beyond month 21. That some reported program participation in later months reflects either data errors or failure by program operators to terminate individuals on schedule.

[b] Five percent of the experimentals never showed up for their Supported Work jobs, and another 9 percent were in the program for less than 30 days. Since employment intervals less than two weeks were not recorded in interviews, the percentage actually understates slightly program participation.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

compared to 32 percent of the controls were employed, a difference which is statistically significant.

These results suggest that the program did have an impact on the post-program employment of at least those experimentals followed for the full three years. However, the time trend in the experimental-control differences is due to changes over time in the employment of both experimentals and controls, as well as to differences in the samples used to estimate program impacts for the various time periods. We examine possible reasons for these changes in subsequent sections.

The same basic pattern of effects observed for differences in employment rates was also observed for differences in the average hours of work per month (see Table IV.2). The experimentals initially worked substantially more hours than controls, but this difference narrowed sharply as experimentals left their Supported Work jobs. In months 16 to 24, both experimentals and controls worked virtually the same number of hours, between 50 and 62 hours per month, but after that time a differential was again evident. By months 31 to 36, the differential was quite large (about 20 hours per month) and statistically significant.

Where differentials in hours worked between the experimentals and controls occurred (months 1 to 15 and 31 to 36), about 60 percent of the difference was attributable to higher employment rates among experimentals than among controls, and about 40 percent was attributable to higher levels of employment among those experimentals with jobs.[1/]

[1/] The experimental-control differences in hours worked during each 9-month period were estimated by a maximum-likelihood technique (Tobit) that accounts for the fact that individuals cannot work less than zero hours. The estimated differences, presented in Appendix Tables A.3 and A.4, are consistent with the differences presented in Table IV.2.

TABLE IV.2

HOURS WORKED PER MONTH
EX-ADDICT SAMPLE

Months	Experimental Group Mean	Control Group Mean	Experimental-Control Differential	Supported Work Hours	
				Number[a/]	As Percentage of Total Hours of Experimentals
1 - 3	138.4	32.4	106.0**	126.8	91.6
4 - 6	116.7	46.7	70.0**	98.9	84.7
7 - 9	97.3	42.9	54.4**	77.1	79.2
10 - 12	80.2	46.7	33.5**	51.4	64.1
13 - 15	64.9	51.4	13.5**	21.4	33.0
16 - 18	50.4	52.3	-1.9	5.5	10.9
19 - 21	55.1	55.4	-0.3	1.7	3.1
22 - 24	61.6	60.2	1.4	1.1	1.8
25 - 27	63.7	58.9	4.8	0.0	0.0
28 - 30	66.6	56.3	10.3	0.0	0.0
31 - 33	73.1	51.9	21.2**	0.0	0.0
34 - 36	70.4	50.0	20.4**	0.0	0.0

NOTE: Except as noted, all data are regression-adjusted. Control variables used in the regressions are listed in Appendix Table A.11. The samples used are defined in Table II.3. Overall experimental-control differentials and control-group means may vary somewhat from the averages reported in Tables IV.6 and IV.7 due to slight differences in samples.

[a/] These data are not regression-adjusted. No experimentals should have been in Supported Work beyond month 21. That some reported program participation in later months reflects either data errors or failure by program operators to terminate individuals on schedule.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

Thus, the time path of experimental-control differences in hours of work among those employed was generally the same as that in hours worked among the entire sample: the differential narrowed to zero and then, after a period, began again to widen. Only in the last 3-month period was there an exception to this pattern: experimentals who were employed worked slightly less than controls who were employed, and, thus, the overall differences in hours worked were accounted for by the differences in the employment rates.

Table IV.3 presents the differences in the earnings of the experimentals and controls. Once again the same basic time path is observed for earnings as was observed for employment rates.[1/] The difference in average earnings is the product of three factors: (1) differences in the probability of being employed, (2) differences in the hours worked among those employed, and (3) differences in the wage rates of those employed. During months 1 to 15, earnings differentials were low relative to hours differences because program wage rates, which constitute the majority of experimentals' earnings in the period, were purposely set below market wage rates and because controls who found jobs earliest were likely to have been the most employable. In contrast, earnings differentials were relatively high during months 31 to 36 due to both higher employment rates and slightly higher average hourly wage rates among experimentals relative to controls (see Table IV.4).

In summary, we observe a pattern of program effects in which experimentals initially did better relative to controls, but in which the

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[1/] Converting earnings data to inflation/deflation-adjusted dollars as of the fourth quarter of 1976 results in overall experimental-control differences that are between 0 and 11 percent smaller than those presented in Table IV.3.

TABLE IV.3

AVERAGE GROSS EARNINGS PER MONTH (DOLLARS)
EX-ADDICT SAMPLE

Months	Experimental Group Mean	Control Group Mean	Experimental-Control Differential	Supported Work Earnings	
				Dollars[a/]	As Percentage of Total Earnings of Experimentals
1 - 3	395.31	122.30	273.01**	355.31	89.9
4 - 6	348.00	184.58	163.42**	284.68	81.8
7 - 9	306.08	166.99	139.09**	224.83	73.5
10 - 12	280.71	205.37	75.34**	154.02	54.9
13 - 15	251.65	211.81	39.84*	66.17	26.3
16 - 18	215.51	222.22	-6.71	17.27	8.0
19 - 21	243.78	250.25	-6.47	5.20	2.1
22 - 24	281.02	270.36	10.66	3.81	1.4
25 - 27	287.05	259.88	27.17	1.78	0.6
28 - 30	304.09	237.45	66.64	0.00	0.0
31 - 33	332.18	221.85	110.33**	0.00	0.0
34 - 36	318.60	218.76	99.84**	0.00	0.0

NOTE: Except as noted, all data are regression-adjusted. Control variables used in the regressions are listed in Appendix Table A.11. The samples used are defined in Table II.3. Overall experimental-control differentials and control group means may vary somewhat from the averages reported in Table IV.10 due to slight differences in the samples.

[a/] These data are not regression-adjusted. No experimentals should have been in Supported Work beyond month 21. That some reported program participation in later months reflects either data errors or failure by program operators to terminate individuals on schedule.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE IV.4

AVERAGE HOURLY WAGE RATES OF THOSE EMPLOYED
EX-ADDICT SAMPLE
(dollars)

Months	Experimental Group Mean	Control Group Mean	Non-program Wage Rates of Experimentals	Program Wage Rates of Experimentals	
				Amount	Percentage of Non-Program Wage Rates
1 - 3	2.86	3.77	3.45	2.80	81.2
4 - 6	2.98	3.95	3.56	2.88	80.9
7 - 9	3.15	3.89	4.02	2.92	72.6
10 - 12	3.50	4.40	4.40	3.00	68.2
13 - 15	3.87	4.12	4.25	3.09	72.2
16 - 18	4.28	4.25	4.42	3.14	71.0
19 - 21	4.42	4.52	4.47	3.06[a/]	68.5
22 - 24	4.56	4.49	4.58	3.46[a/]	75.6
25 - 27	4.51	4.41	4.48	n.a.	n.a.
28 - 30	4.57	4.22	4.57	n.a.	n.a.
31 - 33	4.54	4.27	4.54	n.a.	n.a.
34 - 36	4.53	4.38	4.53	n.a.	n.a.

NOTE: The wage rate figures were calculated by dividing the average earnings for those in the experimental group by their average hours, and similarly for those in the control group. Since these differences are based on aggregate data, significance tests were not calculated.

[a/] These data are based on very small sample sizes.

experimentals' advantage diminished as they left their Supported Work jobs. After a period in which experimentals had the same employment and earnings as controls, a differential again became evident and, by the 31- to 36-month period, experimentals did significantly better than controls. Furthermore, the differentials in hours and earnings were not due solely to the differences in employment rates: in these later periods, among those who were employed, experimentals tended to work more hours and have somewhat higher wage rates than did controls.

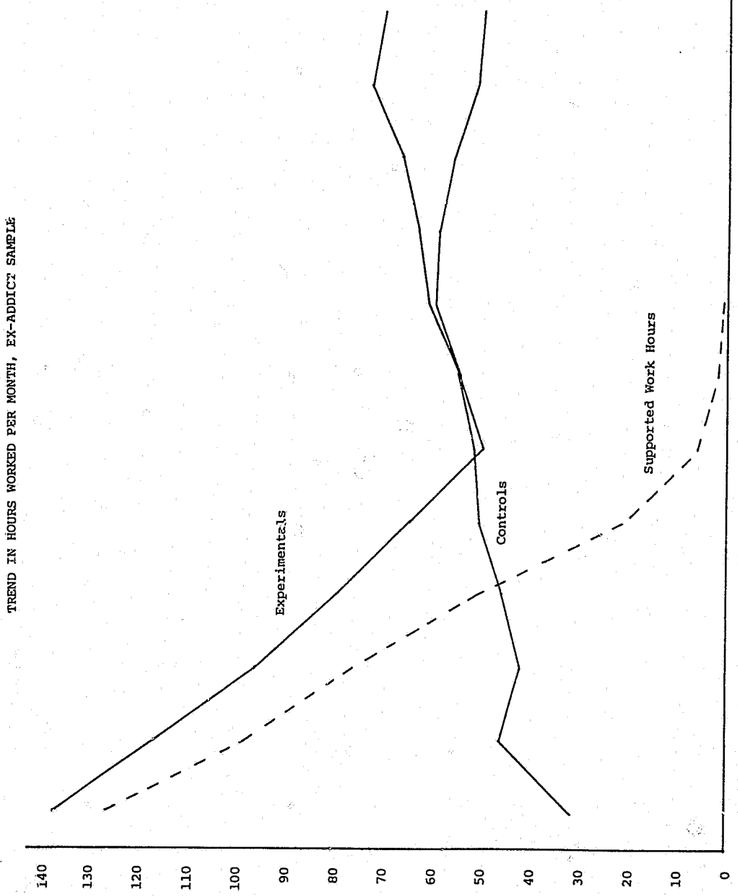
This pattern of employment results, which can be readily seen in Figure IV.1, raises a number of questions, the most important of which is whether the upturn during the last half of the third year is representative of the results we would have observed had the full sample been followed for as long as 36 months. Moreover, what was the cause of the long delay between participants leaving Supported Work and realizing these longer-term benefits?

2. Generalizability of Estimated Long-Run Impacts

As noted previously, the amount of follow-up data available for various sample members depends on the calendar date when they enrolled in the demonstration: those enrolled prior to April 1976 were followed for 36 months (the 36-month cohort), those enrolled between April 1976 and December 31, 1976 were followed for 27 months (the 27-month cohort), and those enrolled in 1977 were followed for only 18 months after their enrollment (the 18-month cohort). Thus, the trend in experimentals' and controls' hours plotted in Figure IV.1 reflects both changes in employment of individuals over time and in sample composition over time.

FIGURE IV.1

TREND IN HOURS WORKED PER MONTH, EX-ADDICT SAMPLE



NOTES: Data plotted in this figure are reported in Table IV.2. Experimental-control differentials are significant only for months 1-15 and 31-36. No experimentals should have been in Supported Work beyond the 21st month. That some report Supported Work hours during months 22-24 may be attributed to either data errors or to occasional failure on the part of program operators to terminate individuals promptly upon expiration of their eligibility period.

These two components of the overall effect can be seen from Figure IV.2, which depicts the average number of hours worked by experimentals and controls in each cohort over time. To abstract from any changes in sample composition, only individuals for whom we have continuous data are included in this figure. Thus, for example, only those who responded to all five interviews are included in the 36-month cohort time trend. (Data from which the time trends in Figure IV.2 were plotted are contained in Appendix Table A.5.)[1/]

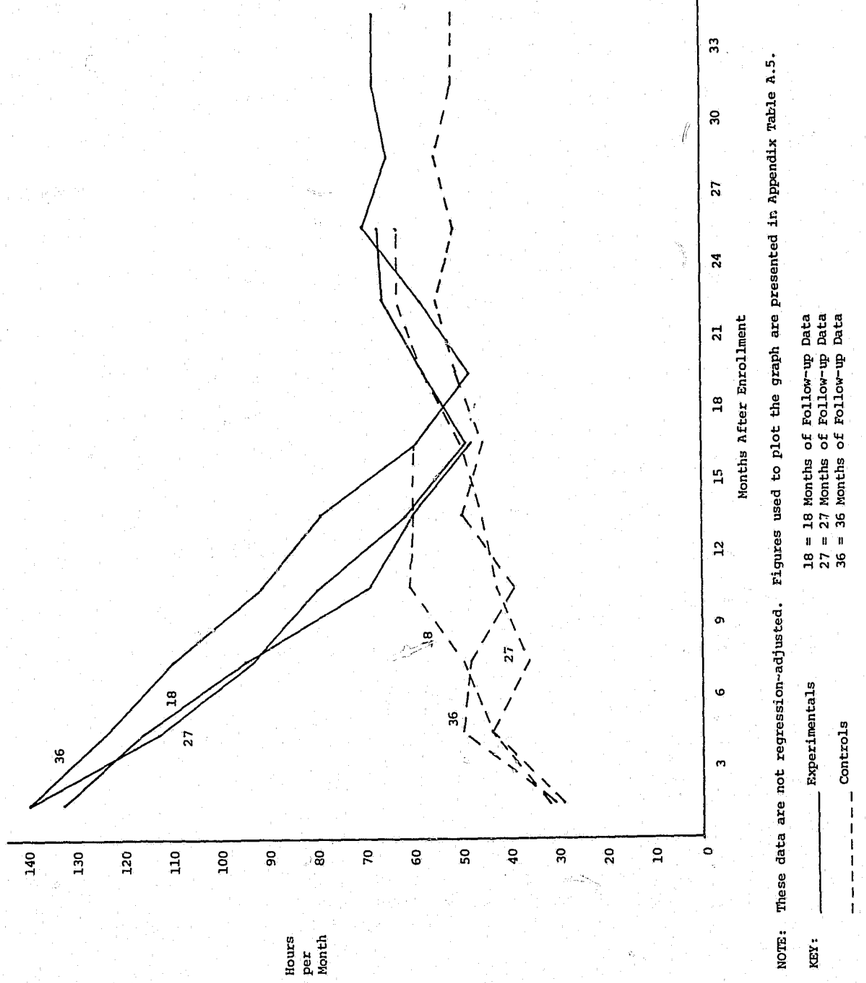
During the initial months following enrollment, all three cohorts exhibited large experimental-control differentials that decreased over time and became insignificant at varying points: significant positive differentials persisted throughout months 13 to 15 among the 36- and the 27-month cohorts, and only through months 7 to 9 among the 18-month cohort. There are two primary reasons for differences in the timing of the decreases in experimental-control differentials. Part of the difference is due to controls' employment increasing more rapidly for the 18- and 27-month cohorts than for the 36-month cohort. Part of the difference is also due to experimentals in the 36-month cohort staying in the program 1.5 months longer than those in the other cohorts. However, it should be noted that this longer participation in the program was found not to lead to significantly larger effects on post-program earnings.[2/]

[1/] The data tabulated in Appendix Table A.5 differ somewhat from those in Table IV.2 because of the different samples and because data in Table IV.2 are regressions-adjusted. However, qualitative assessments of both sets of data yield the same conclusions, even though the statistical significance of the estimated experimental-control differentials vary slightly.

[2/] Appendix D presents an analysis of the impact of length-of-stay in Supported Work on post-program behavior.

FIGURE IV.2

TREND IN HOURS WORKED PER MONTH BY SUBSAMPLES WITH VARYING AMOUNTS OF FOLLOW-UP DATA



NOTE: These data are not regression-adjusted. Figures used to plot the graph are presented in Appendix Table A.5.

KEY: _____ Experimentals
18 = 18 Months of Follow-up Data
27 = 27 Months of Follow-up Data
36 = 36 Months of Follow-up Data
_ _ _ _ _ Controls

For the 36-month cohort, no significant employment differentials were observed again for 9 months, until months 25 to 27, when experimentals increased their employment relative to controls, resulting in a significant differential of 18 hours per month. The differential was somewhat smaller, but persisted, through the 28- to 36-month period. Controls' employment tended to stabilize at around 50 to 55 hours per month, while experimentals' employment averaged 65 to 70 hours per month.

For the 18-month and 27-month cohorts, no significant post-program effects were observed. Whether such effects occurred beyond the period of observation for these groups is simply not known. If any post-program benefits did result, it is likely that they would have been smaller than those observed for the 36-month cohort. This conclusion is based on the fact that, in the earlier periods, the program tended to be less effective for these later cohorts than for the 36-month cohort, and on the fact that controls in the later cohorts exhibited substantially higher employment levels than did controls in the 36-month cohort.

In a subsequent section, we examine possible explanations for these cohort differences. We found that they are not attributable to length of site operation or identified individual characteristics. Variations in the composition of the sample by site explains part of the cohort differences for some time periods. Differences in the economic environment over time may also provide a partial explanation, although, as discussed below, we cannot directly estimate the impact of economic conditions.

The pattern of cohort results observed for the ex-addicts is quite similar to that observed for the other target groups. Generally, the program tended to have a greater impact on the early enrollees, largely

because controls who applied to the program early worked relatively fewer hours than did other controls.

The overall conclusion, then, is that under some conditions, such as those experienced by the 36-month cohort, Supported Work will have long-term employment impacts. However, these effects will tend to be smaller, and perhaps nonexistent, if the programs operate during times when, or enroll individuals among whom, employment in the absence of the program experience will be relatively high.

Having concluded that the longer-term impacts of the program are quite favorable for the earliest enrollees in the demonstration and less favorable or nonexistent for the later enrollees, the question remains as to why there appears to be a delay in the timing of these effects. In the next section, we consider whether job-placement and job-search behavior or the availability in some sites of unemployment compensation benefits may account for the observed time-path of effects.

3. Impact of Job Placement and Job Search Behavior

As noted in Chapter II, fewer than 15 percent of the ex-addict experimentals left Supported Work because they had another job. Yet, over 70 percent found a non-program job at some time during the follow-up period. Excluding the "positive terminees," who left specifically to take another job, there was an elapsed time of 4 to 7 months, on average, between the time these individuals left Supported Work and the time they found their first non-program job.[1/] Johnson (1978) suggests that observed short-run benefits of employment programs may understate the

[1/] See Appendix Table A.14.

longer-run effects because of transition difficulties. Participants not placed in jobs by the program must re-enter the job market, while controls have had the entire program period to search for jobs.

The trend in controls' hours of work is consistent with this hypothesis. The number of hours that the controls worked increased steadily over the first two years until it averaged 50 to 60 hours per month. Only part of this trend can be attributed to changes in economic conditions over time; the rest may be due to controls gradually finding suitable jobs.[1/]

Further support for this explanation for the delay in observing post-program impacts is gained by looking directly at the labor force participation rate and job-search behavior of sample members during the four weeks preceding each interview (see Table IV.5). In month 18, when there were essentially no experimental-control differentials in employment, a significantly higher percentage of experimentals than controls reported that they were looking for a job, and experimentals made significantly more employer contacts than did controls.[2/] These differences in job search activity are also evident, although not significant, in months 27 and 36.

[1/] When differences over calendar time in hours worked were controlled for (a proxy for changes in economic conditions), there remained a substantial increase in controls' hours over the 24 months following application to the Supported Work program.

[2/] With respect to methods of search, significantly more experimentals than controls used the state employment agency in month 18. There were no significant differences in the methods of job search in the later periods.

TABLE IV.5

LABOR FORCE STATUS AND JOB SEARCH DURING THE LAST FOUR WEEKS IN RESPECTIVE TIME PERIODS
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Job Force Status (percentage)								
In labor force	11.35**	65.30	4.73	62.95	2.06	62.35	13.04**	32.24
Employed[a/]	27.61**	48.95	-6.53	56.68	1.02	60.00	-2.78	100.00
Unemployed[a/]	-27.61**	51.05	6.53	43.32	-1.02	40.00	2.78	0.0
Not in labor force	-11.35**	34.70	-4.73	37.05	-2.06	37.65	-13.04**	67.76
Job Search Activity								
Percentage looking for work	-7.54**	47.16	6.17*	39.26	2.46	33.77	4.38	27.41
With help of:								
Supported Work	3.69**	n.a.	1.47*	n.a.	0.0	n.a.	0.0	n.a.
State employment agency	-8.31*	34.21	9.66*	29.56	-8.08	41.41	-9.29	40.54
CETA	-4.36*	7.37	0.25	7.59	1.20	5.47	-0.62	2.70
Community organization	0.60	4.21	2.02	1.90	-1.91	3.91	5.63	2.70
Private agency	-6.54**	8.95	0.50	4.40	-2.36	7.03	-1.24	5.41
Other	-0.13	88.42	2.39	89.24	3.78	87.50	6.64	89.19
Average Number of Contacts with Employers	-1.00**	2.59	1.06*	2.21	0.55	1.52	0.67	1.81
Hours per Week Spent Looking for Work	-1.49**	3.61	0.22	3.26	0.15	2.64	0.56	1.96
Reservation Wage (dollars per week)	6.26**	126.43	0.03	137.82	-10.55**	150.46	-1.50	146.35
Among those employed	3.80	130.97	5.59	141.97	-9.68	158.18	-0.88	154.21
Among those unemployed	3.82	123.22	4.88	126.43	-9.25*	137.75	[b/]	[b/]
Among those not in the labor force	6.50	125.47	-6.94	142.45	-12.93	151.49	-4.22	142.61

NOTE: These data are not regression-adjusted. The samples used are defined in Table II.3. Except as noted, all figures apply to the full sample.

[a/] These figures apply to only those sample members who were in the labor force.

[b/] There are no control group members among the unemployed.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

n.a. = not applicable.

The respondents were also asked about the lowest weekly wage they would be willing to take on a job. In month 9 the "reservation wage" of experimentals was significantly higher than that of controls. This difference narrowed over time and, by month 27, the experimentals' reservation wage was significantly lower than that of controls--particularly among those who were unemployed and among those not in the labor force. This lowering of experimentals' reservation wage rates may have been partly responsible for their employment gains in the later months.

Thus, there is some evidence to suggest that some of the explanation for the delay in post-program impacts of Supported Work is attributable to experimentals experiencing problems when making the transition from Supported Work to other employment. Over time, however, their more active job search relative to that of controls appears to have had some payoff.

4. The Impact of Unemployment Compensation

Another explanation for the delay in post-program impacts, which is related to job search, may be the availability of unemployment compensation (UC) benefits to some experimentals upon their leaving the program. While none of the programs enrolling ex-addicts participated in the state UC programs, as noted previously, experimental-group members in some sites gained eligibility for Special Unemployment Assistance (SUA) benefits on the basis of their Supported Work employment. Thus, because of controls' substantially lower employment rates during the early months following enrollment, receipt of UC benefits was significantly higher among

experimentals than among controls during both months 10 to 18 and 19 to 27 (15 versus 4 percent, and 11 versus 6 percent, respectively). By the 28- to 36-month period, when experimentals were no longer qualifying for benefits on the basis of program employment, no differentials in receipt rates remained between the two groups.[1/]

Those individuals who received UC benefits undoubtedly had less incentive to find alternative employment in the short run and, perhaps, used this period to search longer for a desirable job.[2/] Thus, the differential receipt rates between experimentals and controls could potentially account for the convergence of employment levels of the two groups during the 16- to 21-month period and the gradual increase in experimentals' employment relative to that of controls in subsequent periods.

An attempt was made to assess the impact of UC coverage on the effectiveness of Supported Work, and in particular, to determine the extent to which the delay in post-program effects could be attributed to the institution of the SUA program. However, rigorous analysis of this issue was precluded by the fact that, among the ex-addict sample, Jersey City was the only site in which receipt rates among experimentals were high (34 and 18 percent in months 10 to 18 and 19 to 27, respectively) and

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[1/] Receipt rates and experimental-control differentials in receipt rates were highest among the 36-month cohort and lowest among the 18-month cohort.

[2/] Solon (1979), Dillon and Nicholson (1976), and others provide evidence of the general disincentive effects of unemployment compensation. However, there are no reliable estimates of the magnitudes of these disincentive effects.

experimental-control differentials were significant.[1/] However, estimates based on observed experimental-control differentials among non-UC recipients and estimates based on reasonable assumptions about the employment levels of UC recipients had they not received UC, both suggest that the differential availability of UC benefits to experimentals and controls had relatively little impact on the overall pattern of results.[2/] The delay between leaving Supported Work and finding alternative employment would tend to persist even in the absence of any UC coverage of program employment.

C. DIFFERENTIAL IMPACTS ACROSS SITES AND AMONG SUBGROUPS OF EX-ADDICTS

As discussed in Chapter II, there was considerable diversity among the four Supported Work programs that enrolled ex-addicts and in the types of people who applied for Supported Work. In this section, we investigate whether the effectiveness of the program varies by site and site characteristics and by demographic and background characteristics of the individual. The results of such an analysis may be helpful in determining

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[1/] Data on UC receipt by experimentals and controls in the various sites are presented in Appendix Table A.15. Among the methods considered to address this issue were (1) obtaining a predicted value of employment for UC recipients, based on an employment equation estimated for nonrecipients, and (2) estimating experimental-control differences for recipients and nonrecipients using selection-bias correction methods (e.g., see Heckman, 1976).

[2/] These estimates are, of course, subject to selection bias of unknown direction, because both those experimentals and those controls who received UC benefits are likely to be among the more employable in their respective groups, given that job tenure affects UC eligibility. When we estimated program impacts based only on the Chicago, Oakland, and Philadelphia samples, the pattern of effects over time for the various cohorts was quite similar to that estimated for the full sample, except that small positive, as opposed to small negative, values of the differentials were estimated for months 16 to 21 (see Appendix Figure A.1).

whether the program would be more effective if modelled after certain demonstration programs rather than others or if targeted at a specific subgroup of ex-addicts. In addition, we noted that Supported Work also enrolled ex-offenders, a group that shares many characteristics with the ex-addicts. The Supported Work program had a different effect on these two target groups, and we consider whether subgroup differences within the ex-addict sample might possibly explain these target-group differences.

In this analysis, we have used two tests of statistical significance. One is whether, for a particular subgroup (e.g., those 26-35 years old), there is a significant difference between experimentals and controls. Asterisks beside the particular experimental-control differential are used to indicate when those results are statistically significant. The other test is whether the differences in experimental effects across a particular grouping (e.g., by age groups) are statistically significant. In other words, can we reject the hypothesis that the program's effect is similar for those under 26, those 26-35, those 36-44, and those over 44? The symbol "#" above a set of experimental-control differentials indicates that there is a statistically significant difference in the experimental effects across the groupings in question.

1. Site and Length of Site Operation

In Table IV.6, the results with respect to average monthly hours of work are presented separately by site and by the length of site operation at the time an individual enrolled. For each 9-month period, two numbers are presented for each site. The first is the difference between the average hours worked per month by experimentals and controls in that site; the second is the average hours worked by controls.

TABLE IV.6

HOURS EMPLOYED PER MONTH, BY SITE AND SITE CHARACTERISTICS
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Ex-addicts a/	78.2**	40.5	16.4**	50.0	1.5	58.6	18.3**	52.6
Site	#							
Chicago	90.8**	30.8	20.5**	51.5	11.7	55.8	39.7**	45.8
Jersey City	87.4**	49.7	16.9**	56.4	-4.9	68.1	6.9	73.8
Oakland	59.1**	41.3	16.0	38.4	-20.4	67.2	b/	b/
Philadelphia	61.4**	35.9	12.5	43.9	7.1	47.9	17.4	35.8
Years of Site Operation at Time of Enrollment			#					
Less than 1	73.1**	45.8	27.5**	51.1	3.7	55.1	n.a.	n.a.
1 - 1.5	79.1**	35.0	4.2	46.8	-1.0	60.0	n.a.	n.a.
More than 1.5	84.2**	41.6	19.9**	54.1	2.3	66.5	n.a.	n.a.

NOTE: All data are regression-adjusted. Control variables used in the regressions are listed in Appendix Table A.11. The samples used are defined in Table II.3. Sample sizes for various subgroups can be calculated by multiplying the proportion of the sample in the subgroup (see Appendix Table A.11) by the total sample size.

a/ These overall sample results were estimated from an equation that did not include variables interacting experimental status with site or years of operation. Thus, the subgroup results may not weight up to these overall sample values.

b/ There are only four persons in this Oakland sample.

#Experimental-control differentials for this time period differ significantly from one another.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

n.a. = not applicable.

In the first nine months, the effectiveness of the program did vary significantly by site, largely as a result of differentials in the length of time experimentals in the various sites stayed in Supported Work. In all sites, experimentals worked significantly more than did controls in this period, the difference being largest in Chicago and Jersey City, and smallest in Philadelphia and Oakland. In the later periods, estimated program impacts did not vary significantly among the sites. The differential between experimentals and controls continued to be largest in Chicago, however, and was significant in months 28 to 36. It is noteworthy that, although the widening experimental-control difference in employment after month 27 (discussed above) was largest in Chicago, it was also evident in the other sites.

These site-specific post-program results follow a pattern which is found consistently in other subgroup differences. Throughout the 19- to 36-month period, experimental-control differences were most positive in those sites where controls worked fewer hours than average (such as Philadelphia and Chicago).[1/]

In months 10 to 18 there was also significant variation in program effects by the length of site operation at enrollment. Those experimentals who enrolled in programs that had been in operation between 12 and 18

[1/] It should be noted that the variation across sites in the area unemployment rate is not reflected in the variation in employment patterns of controls. During the study period the unemployment rate in Jersey City was exceptionally high, ranging from 13 percent to 17 percent, while the unemployment rate in Chicago was approximately half that. In all periods, however, controls in Jersey City worked more hours than did controls in Chicago, perhaps in part because public-sector jobs were much more prevalent in Jersey City. (Unemployment rates comes from Bureau of Labor Statistics, 1978.)

months worked only four hours more per month than did comparable controls, while those who enrolled when programs had been in operation for less than 12 or more than 18 months worked significantly more hours than did controls. Since, in the second year of operation, the Supported Work programs generally were expanding, these results suggest that the programs may have suffered adjustment difficulties during the transition period.[1/]

2. Subgroups of Ex-addicts

In Table IV.7, experimental-control differences for subgroups defined by demographic and background characteristics are presented. Since there are many comparisons, and some are significant in only one period, the discussion focuses on those effects that are sustained and consistent across several periods. These consistent differences suggest a pattern in which Supported Work is more effective for those subgroups who might be expected to work less than average (that is, subgroups in which the controls worked fewer hours than average).

With respect to demographic characteristics, there is a consistent variation in program effects according to whether the individual had at least one dependent: those experimentals with dependents tended to work more hours, relative to comparable controls, than did experimentals without dependents. The tendency is evident in all periods, and the differences between the subgroups are significant in months 1 to 9 and months 10 to 18. Also, after the initial 9-month period, effects tended to be more favorable than average among those with some longer-lasting jobs prior to

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[1/] See MDRC (1978).

TABLE IV.7

HOURS EMPLOYED PER MONTH, BY INDIVIDUAL DEMOGRAPHIC AND BACKGROUND CHARACTERISTICS
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Ex-addicts[a]	78.2**	40.5	16.4**	50.0	1.5	58.6	18.3**	52.6
Years of Age								
Under 21	69.8**	49.9	-5.7	68.4	8.6	69.3	80.9*[e]	0.1[e]
21 - 25	75.8**	43.2	12.3*	51.0	-6.4	60.5	3.8	57.5
26 - 35	80.1**	38.7	21.1**	49.0	9.4	58.6	32.8**	44.0
Over 35	82.4**	29.7	24.2*	37.5	-6.0	48.2	-15.6	66.0
Sex					#			
Male	75.0**	41.4	14.5**	51.0	5.4	60.2	15.7*	55.9
Female	90.5**	34.4	24.0**	44.0	-18.9	52.6	37.0	7.6
Race/Ethnicity	#							
White, not Hispanic	65.7**	46.2	25.0**	53.4	5.1	79.2	-35.2	95.9
Black, not Hispanic	83.3**	36.5	15.1**	45.7	2.7	51.8	22.0**	44.9
Hispanic	48.5**	63.8	14.8	80.9	-17.2	90.8	62.1[e]	36.4[e]
Years of Education								
8 or less	84.1**	39.2	25.5**	37.4	8.4	42.3	-4.6	48.9
9 - 11	78.3**	34.7	16.2**	43.8	5.1	56.9	15.8	49.1
12 or more	74.7**	51.2	12.4	67.0	-9.3	70.4	35.6**	50.8
Welfare and Food Stamp Receipt in Month Prior to Enrollment[b]	#							
None	86.4**	38.7	15.8**	53.1	-3.9	65.6	28.6**	42.0
Some	69.8**	41.3	17.0**	46.1	6.2	52.6	9.9	55.9
Dependents	#		#					
None	72.2**	47.1	9.2*	53.7	-5.3	61.9	17.7	51.4
One or more	87.4**	29.0	27.5**	43.2	12.2	53.9	19.9	46.3
Months in Longest Job								
0	104.5**	16.5	-9.5	57.1	-5.7	56.0	-12.5[e]	70.8[e]
1 - 12	72.7**	39.9	14.3**	50.0	2.9	56.6	28.0*	43.9
More than 12	79.3**	42.5	20.5**	48.5	0.8	60.8	15.4	51.1
Weeks Worked in Year Prior to Enrollment[c]								
0	82.2**	38.6	18.1**	44.3	1.9	54.3	9.0	50.2
5	80.2**	39.3	17.3**	46.9	1.6	56.5	12.6	49.9
10	78.2**	40.0	16.4**	49.5	1.3	58.7	16.3*	49.7

Table IV.7 (continued)

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Weeks of Job Training in Year Prior to Enrollment	#							
Less than 8	75.2**	41.4	17.0**	48.1	2.5	57.5	19.0**	49.9
8 or more	106.8**	26.1	9.8	65.0	-9.7	71.7	13.3	45.4
Prior Drug Use[d]								
Used heroin and cocaine regularly	71.4**	44.2	19.4	55.2	-21.3	85.3	10.2	48.8
Used heroin regularly but not cocaine	82.5**	36.6	19.6**	47.8	1.4	56.3	25.4**	46.8
Did not use heroin regularly	61.7**	53.6	-2.2	53.7	20.6	49.6	-19.1	68.3
Drug Treatment in Last Six Months			#		#			
Methadone maintenance	80.8**	33.1	24.1**	38.1	14.8**	48.5	28.6**	42.7
Drug-free program	66.1**	55.5	-6.0	71.1	-6.8	75.9	-14.3	83.0
Other type of program	75.1**	47.0	8.3	64.1	-17.0	66.9	43.5*	41.1
Not in treatment	94.4**	28.6	39.2**	34.1	-15.2	59.2	-25.8	38.8
Prior Arrests[c]					#			
0	82.1**	37.5	15.9	46.0	12.2	49.2	-33.5	86.6
4	76.6**	41.8	14.8**	52.8	-3.4	63.1	20.5*	49.9
9	78.9**	39.2	17.2**	50.5	2.1	59.5	27.2**	43.8
Months since Incarceration					#		#	
Never incarcerated	90.1**	29.3	24.1**	53.0	22.2**	52.4	45.6	33.3
12 or less	77.4**	37.1	20.2**	39.0	11.1	52.9	60.1**	29.7
More than 12	71.6**	48.2	9.8	56.1	-17.4	68.0	4.4	54.6

NOTE: All data are regression-adjusted. Control variables used in the regressions are listed in Appendix Table A.11. The samples used are defined in Table II.3. Sample sizes for various subgroups can be calculated by multiplying the proportion of the sample in the subgroup (see Appendix Table A.11) by the total sample size.

[a] These overall sample results were estimated from an equation that included only the standard control variables and an experimental-status variable. Thus, the subgroup results may not always weight up to these overall sample values.

[b] Welfare includes AFDC, General Assistance, other welfare, and welfare income for which respondents could not identify the source.

[c] These estimates of subgroup effects and means are based on a linear (or piecewise linear) specification of the sample characteristic, evaluated at specified points.

[d] Regular use is defined as use on a daily or almost daily basis for two months or more.

[e] Sample size is less than 20.

#Experimental-control differentials within this subgrouping for this time period differ significantly from one another.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

enrollment in Supported Work and among those with little or no recent job training. Another consistent pattern of effects was that the program tended to be less effective for those who, during the six months prior to enrolling in Supported Work, had been in drug-free treatment programs, and to be relatively more effective for those in methadone maintenance programs; the effects for other types of treatment and for those not in treatment were not consistent over time. A common characteristic of each of these subgroups where effects were largest is that controls tended to work less than the overall average for controls--a characteristic which, as previously noted, also distinguished the 36-month cohort from the full sample and the Chicago and Philadelphia samples from those in Jersey City and Oakland.

As was also noted above, the program effect on hours was larger in months 28 to 36 than in months 19 to 27. However, the fact that almost all sample subgroups exhibit this upward trend over time in estimated program effects, and the fact that the 27-month estimates do not vary much when adjusted to reflect effects for a sample with the characteristics of the 36-month cohort, suggest that these measured sample characteristics per se are not responsible for the observed time path of effects.

Supported Work also enrolled ex-offenders, and the ex-offender target group report (Piliavin and Gartner, 1981) indicates that the program's impact on employment was smaller for ex-offenders than for ex-addicts. The subgroup differences presented in Table IV.7 do not offer an explanation for this target group difference. There are some significant differences by arrest and incarceration histories, but they are inconsistent over time. However, the overall pattern of the program

having a greater impact for groups among which controls do less well does provide an explanation: in every period, ex-offender controls worked more than did ex-addict controls.

In summary, we have found several subgroup differences in the effectiveness of Supported Work. Together, these differences form a consistent pattern in which Supported Work is more effective for those who would have done less well on their own, although no individual difference is so large as to suggest that targeting Supported Work at a specific subgroup of ex-addicts would result in substantially more favorable impacts than observed for the demonstration sample.

3. Further Exploration of Differences Among Cohorts

In the discussion of the overall results, it was noted that there were differences among cohorts in the effect of the program on hours worked and that the program was most effective for those in the 36-month cohort. Having presented the subgroup differences for other variables, we now can examine possible explanations for these cohort differences.

The Supported Work programs had been in operation for varying lengths of time when individual members of the research sample were enrolled. In particular, members of the 36-month cohort tended to enroll in programs when they had been in operation for relatively short periods of time and when the total number of participants was small. Thus, a plausible explanation for the observed differences in program impacts among the sample cohorts was that programs were most effective during the initial period of operations. In fact, this appears not to be the case, as individuals who enrolled in programs when they had been in operation for

longer than a year and a half also exhibited relatively favorable responses to the program.

Another explanation for the cohort results that was considered is that it related to differential impacts across sites. This did turn out to account for part of the difference between the cohorts during the 19- to 27-month period; controlling statistically for differential program impacts among the sites did lead to a reduction in the difference between the estimated program impacts for the 27- and 36-month cohorts. However, the site composition of the cohort samples did not account for cohort differences in program impacts during the 10- to 18-month period.

It seems likely that the changes over time in economic conditions contributed to the observed cohort differences. The fact that in months 10 to 27, controls in the 36-month cohort worked substantially fewer hours than did controls in the 27-month cohort, is consistent with this hypothesis. However, because of the correlations between calendar time, program time, and cohort, it was not possible to produce more than crude estimates of the extent to which changing labor markets affected program outcomes.[1/] However, when we controlled for time since program enrollment, we found that employment increased over calendar time. Thus, changes in economic conditions over time seem to provide a partial explanation for differences in program impacts among the cohorts.

Yet another explanation for the observed differences in outcomes among the three cohorts could be that the type of people applying to

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[1/] Because only four sites enrolled ex-addicts and because labor market conditions exhibited relatively large variations across sites compared to variations over time, we were not able to estimate directly the effects of changes in local unemployment rates.

Supported Work changed over time. There are some differences in the characteristics of the individuals in the various cohorts, and we have noted that, in general, the program was more effective for some types of individual than for others. However, differences in measured sample characteristics do not appear to account for the differences among the cohorts.[1/]

It is possible that those who applied for the program in its initial phases were different from later applicants in ways which are not identified by demographic or background characteristics. For example, in the early phases there might have been a stock of people who had been in drug treatment for whom Supported Work was particularly appropriate, whereas, after this stock was depleted, applicants were drawn from the flow of people coming out of treatment who, on average, were less appropriate candidates for Supported Work. It is not possible to test this hypothesis, however, since it postulates the existence of unobserved differences among the cohorts.

In summary, several explanations for the cohort differences in the observed program effects on hours of work have been investigated, yet only partial explanations have been identified. These include differences in the sample allocations across sites and changes in employment opportunities over calendar time. The remaining differences may be due to unobservable differences in the types of individuals in the various cohorts.

[1/] This conclusion is based on the observation that simulated results for the various cohorts, assuming that the demographic characteristics of the three groups were similar, are much more similar to one another than are the overall estimates of program impacts for the various cohorts.

D. OTHER EMPLOYMENT-RELATED IMPACTS

We have found that Supported Work did have an impact on the employment and earnings of the experimentals, particularly during the early months when experimentals were in Supported Work. Supported Work also had a post-program impact on those earliest enrollees in the demonstration among whom the controls exhibited particularly limited employment. In this section, we examine other indications that Supported Work had long-run impacts. First, we consider post-program employment experiences of experimentals and controls; then we consider the extent and nature of experimentals' and controls' participation in education and training programs during the evaluation period.

1. Non-Supported Work Employment of Experimentals and Controls

In Table IV.8, we present data describing several aspects of the experimentals' non-program employment experience and, where feasible, compare it to that of controls.[1/] Supported Work did provide some placement help to the experimentals: 14 percent of those with nonprogram jobs reported finding these jobs through Supported Work.[2/] Experimentals were also asked whether Supported Work helped them look for a job, and their responses indicated that there were some differences by site in the extent to which the programs provided such assistance: reports of assistance were most prevalent among the Chicago sample and least prevalent

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[1/] See Appendix Table A.18 for similar data broken down by the length of the post-enrollment period for which data are available.

[2/] Very few ex-addicts reported that they had "rollover" jobs--that is, the same job they had during Supported Work minus the wage subsidy and supervision provided by the Supported Work program.

TABLE IV.8

NON-SUPPORTED WORK EMPLOYMENT EXPERIENCE
OF THOSE WITH NON-SUPPORTED WORK JOBS
EX-ADDICT SAMPLE

	Experimentals	Controls	Experimental-Control Differential
Percentage who found job through:			
Supported Work	13.9	n.a.	n.a.
Employment Service	5.7	9.9	-4.2
Percentage with rollover jobs[a/]	2.2	n.a.	n.a.
Percentage with CETA or WIN jobs	13.3	12.3	1.0
Percentage with CETA, WIN, or government jobs	18.4	22.7	-4.3
Percentage distribution by occupation of first non-Supported Work job			
Professional, technical, managerial	8.8	6.8	2.0
Clerical or sales	16.1	17.9	-1.8
Service	20.5	20.6	-0.1
Agriculture, fishing, forestry	1.2	0.6	0.6
Processing	2.9	2.4	0.5
Machine trades	8.8	7.4	1.4
Benchwork	5.0	4.7	0.3
Structural work	12.9	9.7	3.2
Miscellaneous	24.0	30.0	-6.0
(Percentage of Sample with Non-Supported Work Job)	(71.2)	(75.7)	(-4.5)

NOTE: These data are not regression-adjusted; tests of statistical significance were not computed. These data are based on the sample of individuals with at least a baseline, a 9-month, and an 18-month interview. Among these individuals, some will also have a 27- and/or 36-month interview. For an examination of these data by amount of follow-up data, see Appendix Table A.18.

[a/] A rollover job is the same job held during Supported Work participation, but without a wage subsidy from or supervision by the Supported Work program.

n.a. = not applicable.

among the Philadelphia sample. Placement help may partially explain why post-program employment differences between experimentals and controls tended to be consistently largest in the Chicago sample. Placement efforts also varied over time, with both the early and later enrollees reporting more job-search help from the program than those who enrolled in the intermediate periods. This may partially explain the differences in effects by program age that were discussed above, although it does not explain the differences among cohorts.

We also examined the extent to which both experimentals and controls worked in other subsidized jobs. If experimentals tended to work more in such jobs, the experimental-control differences in employment will tend to overstate the social benefits of the program; if controls tended to work more in such jobs, the estimated differences in overall employment will understate the social benefits of Supported Work. Overall, between 12 and 13 percent of those with non-Supported Work jobs reported working specifically in CETA or WIN jobs. If we also include the percentage who reported working on a government job, between 18 and 23 percent worked in potentially subsidized jobs.

There were some site differences in the percentage employed in CETA and WIN jobs, particularly in months 28 to 36. Among those in Chicago, 15 percent of the experimentals were working in CETA or WIN jobs in months 28 to 36, compared to 3 percent of the controls; and in that periods, experimentals earned an average of $32 more per month from subsidized jobs than did controls.[1/] Since the overall earnings differential for all jobs

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[1/] These results are presented in Appendix Tables A.6, A.7, and A.16.

was $183 in Chicago in months 28 to 36, differences in subsidized jobs account for only a small fraction of the total difference. However, these results do qualify somewhat the positive results found for the Chicago site.

There were also some experimental-control differences in the occupation of the first non-program job. Approximately 30 percent of the controls worked in jobs classified as miscellaneous occupations, compared to 24 percent of the experimentals. These jobs were generally related to packaging and materials handling. The experimentals were somewhat more likely than controls to be employed in structural work or in professional, technical, or managerial work, where the structural work primarily involved construction and maintenance activities and jobs in the latter category were related to social welfare. This evidence, though not conclusive, suggests that Supported Work may have increased the participants' access to more skilled jobs.

The above data suggest that the placement help received by participants offers a partial explanation for the differences in program effects by site and by the age of the program. The experimentals in Chicago were also more likely to work in CETA or WIN jobs than were controls, although this difference does not fully account for the larger than average experimental-control differences in employment in Chicago. There is also evidence that Supported Work affected favorably the type of occupation in which the participants were employed after leaving the program.[1/]

[1/] The previously noted wage-rate differences between experimentals and controls may be due to this program-induced differential in occupations of experimentals versus controls.

2. Enrollment in Education and Training

As was noted in Chapter II, members of the ex-addict sample generally had limited education and training experiences prior to applying to Supported Work. It is possible that Supported Work might have increased the enrollment in education or training programs among experimentals by changing their attitudes toward work and, thus, toward the benefits of these programs, by providing information about program availability, and by providing additional income to help support them during program participation. In contrast, if Supported Work is viewed as providing some of the same benefits as education and training programs, controls might have been more likely to seek out such programs after having been denied the opportunity to participate in Supported Work. To the extent that there were experimental-control differences in enrollment in these programs, they could affect the earnings differences in both the short- and long-run.

As can be seen from the data in Table IV.9, participation in education and training programs was limited for both experimentals and controls.[1/] Between 7 and 11 percent of the samples were in educational programs (mainly high school and college) during any 9-month period, and fewer than 5 percent were in training programs. Furthermore, there was generally little difference in participation rates between experimentals and controls. The only exception is that during months 10 to 18, only 7 percent of experimentals as compared with 10 percent of controls were

[1/] Participation was about equally divided among those in Chicago, Jersey City, and Philadelphia. Fewer than 13 percent of the participants in education and training programs were from Oakland.

TABLE IV.9

PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS
EX-ADDICT SAMPLE

	Months 1-9			Months 10-18			Months 19-27			Months 28-36		
	Experimental-Control Differential	Control Group Mean		Experimental-Control Differential	Control Group Mean		Experimental-Control Differential	Control Group Mean		Experimental-Control Differential	Control Group Mean	
Education Programs												
Percentage participating	-1.4	11.2		-3.2*	10.2		2.0	6.8		1.4	6.5	
Average number of weeks	-0.1	1.9		-0.8*	2.1		0.1	1.3		0.7	1.1	
Of those participating[a/]												
Percentage in high school program[b/]	-2.0	35.3		-15.9	34.4		1.3	21.7		-12.5	25.0	
Percentage in vocational program[c/]	-5.5	17.6		13.4	12.5		-21.7**	21.7		25.0	0.0	
Percentage in college program	10.2	35.3		-12.4	53.1		17.1	52.2		-12.5	75.0	
Percentage in other program	-2.7	11.8		14.8**	0.0		3.3	4.3		0.0	0.0	
Percentage receiving diploma or degree[a/]	0.9	0.7		-1.7**	2.3		-0.8	1.0		1.2	0.7	
Training Programs												
Percentage participating	-0.5	3.5		-1.4	4.1		0.6	3.6		0.1	2.2	
Average number of weeks	-0.2	0.6		-0.1	0.7		0.1	0.6		-0.2	0.5	
Of those participating, percentage in programs sponsored by[a/]												
Supported Work	14.3[d/]	0.0		7.7	0.0		0.0	0.0		0.0	0.0	
CETA or WIN	-14.3[d/]	57.1		-14.4	37.5		-4.8	21.4		50.0	0.0	
Jail or prison	-14.3[d/]	14.3		12.0	18.8		3.2	35.7		-25.0	25.0	
Other	14.3[d/]	28.6		-5.3	43.8		1.6	42.9		-25.0	75.0	
Percentage receiving certificate[a/]	0.1	0.4		0.3	0.9		-0.4	2.6		-0.7	2.0	

NOTE: Except where noted, all data are regression-adjusted. Control variables used in the regressions are listed in Appendix Table A.II. The samples used are defined in Table II.3.

[a/] These data are not regression-adjusted.

[b/] High school is an educational program for which one receives a high school diploma or GED and may include a vocational curriculum.

[c/] Vocational training may lead to a diploma or a certificate, but not to a high school diploma or GED.

[d/] These percentages represent a difference of only two individuals.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

enrolled in school.[1/] This result is not large enough to be expected to have any noteworthy impact on overall long-term employment outcomes.

E. PROGRAM EFFECTS ON TOTAL INCOME AND ITS SOURCES

In the previous sections, we found that Supported Work increased the earnings of experimentals both in the periods when a substantial proportion were participating in the program and in the 28- to 36-month period. At the time that the ex-addicts applied to Supported Work, over 50 percent were receiving welfare and/or food stamps. Undoubtedly these income-conditioned transfers as well as other sources of income would be affected by the changes in earnings due to participation in Supported Work. In this section, we report the extent to which the program benefited participants through increasing their total income and the extent to which it benefited taxpayers through reducing public assistance costs. (This section is a summary of a detailed analysis presented in Appendix B.)

We consider income from five sources: earnings, unemployment compensation, welfare, the bonus value of food stamps,[2/] and other types of unearned income. Table IV.10a presents the experimental-control differences in whether income was received from these sources; Table IV.10b presents differences in the average amounts received from the various sources.[3/]

[1/] This difference in enrollment rates accounts for the observed differential in the percentage receiving a diploma or degree (0.6 versus 2.3 for experimentals and controls, respectively).

[2/] The bonus value of food stamps is the face value of the food stamps minus whatever the respondent paid for them.

[3/] The earnings results presented in this section differ somewhat from those presented earlier due to differences in samples. Only individuals for whom valid data on all income sources are available are considered in Table IV.10b.

TABLE IV.10a

PERCENTAGE RECEIVING INCOME FROM VARIOUS SOURCES
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Earnings[a/]	44.8**	50.2	10.8**	53.1	3.5	53.0	10.1*	53.9
Unearned Income								
Unemployment Compensation	-5.0**	7.4	10.7**	4.3	5.2**	6.0	0.5	7.4
Welfare[b/]	-21.7**	50.7	-6.1*	46.7	-0.4	40.2	-3.6	45.1
Food Stamps	-10.1**	45.7	-3.5	43.3	1.8	38.8	0.6	42.0
Other[c/]	-4.2**	7.1	-2.4*	4.2	-2.1	5.3	2.3	2.2

TABLE IV.10b

INCOME RECEIVED PER MONTH FROM VARIOUS SOURCES
EX-ADDICT SAMPLE
(dollars)

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Sources	134.09**	295.50	36.00	344.53	23.17	373.98	92.03**	352.40
Earnings[a/]	201.44**	159.79	39.20*	220.42	16.42	261.33	101.73**	224.36
Unearned Income								
Unemployment Compensation	-6.59**	10.86	17.84**	8.42	15.11**	10.31	1.16	16.62
Welfare[b/]	-48.49**	92.88	-12.50*	86.99	-3.12	74.70	-9.83	82.84
Food Stamp Bonus Value	-6.01**	20.89	-3.47	22.60	0.37	18.56	0.48	20.90
Other[c/]	-4.66	10.24	-2.21	4.61	-3.30	7.86	-0.43	7.14

NOTE: See note to Table IV.1. All data pertain to the full sample, not only to recipients.

[a/] Earnings data reported in this table vary somewhat from those in Table IV.3 because of slight differences in the samples used. Only individuals who had valid data for all income sources were included in this table.

[b/] Welfare includes AFDC, GA, SSI, and other unspecified welfare income.

[c/] Other unearned income includes Social Security, pensions, alimony, child support, and training income.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

In periods when experimentals had higher earnings than controls they also tended to have higher total incomes. For example, in months 1 to 9 the total income of experimentals averaged $430 per month; 45 percent more than that of controls who averaged only $296 per month. During months 10 to 27, the income gains of experimentals were modest ($23 to $36 per month), but in months 28 to 36 experimentals received 26 percent more income than did controls ($444 versus $352 per month). Thus, the program did result in a benefit to participants in the form of higher total incomes.

Concurrent with the earnings differences, we also observed experimental-control differences in transfer income. In both the first and second 9-month periods, significantly fewer experimentals than controls received welfare.[1/] For example, in months 1 to 9, 51 percent of the controls received welfare, as compared to 29 percent of the experimentals, and average welfare payments to experimentals were $48 per month less than payments to controls. However, these differences were considerably smaller in months 10 to 18 and not significant beyond that period. Experimentals were also significantly less likely than controls to receive food stamps in months 1 to 9, although not in the later periods.

The picture is different with respect to unemployment compensation. During months 10 to 18 and months 19 to 27, a significantly higher pecentage of experimentals than controls received unemployment compensation, primarily because some experimentals became eligible for the

[1/] Of those ex-addicts who received welfare, 63 to 76 percent received general assistance and 24 to 35 percent received AFDC. See Appendix Table A.21 for further detail.

temporary, Special Unemployment Assistance program.[1/] For example, in months 10 to 18, only 4 percent of the controls compared with 15 percent of the experimentals received income from unemployment compensation, and experimentals received an average of nearly $18 more per month from this source than did controls.

In the first nine months, therefore, we find that Supported Work reduced transfer income (including welfare, food stamps, and unemployment compensation) by approximately $61 per month. In the intermediate periods, the program actually increased the receipt of transfers by between $2 and $12 per month, due to the higher unemployment compensation paid to experimentals as compared with controls. As we noted earlier, however, it is not clear what the impact of any future Supported Work program would be on this type of transfer income. In months 28 to 36, the program once again reduced transfer income by an estimated $8 per month, a suggestive, though not significant, difference.

F. SUMMARY

We have investigated the effects of Supported Work on the employment rates, hours of work, and earnings of experimentals. We found a similar time path of effects for all three measures. Initially, when a substantial proportion of experimentals were working in their Supported Work jobs, experimentals had significantly higher employment and earnings than did controls. As the proportion working in Supported Work decreased,

[1/] SUA was a temporary program enacted in 1974 to extend unemployment compensation coverage to individuals who met the standard eligibility criteria but who were employed by businesses not covered by the regular unemployment compensation program.

the differentials narrowed and, by months 16 to 18, were no longer significant. However, for those followed for 36 months after their enrollment in Supported Work, significant differentials reappeared during the 31- to 36-month period (see Tables IV.1 to IV.3).

Several explanations for these time paths of program effects were investigated. The effects in the 28- to 36-month period were estimated with a substantially smaller sample than were the effects in the earlier periods. And there is some evidence that the program was generally more effective for this subsample for whom we have 36 months of data. For other subsamples, we observed a similar pattern of large initial differentials that became insignificant as experimentals left Supported Work. After some period of no experimental-control differences, small and not statistically significant differences did reappear for the 27-month cohort. However, the differentials for the 27-month cohort fell to zero sooner, reemerged later, and were substantially smaller in magnitude than were those for the 36-month cohort (see Figure IV.2).

The delay in the observed post-program effects for the 36-month cohort may have been due to the need for experimentals to search for jobs after leaving the program. There is some evidence that more experimentals than controls were actively looking for work, and that in the twenty-seventh month the reservation wage of experimentals--the lowest wage they would be willing to take--had fallen below that of controls. The receipt of unemployment compensation by many experimentals in Jersey City undoubtedly prolonged their job search. However, this latter effect explains relatively little of the delay in observing overall post-program impacts.

From this analysis we conclude that Supported Work did affect the post-program earnings of participants in the 36-month cohort. It may also have had some impact for the later enrollees. However, because of the limited follow-up data for these later enrollees it is not possible to determine whether, in fact, they experienced long-run employment benefits from their Supported Work experience. Most likely, if such long-run benefits did result, they were considerably smaller than those estimated for the 36-month cohort.

Throughout all aspects of this analysis, we found a consistent pattern of Supported Work resulting in larger program impacts on employment among those subgroups where control-group members worked fewer hours than average. This suggests that future Supported Work programs might have greater impact if they were implemented in sites where and during time periods when alternative employment opportunities were most limited, and if they continued to be targeted toward those with the most serious employment problems.

CHAPTER V

EFFECTS ON DRUG USE

As was discussed in detail in Chapter III, Supported Work may decrease the experimentals' drug use through several mechanisms. The increase in employment opportunities may make those drug-use patterns that interfere with work more expensive. Having a job may reduce the experimentals' contacts with their past environments and may change their self-concepts from that of drug-addict to worker. The peer support for work offered in Supported Work may counterbalance the peer support for drug use in the ex-addicts' former environments. The additional income from the program may reduce the experimentals' feelings of deprivation to which drug use may have become a general response. If participants choose to spend some of their additional income on drugs, however, program participation may cause an increase in their drug use.

In this chapter, we present the overall experimental-control differences in the use of several types of drugs and examine the extent to which the effect of the program varies by subgroups. The impact of the program on multi-drug use and on the relationship between drug use and employment are also examined. Since the use of drugs involves several dimensions, we begin with a discussion of various measures of drug use.

A. MEASURES OF DRUG USE

The ex-addicts were asked about their use of alcohol and of eight types of drugs: heroin, other opiates, cocaine, amphetamines,

barbiturates, illegal methadone, psychedelics, and marijuana.[1/] This yields a large number of potential outcome measures by which program effects could be gauged. The outcomes analyzed were chosen on the basis of importance and reliability.[2/]

On the premise that society's concern with the use of a particular drug is due to the costs it imposes on others, drug-use patterns can be ranked in terms of the seriousness of their social consequences. For example, such a ranking potentially could be developed from measures of the association between drug use patterns and employment experiences, health status or criminal activities. For this study, we developed such a ranking based on the relationship between drug use and criminal activities, as measured by the respondent's arrest record, and, from this ranking we constructed what we refer to as an additive index of drug use.[3/] In this index, heroin use--particularly daily use--is given the most weight.

[1/] Respondents reported whether they ever used each substance in the 9-month period and, if so, the frequency, duration of use, and dosage. The duration of use and dosage are not considered in this report, since there is some evidence that these measures are not reliably reported (see Dickinson, 1979a).

[2/] All of the analysis is based on the respondents' self-reported drug use. We did not attempt to verify their responses through chemical testing, since (1) such procedures would pose administrative problems and (2) the less costly and more easily administered tests tend to be unreliable and detect only current use. Other studies that have validated interview responses concerning drug use with chemical tests have found a high rate of agreement (for example, see O'Donnell, 1969).

[3/] Dickinson (1979b) describes the development of the drug use index. Attempts to obtain reliable estimates of the relationships between drug use and employment experiences and between drug use and health status were not successful.

Cocaine and other opiate use are ranked next.[1/] Use of amphetamines, barbiturates, psychedelics, and illegal methadone each have small weights in the index.[2/] Finally, daily use of marijuana has only a small weight, and occasional use has no weight in the index, since it showed no statistical association with arrests.[3/]

In this chapter, results are reported separately for heroin use, daily heroin use, cocaine use, alcohol use, and daily alcohol use. In addition, two summary measures of drug use are reported. The first is simply whether any drug other than marijuana was used. The second is the above-mentioned additive index, which serves as a proxy for the social costs of drug use in that it weights the use of each type of drug by its association with arrests.

[1/] There was relatively little variation in criminal history by how frequently these drugs were used and frequent use of these drugs was not common in this sample.

[2/] Use of these drugs during the 36-month study period was not common. For example, in the first nine months, 3 percent of the ex-addicts used amphetamines, 4 percent used barbiturates, 3 percent used illegal methadone, and 1 percent used psychedelics. None of the experimental-control differences in use of these drugs was significant for any period.

[3/] The specific weights assigned to use of each type of drug are presented in Appendix Table A.9. Alcohol use was not considered in constructing the drug use index because the data on alcohol use pertained to different time periods than did the drug-use data. In particular, the questions about alcohol pertain to the current period, while those about drugs pertain to pre-enrollment use and use during the period covered by each 9-month interview.

B. EFFECTS ON DRUG USE

Overall, Supported Work did not have a significant influence on the participants' use of drugs, as shown in Table V.1.[1/] In months 1 to 9, about one-fifth of both experimentals and controls reported using heroin. Fewer respondents reported using heroin in the later periods, reflecting, in large part, a general decline in the use of this drug (the extent of which was similar for both experimentals and controls).[2/]

Measures of the frequency of heroin use for the full sample are not available for the first 9-month period,[3/] but in months 10 to 18 a lower proportion of experimentals than controls used heroin daily or almost daily: 5.2 percent of the experimentals compared to 7.7 percent of the controls. However, this difference is not significant and does not persist into the later periods.

With respect to the other drugs, 5 to 10 percent of the sample (both experimentals and controls) used opiates other than heroin, and

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[1/] The figures presented in this table are estimated by ordinary least squares regressions. We have also estimated the program effects on the likelihood of using some of the drugs using a maximum likelihood technique (probit) that accounts for the binary nature of these dependent variables. Generally, the results were quite similar, although the maximum likelihood estimates tended to be somewhat smaller and less significant than the estimates presented in Table V.1.

[2/] Some of the decline was due to a difference in drug-use patterns of those enrolled at various points in time: use was substantially less prevalent among the earliest enrollees, who have proportionally greater representation in the 19- to 27- and 28- to 36-month results (see Table V.4). The decline in reported use was not due to the respondents becoming tired of repeated interviewing. The extent of the decline over time within the 9-month interview was almost fully accounted for by the decline between the 9- and 18-month interviews (see Dickinson, 1979a).

[3/] In the 9-month interview, only those who were currently using drugs were asked about the frequency of their use.

TABLE V.1

PERCENTAGE REPORTING USE OF VARIOUS DRUGS
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Any Drug (other than marijuana or alcohol)	-2.1	38.2	1.4	32.7	0.5	27.5	2.7	20.7
Heroin								
Any use[a]	-1.3	21.5	-1.0	17.8	1.7	11.7	1.3	8.8
Daily use[b]	n.a.	n.a.	-2.5	7.7	-0.2	4.8	-0.8	4.3
Any Use of Opiates Other than Heroin[a/c]	-0.7	10.1	1.8	5.5	0.9	5.2	-2.4	7.4
Any Use of Cocaine[a/c]	2.6	16.2	2.6	15.3	1.5	15.2	-1.4	13.7
Any Use of Amphetamines, Barbiturates, Psychedelics or Illegal Methadone[a/c]	0.2	9.2	1.0	9.2	0.3	8.5	-1.7	6.6
Marijuana								
Any use[a]	-0.2	65.4	-0.5	66.3	2.2	62.5	0.8	61.8
Daily use[b]	n.a.	n.a.	-1.4	25.4	-0.2	23.4	2.0	20.7
(Additive Index of Drug Use)[d]	(0.2)	(33.1)	(0.6)	(32.8)	(2.6)	(21.9)	(-0.2)	(18.3)

NOTE: See note to Table IV.1.

[a] Any use is defined as use at any time during the 9-month period.

[b] Daily use is defined as use on a daily or almost daily basis at any time in the 9-month period.

[c] Daily use of other opiates, cocaine, amphetamines, barbiturates, and psychedelics was reported by less than 5 percent of the ex-addict sample and so is not included in this table.

[d] This index weights the use of each drug by its association with arrests. See Dickinson (1979b) for a description of the methodology used to develop the index and see Appendix Table A.9 for the actual weights used.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

n.a. = not available

between 12 and 19 percent of the sample used cocaine, during each 9-month period.[1] Experimental-control differences were neither large nor significant. The use of marijuana was widespread, with approximately 65 percent of both experimentals ansd controls reporting its use in each 9-month period.

Experimental-control differences in the additive index of drug use are also presented in Table V.1. These differences are very small and never statistically significant. Thus, the social cost of the experimentals' drug use, as measured by the index, is virtually identical to that of the controls.

There were also no significant differences in the prevalence of enrollment in drug-treatment programs as a result of Supported Work nor any persistent differences in the types of treatment used.[2]

It should be noted that this similarity in drug use between experimentals and controls occurred despite the fact that there were substantial differences in their employment in some periods. For example, although 94 percent of the experimentals, compared to 50 percent of the controls, were employed at some time in months 1 to 9, 36 percent and 38 percent of experimentals and controls, respectively, reported using some drug other than marijuana or alcohol.

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[1] Unlike for heroin use, there was no sizable decline over time in reported use of cocaine.

[2] These results are presented in Appendix Table A.8.

1. Subgroup Effects on Heroin Use

These overall results on drug use do mask some important effects for certain subgroups. In this section, we examine variation in heroin use by site and by demographic subgroup.

As shown in Table V.2, there are significant differences in the program effects on heroin use by site. The Oakland program significantly reduced the heroin use of experimentals relative to controls in both the first and second 9-month periods. However, between months 10 to 18 and 19 to 27, heroin use among Oakland experimentals remained quite stable (27 to 30 percent of the sample), while the percentage of Oakland controls reporting heroin use fell substantially (from 48 to 25 percent), resulting in a small, and not significant, differential in the later time-period. It should be noted that the overall rate of use among controls was much higher in Oakland than in other sites. Presumably, a similarly high percentage of Oakland experimentals would have used drugs in the absence of the program. Thus, the program did reduce heroin use in the site where and during the time periods when participants were at the highest risk of such use. The same pattern was found with the subgroup effects on hours of work: the program tended to be most effective for those who would have done less well on their own.

Table V.3 presents the subgroup differences in program effects on heroin use according to various background and demographic characteristics of the sample. Again, we find that the program was relatively more effective in reducing heroin use among those whose control-group counterparts were particularly likely to use the drug. Particularly in the early period, the program did lead to reduced heroin use among those over 35,

TABLE V.2

PERCENTAGE REPORTING ANY USE OF HEROIN BY SITE
EX-ADDICT SAMPLE

	Months 1-9			Months 10-18			Months 19-27			Months 28-36		
	Experimental-Control Differential	Control Group Mean		Experimental-Control Differential	Control Group Mean		Experimental-Control Differential	Control Group Mean		Experimental-Control Differential	Control Group Mean	
All Ex-Addicts	-1.3	21.5		-1.0	17.8		1.7	11.7		1.3	8.8	
Site	#											
Chicago	5.7	22.3		3.1	16.9		6.3	15.0		12.6*	9.4	
Jersey City	-0.3	9.6		-1.1	14.6		0.4	7.5		0.5	3.0	
Oakland	-25.2**	56.0		-20.4**	47.8		5.1	24.6		a/	a/	
Philadelphia	-0.6	26.4		2.1	13.5		-0.1	12.2		-3.8	12.5	

NOTE: Together, these subsamples include the same individuals as were included in the sample used to generate the data presented in Table V.1. See note to Table IV.6.

a/There are only four persons in this Oakland sample.

#Experimental-control differentials within this subgrouping for this time period differ significantly from each other.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE V.3

PERCENTAGE REPORTING ANY USE OF HEROIN, BY INDIVIDUAL DEMOGRAPHIC AND BACKGROUND CHARACTERISTICS
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Ex-Addicts	-1.3	21.5	-1.0	17.8	1.7	11.7	1.3	8.8
Years of Age								
Under 21	4.9	10.2	13.6	6.6	-13.0	12.5	14.9	0.1
21 - 25	2.4	17.9	-1.8	16.2	7.6*	-8.8	3.0	10.0
26 - 35	0.2	23.7	3.6	17.4	0.1	14.8	-1.0	10.7
Over 35	-20.9**	30.3	-21.3**	29.8	-4.6	10.2	-5.7[a]	1.4
Sex					#			
Male	-0.3	20.9	-0.5	19.1	-0.4	11.8	0.0	8.9
Female	-5.4	24.0	-3.1	12.3	11.8*	11.4	4.9	8.3
Race/Ethnicity	#							
White, not Hispanic	-12.8*	26.8	-10.1	26.9	-3.2	12.0	-8.8[a]	7.7
Black, not hispanic	2.1	19.0	1.6	15.8	2.8	11.3	1.8	9.5
Hispanic	-15.6*	37.4	-10.6	22.2	-1.5	13.6	0.7	0.2
Years of Education							#	
8 or less	3.4	19.6	0.8	18.0	1.5	11.0	-3.8	22.7
9 - 11	-3.9	24.3	-4.1	18.8	-1.2	12.9	-4.3	8.8
12 or more	1.2	16.5	4.7	15.6	7.2*	9.7	14.7**	2.3
Welfare and Food Stamp Receipt in Month Prior to Enrollment								
None	2.0	22.1	-1.2	17.3	2.9	11.3	-7.8	13.9
Some	-4.6	20.8	-0.7	18.3	0.5	12.1	7.9	4.5
Dependents			#					
None	-0.2	22.4	4.3	13.9	3.7	13.2	4.4	7.2
One or more	-3.1	20.0	-9.2**	23.9	-1.7	9.3	-5.5	11.3
Months in Longest Job								
0	5.6	15.0	-2.7	17.1	2.7	15.2	-2.2[a]	0.4
1 - 12	-4.4	24.0	-3.1	21.8	2.3	11.5	-1.5	8.6
More than 12	0.2	20.3	0.7	14.9	1.1	11.5	2.3	9.8
Weeks Worked in Year Prior to Enrollment[b]								
0	1.0	19.5	-0.8	19.8	0.2	12.6	0.8	10.0
5	-0.2	20.5	-0.9	18.8	0.9	12.2	0.8	9.5
10	-1.3	21.5	-1.0	11.8	1.6	11.8	0.7	9.1
Prior Drug Use								
Used heroin and cocaine regularly	2.3	19.6	6.0	16.6	-1.0	16.2	-7.2[a]	6.0
Used heroin regularly but not cocaine	-3.2	24.0	-0.7	18.2	1.9	11.8	3.7	7.7
Did not use heroin regularly	5.3	10.5	-8.3	16.8	2.5	7.4	-12.7	19.4

TABLE V.3 (Continued)

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Years of Prior Heroin Use[b]								
1	-7.7*	25.4	-4.0	19.1	-2.5	14.4	-2.2	13.4
3	-5.3	23.9	-2.8	18.6	-1.0	13.4	-1.2	12.6
9	1.9	19.6	0.5	17.2	3.5	10.5	1.7	10.5
Drug Treatment in Last Six Months								
Not in treatment	-5.8	21.5	-11.6	20.0	1.4	15.4	7.4	0.0
Methadone maintenance	-1.2	20.4	-2.1	20.3	-1.6	12.3	-2.7	14.9
Drug-free program	-3.9	28.8	-0.3	16.9	8.8*	9.6	17.8*[a]	0.0
Other type of program	4.4	17.1	7.7	11.8	2.2	9.5	-9.8[a]	8.8
Enrollment in Treatment	#							
Voluntary	-4.2	23.1	-0.8	18.0	0.2	10.9	1.4	10.8
Involuntary	8.0	15.7	3.8	14.2	5.8	11.5	-0.7	9.7
Addicts in Neighborhood[c]					#		#	
Few or None	2.6	18.0	-0.2	14.0	-3.5	12.0	-6.5	12.2
Many	-3.3	24.4	-0.6	21.0	6.1*	11.5	8.1*	5.3
Best Friend[c]								
Does not use drugs and is not involved in crime	1.2	16.5	0.5	14.7	3.1	9.9	1.7	9.4
Uses drugs or is involved in crime	-5.6	35.8	-3.2	26.6	-1.6	16.8	-1.5	7.3
Prior Arrests[b]								
0	1.0	14.3	-6.2	18.0	-1.9	4.5	-28.9[a]	27.9
4	0.6	20.8	0.0	18.3	2.1	12.0	0.4	7.5
9	-1.5	22.3	-0.3	17.8	2.1	12.5	4.0	6.5
Months Since Incarceration								
Never incarcerated	3.1	21.1	-5.2	23.1	4.9	12.1	28.8*	0.0
12 or less	-7.1	25.0	-1.2	18.9	-0.5	9.3	-10.2	16.6
More than 12	-0.3	19.3	2.3	13.1	1.0	13.0	-1.7	10.8

NOTE: See note to Table IV.6.

[a] Negative point estimates of experimental or control group means arise because linear regression analysis rather than probit analysis was used.

[b] These estimates of subgroup effects and means are based on a linear (or piecewise linear) specification of the sample characteristic, evaluated at the specified points.

[c] These results were obtained from a regression that did not include the full set of variables interacting status with background characteristics.

#Experimental-control differentials within this subgrouping for this time period differ significantly from one another.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

among whites and Hispanics, and among short-term heroin users; controls in all of these subgroups had high rates of use.[1/] In the later periods, among those with more than 12 years of education and among those who were enrolled in drug-free treatment programs, experimentals were significantly more likely to use heroin than were controls--and controls with these characteristics had lower than average rates of use.

For the ex-addict sample, estimates of program effects on heroin use varied little by the extent of one's criminal history. In contrast, there is some evidence that Supported Work led to reductions in heroin use among the ex-offender target group members (see Piliavin and Gartner, 1981). However, the subgroup results presented here for the ex-addict sample provide no indication as to why the results differ between the two target groups.

Table V.4 presents the cohort differences in program effects on heroin use. These differences are substantial and follow a complex pattern. Among those in the 36-month cohort, the program resulted in a significant increase in heroin use in the first and second 9-month periods. For example, 23 percent of the experimentals compared to 14 percent of the controls in the 36-month cohort used heroin in months 1 to 9. In contrast, among those in the 18-month cohort, the significant decrease in months 1 to 9 did not persist into months 10 to 18. For those in the 27-month

[1/] In the early periods, the recidivism rates among controls over 35 were higher than among those who were younger. Several previous studies have found that those over 30 are less likely to return to drug use after treatment (see, for example, Duvall et al., 1963). The Supported Work sample has substantially fewer people over 35 than does the DARP sample (see Table II.2). Those older ex-addicts in the Supported Work sample may have been selected by referral agencies as being more likely to return to heroin use.

TABLE V.4

PERCENTAGE REPORTING USE OF HEROIN BY LATEST FOLLOW-UP INTERVIEW
EX-ADDICT SAMPLE

	Months 1-9			Months 10-18			Months 19-27			Months 28-36		
	Experimental-Control Differential	Control Group Mean		Experimental-Control Differential	Control Group Mean		Experimental-Control Differential	Control Group Mean		Experimental-Control Differential	Control Group Mean	
Total Sample	-1.3	21.5		-1.0	17.8		1.7	11.7		1.3	8.8	
Latest Follow-up Interview	#			#								
18-month	-8.8**	27.4		2.3	14.8		n.a.	n.a.		n.a.	n.a.	
27-month	-3.1	23.4		-9.1**	20.1		0.4	10.8		n.a.	n.a.	
36-month	8.8*	14.4		11.8**	14.7		4.3	13.5		1.3	8.8	

NOTE: Together, these subsamples include the same individuals as were included in the sample used to generate the data presented in Table V.1. This total sample has been partitioned according to the most recent scheduled interview completed. This partitioning of the analysis sample does not yield subgroups that are exactly the same as would the formal definition of cohorts introduced in Chapter II, in that it is based on interviews completed (as opposed to interviews assigned based on the date of enrollment in the demonstration).

#Experimental-control differentials within this subgrouping for this time period differ significantly from one another.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

n.a. = not applicable.

cohort, this decrease is evident in both periods but is significant only in the latter. Again, the decreases are observed among those groups in which rates of use among controls are relatively high and vice versa.

We have also investigated cohort differences in the daily use of heroin. For all cohorts, the program resulted in a decrease in daily heroin use in months 10 to 18, but this decrease was statistically significant only among those with more than 18 months of follow-up data. Thus, despite the fact that significantly more experimentals than controls in the 36-month cohort used some heroin in months 10 to 18, fewer experimentals used heroin on a daily basis.

These cohort differences in heroin use were not due to any demographic or background differences that we can identify. When the variation in program effects for the demographic subgroups presented in Table V.3 is controlled for, the cohort effects remain. Neither can they be accounted for entirely by differences in site characteristics or length of site operation. When variation in program effects by site is controlled for, the cohort differences in the first nine months are somewhat smaller and not significant, but the cohort differences are unchanged in the second nine months. When differences in program effects by length of site operation are controlled for, the cohort differences persist and, in fact, become larger.

As we discussed above, Supported Work could be expected to either reduce drug use through the provision of employment or increase drug use through the provision of additional income. It is possible that the increase in any heroin use among experimentals in the 36-month cohort was due to their increased earnings and that the decrease in daily heroin use

was due to their increased employment. The observed decreases in any heroin use and daily heroin use for the 18- and 27-month cohorts cannot be explained in this way since the program did not seem to increase their employment after month 9. Moreover, we are unable to explain the differences in behavior between the 36-month and the other cohorts in terms of measured characteristics.

2. Subgroup Effects on Cocaine Use

There were fewer significant subgroup differences in the program effects for cocaine use than for heroin use (see Tables V.5 and V.6). Further, for cocaine we do not observe the relationships we observed for heroin use and for employment, namely that the program tended to increase employment and reduce heroin use most for individuals at highest risk. The largest subgroup difference in cocaine use occurred among those who had previously used both heroin and cocaine regularly.[1/] In the first nine months, 37 percent of the experimentals with this history used cocaine, while only 19 percent of the comparable controls did so; in months 10 to 18, there was a similar effect. These differences did not, however, persist into the later periods when all the experimentals were out of the program. It may be that people with a history of regular cocaine use purchased cocaine with some of their additional income from the program.

Among those who had previously used heroin for fewer than 3 years, a higher percentage of experimentals than controls used cocaine during months 1 to 18. However, the differential rate of use was significant

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[1/] Regular use is defined as use for at least two months on a daily or almost daily basis.

TABLE V.5

PERCENTAGE REPORTING ANY USE OF COCAINE, BY SITE
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Ex-Addicts	2.6	16.2	2.6	15.3	1.5	15.2	-1.4	13.7
Site								
Chicago	3.6	13.0	1.3	18.0	2.0	22.5	7.6	2.4
Jersey City	2.8	15.1	-1.1	21.3	-0.9	15.9	-3.0	14.5
Oakland	-10.0	29.2	0.5	18.4	7.9	25.4	a/	a/
Philadelphia	5.5	16.2	9.3**	3.6	2.9	8.6	-6.0	19.4

NOTE: Together, these subsamples include the same individuals as were included in the sample used to generate the data presented in Table V.1. See note to Table IV.6.

a/ There are only four persons in this Oakland sample.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE V.6

PERCENTAGE REPORTING ANY USE OF COCAINE, BY INDIVIDUAL DEMOGRAPHIC AND BACKGROUND CHARACTERISTICS
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Ex-Addicts	2.6	16.2	2.6	15.3	1.5	15.2	-1.4	13.7
Years of Age								
Under 21	4.4	11.2	7.6	13.3	4.7	13.2	2.8	0.0
21 - 25	0.3	18.4	-2.2	16.2	5.0	13.6	-13.2*	16.1
26 - 35	5.3	13.5	3.7	15.9	-2.6	17.5	5.4	18.1
Over 35	-0.9	21.5	10.7	10.9	4.6	13.5	1.0	0.0
Sex	#							
Male	-0.7	17.8	1.5	16.6	0.9	15.9	-0.9	13.3
Female	16.0**	9.4	6.9	10.0	6.5	11.4	-10.0	16.6
Race/Ethnicity								
White, not Hispanic	-0.1	13.3	-0.4	10.5	-6.6	15.4	4.2	18.8
Black, not Hispanic	3.1	17.2	4.1	16.0	2.4	15.6	-2.2	12.3
Hispanic	1.7	11.1	-6.8	16.1	8.5	10.8	-12.2	26.0
Years of Education								
8 or less	3.2	14.4	6.2	13.2	1.6	17.0	-20.3	24.7
9 - 11	4.4	13.7	1.9	13.9	0.6	15.1	-1.1	12.9
12 or more	-1.6	22.2	2.2	19.3	4.2	14.5	3.8	10.5
Welfare and Food Stamp Receipt in Month Prior to Enrollment					#			
None	2.7	20.4	0.4	18.2	-3.1	19.3	-6.2	21.2
Some	2.4	12.0	4.7	12.4	6.4	11.3	1.2	7.4
Dependents								
None	4.7	14.5	6.0*	13.2	3.0	15.3	2.2	14.2
One or more	-0.8	18.9	-2.7	18.6	-0.2	15.0	-9.5	12.9
Months in Longest Job								
0	6.9	19.2	-5.6	18.6	-15.4	28.6	12.8	0.0
1 - 12	2.2	18.1	2.4	13.8	2.0	15.2	1.6	9.7
More than 12	2.4	14.6	3.6	16.0	3.1	14.0	-5.9	18.2
Weeks Worked in Year Prior to Enrollment a/								
0	0.7	17.2	3.2	15.3	1.1	15.2	0.0	12.8
5	1.6	16.7	2.9	15.3	1.4	15.2	-1.0	13.1
10	2.5	16.2	2.6	15.3	1.7	15.2	-1.6	13.6
Prior Drug Use	#							
Used heroin and cocaine regularly	18.0**	19.0	14.3**	11.3	1.2	20.3	-11.2	28.9
Used heroin regularly but not cocaine	1.0	16.3	1.8	16.3	0.5	15.1	-0.6	10.9
Did not use heroin regularly	-3.0	13.1	-3.8	13.5	9.3	11.5	-3.5	16.8

Table V.6 (continued)

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Years of Prior Heroin Use[a]			#					
1	3.1	18.8	8.0**	15.9	-1.4	15.9	-1.0	10.6
3	2.9	17.8	6.0*	15.7	-0.3	15.6	-1.4	11.1
9	2.3	14.9	-0.1	15.0	3.2	14.9	-2.6	12.6
Drug Treatment Last Six Months								
Not in treatment	-11.5	20.7	-3.3	18.1	-1.5	10.9	-3.8	18.3
Methadone maintenance	2.7	19.0	4.1	16.9	-2.7	20.5	-2.0	15.8
Drug-free program	7.1	12.9	5.8	11.5	7.1	10.6	-1.0	16.4
Other type of program	5.5	10.9	-1.3	14.6	9.7	8.7	-2.6	2.0
Enrollment in Treatment								
Voluntary	1.7	17.6	1.8	16.4	0.8	16.9	-3.7	14.7
Involuntary	10.9*	10.8	6.9	11.1	6.8	11.7	2.3	8.4
Addicts in Neighborhood[b]					#			
Few or none	1.7	13.9	5.3	11.7	-3.5	17.4	3.8	18.3
Many	4.2	18.1	0.7	18.3	5.8*	13.5	-6.2	9.0
Best Friend[b]								
Does not use drugs and is not involved in crime	1.4	14.8	4.9*	13.2	3.0	13.6	-2.4	12.4
Uses drugs or is involved in crime	7.6	20.3	-3.3	21.2	-2.1	19.8	1.7	16.8
Prior Arrests[a]								
0	#	24.6	1.3	14.8	4.9	12.7	-39.6*	49.0
4	0.6	13.6	3.4	14.8	0.6	16.6	-0.4	12.6
9	5.2*	15.1	2.8	15.3	1.4	15.6	2.3	9.6
Months Since Incarceration								
Never incarcerated	3.0	17.4	0.6	19.2	-0.1	20.0	11.2	3.6
12 or less	3.0	17.0	6.2	15.8	7.8	10.9	-11.8	18.2
More than 12	1.9	14.9	1.5	12.1	-1.1	15.1	-1.1	14.3

NOTE: See note to Table IV.6.

[a]These estimates of subgroup effects and means are based on a linear (or piecewise linear) specification of the sample characteristic, evaluated at the specific points.

[b]These results were obtained from a regression that did not include the full set of variables interacting status with background characteristics.

#Experimental-control differentials within this subgrouping for this time period differ significantly from one another.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

only for the second 9-month period. Since, among these short-term heroin users, fewer experimentals than controls used heroin during the period, the program may have induced those whose habituation to drugs was more recent to switch from heroin to cocaine. In the 19- to 27- and 28- to 36-month periods, however, these effects are no longer evident.

Although significant reductions in drug use and large, though not significant, reductions in cocaine use were observed among the Supported Work ex-offender sample (see Piliavin and Gartner, 1981), the subgroup differences in cocaine use by criminal history among the ex-addict sample do not offer any explanations for the differences in program effects between the two target groups.

C. MULTI-DRUG USE

Up to this point we have examined whether Supported Work had an impact on the use of specific drugs. In this section we examine whether the program had an effect on the combinations of drug used.[1/]

The ranking of drug-use patterns by their association with arrests indicates that using heroin with cocaine or amphetamines has more serious social consequences than using heroin without other drugs.[2/] In the first three 9-month periods, there are no experimental-control differences

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[1/]The evidence is examined by 9-month periods. Since there is some evidence that responses recording drug use in specific months (particularly in the month of interview) were likely to be misreported by experimentals (Dickinson, 1979a), we do not examine whether the respondents report using more than one type of drug within the same month.

[2/]See Dickinson (1979b). The interaction between heroin and cocaine or amphetamine use was significantly greater than the sum of their independent effects.

in multi-drug use patterns.[1/] In the 28- to 36-month period, however, significantly more experimentals than controls used heroin by itself (without cocaine, amphetamines, or barbiturates). Since there was no significant experimental-control difference in overall heroin use during that period, this was due, at least in part, to the (nonsignificant) experimental-control difference in combined use of heroin and cocaine or amphetamines. This result again reflects a persistent cohort difference. Among those in the 36-month cohort, more experimentals than controls used heroin. However, experimentals' pattern of use tended to be such that the estimated social costs of this more frequent use was not significantly different from that of the less frequent use of heroin by controls, because controls were more likely than experimentals to use heroin in combination with other drugs.

D. EFFECTS ON ALCOHOL USE

In months 1 to 9 significantly fewer experimentals than controls used alcohol daily. As shown in Table V.7, approximately 12 percent of the experimentals and 16 percent of the controls reported drinking alcohol daily. In the later periods, however, the effect is reversed: a higher percentage of experimentals than controls used alcohol daily, although results for these later periods are not statistically significant.

The effect observed in the first nine months is primarily the result of a large difference in Jersey City.[2/] In that site,

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[1/] These figures are presented in Appendix Table A.9.

[2/] If Jersey City is excluded from the overall comparison, the experimental-control difference in months 1 to 9 is -1.5 percentage points and not significant.

TABLE V.7

PERCENTAGE REPORTING DAILY USE OF ALCOHOL, BY SITE
EX-ADDICT SAMPLE

	Months 1-9			Months 10-18			Months 19-27			Months 28-36		
	Experimental-Control Differential	Control Group Mean		Experimental-Control Differential	Control Group Mean		Experimental-Control Differential	Control Group Mean		Experimental-Control Differential	Control Group Mean	
All Ex-Addicts	-4.1*	15.7		4.7	12.1		4.4*	13.3		3.4	13.1	
Site												
Chicago	-4.3	9.0		3.3	16.3		-0.9	17.8		-6.4	13.1	
Jersey City	-8.2**	24.0		6.4*	11.3		8.8**	13.5		-6.6	13.5	
Oakland	6.5	14.7		4.0	13.6		-5.8	29.4		a/	a/	
Philadelphia	-1.9	10.0		3.8	9.2		4.3	7.7		10.8*	10.2	

NOTE: See note to Table IV.6. The precise wording of the question concerning alcohol use in the follow-up interviews is "Do you ever drink alcohol and, if so, how often?" Thus, responses may not apply precisely to the entire 9-month period.

a/ There are only four persons in this Oakland sample.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

both experimentals and controls had relatively high rates of daily alcohol use, but the use among experimentals was significantly lower. The higher percentage of experimentals than controls reporting daily use in subsequent periods was due in part to a decrease in reported use among controls (particularly controls in Jersey City) and in part to an increase in reported used among experimentals in all sites.

This increase in daily alcohol use among experimentals after program participation might have been expected if experimentals had altered their drug use patterns.[1/] However, Supported Work did not have an overall impact on the experimentals' drug use. Furthermore, the subgroups for which there were significant decreases in drug use were not, in general, the same subgroups for which there was an increase in daily alcohol use.[2/] The experimentals' increase in daily alcohol use during months 10 to 36 was particularly evident among those who were 21 to 25 years old and among those who were in drug-free treatment programs (Table V.8). There was no clear pattern of experimental effects on drug use for individuals with other characteristics.

Another possible explanation for the increase in alcohol use involves the relationship between alcohol use and employment. This possibility is discussed below.

1/ Several studies have found that those who remain abstinent from heroin following treatment are more likely to use alcohol. For example, see O'Donnell (1969).

2/ We also examined whether there was an increase in alcohol use among those reporting current heroin use on the baseline interview, but found a significant experimental decrease in both daily alcohol use and heroin use in months 10 to 18 for this subgroup. However, since we have some evidence that current use is less accurately reported than any use (Dickinson, 1979a), we do not place much reliance on these results.

TABLE V.8

PERCENTAGE REPORTING DAILY USE OF ALCOHOL, BY INDIVIDUAL DEMOGRAPHIC AND BACKGROUND CHARACTERISTICS
EX-ADDICT SAMPLE

	Months 1-9	Months 10-18			Months 19-27			Months 28-36		
	Experimental-Control Group Differential	Experimental Group Mean	Control Group Mean	Experimental-Control Group Differential	Experimental Group Mean	Control Group Mean	Experimental-Control Group Differential	Experimental Group Mean	Control Group Mean	Experimental-Control Group Differential
All Ex-Addicts	-4.1*	15.7	12.1	4.4*	13.3	13.3	3.4	13.1	15.7	4.7
Years of Age							#			
Under 21	6.0	4.1	9.6	8.3	12.9	12.9	-16.3[a/]	16.1	4.1	2.9
21 - 25	-5.3	15.2	12.5	6.5	11.1	11.1	17.5**	10.4	15.2	7.8**
26 - 35	-4.8	17.8	12.5	0.7	14.1	14.1	-3.7	16.1	17.8	2.5
Over 35	-3.4	15.6	10.8	7.8	17.8	17.8	-10.7[a/]	8.9	15.6	3.1
Sex										
Male	-5.3**	17.3	13.7	4.2*	12.4	12.4	3.9	13.6	17.3	3.8
Female	0.9	8.9	5.3	5.2	17.6	17.6	-5.3	10.0	8.9	8.0
Race/Ethnicity										
White, not Hispanic	-8.8	16.3	13.2	-0.1	13.7	13.7	-20.7	23.5	16.3	1.1
Black, not Hispanic	-2.7	14.8	12.2	4.8*	13.4	13.4	4.0	11.7	14.8	4.5*
Hispanic	-10.5	23.7	9.2	6.3	12.3	12.3	23.2	15.4	23.7	11.2
Years of Education										
8 or less	-13.6*	20.2	12.8	3.1	12.7	12.7	-4.6	24.1	20.2	4.6
9 - 11	-3.4	16.3	12.4	4.6	14.8	14.8	6.3	10.4	16.3	4.7*
12 or more	-1.0	12.3	11.1	4.4	11.7	11.7	-2.6	14.4	12.3	4.4
Welfare and Food Stamp Receipt in Month Prior to Enrollment										
None	-1.2	15.9	10.1	7.8**	13.4	13.4	6.5	9.6	15.9	7.6*
Some	-7.1**	15.4	14.1	1.1	13.2	13.2	-0.5	16.0	15.4	1.6
Dependents										
None	-3.7	15.8	11.3	4.0	13.8	13.8	-0.8	15.5	15.8	4.1
One or more	-5.0	15.6	13.4	4.9	12.6	12.6	8.5	9.1	15.6	5.1
Months in Longest Job										
0	-13.6	19.8	7.5	9.9	7.9	7.9	22.4	0.0	19.8	13.2
1 - 12	-3.1	12.5	12.4	5.2	13.2	13.2	1.1	16.9	12.5	3.5
More than 12	-3.8	17.4	12.3	3.2	13.8	13.8	1.8	13.0	17.4	4.5
Weeks Worked in Year Prior to Enrollment[b/]										
0	-5.9**	17.7	12.1	2.3	12.6	12.6	-0.5	10.5	17.7	5.7**
5	-5.0**	16.7	12.1	3.3	12.9	12.9	0.7	11.5	16.7	5.2**
10	-4.2**	15.7	12.1	4.2*	13.3	13.3	1.9	12.5	15.7	4.6**
Prior Drug Use										
Used heroin and cocaine regularly	-5.1	14.7	14.4	2.2	11.7	11.7	-21.5[a/]	12.8	14.7	-1.1
Used heroin regularly but not cocaine	-4.6*	15.6	11.0	4.7*	14.2	14.2	5.8	13.6	15.6	6.3**
Did not use heroin regularly	-1.0	17.2	15.0	4.6	9.8	9.8	6.9	9.5	17.2	1.1

Table V.8 (continued)

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Years of Prior Heroin Use[b]								
1	-3.9	15.6	4.0	11.5	2.5	14.6	18.6**	0.0
3	-4.0	15.7	4.2	11.7	3.2	14.1	13.1**	4.0
9	-4.2*	15.9	5.0*	12.4	5.2*	12.7	-3.2	18.5
Drug Treatment Last Six Months							#	
Not in treatment	-21.3**[a]	20.4	4.6	10.8	-3.9	16.8	15.6	21.8
Methadone maintenance	-1.5	12.7	1.3	14.2	7.0	12.6	2.8	9.9
Drug-free program	3.3	12.6	7.6	10.9	8.1	7.4	35.7**	-1.6
Other type of program	-8.9*	23.3	9.3*	8.8	-3.0	20.4	-24.8**	35.0
Enrollment in Treatment								
Voluntary	-1.0	14.4	4.1	11.5	5.3	13.1	8.6	13.3
Involuntary	-4.5	20.0	6.4	12.7	4.2	15.0	-4.5	16.5
Addicts in Neighborhood[c]								
Few or none	-5.4*	16.1	2.5	11.6	4.7	10.5	6.3	10.1
Many	-2.9	15.4	7.0**	12.5	4.1	15.6	1.6	16.2
Best Friend[c]								
Does not use drugs and is not involved in crime	-4.2*	15.1	2.5	12.2	4.1	13.6	3.4	12.7
Uses drugs or is involved in crime	-3.5	17.4	11.7**	11.7	5.1	12.3	5.4	14.1
Prior Arrests[b]								
0	-16.6**	17.8	8.9	7.9	3.6	8.3	20.3	-3.3
4	-4.2*	12.5	3.6	12.8	4.6	13.9	-3.5	15.7
9	-2.7	11.5	4.1*	12.6	4.4*	13.9	0.0	15.2
Months Since Incarceration			#					
Never incarcerated	-5.4	19.0	-4.2	15.4	1.4	10.9	-24.3	27.3
12 or less	3.2	12.0	6.4	11.6	10.7**	13.2	-0.3	20.5
More than 12	-8.4**	16.0	9.7**	10.1	1.9	14.9	11.3*	5.8

NOTE: See note to Table IV.6.

[a] Negative point estimates of experimental or control group means arise because linear regression analysis rather than probit analysis was used.

[b] These estimates of subgroup effects and means are based on a linear (or piecewise linear) specification of the sample characteristic, evaluated at the specified points.

[c] These results were obtained from a regression that did not include the full set of variables interacting status with background characteristics.

#Experimental-control differentials within this subgrouping for this time period differ significantly from one another.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

E. ALCOHOL USE, DRUG USE, AND EMPLOYMENT

Up to this point we have examined the relationship between drug or alcohol use and employment by comparing experimental-control differences in drug and alcohol use in the four 9-month observation periods with experimental-control differences in employment in the corresponding periods. We noted that during months 1 to 9, experimentals were much more likely to be employed than were controls (the differential was in the neighborhood of 45 percentage points), but that this difference was due largely to experimentals' participation in Supported Work. As experimentals left the program this differential decreased to zero. Then in the final 9-month period experimentals' employment increased again. As pointed out in Chapter IV, this observed increase was due primarily to unmeasured characteristics in the subsample of ex-addicts who were interviewed over the full 3-year post-enrollment period. In this section, we will examine directly the use of drugs and alcohol among those who were employed and among those who were not employed.[1/]

The types of experimental-control comparisons we present in this section are somewhat different from those presented in previous sections. When we examine experimental-control differences for the entire sample or for subgroups that are defined by pre-enrollment characteristics, random assignment assures us that we are comparing individuals who are not systematically different except for their participation in Supported Work. When we examine differences among groups selected on post-enrollment characteristics, such as employment, there is no such assurance. This is

[1/] We examine the relationship between drug use and crime in the next chapter.

particularly true in the first nine months. In that period, only 50 percent of the controls were employed, but 95 percent of the experimentals were employed. Therefore, it is quite likely that employed controls differed systematically from employed experimentals in this period. Thus, the reader should be aware of these potential selection biases in interpreting the results in this section.

We noted in the previous section that Supported Work resulted in a a decrease in daily alcohol use in months 1 to 9 and an increase in daily alcohol use in the later periods. The low initial and only gradually increasing stress of Supported Work may have lessened the daily use of alcohol among participants relative to controls. However, when experimentals tried to move into nonprogram jobs, they may have experienced more stress and may have increased their alcohol consumption as a consequence. Table V.9 shows that among the controls there was no consistent relationship between alcohol use and employment. However, in every period, employed experimentals were more likely to consume alcohol than were unemployed experimentals. Among the employed, the experimental-control differential in daily alcohol use was somewhat larger in the third and fourth 9-month periods than it had been earlier. For example, in months 19 to 27 the differential in alcohol use among those employed was 6.3 percentage points. In months 10 to 18, the differential was 2.2 percentage points, but one-third of the experimentals were still employed in Supported Work at some time in that period. There is some weak evidence, therefore, that Supported Work may have altered the relationships between alcohol use and post-program employment.

TABLE V.9

PERCENTAGE REPORTING DAILY USE OF ALCOHOL, BY CURRENT EMPLOYMENT STATUS

EX-ADDICT SAMPLE

Employment Status	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Not Employed	-9.1	17.4	6.5*	9.1	1.4	14.1	1.9	12.1
Employed	-3.3	14.7	2.2	15.1	6.3*	12.7	3.5	13.5
(Percentage Not Employed)[a]	(-44.4)**	(49.6)	(-13.5)**	(46.7)	(-3.3)	(44.3)	(-10.6)*	(43.9)

NOTE: These data are not regression-adjusted.

[a]These data may differ somewhat from those reported in Chapter IV because of the different samples included in the analysis.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

Table V.10, which contrasts drug use among those who were employed with use among those who were not employed, shows only small differences among controls: generally there was only a slightly higher incidence of drug use among unemployed controls than among those who were employed. In months 10 to 18, for example, 32 percent of those controls who were employed, versus 35 percent of those who were not employed, used drugs. Moreover, the program had no impact on the relationship between drug use and employment. There were no significant experimental-control differences in drug use among those who were employed, despite substantial differences in the type of employment obtained in the early periods. There were also no significant experimental-control differences in the extent of drug use among those not employed.

Thus, these results provide little support for the commonly held belief that providing employment to ex-addicts will help reduce their drug use and no support for the hypothesis that Supported Work would be especially effective in reducing drug use among ex-addicts. We found that the drug use among those who were employed was somewhat less than the drug use among those not employed, but that there were no significant experimental-control differences in drug use among either group.

F. SUMMARY AND CONCLUSIONS

Overall, Supported Work had no effect on the drug use of ex-addicts. Despite the fact that significantly more experimentals than controls were employed in the early periods (44 percentage points more during months 1 to 9) and in the last nine months (11 percentage points), there were no significant differences in the use of heroin, other opiates, cocaine, or other types of drugs in any period for the sample as a whole;

TABLE V.10

OTHER THAN MARIJUANA OR ALCOHOL, BY CURRENT EMPLOYMENT STATUS
EX-ADDICT SAMPLE

PERCENTAGE REPORTING USE OF ANY DRUG, OTHER THAN MARIJUANA OR ALCOHOL, BY CURRENT EMPLOYMENT STATUS
EX-ADDICT SAMPLE

Employment Status	Months 1-9 Experimental-Control Differential	Months 1-9 Control Group Mean	Months 10-18 Experimental-Control Differential	Months 10-18 Control Group Mean	Months 19-27 Experimental-Control Differential	Months 19-27 Control Group Mean	Months 28-36 Experimental-Control Differential	Months 28-36 Control Group Mean
Not Employed	9.1	40.9	2.5	35.2	-0.2	30.8	-0.7	20.0
Employed	1.0	36.0	0.9	31.5	-0.2	26.5	1.2	22.8
(Percentage Not Employed)a/	(-44.3)**	(50.6)	(-13.4)**	(48.6)	(-4.1)	(47.6)	(-10.7)*	(47.0)

NOTE: These data are not regression-adjusted.

a/These data may differ somewhat from those reported in Chapter IV because of the different samples included in the analysis.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

nor were there any significant experimental-control differences in variables that measure the relationship between drug use and employment directly.

There were some significant effects for certain subgroups. For heroin use, these differences suggest a pattern also found in the employment results: the program tended to have a more favorable impact among those who would have done less well on their own. For the early periods, there were also some significant differences among cohorts in the estimates of program impacts on heroin use. For cocaine use, there was some evidence that during months 1 to 18 the program increased the cocaine use of those who had previously been regular users of heroin and cocaine and of those who had used heroin for a relatively short time.

The program did significantly affect the daily use of alcohol. In the first nine months, significantly fewer experimentals than controls used alcohol daily, but this effect was largely concentrated in the Jersey City site. In the later periods, a higher percentage of experimentals than controls used alcohol daily. However, this increase did not appear to be related to changes in drug use. The experimental-control difference in alcohol consumption was somewhat larger among those who were employed, suggesting that the program may have changed the relationship between alcohol use and employment among experimentals.

CHAPTER VI

PROGRAM EFFECTS ON CRIME AND CRIMINAL JUSTICE EXPERIENCES

Supported Work is hypothesized to reduce the criminal activity of ex-addicts through two main mechanisms. First, the program may reduce economically motivated crime by increasing the participants' employment opportunities. Raising the economic payoff of work relative to the payoff of crime may lead participants to shift to legal work, and the additional income resulting from participation may reduce ex-addicts' willingness to take the risks associated with crime. Second, Supported Work may increase the non-economic benefits of work relative to those of crime. By providing opportunities for participants to succeed at conventional behavior, Supported Work may counteract the peer and cultural support for criminal activity found in the ex-addict's previous environment.

In this chapter, we begin by describing measures of criminal activity and their validity. We then investigate the effects of Supported Work on several indicators of criminal activity for the ex-addict sample as a whole and for various subgroups. The final section examines whether Supported Work had an effect on the relationships between crime and drug use and between crime and employment.

A. MEASURES OF CRIMINAL ACTIVITIES

When measures of criminal activity were chosen, three options were considered.[1/] The first was the respondents' own reports of the crimes

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[1/] See Piliavin and Gartner (1981) for further discussion of measures of criminal activity.

that they had committed. The second was the respondents' reports of contacts with the criminal justice system, including arrests, convictions, and incarcerations. The third was officially recorded criminal justice contacts.

Of the three, we place the least reliance on self-reports of criminal behavior. Other studies have found these to provide less adequate measures of criminal activity than do self-reports of arrest,[1/] and our own experience with such measures in this study confirmed that finding.

We place the greatest emphasis on the self-reports of contacts with the criminal justice system, particularly arrests. While clearly arrests are not perfectly correlated with criminal activity, they are less dependent on the abilities and workload of criminal justice personnel than are convictions and incarcerations. Further, due to delays in the adjudication of arrests, the disposition of some arrests will not have occurred during the period covered by the follow-up interviews.

While officially recorded criminal justice contacts were conceptually preferable to self-reports, administrative and legal problems prohibited us from obtaining such records for all sample members. We did obtain official records for a subsample of 774 ex-offenders and ex-addicts from San Francisco, Oakland, and Hartford.[2/] When these official records were compared with interview data, it was found that respondents substantially underreported their arrests, but that there were no

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[1/] It should be noted that we are not assuming that all those who are arrested are guilty, but only that the relationsip between arrests and criminal activity is the same for experimentals and controls.

[2/] See Schore et al. (1979).

experimental-control differences in the extent of underreporting. This evidence suggests that any experimental effects on self-reported arrests are not due to a differential underreporting.[1/]

B. OVERALL EFFECTS BY TIME PERIOD

Supported Work did substantially reduce the criminal activity of the ex-addicts, as measured by their contacts with the criminal justice system. The results presented in Table VI.1 indicate that, in every time period, a lower percentage of experimentals than controls were arrested. This difference was significant in months 10 to 18, when 19 percent of the controls were arrested compared to 13 percent of the experimentals. The number of arrests for experimentals was also somewhat less than for controls, although these differences were smaller and not significant.

Table VI.1 also presents the effects of Supported Work on arrests for two specific offenses: drug-related crimes and robbery. Although the program did not significantly reduce the use of drugs, in months 10 to 18 and 19 to 27, Supported Work significantly reduced the numbers who were arrested for drug-related crimes.[2/]

We considered the effect of the program on robbery separately because it has a very high social cost[3/] and because other studies have found that drug users are particularly likely to commit this type

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[1/] However, proportional underreporting will reduce the size of the experimental differences and, in the case of binomial outcomes, tests of statistical significance will be conservative.

[2/] Individuals who were arrested for multiple charges are included under the most serious charge.

[3/] For example, see Kemper, Long, and Thornton (forthcoming), who estimate that the social cost of an arrest for robbery is over $13,000.

TABLE VI.1

ARRESTS, CONVICTIONS, AND INCARCERATIONS
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Percentage with any Arrest	-2.5	19.5	-5.9**	18.6	-2.3	18.2	-5.0	13.5
Number of Arrests	0.00	0.23	-0.09	0.23	-0.04	0.22	-0.04	0.15
Percentage with Robbery Arrests[a]	-3.4**	4.5	-2.0**	3.3	0.5	2.2	-0.9	1.9
Number of Robbery Arrests[a]	-0.04**	0.05	-0.02**	0.03	0.01	0.02	-0.01	0.02
Percentage with Drug-related Arrests[b]	-1.4	3.6	-3.0**	4.1	-2.4*	4.9	-2.2	3.5
Percentage Convicted	-0.2	8.7	-3.4*	9.6	-1.6	8.0	-3.4	6.5
Percentage Incarcerated	0.3	10.3	-4.9**	16.2	-2.9	18.6	-5.9	21.2
Number of Weeks Incarcerated	-0.20	1.81	-1.40**	2.90	-0.66	4.14	-1.20	4.36

NOTE: See note to Table IV.1. All data pertain to the full sample.

[a] Robbery arrests are defined as those for which robbery was the most serious charge. Only murder and felonious assault are considered to be more serious than robbery.

[b] Drug-related arrests are defined as those for which narcotics law violation is the most serious charge. More serious charges include murder, felonious assault, robbery, burglary, larceny, motor-vehicle theft and other property crimes, and other crimes against persons.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

of offense.[1] In the first and second 9-month periods, Supported Work did have a significant effect on robbery arrests--in months 1 to 9, for example, 4.5 percent of the controls were arrested for robbery compared to only 1.1 percent of the experimentals. (See Tables A.24 and A.25 for a more detailed breakdown of arrests by type of charge.)

The proportional reductions in arrests for robbery and drug-related crimes were greater than the proportional reduction in arrests in general. As we discussed in Chapter III, the additional income provided by Supported Work may make its participants less willing to take the risks of crime. If this is the case, then we would expect Supported Work to have the greatest effect on the riskiest crimes. As part of the baseline interview, respondents were asked what they felt the risks were of being caught for the various types of crime they had committed. Among the ex-addicts, 53 percent reported that the risks of being caught for robbery were high, and 47 percent reported that the risks of drug offenses were high. Only a third of the sample reported that property crimes other than robbery or burglary, which were the types of offenses most commonly committed by ex-addicts in the six months prior to enrollment, carried high risks. Thus, Supported Work did result in a greater reduction in arrests for crimes that were judged to have the highest risks.

Supported Work also had an effect in reducing the percentage of ex-addicts who were convicted and incarcerated. These results were largest and statistically significant in months 10 to 18, when 9.6 percent of the controls were convicted compared to 6.2 percent of the experimentals. Over

[1] See Greenberg and Adler (1974).

16 percent of the controls spent some time in jail or prison during this period compared to 11.3 percent of the experimentals.

Up to this point we have considered the impact of Supported Work on the experimentals' contacts with the criminal justice system. The respondents were also asked explicitly whether they committed any crimes or made money illegally and, if so, how much they made. However, because there is likely to be more underreporting of actual crimes than of arrests and convictions, we place less reliance on these measures and present only illustrative results. Supported Work did tend to reduce illegal activity, as measured by self-reported crimes, although the differences were small and not significant.[1/] For example, 21 percent of the controls compared to 19 percent of the experimentals reported that they made money illegally in months 1 to 9. In that period, the average control reported making $52 per week illegally, while the average experimental reported making $39 per week illegally. While these numbers are subject to a great deal of error, they allow us to calculate the experimental-control difference in total income, both legal and illegal. In months 1 to 9, experimentals received an average of $585 per month, while controls received $503 from legal and illegal sources. It should be noted that this difference of $82 was less than the difference of $134 observed in legal income alone.

In summary, Supported Work resulted in fewer experimentals than controls being arrested, convicted, or incarcerated. These results for contacts with the criminal justice system are reinforced by the results

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1/ As noted previously, if there is proportional underreporting of whether illegal income was received, this will result in an understatement of both the size of the experimental-control differences and of the significance of those differences.

for self-reported criminal activity. The effect on criminal justice contacts tended to be largest and statistically significant in months 10 to 18, which is a period when approximately one-third of the experimentals were still participating in the program. There were also significant reductions in the percentage arrested for drug offenses in months 19 to 27, and the point estimates for the other measures suggest that there may be a continued effect on criminal activity in the later periods when all of the participants have left the Supported Work program.

C. SUBGROUP DIFFERENCES BY TIME PERIOD

Tables VI.2 and VI.3 present the site and demographic subgroup differences in the percentage who were arrested in each period. In examining the subgroup differences in hours of work and in drug use we found a consistent pattern whereby Supported Work was more effective for those who did less well on their own. There is some evidence of this pattern for the subgroup differences in arrests, although the results are less consistent than those found for the other outcomes.

There were some site differences in the effects on criminal activity, but the pattern of results was inconsistent across time periods. Also, the site differences in arrests generally did not coincide with the site differences in employment. The experimental-control difference in hours worked was consistently larger in Chicago than in other sites, but the difference in arrests was not largest in Chicago, except in the last period.

The site differences found for heroin use were reflected in the site differences for arrests. In the first and second 9-month periods, the program in Oakland did result in decreases in heroin use among

TABLE VI.2

PERCENTAGE WITH ANY ARRESTS, BY SITE
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Ex-Addicts	-2.5	19.5	-5.9**	18.6	-2.3	18.2	-5.0	13.5
Site								
Chicago	-4.3	17.4	-1.4	16.7	-4.2	16.4	-11.8	16.9
Jersey City	-2.7	19.3	-4.0	12.8	-6.5	19.9	-2.6	11.1
Oakland	-8.9	26.8	-13.6*	41.4	5.2	22.2	a/	a/
Philadelphia	1.5	19.6	-10.3**	21.0	2.0	16.6	-3.6	15.1

NOTE: Together these subsamples include the same individuals as were included in the sample used to generate the data presented in Table VI.1. See the note to Table IV.6.

a/ There are only four persons in this Oakland sample.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE VI.3

PERCENTAGE WITH ANY ARRESTS, BY INDIVIDUAL DEMOGRAPHIC AND BACKGROUND CHARACTERISTICS
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Ex-Addicts	-2.5	19.5	-5.9**	18.6	-2.3	18.2	-5.0	13.5
Years of Age								
Under 21	-3.0	32.6	4.4	16.2	4.9	15.7	3.6a/	6.7
21 - 25	-3.3	19.9	-9.0**	22.8	-2.5	19.3	4.1	11.6
26 - 35	0.0	18.1	-3.6	16.7	-0.7	18.1	-16.3**	18.8
Over 35	-8.1	16.2	-10.4	13.9	-11.4	15.9	-4.5	4.6
Sex								
Male	-1.9	20.5	-6.3**	20.7	-2.1	19.5	-6.9	14.7
Female	-4.6	15.3	-4.4	10.6	-3.5	11.5	-2.8	7.8
Race/Ethnicity					#			
White, not Hispanic	-4.7	19.9	-1.9	13.6	-21.0	33.1	-6.7	9.9
Black, not Hispanic	-2.7	19.6	-7.0**	19.8	2.4	14.7	-5.8	13.2
Hispanic	3.8	15.1	-2.9	16.3	-16.3*	27.3	-15.8a/	31.1
Years of Education								
8 or less	3.3	20.6	4.1	14.7	-2.2	-14.6	-11.3	14.7
9 - 11	-1.9	18.6	-8.2**	21.3	-3.5	21.6	-10.8**	19.4
12 or more	-6.3	20.6	-6.6	15.5	0.1	13.1	6.3	0.2
Welfare and Food Stamp Receipt in Month Prior to Enrollment								
None	-6.4	23.5	-9.2**	20.1	#			
Some	1.9	15.1	-2.4	17.1	-8.6	21.4	-13.0**	16.7
Dependents					4.0	14.9	-0.3	11.3
None	-1.1	19.2	#					
One or more	-4.5	20.0	-2.0	16.8	-2.1	17.1	-5.7	15.7
Months in Longest Job			-12.3**	21.7	-2.7	19.9	-7.7	10.7
0	8.0	6.2	-18.3	19.5	-16.2	34.7	-14.5a/b/	13.8
1 - 12	-1.1	19.5	-8.2**	20.3	-6.8	20.3	-8.8	12.3
More than 12	-4.5	20.8	-3.1	17.3	2.1	15.3	-4.3	14.9
Weeks Worked in Year Prior to Enrollment c/								
0	-2.7	19.3	-3.5	16.5	-3.6	20.1	-9.6*	16.2
5	-2.6	19.4	-4.8*	17.6	-3.0	19.2	-8.4*	15.3
10	-2.4	19.5	-6.1**	18.8	-2.4	18.2	-7.2*	14.5
Prior Drug Use			#					
Used heroin and cocaine regularly	-8.0	23.9	1.3	13.5	3.0	16.9	-26.1**b/	23.9
Used heroin regularly but not cocaine	-3.9	20.0	-4.2	17.3	-3.5	18.7	-3.6	13.6
Did not use heroin regularly	9.3	13.2	-20.1**	29.4	-0.5	16.6	-4.2	5.6

Table VI.3 (continued)

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Drug Treatment in Last Six Months	#							
Not in treatment	-5.7	21.2	-9.9	19.4	2.7	11.4	-30.3*[b]	29.4
Methadone maintenance	-4.9	22.4	-1.6	18.9	-1.8	18.0	-4.4	11.3
Drug-free program	11.0*	8.9	-12.6**	20.7	0.7	16.6	6.2	12.4
Other type of program	-9.1	22.6	-7.3	15.4	-10.2	24.4	-14.7	16.8
Prior Arrests[c]					#			
0	-2.3	16.2	-3.3	7.6	1.7	6.7	-9.2	12.3
4	-2.4	17.8	-4.6*	18.1	-0.2	18.0	-7.7*	14.7
9	-2.5	19.8	-6.2*	19.9	-2.6	19.4	-6.0	14.0
Months Since Incarceration								
Never incarcerated	0.2	16.8	-7.7*	15.2	-1.8	17.6	-24.7**	26.9
12 or less	-4.2	26.8	-1.8	23.3	-2.9	20.6	9.5	4.6
More than 12	-4.1	15.8	-7.7	16.1	-2.2	16.4	-8.3	13.5
Parole or Probation at Enrollment					#			
Not on parole or porbation	-0.1	16.5	-5.2	15.5	-7.3*	20.7	-2.0	9.6
On parole or probation	-4.9	22.7	-6.8*	22.0	2.9	15.5	-10.9*	18.3

NOTE: See note to Table IV.6.

[a] These data are based on a sample of fewer than 20 persons.

[b] Negative point estimates of experimental or control group means arise because linear regression analysis rather than probit analysis was used.

[c] These estimates of subgroup effects and means are based on a linear (or piecewise liner) specification of the sample characteristic, evaluated at the specified points.

#Experimental-control differentials within this subgrouping for this time period differ significantly from one another.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

experimentals relative to controls. The effect on arrests was also largest for Oakland in these two time periods. However, these favorable results with respect to drug use and arrests did not persist into the third 9-month period, when virtually all the participants had left the program.

With respect to individual characteristics, there was a consistent pattern in which the program had a greater impact on arrests among those who were not receiving welfare at enrollment. The pattern was evident for all time periods and statistically significant for months 10 to 18 and 27 to 36. Since the program would result in a larger change in income among those who were not receiving welfare than among those who were, these results suggest that the extra income provided by the program may have been an important mechanism in reducing arrests. The program also tended to be more effective for those with at least one dependent. For example, in months 10 to 18, 22 percent of the controls with dependents were arrested, as compared to 9 percent of the experimentals.

There is a common belief among correctional authorities that older offenders "burn out" and are prepared to begin conventional lives if given the opportunity. There is also considerable evidence that crime-control programs for youth are not effective.[1] The results by age presented in Table VI.3 tend to be consistent with this literature. In general, the arrest rates among those over 35 were lower than average, and the experimental-control differences in arrests were generally considerably larger for these older ex-addicts. The program tended to be less effective than average for those under 21. In fact, among these younger sample

[1] See Piliavin and Gartner (1979).

members a higher percentage of experimentals than controls was arrested in each period except months 1 to 9, although these differences are not significant. It should be noted that the results by age did not follow the overall pattern of Supported Work results. In this case the program had the most favorable impact on those with the lowest risks of being arrested.

Piliavin and Gartner (1981) reported that Supported Work did not significantly affect criminal activity among ex-offenders. The subgroup differences presented in Table VI.3 do not offer an explanation for this difference between the ex-addict and ex-offender target groups, because there is no strong evidence that the effectiveness of the program in reducing arrests varied consistently by criminal history. Furthermore, the pattern noted earlier of individuals at greatest risk benefitting most from Supported Work does not apply to crime: ex-addicts were less likely to be arrested than were ex-offenders; yet the program was more effective in reducing arrests among ex-addicts.[1/] However, an analysis of the subgroup differences for the likelihood of arrest among the ex-offender group revealed a (nonsignificant) reduction in arrests for experimentals who used heroin regularly prior to enrolling in Supported Work. These results suggest that it may be the prior use of heroin rather than severity of criminal history that relates to the effect of Supported Work on crime.

If Supported Work reduced criminal activity solely because it increased employment, then we would expect that those subgroups with the largest

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[1/] However, the probabilities of being arrested for a robbery and for a drug offense were quite similar between the two target-group samples.

decrease in arrests. It was noted above that this was not the case for the site differences, nor was it the case for other subgroup differences in arrests.[1/] We have found some evidence that the additional income from the program was an important mechanism by which Supported Work affected criminal activity, but the lack of consistency between the subgroup differences in employment and arrests suggests that economic factors are not the sole mechanisms.

D. CUMULATIVE EFFECTS ON CRIMINAL ACTIVITY

Examining the experimental-control differences in each of four 9-month periods creates a potential problem for gauging the effects of Supported Work on criminal activity. If an individual is arrested for a serious crime early in the study period, that person may not be free to commit crimes in the later periods.[2/] To abstract from this potential difficulty, in Table VI.4 we also present the cumulative differences in the measures of criminal activity for the first 18 months, the first 27 months, and the full 36-month period following enrollment.[3/]

[1/] The one exception occurs for those with at least one dependent: among those individuals, the program resulted in a larger than average increase in employment and a larger than average reduction in arrests.

[2/] Differences in time spent in jail or prison had only a small effect on the experimental-control differences in hours worked. For example, in months 28 to 36 the observed difference in hours worked per month was 17.1. When time in prison is adjusted for, the experimental-control difference in hours worked per month not in prison is 16.5.

[3/] The cumulative results are estimated on somewhat smaller samples than are the results by 9-month period. The 1- to 36-month sample, for example, includes only those for whom we have all four follow-up interviews.

TABLE VI.4

CUMULATIVE ARRESTS, CONVICTIONS, AND INCARCERATIONS
EX-ADDICT SAMPLE

	Months 1-18			Months 1-27			Months 1-36		
	Experimental	Control Group	Differential	Experimental	Control Group	Differential	Experimental	Control Group	Differential
Percentage with any Arrest	25.4	33.5	-8.2	33.3	43.3	-10.9**	35.0	53.1	-18.1**
Number of Arrests	0.40	0.48	-0.08	0.51	0.70	-0.19**	0.58	1.01	-0.43**
Months to first arrest among those arrested	9.0	8.9	0.1	11.8	11.5	1.3	12.3	12.8	4.5
Percentage with Robbery Arrests[a]	1.6	7.5	-5.2**	2.2	8.8	-6.9**	2.2	13.4	-13.2**
Number of Robbery Arrests[a]	0.02	0.08	-0.06**	0.03	0.11	-0.08**	0.02	0.15	-0.14**
Percentage with Drug-related Arrests[b]	3.7	7.9	-3.8**	6.0	10.5	-4.6**	6.8	14.0	-7.2
Percentage Convicted	13.5	17.8	-4.3*	16.4	22.1	-5.7*	19.2	32.9	-13.6*
Percentage Incarcerated	15.8	20.2	-4.4*	20.3	28.4	-8.1**	22.5	36.6	-14.1*
Number of Weeks Incarcerated	3.9	5.4	-1.5*	5.7	9.7	-4.0**	6.7	13.8	-7.1*

NOTE: See note to Table IV.1. All data pertain to the full sample. The sample for each period includes individuals who have completed all relevant interviews. The 1-18 month sample includes all individuals who completed at least the baseline, 9-month, and 18-month interviews; the 1-27 month sample includes those who completed at least the baseline, 9-month, 18-month, and 27-month interviews; and the 1-36 month sample includes those who completed the baseline and all four follow-up interviews.

[a]/Robbery arrests are defined as those for which robbery was the most serious charge. Only murder and felonious assault are considered more serious than robbery.

[b]/Drug-related arrests are defined as those for which narcotics law violation is the most serious charge. More serious charges include murder, felonious assault, robbery, burglary, larceny, motor-vehicle theft and other property crimes and other crimes against persons.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

The cumulative results also indicate that Supported Work produced a significant reduction in the experimentals' contacts with the criminal justice system. In all the time periods considered, significantly fewer experimentals were arrested, convicted, or incarcerated. Over the full 36-month period, for example, 53 percent of the controls were arrested at least once, as compared to 35 percent of the experimentals. Over the three-year period, controls spent an average of nearly 14 weeks in jail or prison, while the experimentals spent an average of less than 7 weeks. The persistence of experimental effects in the cumulative results, which were weak or absent in the results by 9-month period, indicates that among the experimentals there was a higher incidence of the same individuals reporting arrests in more than one time period, compared with controls.

The cumulative arrest results show a significant difference among cohorts with different amounts of follow-up data (see Table VI.5). These results follow the typical cohort pattern, which shows the strongest experimental effect for the 36-month cohort, among whom the "risk" of arrest is highest, as indicated by the control group's arrest rate.[1/] Differential program effects on cumulative arrest rates among other subgroups follow the general pattern of results described for the 9-month periods.[2/]

[1/] Cohort effects in each 9-month period were less consistent; in months 19 to 27 there was a significant decrease in arrests among the 27-, but not the 36-month cohort (see Appendix Table A.10).

[2/] These other subgroup differences in cumulative arrests are presented in Appendix Tables A.17 and A.23.

TABLE VI.5

CUMULATIVE PERCENTAGE WITH ANY ARRESTS BY LATEST FOLLOW-UP INTERVIEW
EX-ADDICT SAMPLE

	Months 1-18		Months 1-27		Months 1-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Total Sample	-8.2	33.5	-10.9**	43.3	-18.1**	53.1
Latest Follow-up Interview	#					
18-month	3.1	31.4	n.a.	n.a.	n.a.	n.a.
27-month	-9.5**	32.2	-9.4	42.1	n.a.	n.a.
36-month	-17.3**	38.4	-14.2**	46.1	-18.1**	53.1

NOTE: Together these subsamples include the same individuals as were included in the sample used to generate Table VI.4. This total sample has been partitioned according to the most recent scheduled interview completed. This partitioning of the analysis sample does not yield subgroups that are exactly the same as would the formal definition of cohorts introduced in Chapter II, in that it is based on interviews completed (as opposed to interviews assigned as determined by the date of enrollment in Supported Work).

#Experimental-control differentials within this subgrouping for this time period differ significantly from one another.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

E. RELATIONSHIPS BETWEEN PROGRAM EFFECTS ON CRIME AND OTHER OUTCOMES

In this section we investigate the relationship between the effects of Supported Work on crime and other aspects of the ex-addict's behavior. Specifically, for each period we examine whether the effect on arrests varied by whether the individual was employed and by whether the person was using drugs during that period. Since we are dividing the sample by variables that themselves may be affected by the program, these comparisons are subject to a potential selectivity bias, as we noted above.

1. Employment and Arrests

If Supported Work affects crime only because it affects employment, then we would not expect it to change the relationship between crime and employment. If this hypothesis is correct, the arrest rate would be lower among those employed than among those unemployed. However, employed experimentals would not behave differently than employed controls.

The results in Table VI.6 suggest that Supported Work did change the relationship between crime and employment. Among the employed, lower percentages of experimentals than controls reported arrests. In months 10 to 18 the difference was significant; 17 percent of the employed controls reported having been arrested, compared with 12 percent of employed experimentals. Among those who were not employed, in contrast, the arrest rates of experimentals and controls were generally similar; approximately 20 percent of both experimentals and controls who were not employed were arrested.[1/]

[1/] This pattern is not evident in months 28 to 36, but the number of cases is quite small in this two-way classification for this period. Only seventeen individuals who were not employed reported arrests in this 28- to 36-month period.

TABLE VI.6

PERCENTAGE REPORTING ANY ARRESTS, BY CURRENT EMPLOYMENT STATUS
EX-ADDICT SAMPLE

Employment Status	Months 1-9			Months 10-18			Months 19-27			Months 28-36		
	Experimental-Control Differential	Control Group Mean		Experimental-Control Differential	Control Group Mean		Experimental-Control Differential	Control Group Mean		Experimental-Control Differential	Control Group Mean	
Not Employed	2.2	21.9		0.5	21.0		0.6	20.3		-5.2	15.7	
Employed	-1.6	19.6		-5.4*	17.2		-4.0	16.7		-4.7	12.7	
(Percentage Not Employed)a/	(-45.1)**	(51.2)		(-13.5)**	(48.6)		(-4.0)	(47.8)		(-10.9)*	(47.0)	

NOTE: These data are not regression-adjusted.

a/These data may differ somewhat from those reported in Chapter IV because of the different samples included in the analysis.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

These results suggest that Supported Work did not reduce criminal activity simply by increasing the participants' employability. The program also changed the relationship between crime and employment; it had very little effect on the criminal activity of those who did not find a job after leaving Supported Work. However, among those who were employed, Supported Work may have strengthened the commitment to more conventional behavior, and thereby reduced involvement in criminal activity.

2. Drug Use and Arrests

Table VI.7 presents the experimental effects on arrest rates by whether or not the person was using any drug other than marijuana or alcohol. If the program reduced crime primarily because it provided drug users with a legal source of income, then the program effects on crime should have been largest among those who were using drugs.

In general, controls who used drugs were more likely to be arrested than those who did not. Furthermore, the experimental-control differences were larger among drug users than among non-users in every period. For example, in months 10 to 18, among those who used drugs, 28 percent of the controls were arrested compared to 22 percent of the experimentals. Among those who did not use drugs, 15 percent of the controls were arrested compared to 12 percent of the experimentals. However, none of the differences was statistically significant.

Thus, while the program did produce the larger decrease in arrests among those who used drugs, which might indicate that the provision of legal income to those with a strong economic motivation to commit crime may be an important mechanism by which Supported Work reduced criminal

TABLE VI.7

PERCENTAGE REPORTING ANY ARRESTS, BY CURRENT USE OF DRUGS OTHER THAN MARIJUANA OR ALCOHOL
EX-ADDICT SAMPLE

Drug-Use Status	Months 1-9			Months 10-18			Months 19-27			Months 28-36		
	Experimental-Control Differential	Control Group Mean		Experimental-Control Differential	Control Group Mean		Experimental-Control Differential	Control Group Mean		Experimental-Control Differential	Control Group Mean	
Using Drugs	-0.2	25.8		-5.9	28.0		-7.2	30.8		-7.1	24.2	
Not Using Drugs	-2.7	17.4		-2.9	14.6		-0.9	13.5		-4.6	11.1	
(Percentage Reporting Drug Use)a/	(-0.6)	(39.2)		(0.9)	(33.2)		(-0.3)	(28.3)		(0.3)	(22.0)	

NOTE: These data are not regression-adjusted.

a/These data may differ somewhat from those reported in Chapter V because of the different samples included in the analysis.

activity, the fact that the program also reduced arrests among non-users suggests that these economic factors may not be the sole mechanism.

F. SUMMARY AND CONCLUSION

Supported Work did significantly reduce the criminal activity of the ex-addicts. In almost every period, a lower percentage of experimentals than controls were arrested, convicted, or incarcerated, and these effects were significant in months 10 to 18.[1/] The cumulative differences in the arrest rates were also large and statistically significant. Furthermore, the program had a significant impact on arrests for two specific types of offenses: robbery and drug-related crimes. The effect on robbery is particularly important since this type of crime has a high social cost.

Two mechanisms by which Supported Work may have produced these effects have been identified: economic factors, such as increased employment opportunities and increased income (which may reduce the economic motive for crime), and non-economic factors, such as graduated stress and peer support for work (which may counterbalance peer and subculture support for criminal activity).

We found evidence that Supported Work did influence criminal activity through economic factors. The program had a larger effect on arrests for those who were using drugs and who, thus, had the greater economic motivation for crime. It also had a greater effect for those who had not been receiving welfare at the time of their enrollment and who,

1/These results differ from the results for the Wildcat Supported Work Program, where there was a reduction in arrests only in the first year following program enrollment (see Friedman, 1978).

thus, had a larger change in income as a result of the program. Further, the reduction in arrests was proportionately larger for "riskier" crimes, perhaps because the additional income reduced the participants' willingness to take risks.

While there is evidence that economic factors were an important mechanism, they do not appear to be the sole mechanism by which Supported Work reduced criminal activity. We have found that, in general, the subgroup differences in arrests did not coincide with the subgroup differences in employment. Further, the experience of participating in Supported Work may change the relationship between employment and arrests, as evidenced by the fact that experimentals who were employed were less likely to be arrested than were controls who were employed. This suggests that experimentals' experiences in Supported Work may have strengthened their commitment to conventional behavior. The fact that the program was more effective for those with dependents provides further support for this hypothesis. Thus, it appears that both economic and other factors served to reduce the criminal activity of the ex-addicts.

CHAPTER VII

CONCLUSION

In this report, we have examined the impact of Supported Work on several dimensions of the ex-addicts' behavior. We found that the program increased the employment and earnings of participants in some periods, and that the program reduced their involvement in criminal activity. The program did not, however, have any overall impact on the drug use of ex-addicts. In this chapter, we review the major findings of the report, compare the results across the outcomes considered, and explore the implications of these findings for the establishment of a permanent Supported Work program for ex-addicts.

A. REVIEW OF FINDINGS

1. Employment and Earnings

Supported Work had a large and significant impact on the employment and earnings of experimentals for periods when a substantial number of experimentals were participating in the program. Thus, Supported Work did provide employment opportunities to the ex-addicts that otherwise would not have been available. As the participants left their Supported Work jobs, the experimental-control differences in employment and earnings narrowed sharply, until, by the 16- to 18-month period, there were virtually no differences in employment rates, hours of work, or earnings between experimentals and controls.

This equality between the employment and earnings of experimentals and controls continued for several months, during which there were some differences in other behaviors. More experimentals than controls reported

that they were looking for work, and the experimentals' average reservation wage--the lowest wage they would be willing to accept--was significantly lower than that of controls. Perhaps due in part to these differences, the experimentals began to show an improvement, relative to controls, and by months 33 to 36, those experimentals who enrolled early enough to receive a 36-month interview had significantly higher employment rates, worked more hours, and earned more than controls. However, the results for the later periods are qualified by the fact that this particular subsample of the respondents consistently exhibited a more favorable employment response to the program than was observed for those later enrollees who were followed for only 18 or 27 months.

2. Drug Use

The favorable program effects on employment were not reflected in the drug-use rates of the ex-addicts. Overall, there were no significant experimental-control differences in the use of heroin, cocaine, and other opiates, or in the summary measures of drug use. There were some significant effects for various subgroups--some positive and some negative--but, in general, these effects did not persist into the later periods when all the experimentals had left the program.

Supported Work did significantly affect the daily use of alcohol of participants during the first 9 months, although this result is dominated by one site in which a lower percentage of experimentals than controls reported drinking alcohol daily.[1/] However, in the later periods,

[1/] This effect does not appear to be related to changes in drug-use patterns.

daily alcohol use was generally more prevalent among experimentals than controls.

3. Criminal Activity

Supported Work did reduce the criminal activity of participants, as indicated by arrest rates, convictions, and time incarcerated. These reductions in criminal activity are evident in every period and significant in the 10- to 18-month period, as well as over the cumulative 1- to 27-month and 1- to 36-month periods. Over the 27-month period, 43 percent of controls were arrested at least once, as compared with 32 percent of experimentals; the figures for the full 36-month period are 53 percent and 35 percent, respectively. Furthermore, the program significantly reduced arrests for two specific types of crimes which are of particular interest--robbery and drug-related offenses. The effect on robbery is particularly important because this crime typically has a very high social cost.

B. PROGRAM EFFECTS ACROSS OUTCOMES

1. Relationship Between Effects on Employment, Drug Use and Criminal Activity

The extent to which Supported Work affected the relationship between employment, drug use, and crime was also examined. It was found that the use of drugs was only slightly less among those who were employed than among those not employed (regardless of experimental status) and that there was no experimental-control difference in drug use among either group. Thus, the program does not appear to have affected the relationship between drug use and employment.

There was no consistent relationship between employment and alcohol use among the controls. However, in the later periods, the increase in

daily alcohol use among experimentals was larger among those who were employed than among those who were not employed. These differences suggest that the program may have altered the experimentals' behavior under the stress of employment or perhaps that experimentals tended to have more stressful jobs than did controls.

The data offer some evidence of the hypothesized relationship between employment and crime: arrest rates tended to be lower for employed individuals (both experimentals and controls) than for those who were not employed. Furthermore, among employed individuals, arrest rates were lower for experimentals than controls. While this latter comparison may be biased by self-selection, it gives some indication that Supported Work employment may be a more effective vehicle for reducing crime than is other employment.

Arrest rate and drug use were also found to be positively correlated: individuals who reported using any drug other than marijuana or alcohol also reported higher arrest rates than did those who were non-users, regardless of experimental status. Moreover, among both users and non-users, experimentals had lower arrest rates than did controls. Thus, it appears that providing drug users with a legal source of income may have been an important mechanism by which Supported Work reduced criminal activity. However, this was not the sole mechanism, because there was also a reduction in arrests among non-users.

2. Subgroup Differences Across Outcomes

We also examined differences in program effects by site and by various demographic subgroups, because consistent subgroup differences

could provide further indications of a relationship between the program effects on employment, drug use, and crime.

There were no consistent site differences for employment outcomes. However, we did find that in Oakland the program resulted in a significant reduction in heroin use, and that the reduction in arrests was larger there than in other sites. Significant reductions in both outcomes occurred in the 10- to 18-month period in this site.

There were some consistent demographic subgroup differences. Among those with at least one dependent, the program had a larger favorable effect on hours of work and produced a larger reduction in heroin use, cocaine use, and in the arrest rate. This more favorable pattern of results for ex-addicts who have dependents may indicate that this group sees greater benefits than average to changing their life-styles.

Some other demographic subgroup differences were evident for more than one outcome. Among those over 35, there was a larger program-related reduction in heroin use and in arrests. Thus, Supported Work may have hastened the "maturing out" of these older ex-addicts. There is also some evidence that among those who were in drug-free programs prior to Supported Work, the program resulted in a smaller effect on hours of work. Moreover, these experimentals used heroin more than did comparable controls.

The subgroup analysis indicated another consistent pattern across all three outcomes: Supported Work was more effective for those who would have done less well on their own, as measured by the control group's experience. In those subgroups where the program resulted in the largest effect on hours of work, controls tended to work fewer than average hours. Similarly, in subgroups where Supported Work produced the largest

reduction in heroin use and in arrests, the controls tended to have high drug use and arrest rates. This result indicates that any future Supported Work program for ex-addicts should continue to be targeted on those with the most severe labor market difficulties and on those with the highest risk of returning to drug use or engaging in crime.

This overall pattern also offers some explanation of why Supported Work had a greater impact on the earnings of ex-addicts than of ex-offenders. In every period, ex-addict controls earned less than did ex-offender controls. However, this pattern does not explain why the program reduced criminal activity only for ex-addicts and reduced drug use only for ex-offenders. The ex-offender subgroup differences suggest that prior heroin use may be the variable that influences the program's impact on crime. The subgroup differences do not provide any explanation for the target group differences in drug-use effects, however.

One other consistent subgroup difference occurred among the various cohorts, defined by the latest follow-up interview completed. Among those with 36-month interviews--the earliest enrollees in the demonstration--the program resulted in larger effects on employment and earnings, produced different patterns of drug use, and larger reductions in cumulative arrest rates than were observed among the other cohorts. The interpretation of these differences has a direct bearing on the generalizability of the results of this study.

C. GENERALIZABILTY OF THE RESULTS

The cohort differences pose the greatest problem for the interpretation of the employment and earnings results, because the significant

post-program results were observed only in the last 9-month period, a period in which we have data only for the 36-month cohort.

We investigated several possible causes for the observed cohort differences. Only small parts of the differences can be explained by the differential sample allocation by site. Observable characteristics of the sample do not explain the differences in outcomes. There is evidence that some of the cohort differences are related to calendar time, and may therefore be due to changes in labor market conditions over time. Members of the 36-month cohort enrolled when the economic conditions were very depressed and control members worked fewer than average hours. Thus, the fact that Supported Work had the largest effect on the 36-month cohort is consistent with the overall pattern noted above. However, as best we can determine, site and labor market conditions do not fully account for the cohort effects. It is possible that the remainder of the effects are due to unobserved differences over time in the characteristics of the individuals who applied for Supported Work or in the changing character of the Supported Work programs themselves.

Based on available data, it is not possible to predict whether Supported Work had long run employment effects for the 27- and 18-month cohorts. At best the impacts are expected to have been smaller than the estimated impact on the 36-month cohort, since the earlier results were consistently smaller for the 18- and 27-month cohorts, and since controls in these cohorts exhibited substantially more favorable employment experiences than did those in the 36-month cohort.

Based on the results presented in this report, a national Supported Work program would be expected to have a large effect on the in-program

employment and earnings of ex-addict participants. The size and timing of impacts on post-program employment are uncertain. The program would also be expected to produce reductions in transfer payments, particularly during the in-program periods. Although no overall effects on drug use should be expected, a national program would be expected to reduce the arrests, convictions, and incarcerations of the ex-addict participants both during and following their participation in the program.

APPENDIX A

SUPPLEMENTARY FIGURES AND TABLES

FIGURE A.1

TREND IN HOURS WORKED PER MONTH BY COHORT
CHICAGO, OAKLAND AND PHILADELPHIA
EX-ADDICT SAMPLES

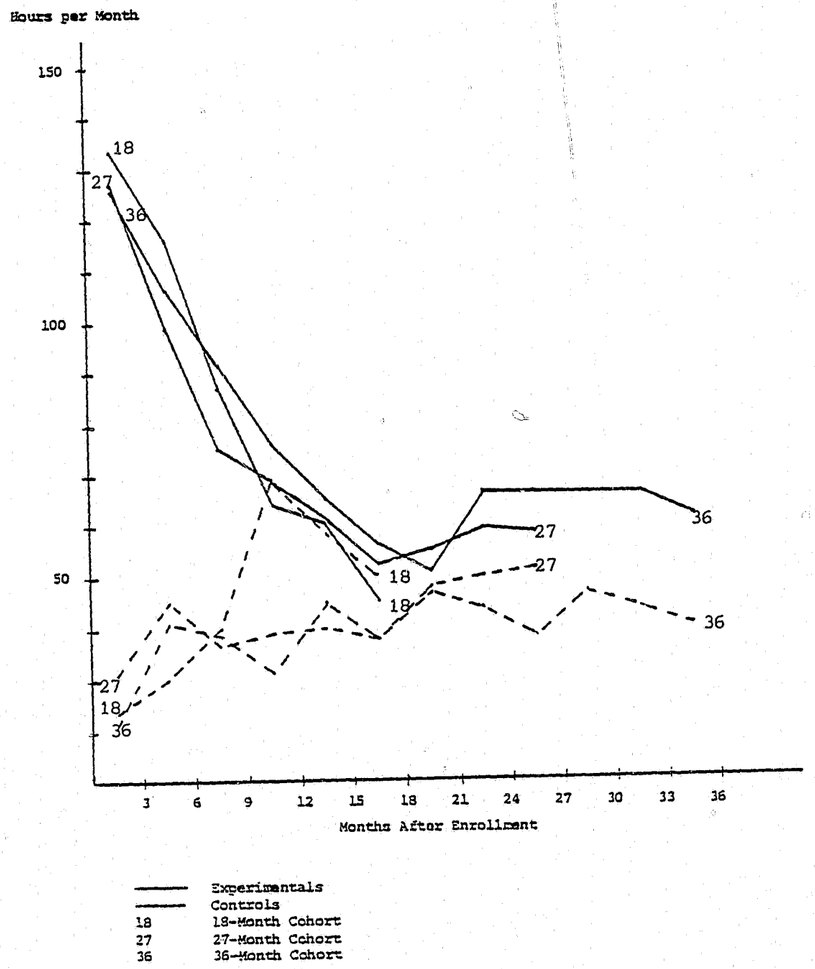

TABLE A.1

PERCENTAGE DISTRIBUTIONS OF INDIVIDUAL CHARACTERISTICS AT ENROLLMENT,
BY LATEST FOLLOW-UP INTERVIEW
EX-ADDICT SAMPLE

	Latest Follow-Up Interview[a]		
	18-Month	27-Month	36-Month
Site			
Chicago	28.3	18.0	21.2
Jersey City	28.7	40.0	37.0
Oakland	18.6	9.2	1.3
Philadelphia	24.3	32.9	40.5
Demographic Characteristics			
Years of Age			
Less than 21	5.4	8.3	4.9
21 - 25	35.3	40.5	37.4
26 - 35			
More than 35	14.5	11.3	12.0
(Average)	(28.6)	(27.3)	(27.8)
Sex			
Male	79.4	81.5	86.8
Female	20.6	18.5	13.2
Race/Ethnicity			
White, Non-Hispanic	13.5	15.5	10.0
Black, Non-Hispanic, and Other	75.8	74.7	84.4
Hispanic	10.7	9.8	5.6
Education			
High School Diploma or Equivalent	39.7	40.5	39.4
Years of Education			
8 or less	15.0	14.1	11.9
9 - 11	58.2	55.4	62.3
12 or more	26.8	30.5	25.8
(Average number of years)	(10.4)	(10.5)	(10.4)
Marital Status			
Married	21.4	23.9	23.5
At least one dependent	38.1	38.5	37.1
Employment Experience			
Worked past year	42.7	50.2	58.7
(Average weeks worked in past year)	8.5	9.4	13.1
Longest Job Ever Held			
No job	7.6	4.4	5.5
1 - 12 months	38.4	41.4	36.9
More than 12 months	54.0	54.2	57.6
(Average dollar earnings last 2 months for those who worked)	(641.4)	(549.1)	(629.8)
Received Welfare Last Month	37.9	39.7	41.8
(Average dollar amount received)	(78.01)	(76.45)	(81.28)
Criminal History			
Number of Arrests			
0	9.6	10.0	10.2
1	10.8	11.5	8.3
2 - 5	31.7	33.6	35.5
6 - 10	17.5	19.3	21.5
More than 10	30.4	25.6	24.5
(Average number of arrests)	(9.2)	(8.4)	(8.4)
At least one conviction	77.8	73.7	78.4
(Average number of convictions)	(3.3)	(2.7)	(3.0)
Average number of weeks incarcerated	153	122	142

Table A.1 (continued)

	Latest Follow-Up Interview[a]		
	18-Month	27-Month	36-Month
Drug Use History			
Most Recent Treatment in Last Six Months			
Methadone maintenance	57.9	46.2	56.6
Drug free	19.2	24.1	18.4
Other	26.4	18.5	16.5
Not in treatment	16.5	11.2	8.5
Number of Treatment Programs Ever Enrolled In			
None	10.4	5.0	5.6
1	47.5	53.4	56.2
2	25.4	23.6	20.2
3 or more	16.7	18.0	18.0
(Average number of treatment programs)	(1.6)	(1.7)	(1.7)
Types of Drugs Ever Used			
Heroin	93.3	93.7	94.8
Other Opiates	24.6	23.5	40.2
Cocaine	66.9	66.7	68.5
Barbiturates	37.7	35.7	37.8
Amphetamines	30.8	30.8	36.3
Psychedelics	28.3	25.7	24.7
Marijuana	91.3	90.5	91.4
Opiates plus other drugs	74.4	74.3	78.0
Opiates only	18.9	20.8	17.4
Other drugs only	3.4	3.6	3.1
Length of Time Used Heroin			
Never used[b]	10.5	9.2	6.7
Less than one year	8.8	6.1	3.8
1 - 5 years	36.0	39.0	40.1
More than 5 years	44.8	45.7	49.4
(Average number of years used heroin)	(5.9)	(6.2)	(6.7)
Number of Times Previously Stopped Using Heroin (for those who ever used)			
0	9.8	13.3	8.7
1 - 2	22.4	23.1	25.7
3 - 4	19.6	18.4	23.1
5 or more	48.1	45.3	42.5
(Average number of times)	(6.1)	(6.3)	(6.3)
Number in Sample	241	592	267

SOURCE: These data are from baseline interviews conducted by MPR staff.

[a] These categories are mutually exclusive; the total sample of 1100 persons includes any individual appearing in any analysis sample.

[b] "Never used" includes people who used heroin less than a few times a month.

TABLE A.2

NUMBERS OF INTERVIEWS ASSIGNED AND COMPLETED
EX-ADDICT SAMPLE

Interview Type	Number Assigned	Number Completed[a]	Percentage Completed
Enrollment	1,433	1,407	98.2
9-month	1,433	1,111	77.5
18-month	1,433	987	68.9
27-month	1,220[b]	885	72.5
36-month	472[c]	317	67.2

NOTE: These data are from Jackson et al. (1979), Tables II.1 and VI.A.1-VI.A.4.

[a] These figures include 13 persons who completed a substitute enrollment interview at the time of a subsequently scheduled follow-up interview. They do not include individuals who completed substitute follow-up interviews.

[b] Only those enrolled prior to January 1977 were assigned a 27-month interview.

[c] Only those enrolled prior to April 1976 were assigned a 36-month interview.

TABLE A.3

TOBIT ESTIMATES OF HOURS EMPLOYED PER MONTH

EX-ADDICT SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Total Sample	82.81**	35.67	18.59**	44.13	3.05	52.87	17.89**	47.51
Site								
Chicago	95.02**	23.77	21.20**	34.48	10.48	36.39	43.43**	42.65
Jersey City	94.21**	47.47	22.11**	63.07	-1.41	77.65	7.03	68.86
Oakland	60.59**	38.34	14.88	32.23	-18.44	56.26	12.70[a/]	94.01
Philadelphia	64.27**	28.78	12.28*	34.56	6.84	38.33	14.05	32.18
Number in Sample	972		969		880		310	

NOTE: See note to Table IV.6.

[a/] There are only four persons in this Oakland sample.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE A.4

COMPONENTS OF EXPERIMENTAL-CONTROL DIFFERENTIAL IN HOURS EMPLOYED

EX-ADDICT SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Value	Percent of Total	Value	Percent of Total	Value	Percent of Total	Value	Percent of Total
Overall Differential	82.81	100.0	18.59	100.0	3.05	100.0	17.89	100.0
Differential Due to:								
Change in Probability of Employment	46.61	56.3	11.36	61.1	1.85	60.6	10.89	60.9
Change in Hours Worked Among Employed	36.20	43.7	7.23	38.9	1.20	39.4	7.00	39.1

NOTE: The decomposition of the overall differential was estimated from a tobit equation in which the overall differential, $\Delta E(Y)$, can be expressed as:

$$X_E \beta F_E(\cdot) + \sigma f_E(\cdot) - X_c \beta F_c(\cdot) - \sigma f_c(\cdot)$$

where: X is a vector of control variables; β is a vector of estimated coefficients; $F(\cdot)$ denotes the cumulative standard normal distribution evaluated at X; $f(\cdot)$ denotes the standard normal density evaluated at X; σ is the standard error of the equation; and E and c denote experimentals' and controls' values, respectively.

The two components, respectively, can be expressed as:

$$E(Y_E^*) * \left[F_E(\cdot) - F_c(\cdot)\right]$$

and

$$\Delta E(Y^*) * F_c(\cdot)$$

where: $E(Y^*)$ is the expected value of Y for observations above the limit.

See McDonald, J. and R. Moffitt, "Uses of Tobit Analysis," Review of Economic Statistics (forthcoming) for a discussion of this decomposition procedure.

Significance levels are not indicated.

TABLE A.5

HOURS EMPLOYED PER MONTH, BY COHORT
EX-ADDICT SAMPLE

	36-Month Cohort[a]			27-Month Cohort[a]			18-Month Cohort[a]		
	Experimental Group Mean	Control Group Mean	Experimental-Control Differential	Experimental Group Mean	Control Group Mean	Experimental-Control Differential	Experimental Group Mean	Control Group Mean	Experimental-Control Differential
Months									
1-3	139.6	30.9	108.7**	139.6	32.3	107.3**	133.2	28.5	104.7**
4-6	124.2	50.3	73.9**	113.4	43.7	69.7**	117.7	43.6	74.1**
7-9	109.9	47.7	62.2**	93.2	36.7	56.5**	95.1	49.5	45.6**
10-12	91.5	38.6	52.9**	79.9	42.9	37.0**	68.7	60.7	8.0
13-15	79.4	49.7	29.7**	61.9	45.6	16.3**	59.6	60.0	-0.4
16-18	58.6	46.2	12.4	49.3	49.7	-0.4	48.1	59.7	-11.6
19-21	47.8	51.5	-3.7	57.6	57.9	-0.3	n.a.	n.a.	n.a.
22-24	58.3	54.7	3.6	65.7	63.4	2.3	n.a.	n.a.	n.a.
25-27	70.1	52.0	18.1*[b]	66.8	62.7	4.1	n.a.	n.a.	n.a.
28-30	64.6	55.2	9.4[b]	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
31-33	68.3	52.4	15.9[b]	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
34-36	68.0	52.4	15.6[b]	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.

NOTE: These data are not regression-adjusted.

[a] Individuals were included in designated cohort only if they had a complete set of employment data from all interviews up to and including the interview of the cohort named.

[b] These data differ from those in Table IV.2 because of differences in samples and because these data are not regression-adjusted.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

n.a. = not applicable.

TABLE A.6

PERCENTAGE HAVING CETA OR WIN JOBS, BY SITE
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Chicago	-2.5	3.2	0.5	6.2	3.2	2.5	12.1	3.0
Jersey City	-2.5	2.5	-2.4	6.3	2.3	5.6	-7.7	7.7
Oakland	-2.5	5.9	-2.6	6.4	0.2	3.3	0.0[a]	0.0
Philadelphia	0.9	1.3	-0.9	3.4	2.8	2.1	-1.6	3.2
Total Sample	-1.4	2.6	-1.3	5.4	2.5	3.6	-0.9	4.7

NOTE: These figures are based on those individuals who report earnings from jobs specifically identified as CETA or WIN jobs. To the extent that individuals do not realize that their jobs are supported by the CETA or WIN programs, these figures understate total CETA/WIN employment. These data are not regression-adjusted. No test statistics were computed.

[a] There are only four persons in this Oakland sample.

TABLE A.7

AVERAGE MONTHLY EARNINGS FROM CETA OR WIN JOBS, BY SITE
EX-ADDICT SAMPLE
(dollars)

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Chicago	-11.86	15.02	-8.07	25.79	42.26	15.00	31.80	9.80
Jersey City	-7.43	7.43	-7.23	18.67	0.95	26.19	-26.80	26.80
Oakland	-8.09	14.54	-26.40	43.33	23.08	1.20	0.00[a]	0.00
Philadelphia	-1.40	7.36	-3.75	12.14	-1.45	12.49	-9.93	13.35
Total Sample	-6.88	9.94	-8.38	20.91	9.54	17.47	-7.15	17.18

NOTE: These figures are based on earnings from jobs that respondents identified specifically as CETA or WIN. To the extent that individuals do not realize that their jobs are supported by the CETA or WIN programs, these figures understate total CETA/WIN earnings. These data are not regression-adjusted. No test statistics were computed.

[a] There are only four persons in this Oakland sample.

TABLE A.8

ENROLLMENT IN DRUG TREATMENT PROGRAMS
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Percentage Enrolled in Any Program	1.6	54.7	-0.2	39.7	-2.8	34.2	0.5	32.6
Of those in treatment, percentage in[a]								
Methadone Maintenance	-1.7	71.4	4.8	72.3	6.9	76.9	0.7	67.4
Drug Free Program	1.4	19.2	-1.2	16.3	-7.3*	16.2	-1.2	22.5
Detoxification Program	-0.3	8.2	-0.8	6.0	0.6	3.1	-4.0	6.1
Alcohol Treatment Program	0.7	1.2	-2.8	5.4	-0.2	3.9	4.4	4.1
Average Number of Months in Drug Treatment	0.1	3.9	0.0	2.9	-0.1	2.3	-0.1	2.4

[a] These data are not regression-adjusted.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE A.9

PERCENTAGES REPORTING VARIOUS DRUG-USE PATTERNS
EX-ADDICT SAMPLE

	Relative Weight[a]	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
		Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Cocaine or ~~Amphetamines~~ Only[b]	.24	2.4	8.7	2.0	8.7	1.6	8.8	0.1	7.2
Barbiturates only[c]	.50	0.1	1.3	-0.7	2.3	-0.6	1.9	0.0	0.0
Heroin only[d]	.40	-0.7	11.8	0.0	7.3	0.2	5.8	5.4**	1.9
Barbiturates and Cocaine or Amphetamines	.56	0.2	0.6	-0.4	0.6	-0.6	1.3	-0.2	2.1
Barbiturates and Heroin	.71	-0.7	0.9	0.6	1.1	0.1	0.2	-0.6	0.6
Heroin and Cocaine or Amphetamines	1.00	1.0	4.6	-1.0	6.2	0.0	5.4	-3.3	6.3
Heroin and Barbiturates and Cocaine or Amphetamines	.87	-0.1	1.2	-0.3	0.9	0.3	0.7	0.0	0.0

[a] These weights reflect the size of the association between each drug use pattern and the number of arrests of an individual, relative to the size of the association between the use of heroin and cocaine or amphetamines and number of arrests.

[b] Individuals in this category did not use heroin or barbiturates, but may have used marijuana, illegal methadone, other opiates, or psychedelics.

[c] Individuals in this category did not use amphetamines or cocaine, but may have used other opiates, or psychedelics.

[d] Individuals in this category did not use barbiturates, or cocaine, but may have used other opiates or psychedelics.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE A.10

PERCENTAGE REPORTING ANY ARRESTS, BY LATEST FOLLOW-UP INTERVIEW
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Total Sample	-2.5	19.5	-5.9**	18.6	-2.3	18.2	-5.0	13.5
Latest Follow-up Interview	#				#			
18-month	8.1	13.9	-2.8	23.4	n.a.	n.a.	n.a.	n.a.
27-month	-3.8	19.8	-5.>	15.8	-5.6*	20.1	n.a.	n.a.
36-month	-10.7*	24.6	-9.1*	19.9	4.4	14.3	-5.0	13.5

NOTE: See note to Table VI.5.

#Estimated program effects vary significantly among subgroups.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

n.a. = not applicable.

TABLE A.11

MEANS OF CONTROL VARIABLES USED IN REGRESSIONS
EX-ADDICT SAMPLE
(Standard Deviations of Continuous Variables are in Parentheses)

	Analysis Sample		
	1-18 Month Outcomes	19-27 Month Outcomes	28-36 Month Outcomes
Experimental Group	0.527	0.524	0.511
Amount of Follow-up Data[a/]			
27 Month	0.503	---	---
36 Months	0.261	0.326	---
Pressured by Welfare, Drug-Treatment, or Criminal Justice Agency to Apply to Program[a/]	0.122	0.129	0.125
Site			
Chicago	0.232	0.194	0.208
Jersey City	0.396	0.393	0.375
Oakland	0.091	0.064	0.013
Philadelphia	0.282	0.349	0.404
Months of Program Operation			
13 - 18	0.398	0.445	---
> 18	0.245	0.119	---
Area Unemployment Rate During Follow-up Period[b/]	9.489	8.299	7.188
	(2.562)	(2.311)	(1.779)
Complies with Formal Program Eligibility Criteria	0.777	0.777	0.771
Age			
21 - 25	0.383	0.395	0.372
26 - 35	0.429	0.418	0.458
36 or older	0.125	0.119	0.121
Male	0.805	0.836	0.866
Race/Ethnicity			
White, non-Hispanic	0.134	0.131	0.098
Hispanic	0.081	0.084	0.056
Years of School			
9 - 11	0.582	0.570	0.621
≥ 12	0.279	0.294	0.258
Number of Persons in Household	3.596	3.547	3.384
	(2.283)	(2.239)	(2.068)
Currently Married	0.234	0.240	0.238
Any Dependents	0.390	0.384	0.374
Any Food Stamps or Welfare Last Month	0.498	0.516	0.541
Total Income Last Month (dollars)[c/]	228.967	225.037	267.301
	(234.231)	(230.245)	(257.170)
Earnings Last Month[c/]			
Any	0.493	0.522	0.578
Dollar Amount	100.076	107.088	138.481
	(171.160)	(178.295)	(200.908)
Unemployment Compensation Last Month[c/]			
Any	0.050	0.047	0.054
Dollar Amount	15.815	14.369	16.790
	(73.679)	(69.234)	(75.164)
Welfare Last Month[c/]			
Any	0.413	0.400	0.417
Dollar Amount	83.781	76.324	82.068
	(116.658)	(108.573)	(109.216)
Food Stamps Last Month[c/]			
Any	0.333	0.316	0.335
Dollar Bonus Value	20.085	18.150	18.560
	(34.981)	(32.237)	(32.306)

Table A.11 (continued)

	Analysis Sample		
	1-18 Month Outcomes	19-27 Month Outcomes	28-36 Month Outcomes
Other Unearned Income Last Month[c/]			
Any	0.040	0.039	0.058
Dollar Amount	9.210	9.106	11.441
	(61.637)	(60.822)	(57.052)
Weeks Worked Prior Year	10.165	10.654	13.150
	(14.105)	(14.210)	(14.741)
Length of Longest Job Ever			
12 Months or Less	0.393	0.394	0.364
> 12 Months	0.550	0.557	0.581
8 or More Weeks of Job Training Prior Year[d/]	0.091	0.099	0.088
Used Any Drugs (other than marijuana)[c/]	0.980	0.985	0.985
Used Heroin Regularly but Not Cocaine Regularly	0.733	0.738	0.772
Used Heroin Regularly and Used Cocaine Regularly	0.124	0.123	0.119
Used Cocaine[c/]	0.679	0.676	0.682
Used Alcohol Daily[c/]	0.115	0.119	0.120
Months of Past Cocaine Use[c/]	21.970	22.163	24.199
	(45.103)	(45.518)	(43.413)
Months of Past Heroin Use[e/]	76.384	78.392	81.955
	(67.875)	(68.974)	(64.615)
Drug Treatment Last 6 Months			
In treatment	0.886	0.900	0.915
In methadone maintenance	0.482	0.503	0.571
In drug free	0.217	0.218	0.179
In treatment involuntarily	0.258	0.262	0.256
Best Friend Does Not Use Drugs and Is Not Involved in Crime[a/]	0.739	0.736	0.712
Many Addicts in Neighborhood[a/]	0.540	0.558	0.500
Ever Arrested	0.891	0.896	0.888
Number of Arrests	8.255	8.400	8.432
	(11.083)	(11.114)	(10.298)
Time Since Incarcerated			
12 Months or Less	0.291	0.295	0.272
> 12 Months	0.412	0.430	0.575
On Parole or Probation[f/]	0.488	0.496	0.512
Maximum Number of Cases in Regressions	974	885	311

NOTE: Means of these variables will vary slightly from one set of regressions to another because of slightly different sample sizes for analyses of various outcome measures. Sample sizes for various subgroups can be calculated by multiplying the proportion of the sample in the subgroup by the total sample size.

[a/] These variables were included only in regressions to estimate subgroup effects for individuals with the various attributes.

[b/] Area unemployment rate was ultimately excluded from regressions because of its high correlation with the site variables. The 1-18 month value pertains to months 10-18.

[c/] These variables were included only in regressions where the dependent variable was the post-enrollment value of the same.

[d/] This variable was included only in employment-related regressions.

[e/] This variable was included only in regressions where indicators of drug use were the dependent variables.

[f/] This variable was included only in regressions where indicators of drug use and criminal activities were the dependent variables.

TABLE A.12

ESTIMATED COEFFICIENTS ON CONTROL VARIABLES USED IN SELECTED REGRESSION EQUATIONS
EX-ADDICT SAMPLE

	Dependent Variable		
	Hours Employed Per Month[a] (Months 19-27)	Used Any Drugs (x100)[b] (Months 19-27)	Had Any Arrest (x100)[c] (Months 19-27)
Site			
Chicago	-3.4	23.9	-2.6
Jersey City	n.a.	n.a.	n.a.
Oakland	-8.4	35.4	8.1
Philadelphia	-14.1**	7.1	0.9
Months of Program Operation			
13 - 18	2.4	0.4	1.9
> 18	10.8	-8.3	-2.3
Complies with Formal Program Eligibility Criteria	-1.5	-5.4	-4.5
Age			
21 - 25	-15.2	2.5	-0.2
26 - 35	-11.1	3.2	0.4
36 or older	-25.3*	0.5	-7.0
Male	20.5**	-9.8**	9.3
Race/Ethnicity			
White, non-Hispanic	28.2**	-1.0	5.7
Hispanic	27.2**	2.3	1.3
Years of School			
9 - 11	10.3	-3.1	5.8
≥ 12	16.7**	-3.0	-1.1
Number of Persons in Household	-0.7	2.2**	-0.7
Currently Married	10.1	1.6	-2.1
Any Dependents	0.9	-6.7*	3.5
Any Food Stamps or Welfare Last Month	-6.6	-1.5	0.3
Weeks Worked Prior Year	0.4*	-0.0	-0.2
Length of Longest Job Ever			
12 Months or less	8.4	-5.1	2.1
> 12 Months	12.0	-5.2	-1.4
8 or More Weeks of Job Training Prior Year	6.1	n.a.	n.a.
Used Heroin Regularly But Not Cocaine Regularly	-5.2	7.1	-0.4
Used Heroin Regularly and Used Cocaine Regularly	9.3	14.2**	1.8
Duration of Past Heroin Use	n.a.	0.0	n.a.
Drug Treatment Last Six Months			
In treatment	9.0	-2.4	n.a.
In methadone maintenance	0.1	9.5**	4.7
In drug free	14.5*	8.8*	6.7
In treatment involuntarily	n.a.	0.2	n.a.

Table A.12 (continued)

	Dependent Variable		
	Hours Employed Per Month[a] (Months 19-27)	Used Any Drugs (x100)[b] (Months 19-27)	Had Any Arrest (x100)[c] (Months 19-27)
Ever Arrested	8.9	14.5**	11.2*
Number of Arrests	-0.1	0.0	-0.0
Time Since Incarcerated			
12 Months or less	-4.3	-15.3**	2.1
> 12 Months	-5.6	-9.7**	-1.4
On Parole or Probation	n.a.	4.2	-1.0
Number of Cases in Regressions	879	862	873
Mean of Dependent Variable	59.4	27.8	17.0
$\bar{R}^2$	0.076	0.035	0.012

SOURCE: See Table II.3.

[a] This equation included a binary variable indicating experimental status. An estimate of the program impact, based on this equation, is presented in Table IV.6.

[b] This equation included binary variables interacting experimental status with site and experimental status with program age. Overall program impacts estimated from this equation are presented in Table V.1.

[c] This equation included a binary variable indicating experimental status. An estimate of the program impact, based on this equation, is presented in Table VI.1.

*Statistically significant at the 10 percent level, two tailed test.
**Statistically significant at the 5 percent level, two tailed test.

n.a. = not applicable.

CONTINUED

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TABLE A.13

PERCENTAGE DISTRIBUTION OF THE SAMPLE BY AVERAGE NUMBER OF HOURS WORKED AND AVERAGE EARNINGS PER MONTH
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimentals	Controls	Experimentals	Controls	Experimentals	Controls	Experimentals	Controls
Hours Worked per Month								
0	6.4	50.4	34.9	48.5	43.9	46.9	36.5	46.4
1 - 43	9.7	15.3	9.7	12.1	13.9	11.0	13.8	12.6
44 - 86	15.2	13.5	19.3	13.4	10.2	9.5	13.2	9.9
87 - 129	16.5	9.8	14.2	8.1	8.9	9.8	9.8	10.6
130 - 172	20.4	7.0	9.9	7.7	7.6	10.5	10.7	7.3
173 - 216	30.2	2.6	10.5	9.4	12.8	9.5	14.5	12.6
217 - 259	1.2	0.9	1.0	0.7	1.3	2.1	0.6	0.0
≥ 260	6.4	0.4	0.4	0.2	1.3	0.7	1.9	0.7
(Average hours)	(118.0)	(40.9)	(66.4)	(50.3)	(60.2)	(58.5)	(69.0)	(54.5)
Earnings per Month (dollars)								
0	6.2	50.4	35.0	48.5	43.7	46.9	36.5	46.4
1 - 99	7.8	11.1	7.0	9.2	9.3	8.1	8.8	7.9
100 - 199	13.2	9.8	12.8	6.4	8.4	6.2	8.2	8.6
200 - 299	11.3	9.8	10.7	9.4	4.3	5.7	10.1	6.6
300 - 399	11.1	5.9	7.0	6.8	5.2	3.3	5.7	5.3
400 - 499	30.7	2.8	9.3	4.4	5.4	5.5	6.9	5.3
500 - 599	12.5	3.9	5.6	3.7	4.5	4.8	6.3	6.0
600 - 699	4.1	2.8	4.5	3.5	5.8	6.0	3.1	4.0
700 - 799	1.4	0.7	2.7	2.0	4.1	4.8	3.1	2.6
800 - 899	0.6	0.7	1.6	1.5	3.0	2.9	3.1	2.6
≥ 900	1.2	2.0	3.7	4.6	6.1	6.0	8.2	4.6
(Average dollars)	(350.65)	(158.32)	(253.02)	(211.22)	(271.21)	(261.20)	(313.70)	(232.25)
Number in Sample	974		974		885		311	

NOTE: Samples used are defined in Table II.3. These data are not regression-adjusted. Columns may not sum to 100 due to rounding.

TABLE A.14

PERCENTAGE OF EX-ADDICT EXPERIMENTAL SAMPLE WITH POST-PROGRAM JOB,
BY REASON FOR LEAVING SUPPORTED WORK AND AMOUNT OF POST-SUPPORTED WORK FOLLOW-UP DATA

(For Those With Job, Average Number of Months to First Post-program Job in Parentheses)

Reason for Leaving Supported Work	Amount of Post-Supported Work Follow-up Data				Total Sample
	≤ 6 months	7 to 12 months	13 to 18 months	> 18 months	
Exhausted Allowable Time in Program[a]	71.4 (0.6)	80.0 (0.1)	86.5 (5.2)	75.0 (3.7)	82.5 (4.1)
To Take Another Job or to Enroll in School or Job Training	100.0 (0.0)	91.7 (0.5)	100.0 (0.5)	81.8 (2.3)	89.1 (1.2)
Poor Performance[b]	0.0 (n.a.)	29.4 (3.2)	69.2 (3.9)	66.7 (8.3)	61.9 (7.1)
Other[c]	50.0 (2.3)	44.4 (1.6)	60.7 (2.9)	70.7 (10.2)	63.1 (6.8)

NOTE: The amount of post-Supported Work follow-up data is the number of months between the time the sample member left the Supported Work job and the date of the latest month of continuous follow-up data.

[a] This includes individuals not leaving Supported Work to take another job, to enroll in school or job training, or because of poor performance, but who either spent the maximum number of months in the program or exceeded the maximum calendar time for participation.

[b] This category includes those terminated because of conflicts with the boss or crew members, use of drugs or alcohol, illegal activities or incarcertaion, absenteeism, poor punctuality, or low productivity.

[c] This includes reasons such as low pay and health, childcare, or transportation problems.

n.a. = not applicable.

TABLE A.15a

PERCENTAGE RECEIVING UNEMPLOYMENT COMPENSATION, BY SITE
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Chicago	4.8	3.7	-2.7	5.3	-5.6	9.8	-10.3	18.6
Jersey City	-9.3**	11.4	26.9**	6.6	12.1	6.1	9.6*	2.9
Oakland	-16.3**	17.3	2.2	9.2	-2.5	10.9	-6.5	0.7
Philadelphia	-1.7	3.1	4.1	1.7	5.1	2.0	-2.0	5.7

TABLE A.15b

AVERAGE AMOUNT OF UNEMPLOYMENT COMPENSATION RECEIVED PER MONTH BY SITE
(dollars)

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Chicago	4.90	4.56	-2.69	10.02	-8.95	17.78	-36.00	53.25
Jersey City	-17.80**	20.85	44.86**	16.77	31.24**	8.11	18.25	5.76
Oakland	-8.41	13.95	1.04	8.70	12.91	11.70	-13.39[a]	-3.69[a]
Philadelphia	-1.85	3.02	8.62	2.32	10.90	5.85	4.91	6.32

NOTE: See note to Table IV.1. All data pertain to the full sample, not only to recipients.

[a] There are only four persons in the Oakland sample.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE A.16

PERCENTAGE HAVING CETA, WIN, OR OTHER GOVERNMENT JOBS, BY SITE
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Chicago	-4.1	6.3	-2.4	13.0	2.8	6.2	12.1	3.0
Jersey City	-5.9	6.4	-3.7	12.0	0.0	11.7	3.6	11.1
Oakland	-7.9	11.3	-2.6	10.0	0.2	6.7	0.0[a]	0.0
Philadelphia	0.1	2.5	-1.1	5.4	1.9	4.1	0.1	3.2
Total Sample	-3.9	5.7	-2.5	10.1	1.2	7.7	4.2	5.9

NOTE: These data are not regression-adjusted. No test statistics were computed.

[a] The sample size is four.

TABLE A.17

CUMULATIVE PERCENT ARRESTED, BY SITE AND SITE CHARACTERISTICS
EX-ADDICT SAMPLE

	Months 1-18		Months 1-27		Months 1-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Ex-Addicts[a/]	-8.2**	33.5	-10.9**	43.3	-18.1**	53.2
Site						
Chicago	-6.6	27.3	-7.3	32.9	-18.9	36.7
Jersey City	-5.2	31.4	-14.8**	40.4	-20.2	55.8
Oakland	-8.8	35.4	-24.3*	66.9	[b/]	[b/]
Philadelphia	-23.7**	54.8	-6.0	41.8	-15.0	49.8
Years of Site Operation at Time of Enrollment						
Less than 1	-15.4**	37.0	-12.7	43.5	n.a.	n.a.
1 - 1.5	-6.7	33.6	-8.5	44.8	n.a.	n.a.
More than 1.5	-2.1	29.5	-15.6	36.8	n.a.	n.a.

NOTE: See notes to Table IV.6 for a discussion of subsample sizes and Table VI.4 for a discussion of the periods of observation.

[a/] These overall sample results were estimated from an equation that did not include variables interacting experimental status with site characteristic. Thus, the subgroup results may not weight up to these overall sample values.

[b/] There are only four persons in this Oakland sample.

n.a. = not applicable.

TABLE A.18

NON-SUPPORTED WORK EMPLOYMENT EXPERIENCE, BY AMOUNT OF FOLLOW-UP DATA
EX-ADDICT SAMPLE

	Sample With 18 Months of Follow-up Data		Sample With 27 Months of Follow-up Data		Sample With 36 Months of Follow-up Data	
	Experimentals	Controls	Experimentals	Controls	Experimentals	Controls
Average Month of First Supported Work Termination[a/]	6.7	n.a.	7.1	n.a.	7.9	n.a.
Percentage with Non-Supported Work Employment	58.1	74.8	74.3	74.0	77.9	79.8
Of Those with Non-Supported Work Employment,						
Percentage who found job through						
Supported Work	22.2	n.a.	10.6	n.a.	14.7	n.a.
Employment Service	1.4	9.6	7.0	12.7	6.3	5.3
Percentage with Rollover Jobs[b/]	1.4	n.a.	2.0	n.a.	3.2	n.a.
Percentage with CETA or WIN Jobs	11.0	14.5	15.0	12.5	12.6	10.4
Percentage with CETA, WIN, or Government Jobs	17.8	21.4	21.5	23.8	24.2	21.9
Average Hours Worked per Week[c/]	17.2	16.9	16.0	15.1	16.1	14.1
Average Hours Worked per Week when Worked	34.4	37.8	36.9	35.1	37.7	34.5
Average Wage Per Hour (dollars)[d/]	4.64	4.05	4.21	4.59	4.06	3.94
Average Length of First Continous Spell of Employment (months)	4.5	6.1	5.8	6.6	6.9	8.1
Percent in Their First Job at End of Period	18.1	3.6	6.5	0.6	5.3	1.1
Average Number of Spells of Employment	1.2	1.4	1.5	2.0	1.1	2.2
Average Percentage of Available Weeks Employed	48.6	42.9	42.1	42.0	11.0	40.2

NOTE: These data are not regression-adjusted. Samples used include only those observations for whom continuous data for the indicated length of time (18, 27, or 36 months) were available. Data pertain to the full period covered by the interview data.

[a/] Sixteen percent of the sample left the program more than once. On average, individuals were in Supported Work 6.8 months at the time of their first termination. The overall average length of stay for the full sample (including a few individuals who never showed up for the program) was 6.7 months.

[b/] A participant with a rollover job is one who has the same job as during Supported Work participation, but whose wage is no longer subsidized by Supported Work nor does the Supported Work program provide supervision.

[c/] For experimentals, the average hours worked per week were calculated from the number of weeks since leaving Supported Work. They do not include non-Supported Work hours during program participation.

[d/] These wage rates are calculated as the average, for all individuals who had jobs, of their total earnings divided by the number of hours worked.

TABLE A.19

EXPERIMENTAL-CONTROL DIFFERENCES IN SELECTED PRE-ENROLLMENT CHARACTERISTICS
EX-ADDICT SAMPLE

	Experimental-Control Differential	Control Group Mean
Average Age	0.03	27.79
Proportion Male	-0.00	0.83
Proportion Black	0.02	0.73
Number of Dependents	0.06	0.81
Number of Years of Formal Education	0.02	10.46
Number of Weeks Worked Last 12 Months	-0.27	9.79
Average Wage for Those Employed Last 12 Months	-0.24	3.30
Unearned Income Last 4 Weeks	6.64	114.96
Proportion Receiving Welfare	-0.01	0.37
Number of Arrests	0.51	8.29
Number of Convictions	0.15	2.66
Weeks Incarcerated Last 12 Months	-0.64	6.29
Proportion Ever Used Drugs[a/]	-0.00	0.98
Proportion Ever Used Heroin	0.01	0.94
Proportion Ever Used Drugs Regularly[b/]	0.01	0.89

NOTE: These data pertain to the total ex-addict sample and are taken from Jackson et al. (1978). Significance tests were carried out but no significant experimental-control differentials were found.

[a/] This includes only individuals who have used drugs other than marijuana and alcohol.

[b/] Regular use is defined as use of some drug other than marijuana or alcohol as often as once a day for as long as two months.

TABLE A.20

SAMPLE SIZES FOR ANALYSIS OF VARIOUS OUTCOME MEASURES IN VARIOUS TIME PERIODS
EX-ADDICT SAMPLE

	Months Covered by Outcome Measure			
	1-18	19-27	28-36	1-27	1-36
Employment	967	869	310	n.a.	n.a.
Income Sources and Welfare Dependence	747	835	300	n.a.	n.a.
Any Use of Drugs (other than Marijuana) and Additive Drug-Use Index	948	862	307	n.a.	n.a.
Use of Marijuana, Enrollment in Drug Treatment, Drug Use Among Sample Subgroups	721	831	304	n.a.	n.a.
Indicators of Criminal Activities	858	873	291	640	171
Total Potential Sample[a/]	974	885	311	738	242

[a/] This includes all individuals who completed the required interviews: the potential sample for 1- to 18-month outcomes includes all who completed an enrollment, a 9-month, and an 18-month interview; that for 19- to 27-month outcomes includes all who completed an enrollment and a 27-month interview; that for months 28 to 36 includes all who completed an enrollment and a 36-month interview; those for months 1 to 27 and 1 to 36 include all who completed all scheduled interviews up to the 19- to 27- and the 28- to 36-month interview, respectively.

n.a. = not applicable.

TABLE A.21

PERCENTAGE RECEIVING WELFARE BY TYPE OF WELFARE
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Experimental Group Mean	Control Group Mean	Experimental Group Mean	Control Group Mean	Experimental Group Mean	Control Group Mean	Experimental Group Mean	Control Group Mean
Any Welfare	29.0	50.7	40.6	46.7	39.8	40.2	41.5	45.1
Of Those Receiving Welfare, Percentage Receiving:								
AFDC	34.7	34.8	25.1	28.5	29.4	24.4	19.4	23.8
GA	59.1	62.6	72.2	71.0	72.2	74.4	75.0	76.2
Other[a/]	7.9	5.6	3.7	2.6	2.8	3.2	5.6	2.4
Total Number of Recipients	325		380		336		78	

NOTE: These data are not regression-adjusted.

[a/] Other includes SSI and other unspecified welfare income.

TABLE A.22

ENROLLMENT BY SITE AND TIME PERIOD
EX-ADDICT SAMPLE

	Site				All Sites	
	Chicago	Jersey City	Oakland	Philadelphia	Number	Percentage
Enrollment Period						
March-December 1975	51	86	0	78	215	15.4
January-June 1976	65	183	72	176	496	35.2
July-December 1976	103	169	44	170	486	34.6
January-July 1977	80	61	28	41	210	14.9
Total						
Number	299	499	144	465	1407	100.0
Percentage	21.3	35.5	10.2	33.0	100.0	

NOTE: These figures are only for ex-addicts who completed an enrollment (baseline) interview, including 13 persons who completed a substitute baseline interview at the time of a scheduled follow-up survey. This includes all but 26 of those subjected to random assignment.

TABLE A.23

CUMULATIVE PERCENT ARRESTED, BY INDIVIDUAL DEMOGRAPHIC AND BACKGROUND CHARACTERISTICS
EX-ADDICT SAMPLE

	Months 1-18		Months 1-27		Months 1-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Total Sample	-8.2**	33.5	-10.9**	43.3	-18.1**	53.2
Years of Age						
Under 21	-3.5	36.6	14.5	34.7	39.1[a/]	20.7
21 - 25	-12.0**	37.9	-10.9*	46.8	7.7	64.1
26 - 35	-3.9	31.1	-11.1*	43.4	-30.8**	56.6
Over 35	-14.1	27.1	-26.5**	36.8	-14.6	23.3
Sex						
Male	-8.1**	36.3	-11.4**	46.7	-17.4*	54.7
Female	-8.3	23.1	-9.4	30.0	-14.8	45.3
Race/Ethnicity						
White	-6.6	30.7	-29.4**	50.6	-13.0	56.7
Black	-9.2**	34.7	-7.6*	41.6	-20.3**	54.1
Hispanic	-0.8	27.4	-13.9	49.4	7.0[a/]	39.3
Years of Education						
8 or less	#	29.0	5.4	37.6	-33.7	52.3
9 - 11	7.7	34.1	-14.3**	46.6	-21.6*	63.2
12 or more	-8.8**	34.9	-21.4*	40.1	0.9	29.1
Welfare and Food Stamp Receipt in Month Prior to Enrollment	-14.6**		#		-28.8**	59.3
None	-13.3	38.3	-19.4**	47.9	-5.7	46.9
Some	-2.6	28.5	-2.4	38.7		
Dependents	#		-8.8*	41.7	-24.0**	58.2
None	-3.5	30.5	-14.5**	46.0	-5.4	44.0
One or more	-15.6**	38.5				
Months in Longest Job					-65.9[a/b/]	64.7
Never employed	-4.3	19.7	-32.9*	49.4	-21.3	49.6
1 - 12	-9.6*	36.8	-16.0**	47.6	-10.1	54.8
More than 12	-7.5*	32.6	-5.5	39.7		
Weeks Worked Year Prior to Enrollment[c/]						
0	-6.6*	32.3	-9.0**	42.7	-19.6*	55.6
5	-7.4**	33.0	-10.1**	43.0	-18.7**	54.6
10	-8.3**	33.7	-11.1**	43.4	-17.8**	53.6
Prior Drug Use						
Used heroin and cocaine regularly	-0.3	35.4	0.1	51.2	25.0	59.3
Used heroin regularly but not cocaine	-9.3**	33.2	-12.5**	42.1	21.7**	52.6
Did not use heroin regularly	-6.5	37.7	-10.5	53.6	6.8	50.8

Table A.23 (continued)

	Months 1-18		Months 1-27		Months 1-36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Drug Treatment Last Six Months						
Methadone maintenance	-7.8*	36.0	-8.9	44.8	-15.5	49.9
Other type of program	-12.3*	32.6	-16.9	41.8	-13.0	66.2
Not in treatment	-14.3	39.2	-14.4	46.9	-16.1	40.3
Number of Arrests[c/]						
4	-6.6*	34.1	-8.7**	44.1	-14.1	54.5
9	-8.4**	37.6	-11.5**	47.9	-18.4**	62.4
14	-10.3**	41.1	-14.3**	51.7	-22.9**	70.3
Months Since Incarcerated						
Never incarcerated	-6.2	35.4	-13.3*	49.1	-12.4	47.4
12 or less	-6.4	38.1	-7.3	43.0	-22.0	47.6
More than 12	-12.4*	26.7	-11.8	36.3	-17.5	68.1
Parole or Probation at Enrollment						
Not on parole or probation	-4.7	27.6	-12.9**	39.5	-13.4	56.2
On parole or probation	-11.9**	40.0	-9.9*	47.7	-22.3	49.7

NOTE: See notes to Table IV.6 for a discussion of subsample sizes and Table VI.4 for a discussion of the periods of observation.

[a/] These data are based on a sample of fewer than 20 persons.

[b/] Negative point estimates of experimental or control group means arise because linear regression analysis rather than probit analysis was used.

[c/] These estimates of subgroup effects and means are based on a linear (or piecewise linear) specification of the sample characteristic, evaluated at the specified points.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE A.24

NUMBER OF ARRESTS BY MOST SERIOUS CHARGE
EX-ADDICT SAMPLE

Most Serious Charge[a]	Months 1-9 Experimental-Control Differential	Months 1-9 Control Group Mean	Months 10-18 Experimental-Control Differential	Months 10-18 Control Group Mean	Months 19-27 Experimental-Control Differential	Months 19-27 Control Group Mean	Months 28-36 Experimental-Control Differential	Months 28-36 Control Group Mean
Murder	0.004	0.000	0.002	0.004	-0.0002	0.002	-0.007	0.007
Felonious Assault	0.003	0.012	-0.011	0.024	0.006	0.010	-0.027*	0.027
Robbery	-0.039**	0.049	-0.013	0.029	0.005	0.017	-0.001	0.013
Burglary	-0.011	0.047	-0.018	0.038	-0.017	0.036	-0.007	0.013
Larceny	0.008	0.040	0.004	0.033	0.012	0.031	0.005	0.020
Narcotics	-0.0004	0.042	-0.022*	0.042	-0.025*	0.051	-0.021	0.033
Other Personal Charges	0.002	0.005	0.001	0.007	-0.001	0.007	0.006	0.000
Other Miscellaneous	0.027	0.037	-0.009	0.044	-0.015	0.060	-0.002	0.040
Unspecified[b]	0.002	0.026	0.009	0.022	-0.001	0.005	0.000	0.000
Total Number of Arrests	0.002	0.255	-0.060*	0.243	-0.035	0.219	-0.052	0.153

NOTE: These data are not regression adjusted. Charges were specified for only the three most recent arrests.

[a] Charges, in order of seriousness, are: murder, felonious assault, robbery, burglary, larceny, motor-vehicle theft and other property crimes, other crimes against persons, drug-related crimes.

[b] Arrests for which the charge was not reported are classified as unspecified.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

TABLE A.25

CUMULATIVE NUMBER OF ARRESTS BY MOST SERIOUS CHARGE
EX-ADDICT SAMPLE

Most Serious Charge[a]	Months 1-18 Experimental-Control Differential	Months 1-18 Control Group Mean	Months 1-27 Experimental-Control Differential	Months 1-27 Control Group Mean	Months 1-36 Experimental-Control Differential	Months 1-36 Control Group Mean
Murder	0.002	0.005	0.002	0.006	0.011	0.011
Felonious Assault	-0.012	0.038	-0.023	0.054	-0.009	0.063
Robbery	-0.057**	0.080	-0.074**	0.102	-0.105**	0.126
Burglary	0.028	0.085	-0.070**	0.137	-0.146**	0.200
Larceny	0.014	0.071	0.019	0.099	-0.028	0.179
Narcotics	-0.032	0.087	-0.049	0.127	-0.061	0.147
Other Personal Charges	0.003	0.012	-0.005	0.019	0.011	0.010
Other Miscellaneous Charges	0.017	0.082	0.018	0.127	0.014	0.158
Unspecified[a]	0.019	0.047	0.016	0.057	0.012	0.063
Total Number of Arrests	-0.071	0.504	-0.163**	0.726	-0.292*	0.947

NOTE: These data are not regression adjusted. Charges were specified for only the three most recent arrests.

[a] Charges, in order of seriousness, are: murder, felonious assault, robbery, burglary, larceny, motor-vehicle theft and other property crimes, other crimes against persons, drug-related crimes.

[b] Arrests for which the charge was not reported are classified as unspecified.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

APPENDIX B

EFFECTS OF THE PROGRAM ON INCOME, IN-KIND TRANSFERS, HOUSING, HOUSEHOLD COMPOSITION, AND HEALTH-CARE UTILIZATION

A primary goal of employment and training programs aimed at groups such as the ex-addict population is to improve the economic status of participants and former participants while reducing their dependence on public-assistance programs. During the month prior to their enrollment in the national Supported Work demonstration, ex-addict sample members had reported incomes of approximately $263 per month, over one-third of which was from welfare and food stamp bonuses (see Jackson et al., 1978). In Chapter IV we found that Supported Work had a significant effect on the earnings of experimentals, primarily in the periods in which they were eligible to participate in the program, but also in some later periods. In this Appendix we investigate the extent to which the program benefited participants in the form of increased total income and the extent to which it benefited taxpayers in the form of reduced public-assistance costs.[1/]

We consider program impacts on total income and its sources both during the time when experimentals were in their Supported Work jobs and afterwards. The main income sources for ex-addicts included earnings, welfare (AFDC, GA, SSI, and other welfare),[2/] food stamp bonuses, and

[1/] We consider only income obtained through legitimate means. Differences in illegal income were presented in Chapter VI.

[2/] In the month prior to enrollment, about 28 percent of those receiving welfare received AFDC, 65 percent received GA, and the remainder received SSI and other unspecified assistance.

unemployment compensation. A small amount of income was also received from sources such as pensions, alimony, child support, and job training. Cash transfers were sometimes supplemented by in-kind benefits such as medical and housing assistance.[1/] Figure B.1 identifies the components of these different income sources.

A. PROGRAM EFFECTS ON INCOME

In this section we examine the effects of Supported Work on the receipt of income by the individual from each of five sources: earnings, unemployment compensation, welfare, food stamps, and other programs or private transfers.[2/] The receipt of in-kind assistance is discussed in the next section.

1. Overall Effects on Total Income and Its Sources

Supported Work does tend to improve the economic status of participants, both while they are in the program and subsequently. One summary measure of economic status is the ratio of income to the poverty threshold. The poverty threshold depends on the size and composition of the family, while the income that we compare to this threshold is only the income received by the respondent. To the extent that other family members also received income, the ratios presented in Table B.1 will understate the economic status of the families. However, since the program had no

[1/] Individuals may also have benefited from income received by other members of their household. Because the average value of such income was small and because we found little or no evidence of program effects on receipt of such income, we do not discuss this income source further.

[2/] Results for employment rates and earnings may vary from those presented in Chapter IV, since only individuals with valid data for all income sources are included in analysis samples discussed here.

FIGURE B.1

CATEGORIES OF INCOME AND IN-KIND BENEFITS USED IN THE ANALYSIS

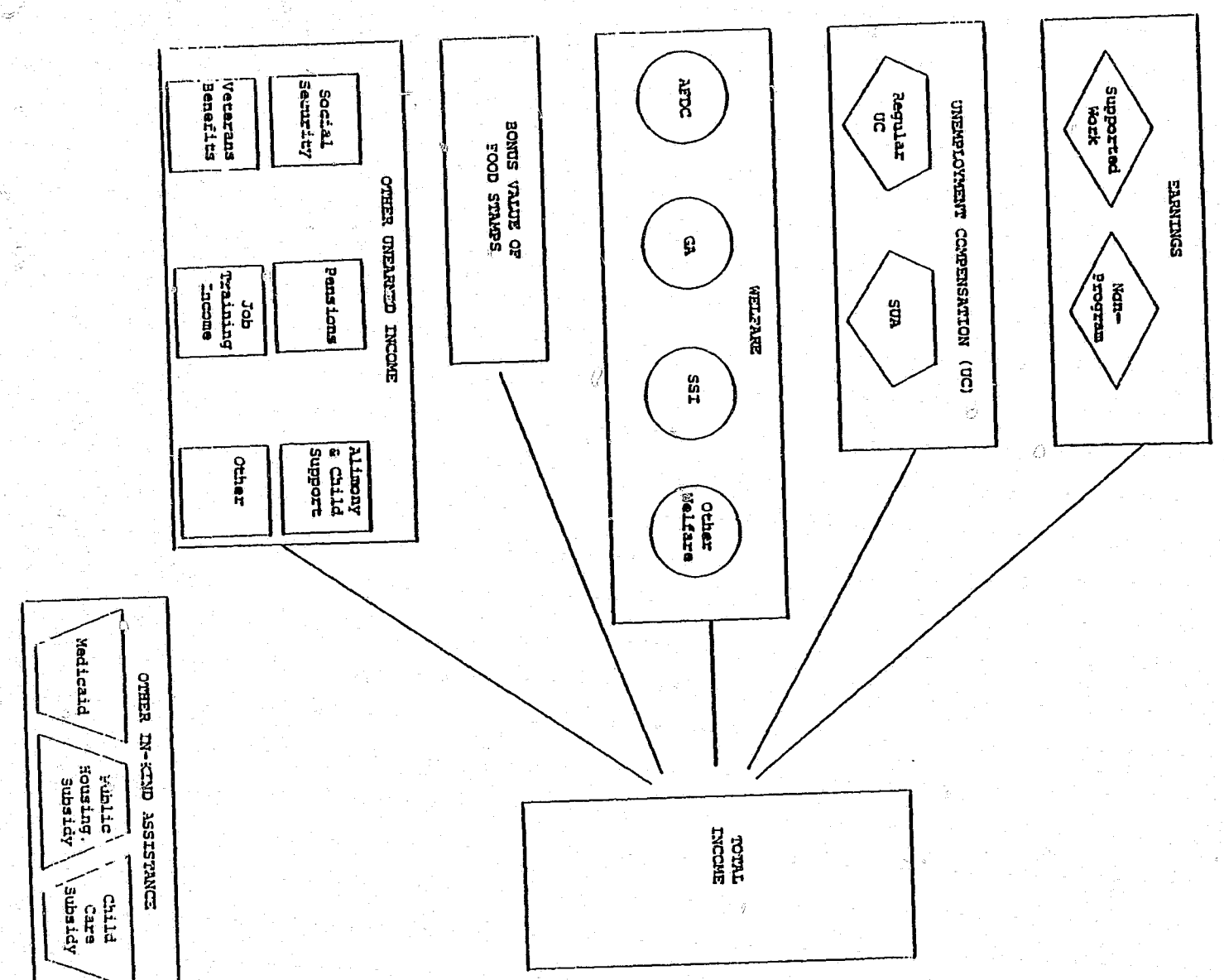

TABLE B.1

TOTAL INCOME IN RELATIONSHIP TO THE POVERTY LEVEL

EX-ADDICT SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Percent of Families With Income Below the Poverty Level	-31.8**	63.6	-12.5**	62.9	-3.7	59.8	-7.6	61.5
Average Income as a Percent of the Poverty Level	43.3**	97.3	9.8	108.6	11.3	109.5	29.4**	103.5

NOTE: These data are based on reported income of respondents only and on the relationship and ages of only immediate family members (spouse and own children).

Poverty level income is calculated based on 1976 standards for families (1) with male and female heads, (2) different numbers of members, and (3) different numbers of children under 18. The poverty-level income thresholds for families with various combinations of the above characteristics are reported in Current Population Reports: Consumer Income, Washington, D.C.: U.S. Department of Commerce, Bureau of the Census, July 1978 (Table A.2). For comparison purposes, incomes of sample members were adjusted by the GNP Index (reported in Business Conditions Digest, Volume 17, Number 1, January 1977, and Volume 19, Number 2, February 1979) to constant fourth-quarter 1976 dollars.

**Statistically significant at the 5 percent level.

effect on the other family members' income or on the family composition, the ratios understate the status of both experimentals and controls by the same percentage.

As can be seen in Table B.1, significantly fewer experimentals than controls had incomes below the poverty threshold in the periods when a substantial number of experimentals were participating in the program. For example, 63 percent of the controls had incomes below the poverty level in months 10 to 18, as compared to 50 percent of the experimentals. While point estimates suggest that this trend of improved economic status persists into later periods, the experimental-control differences in the percentage of families in poverty beyond month 18 are not significant. However, in months 28 to 36 the average income relative to the poverty level of experimentals was significantly higher than that of controls (133 versus 104 percent). The results for the 28- to 36-month sample are larger than those we would expect to observe for the full sample of ex-addicts had they all been followed for this long after their enrollment because of the substantially higher estimate of long-term earnings gains for this cohort as compared with the remainder of the sample.

We investigated two factors that may have produced these program effects on poverty status: the types of incomes that the individuals received and the amounts of income they received from these sources. Table B.2a indicates the percentages receiving income from various sources during each of the four 9-month follow-up periods. As noted in Chapter IV, a significantly higher percentage of experimentals than controls were employed at some time during the first and second nine-month periods after enrollment (95 versus 50 percent, and 64 versus 53 percent, respectively).

TABLE B.2a

PERCENTAGE RECEIVING INCOME FROM DIFFERENT SOURCES

EX-ADDICT SAMPLE

	Months 1 - 9			Months 10 - 18			Months 19 - 27			Months 28 - 36		
	Experimental	Control Group	Differential	Experimental	Control Group	Differential	Experimental	Control Group	Differential	Experimental	Control Group	Differential
Earnings	44.8**	50.2			53.1	3.5		53.0		10.1*	53.9	
Unearned Income												
Unemployment compensation	-5.0**	7.4			4.3	5.2**		6.0		0.5	7.4	
Welfare[a]	-21.7**	50.7			46.7	-0.4		40.2		-3.6	45.1	
Food stamps	-10.1**	45.7			43.3	1.8		38.8		0.6	42.0	
Other[b]	-4.2**	7.1			4.2	-2.1		5.3		2.3	2.2	

TABLE B.2b

AVERAGE INCOME RECEIVED FROM DIFFERENT SOURCES

EX-ADDICT SAMPLE

(dollars)

	Months 1 - 9			Months 10 - 18			Months 19 - 27			Months 28 - 36		
	Experimental	Control Group	Differential	Experimental	Control Group	Differential	Experimental	Control Group	Differential	Experimental	Control Group	Differential
All Sources	134.09**	295.50			344.53	23.17		373.98		93.03**	352.40	
Earnings	201.44**	159.79			220.42	16.42		261.33		101.73**	224.36	
Unearned Income												
Unemployment compensation	-6.59**	10.86			8.42	15.11**		10.31		1.26	16.62	
Welfare[a]	-48.49**	92.88			86.99	-3.12		74.70		-9.83	82.84	
Food stamp bonus value	-6.01**	20.89			22.60	0.37		18.56		0.48	20.90	
Other[b]	-4.66	10.24			4.61	-3.30		7.86		-0.43	7.14	

NOTE: Earnings data reported in this chapter vary somewhat from those reported in Chapter IV, because of a slight difference in the samples used: only individuals who have valid data for all income sources listed in this table were included in the analysis reported here.

[a] Welfare includes AFDC, GA, SSI, and other unspecified welfare income.

[b] Other unearned income includes Social Security, pensions, alimony, child support, and job training income.

*Statistically significant at the 10 percent level.

**Statistically significant at the 5 percent level.

However, the differential narrowed sharply as experimentals left their Supported Work jobs and then increased again in months 28 to 36.[1/]

The trend in the receipt of unemployment compensation reflects the trend in employment rates. Significantly fewer experimentals than controls received unemployment compensation in months 1 to 9. As discussed in Chapter IV, participation in Supported Work could potentially make an individual eligible for unemployment compensation through the Special Unemployment Assistance program. As a result, significantly more experimentals received unemployment compensation in months 10 to 27.[2/] After that time, experimentals had either found new jobs or exhausted their eligibility for benefits; there is virtually no experimental-control difference in the receipt of this type of income in months 28 to 36.

Due to a combination of differences in employment and receipt of unemployment compensation, experimentals were less likely than controls to receive welfare and food stamps during the first 18 months after their enrollment in the demonstration.[3/] However, by the 19- to 27-month

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[1/] This decline in the experimental-control differential was most pronounced in Oakland where, during the 19- to 27-month period, employment rates for experimentals were estimated at 12 percentage points lower than for controls (53 and 65 percent employed, respectively). In the other sites, experimentals maintained a positive differential of 3 to 6 percentage points in months 19 to 27.

[2/] Throughout the 10- to 36-month period, between 60 and 85 percent of those ex-addict experimentals receiving UC were in the Jersey City sample, while receipt among control group members was much more evenly distributed across the sites. This fact may explain at least some of the observed differential in employment results between the Jersey City and the full ex-addict samples.

[3/] Between one-fourth and one-third of those who reported welfare income received AFDC. The remainder received General Assistance or other forms of public assistance.

period, 40 to 45 percent of both groups received welfare and roughly similar percentages received food stamp bonuses.

Receipt of income from other unearned sources, which tend to be less income-conditioned than are welfare and food stamps, was relatively rare in the ex-addict sample (between 2 and 7 percent of the sample received such income). As shown in Table B.2a, fewer experimentals than controls received income from these sources after enrolling in Supported Work, and the differences in the first and second 9-month periods were statistically significant.[1/]

These changes in the types of income received, together with some change in the levels of income among recipients, gave rise to the pattern of income changes depicted by Figure B.2 and reported in Table B.2b. During the first nine months after enrollment, total income of experimentals averaged $430 per month, 45 percent above the control average of $296 per month. Furthermore, 84 percent of the experimental group's income during this period came from earnings, as compared with only 54 percent of the control group's income. Experimentals earned an average of $201 more per month than controls ($361 versus $160) and, as a result, their welfare and food stamp income combined was reduced by an average of $55 per month ($59 versus $114).[2/] During these early months, Supported Work had

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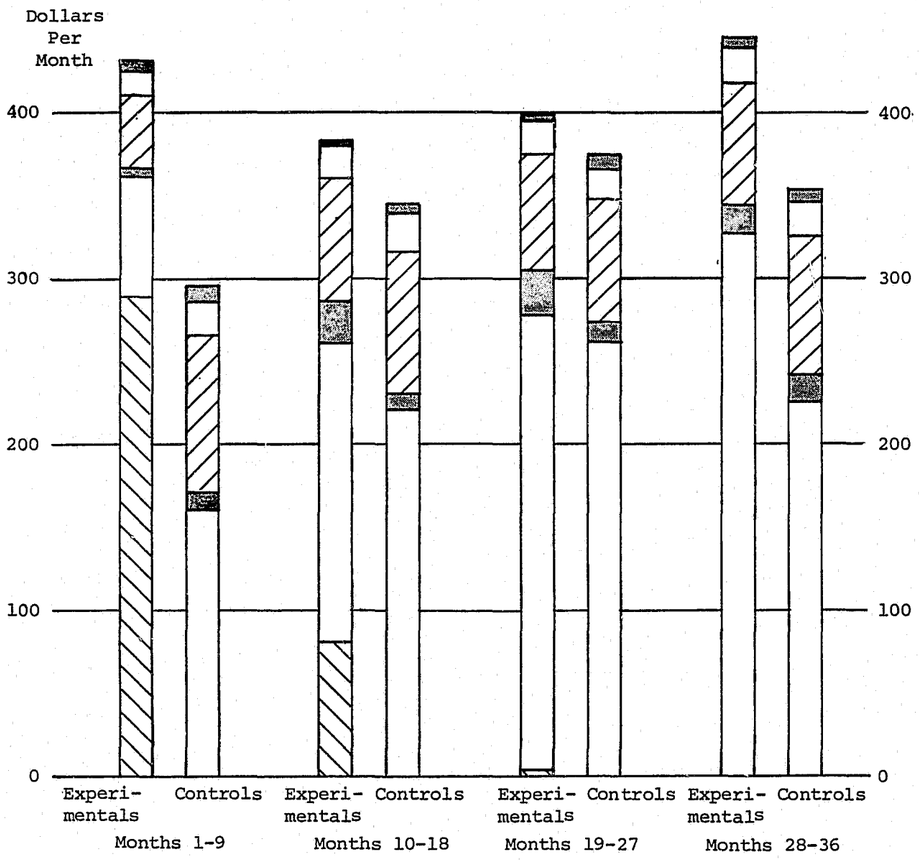
[1/] A large portion of those receiving income from these miscellaneous sources did not report the source to be one of those noted in Figure B.1.

[2/] This reduction in welfare benefits is due almost entirely to a total loss of benefits rather than to a reduction of benefit levels among recipients. Among recipients, experimentals received benefits for seven months, on average, and controls eight months. During those months, recipients in both groups received benefits averaging about $200 per month. Similarly, changes in food stamp bonuses were due mainly to experimentals leaving the rolls rather than to increased costs of the stamps.

FIGURE B.2

COMPONENTS OF TOTAL MONTHLY INCOME
EX-ADDICT SAMPLE



Key:

- Other Unearned Sources
- Food Stamp Bonus
- Welfare
- Unemployment Compensation
- Non-Supported Work Earnings
- Supported Work Earnings

essentially no effect on ex-addict income from other unearned income sources.

By the 10- to 18-month period, when only 28 percent of the experimentals spent any time in Supported Work jobs, average total income of experimentals was not significantly different from that of controls: experimentals received $380 per month, while controls received $345 per month, on average. There were some significant differences between the amounts experimentals and controls received from the various sources, however. Experimentals continued to have significantly higher earnings than controls ($260 versus $220 per month), as well as significantly higher unemployment compensation benefits ($26 versus $8 per month). These increases in employment-related income were only partially offset by a significant $13 per month reduction in welfare benefits. While point estimates suggest small reductions in food stamp bonuses and other unearned income among experimentals relative to controls, these differences were not statistically significant.

For the 19- to 27-month period, the total incomes of experimentals and controls were quite similar ($397 versus $374 per month), with 70 percent of both group's incomes being earnings. The only significant difference in the incomes of experimentals and controls during this period was the experimentals' higher unemployment compensation benefits ($25 versus $10 per month).

For months 28 to 36, experimentals received an average of $93 more per month than controls, a difference that is largely due to experimentals having earned approximately $100 more per month than did controls and having received $10 less per month from welfare than did controls. The

result for this period is estimated with the 36-month cohort, which constitutes less than a third of those for whom 18 months of follow-up data are available. The extent to which this result can be generalized to the full sample depends on how representative the 36-month cohort is of the other groups. As was discussed in Chapter IV, the 36-month cohort consistently had larger experimental-control earnings and welfare income differentials than did the other cohorts. Thus, longer-term effects for the full sample would be expected to be substantially smaller than those observed for the 36-month cohort.

2. Reductions in Public Assistance Income Among Various Sample Subgroups

The results discussed in the preceding section suggest that Supported Work will lead to sizable reductions in transfer payments while the individuals are eligible to participate in the program, but to only modest reductions after that time.[1/] Figure B.3 depicts this trend in benefit levels of experimentals asnd controls.

In an effort to gain greater insight into the welfare impacts of alternative program-targeting strategies, we also investigated the extent of variation in program effects among individuals with various demographic and background characteristics. These results, which are presented in Table B.3, indicate two consistent patterns of differential impacts across subgroups: Supported Work led to significantly greater reductions in welfare benefits among those with one or more dependents and among those

[1/] Among this national demonstration sample of ex-addicts, reductions among participants averaged about $54 per month ($648 per year).

FIGURE B.3

TREND IN RECEIPT OF WELFARE INCOME AND FOOD STAMP BONUSES
EX-ADDICT SAMPLE

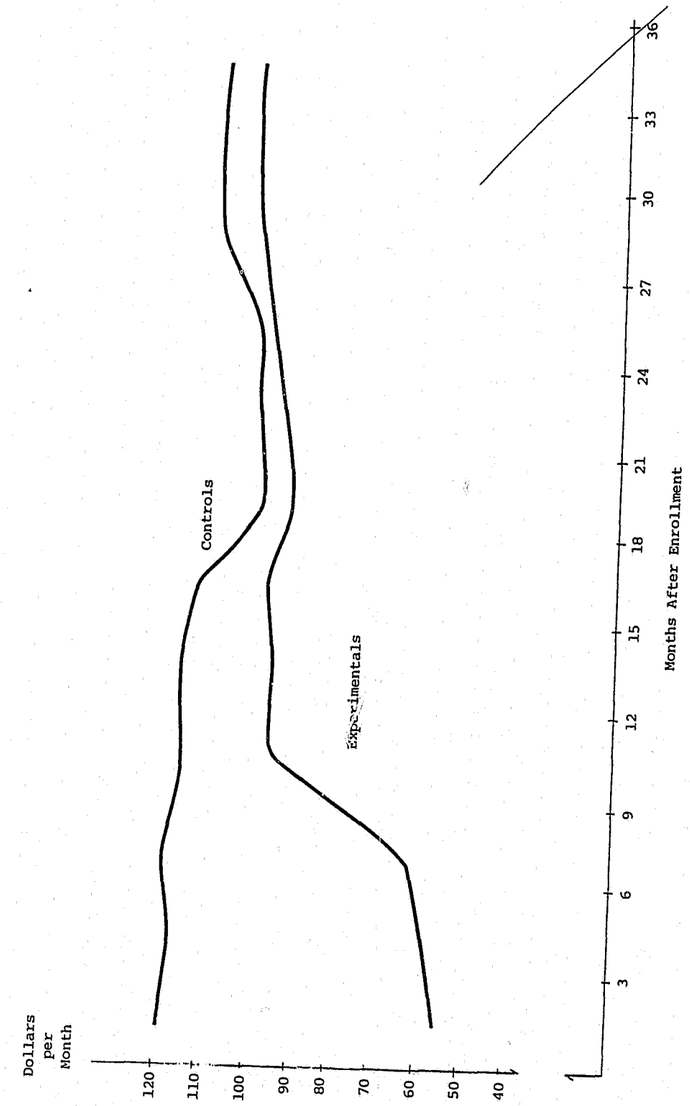


TABLE B.3

AVERAGE MONTHLY WELFARE INCOME PLUS FOOD STAMP BONUS VALUE RECEIVED, BY DEMOGRAPHIC AND BACKGROUND CHARACTERISTICS
EX-ADDICT SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
All Ex-Addicts[a/]	-57.20**	115.18	-18.47**	110.89	-4.04	93.94	-9.45	103.79
Years of Age								
Under 21	-5.96	83.73	40.66	70.98	21.17	47.16	-31.30	134.71
21 - 25	-51.54**	102.66	-8.43	100.57	0.73	84.52	-37.29	120.00
26 - 35	-70.58**	126.71	-36.03**	123.47	-12.80	104.12	-0.20	93.11
Over 35	-55.33**	130.32	-18.80	119.76	-4.56	119.02	37.89	82.00
Sex								
Male	-58.31**	101.05	-17.73*	90.52	-10.54	81.21	-8.69	84.23
Female	-53.50**	169.10	-21.25	188.66	27.18	155.00	-24.03	227.33
Race/Ethnicity			#					
White, non-Hispanic	-61.01**	106.69	-37.77	102.59	-10.03	71.33	48.44	39.16
Black (or other), non-Hispanic	-56.70**	114.71	-6.30	107.16	0.97	96.39	-12.51	106.78
Hispanic	-56.41*	136.92	-108.52**	166.70	-39.15	107.81	-88.64	172.45
Years of Education					#			
8 or less	-89.44**	140.66	-30.43	127.95	44.83**	78.24	-10.62	130.47
9 - 11	-53.39**	120.84	-16.12	117.27	-15.78	107.41	-4.53	93.13
12 or more	-48.78**	91.39	-17.01	89.95	-3.40	74.63	-25.70	116.76
Welfare and Food Stamp Receipt in Month Prior to Enrollment								
None	-43.83**	68.22	-5.83	70.92	4.88	66.10	-27.01	105.55
Some	-69.85**	158.85	-30.21**	148.07	-12.27	119.64	3.48	102.24
Dependents	#		#					
None	-41.20**	94.73	-6.02	91.15	4.23	82.77	11.24	84.94
One or more	-81.66**	146.10	-37.28**	140.75	-17.41	112.00	-48.34	135.93

Table B.3 (continued)

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Months in Longest Job								
0	-8.24	101.66	27.77	126.74	-55.69	152.95	26.30	131.81
1 - 12 months	-56.27**	115.12	-30.78**	116.70	3.48	95.29	-31.68	137.58
More than 12 months	-62.63**	116.48	-14.17	105.33	-4.76	87.63	-0.96	79.30
Weeks Worked Year Prior to Enrollment[b/]	#							
0	-67.23**	121.67	-26.18**	115.55	-7.89	102.93	-21.21	109.67
5	-62.16**	118.35	-22.23**	113.17	-6.04	98.61	-17.19	107.40
10	-57.09**	115.03	-18.29**	110.78	-4.18	94.28	-13.16	105.13
Prior Drug Use								
Used heroin and cocaine regularly	-56.94**	108.01	-29.80	114.33	-1.80	103.71	-6.28	93.00
Used heroin but not cocaine regularly	-52.35**	113.26	-12.93	107.51	3.82	90.14	-17.81	107.62
Did not use heroin regularly	-83.91**	131.15	-38.63	125.99	-47.76*	105.87	31.52	89.62
Drug Treatment in Last Six Months								
Not in treatment	-58.71**	121.02	-17.29	99.23	0.35	59.08	21.80	34.54
In methadone maintenance program	-64.18**	126.98	-23.59*	121.68	-5.62	101.65	-35.65	139.38
In drug-free program	-25.01	82.39	-7.83	97.09	-5.80	84.99	32.82	71.97
In other type of program	-74.88**	118.66	-17.98	105.30	0.06	103.64	9.05	51.94
Prior Arrests[b/]								
0	-57.64**	131.17	-40.88	126.97	5.70	100.62	61.50	81.03
4	-57.28**	112.93	-21.24**	111.50	-6.05	93.59	-11.61	103.08
9	-47.27**	113.28	-15.71	108.93	-5.17	93.23	-18.46	106.13
Months Since Incarcerated	#				#			
Never incarcerated	-25.75	99.49	-6.54	107.93	-36.68*	111.56	-65.95	116.98
12 or less	-49.12**	109.26	-9.79	88.95	25.21	70.05	26.87	72.76
More than 12	-88.86**	132.21	-34.50**	129.47	-3.35	99.30	-13.56	115.14

[a/] These data may differ somewhat from those reported in Table B.2b because of the different samples included in the analysis.

[b/] These estimates of subgroup effects and means are based on a linear (or piecewise linear) specification of the sample characteristic.

Estimated program effects vary significantly among the subgroups.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

who worked fewer weeks during the year before baseline. In part, these results are due to the fact that these groups are more likely to receive benefits than those with no dependents or more weeks worked and they are more likely to receive higher monthly benefits in the absence of the Supported Work experience. Thus, there is the potential for higher reductions in welfare benefits in response to increased employment-related income. This difference also reflects the greater increase in work hours among those experimentals who have dependents (see Chapter IV).

As previously noted, we also considered the extent to which the results observed for the subsample with data for months 19 to 27 and for months 28 to 36 are generalizable to those we would expect to have observed had the full ex-addict sample been interviewed in these later time periods. While the estimated effects for the differential subsamples did not vary significantly, the point estimates suggest that the reductions were much larger for those in the 36-month cohort. Thus, we would expect that the average $9 per month reduction in benefits estimated for the 28- to 36-month time period is higher than we would have observed for the full sample.

B. RECEIPT OF IN-KIND ASSISTANCE

As a result of Supported Work's effect on employment and earnings, the in-kind assistance received by experimentals might be expected to change. However, as can be seen from the data presented in Table B.4, the only significant impact is that, during the first nine-month period when most experimentals were in Supported Work jobs, only 34 percent of the experimentals as compared with 45 percent of controls had a Medicaid

TABLE B.4a

PERCENTAGE RECEIVING IN-KIND ASSISTANCE
EX-ADDICT SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Medicaid Card	-10.8**	44.6	-3.6	41.1	-2.2	36.8	-0.3	41.3
Public Housing	-0.4	15.9	-0.9	14.9	0.8	14.8	-1.1	20.6
Rent Subsidy[a/]	-0.5	1.5	-0.4	1.4	0.7	1.3	0.0	0.0

TABLE B.4b

AMOUNT OF IN-KIND ASSISTANCE RECEIVED
EX-ADDICT SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Number of Months with Medicaid Card	-1.0**	3.4	-0.3	3.3	-0.2	3.0	-0.0	3.3
Number of Subsidized Doctor Visits	-0.7	1.8	0.0	1.5	0.0	1.4	2.4*	1.6
Number of Subsidized Hospital Days	-0.4	2.0	-0.3	2.7	0.1	1.9	-0.2	3.6
Monthly Rent Subsidy (dollars)	-0.45	1.36	0.06	1.14	0.28	1.45	1.02	0.00

NOTE: These data are simple subgroup means, except those pertaining to subsidized doctor visits and hospital stays.

[a/] A rent subsidy is defined as rent paid directly to the landlord by the welfare agency.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

card.[1/] However, there was no significant reduction in the receipt of subsidized medical care, and, furthermore, by the 10- to 27-month period, 35 to 40 percent of both experimentals and controls reported having a Medicaid card. Similarly, through the period of observation, about 15 percent of both the experimentals and controls lived in public housing, paying average rents of $85 to $95 per month.

C. PROGRAM EFFECTS ON HOUSEHOLD COMPOSITION, HOUSING, AND HEALTH-CARE UTILIZATION

In the previous section we reported that the Supported Work program significantly increased the income of experimentals, primarily in the first nine-month period, and that much of this increase came from higher earnings. In this section we examine the impact of this increased income on several aspects of the experimentals' lives, including household composition, housing, and health-care utilization.

1. Household Composition

There is some evidence that income maintenance programs increase marital instability of recipients because the increased income makes the spouses more independent of each other (see Hannan et al., 1977). Bishop (1979) reviews evidence that the opposite is the case for increased employment of the husband. Marriages become more stable as the husbands' employment rates increase, perhaps because they are able to perform their socially expected role as provider. Supported Work provides a test of the

[1/] Among both experimentals and controls, those with a card reported having had it for an average of about seven of the nine months.

hypothesis that employment increases marital stability, although the ex-addicts are a highly specialized population.

There is no evidence that Supported Work had an effect on the household composition of the ex-addicts. The percentage currently married for both experimentals and controls during all time periods was between 18 and 20, with no consistent differences. Average household size also remained constant at about 3.5, with negligible differences in the number of dependents.

2. Housing Status

The income maintenance experiments tended to have a small but positive effect on homeownership, and on the housing quality of participants.[1/] Through increasing participant income, Supported Work may have a similar effect on participant housing. Table B.5 presents the estimated program effects on several aspects of housing consumption.

There is no indication that Supported Work had any impact on the housing expenditures of members of the ex-addict sample. Between 2 and 3 percent of both experimentals and controls reported owning a home during the three years following enrollment in Supported Work. Roughly 70 percent of the sample rented privately-owned dwellings, while 15 percent lived in public housing. Private rentals for controls average $135 per month, and experimentals tended to pay about five dollars more per month. These differences were never statistically significant. Public housing rent was roughly 85 to 90 dollars per month for both groups, despite the

[1/] See Ohls (1979) and Institute for Research on Poverty (1976).

TABLE B.5

TYPES OF HOUSING, RESIDENTIAL MOVES, AND HOUSING CONDITIONS
EX-ADDICT SAMPLE

	Months 1 - 9		Months 10 - 18		Months 19 - 27		Months 28 - 36	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Types of Housing[a/]								
Owner-Occupiers (percent)	-0.6	2.2	1.1	1.7	-0.2	3.3	0.4	3.3
Private Renters (percent)	2.3	70.6	-1.5	72.3	3.0	68.8	-5.0	67.4
Average Monthly Rent (dollars)	4.44	131.31	3.55	135.16	6.29	138.69	11.23	136.29
Public Housing Renters (percent)	-0.4	15.9	-0.9	14.9	0.8	14.8	-1.1	20.6
Average Monthly Rent (dollars)	-5.62	90.96	5.79	86.03	-0.45	94.04	-10.58	93.04
Number of Moves	0.1	0.4	0.1	0.4	0.1**	0.4	0.0	0.4
Housing Conditions								
Mugged (percent)	-1.5	9.9	-4.0**	9.4	-0.9	8.2	0.1	5.2
Number of Rooms	-0.3*	4.7	-0.0	4.6	-0.2	4.7	-0.3	4.9
Number of rooms per person	-0.0	1.5	-0.0	1.7	-0.0	1.6	0.1	1.6
Improvements (percent)	4.5	24.6	5.1*	21.8	2.0	24.1	5.7	23.4
Burglarized (percent)	0.4	8.9	-2.3	9.8	-1.1	6.0	3.0	3.8

NOTE: These data on housing conditions are not regression-adjusted. The test statistics are based on t-tests of differences in means between experimentals and controls.

[a/] Percentages living in these various types of housing may not sum to 100 due to some sample members living in institutions and other dwellings.

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.

significantly higher income that experimentals received in the first and final 9-month periods. Experimentals tended to move more frequently than controls--controls averaged 0.4 moves between interviews as compared with 0.5 for experimentals. However, this difference was signficant only for the third 9-month period.

There is no consistent indication that Supported Work affected the quality of experimental housing. Experimentals did have a somewhat greater tendency to make home improvements than controls. It is possible that this is a result of skills learned in Supported Work jobs, many of which involved construction and housing rehabilitation. There were no significant experimental-control differences in the number who reported their homes had been burglarized between interviews, but there was a slight tendency for experimentals to have been mugged less often than controls. This may be due to the experimentals spending more time on jobs and less time on the street than controls rather than to differences in the safety of their neighborhoods.

3. Health-Care Utilization

To the extent that participants have more income than controls, they may spend more on health care. On the other hand, the lower income of controls may lead to poor health and thus, to more health care. Controls may also be more likely to be eligible for subsidized care than experimentals. The situation is further complicated by potential drug use among ex-addicts. To the extent that there are program effects on drug use, there may also be effects on medical care for drug-related illnesses.

Supported Work failed to have any enduring impact on the health-care utilization of the ex-addicts. Experimentals and controls reported

slightly over two visits to the doctor and between three and four days in the hospital in the nine-month periods. Significantly more experimentals than controls reported having seen a doctor in the first 9-month period and having been in the hospital during that period, despite significantly lower Medicaid eligibility for experimentals during this period. During later periods, however, the experimental-control differentials were smaller and not significant. There did not appear to be any consistent program effects on the reasons for seeking medical care. Working experimentals and controls both reported losing about one work day per month due to illness.

4. Summary

In summary, Supported Work did not have any significant impact on the marital stability of participants or on other aspects of their household composition. Further, the program did not influence their expenditures on housing. Experimentals were somewhat more likely than controls to make home improvements, perhaps because of the related skills they acquired on their Supported Work jobs.

With respect to health-care utilization, in months 1 to 9, experimentals were significantly more likely to visit a doctor and to be hospitalized than controls, despite the fact that a lower percentage of experimentals than controls were eligible for subsidized care. There are no program effects on health-care utilization in the later periods, however.

D. CONCLUSION

Supported Work did increase the net income of the participants. In the first nine-month period the increase in earnings among experimentals

was partially offset by a decrease in transfer payments, but on average, experimentals had incomes 45 percent higher than controls during this period. In the second and third 9-month periods the differences in income were substantially smaller and partially reflected the fact that experimentals received more unemployment compensation than controls, particularly in Jersey City. During the last 9-month period, the income of experimentals was significantly higher than that of controls, primarily because of higher earnings. This result is qualified, however, by the fact that the subsample for whom we have the 28- to 36-month data does not appear to be representative of the full ex-addict sample.

Supported Work also benefited taxpayers by reducing public-assistance costs. Experimentals received significantly less welfare than controls in the first and second 9-month periods, and this effect was particularly strong among ex-addicts with dependents. Although experimentals tended to receive less welfare in the later periods, the differences were small and not statistically significant. There were no program effects on in-kind transfers.

The benefits to taxpayers from decreased welfare payments were partially offset by the increased receipt of unemployment compensation by experimentals relative to controls. This was largely due to some experimentals gaining eligibility for the temporary Special Unemployment Assistance program through participation in Supported work.

We have also investigated whether the differences in income had an impact on the other aspects of the participants' lives, including their marital stability, household composition, housing consumption, and their medical-care utilization. We found little evidence of effects along any of these various dimensions.

APPENDIX C

ASSESSING THE IMPACT OF INTERVIEW NONRESPONSE ON EVALUATION RESULTS FOR THE EX-ADDICT SAMPLE

by

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and

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* This appendix is excerpted from a project report "Assessing the Effects of Interview Nonresponse on Estimates of the Impact of Supported Work," Princeton, New Jersey: Mathematica Policy Research, Inc., 1979.

I. THE PROBLEM

The primary methodology used in the analysis of the effects of the Supported Work program is the single equation multiple regression model. In the simplest case, outcomes of interest (such as earnings, employment, and drug use) are regressed on personal characteristics and on a dummy variable equal to one for experimentals and zero for controls. The coefficient on the experimental status variable reflects the difference between experimentals and controls, while the other explanatory variables account for differences in the other characteristics, so that the comparison of experimental and control groups is for groups with similar composition. Alternative specifications include interaction terms between the experimental-control dummy and personal characteristics, in the belief that the program's impact may depend upon the socioeconomic characteristics of the participant. Interactions of the experimental status variables with location or length of site operation may also be included in the model as explanatory variables. The general regression model can be written as

$$Y = XB + \varepsilon, \qquad (1)$$

where Y is the outcome variable, X is a matrix containing demographic and socioeconomic characteristics as well as program variables, ε is a disturbance term, and B is a vector of unknown parameters to be estimated.

Estimation of B is usually accomplished by the use of ordinary least squares (OLS) regression methods, where the OLS estimator can be

written as:

$$\hat{B} = (X'X)^{-1} X'Y. \tag{2}$$

Substituting (1) into (2) we have

$$\hat{B} = B + (X'X)^{-1} X'\varepsilon. \tag{3}$$

For a sample in which no systematic effect is operating to limit the sample available for analysis--that is, an uncensored sample--the expected value of the regression coefficient is

$$E(\hat{B}|X) = B + (X'X)^{-1} X'E(\varepsilon|X). \tag{4}$$

Thus, $\hat{B}$ is an unbiased estimated of B if $E(\varepsilon|X) = 0$; that is, if the conditional mean of the disturbance term is equal to zero. This condition is usually assumed to be satisfied for a properly specified model.

For a censored sample, however, we have the additional conditioning factor of the sample selection rule. Hence,

$$E(\hat{B}|X \text{ and selection rule}) = B + (X'X)^{-1} X'E(\varepsilon|X \text{ and selection rule}) \tag{5}$$

If the conditional expectation of the disturbance term fails to equal zero, the coefficients will be biased. Thus, attention must focus on the relationship between the sample selection rule and the disturbance term ε.

The censoring mechanism in the case under consideration here is failure to obtain a scheduled follow-up interview (for any reason) for an individual. One way to view this mechanism is to imagine that each

individual has an index of response likelihood, R*. Individuals with values of R* exceeding zero will be locatable and will be able and willing to complete the interview. Those with values of R* below zero will not complete interviews. Furthermore, assume that it is possible to identify some characteristics that affect the likelihood of response, such as whether the individual has moved, whether he or she is incarcerated, and a variety of other personal traits. This model can be described as follows:

$$R^* = Z\delta + \eta, \tag{6}$$

where Z is a vector of personal traits affecting responsiveness,[1/] δ is the coefficient vector, and η is a disturbance term. Of course, R* is not observed directly; we only know whether or not an interview was completed:

$$R = \begin{cases} 1 \text{ for } R^* \geq 0, \text{ (i.e., } \eta \geq -Z\delta) \\ 0 \text{ for } R^* < 0, \text{ (i.e., } \eta < -Z\delta) \end{cases}, \tag{7}$$

where R = 1 for respondents and R = 0 for non-respondents.

From equation (5) it can be seen that in order to obtain unbiased coefficients we require

$$E(\varepsilon|X, \eta \geq -Z\delta) = 0. \tag{8}$$

If ε has zero mean and ε and η are mean independent,[2/] this condition

---

[1/] The vector Z may contain many of the same variables as X contains.

[2/] Mean or conditional independence implies that $E(\varepsilon|\eta) = E(\varepsilon)$, a somewhat stronger requirement than zero correlation, unless ε and η are assumed to have a bivariate normal distribution.

is satisfied (for nonstochastic Z). However, if the probability of nonresponse is affected by Y (and therefore by ε), ε and η are not independent, the expected value in equation (8) is not zero, and the regression estimates of the coefficients in equation (1) will be biased.[1/]

This correlation between ε and η may result in two different ways. If Z contains only nonstochastic variables, and there exists an unmeasured variable (e.g., motivation or attitude) that affects both outcomes and the probability of response, then ε and η will be correlated. However, correlation of the disturbance terms of the estimating equations will result even if the disturbance terms in the structural equations are independent if current outcomes affect the probability of responding to requests for interviews. In this case the structural response model can be written as

$$R^* = X\delta_1 + Z^*\delta_2 + Y\delta_3 + \eta^*, \qquad (9)$$

where Z^* contains exogenous variables not included in X, and η^* is a disturbance term possibly uncorrelated with ε.[2/] Substituting equation (1) in (9) to obtain an equation that can be easily estimated we have

$$R^* = X\delta_1 + Z^*\delta_2 + (X\beta + \varepsilon)\delta_3 + \eta^* \qquad (10)$$

$$= X(\delta_1 + \beta\delta_3) + Z^*\delta_2 + (\eta^* + \varepsilon\delta_3),$$

$$R^* = Z\delta + \eta, \qquad (11)$$

[1/] As noted by Peck (1973) and others, if the probability of nonresponse is related only to the regressors (X's) or is random, no nonresponse bias results.

[2/] Some of the elements of δ_1 will be zero if there are variables in X which affect outcomes but not response.

where $Z = (X, Z^*)$, $\delta = \begin{pmatrix} \delta_1 + \beta\delta_3 \\ \delta_2 \end{pmatrix}$, and $\eta = (\eta^* + \varepsilon\delta_3)$.

Clearly, the disturbance term in the estimating equation (11), which has the same form as equation (6), is correlated with ε, even if the disturbance terms η^* and ε are independent.[1/]

Figure C.1 provides an intuitive explanation of the problem. For a given vector Z, individuals with large negative values of η are more likely to be nonrespondents. If η and ε are positively correlated, the nonrespondents are more likely to be those with large negative deviations (ε) from the true regression line, AB--that is, those corresponding to the circled points in the diagram. Performing regression analysis on the restricted sample would produce an estimated regression line like CD. Comparison of CD with the true regression line AB demonstrates the potential for bias in estimated coefficients arising from nonresponse.

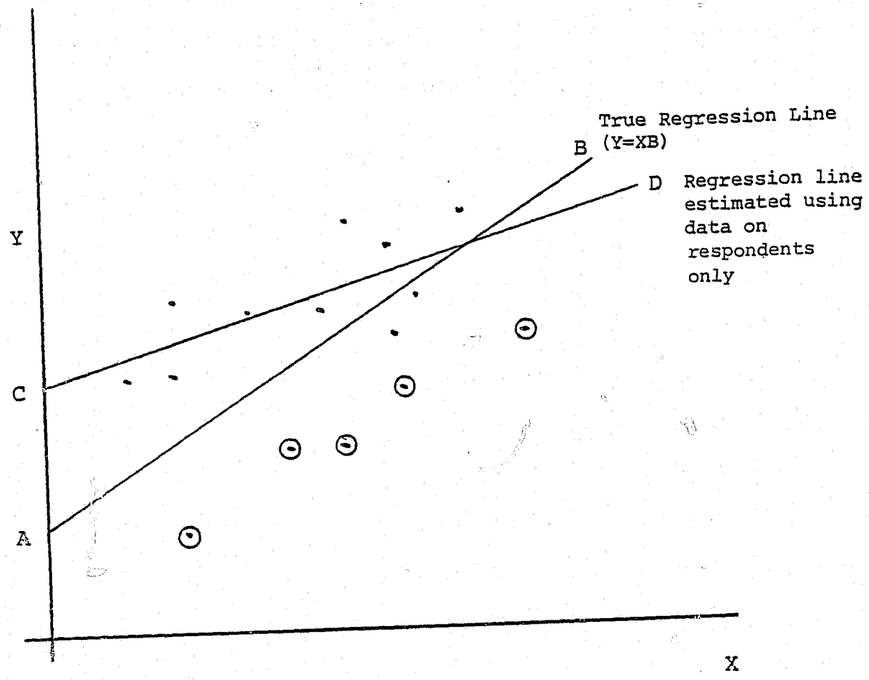
Recent developments in econometric methodology suggest ways of handling the problem of nonresponse bias when data on the variables affecting the probability of response (Z) are observed. Heckman (1976) shows that statistical models characterized by limited dependent variables, sample selection rules, or truncation points have a common structure, and

---

[1/] The only difference between the two behavioral specifications that affects estimation of the model of probability of response is that equations (9) - (11) result in the inclusion of all exogenous variables from the outcome equation (1), including ones not considered to have direct impact on the likelihood of response. Only variables directly affecting response are included in the vector labeled Z under the first specification, (6).

FIGURE C.1

ILLUSTRATED EFFECT OF SAMPLE NONRESPONSE ON ESTIMATES OF REGRESSION PARAMETERS



Note: Circled observations are those omitted from the evaluation sample because of nonresponse.

suggests a simple method of estimating these models that we employ in this analysis.

Heckman's paper deals with the estimation of models like those specified in equations (1), (6), and (7). In an earlier paper (Heckman, 1974), he showed that maximum likelihood methods could be employed to consistently and efficiently estimate the parameters of this model. However, the likelihood method was found to be quite expensive. The more recent paper (Heckman, 1976) shows that consistent estimates can be obtained in a much less costly manner by treating the problem as an "omitted variable" problem. Using our nonresponse model to demonstrate, this can be seen as follows: equation (1) for the i^{th} observation is

$$Y_i = X_i B + \varepsilon_i. \tag{12}$$

Taking expectations, given that the sample available is limited to those who respond ($R^* \geq 0$), gives

$$E\,(Y_i \mid R_i^* \geq 0) = X_i B + E\,(\varepsilon_i \mid R_i^* \geq 0). \tag{13}$$

If we assume that ε and η, the disturbance term in equation (6), follow a bivariate normal distribution, then it can be shown[1/] that

$$E\,(\varepsilon_i \mid R_i^* \geq 0) = \frac{\sigma_{12}}{(\sigma_{22})^{\frac{1}{2}}}\,\lambda_i, \tag{14}$$

where σ_{12} is the covariance between ε and η, σ_{22} is the variance of η, and

---

[1/] See Johnson and Kotz (1972), pp. 112-116

$$\lambda_i = \frac{f\ (Z_i\delta/\sigma_{22}^{\frac{1}{2}})}{F\ (Z_i\delta/\sigma_{22}^{\frac{1}{2}})} \qquad (15)$$

The denominator of λ_i is the probability that $R_i^* \geq 0$ (i.e., the probability that the individual responds to the interview), while the numerator of λ_i is the standard normal density function, evaluated at the point $Z_i\delta/\sigma_{22}^{\frac{1}{2}}$.

Substituting equation (14) in equation (13) we have

$$E\ (Y_i | R_i^* \geq 0) = X_i B + \frac{\sigma_{12}}{(\sigma_{22})^{\frac{1}{2}}}\lambda_i. \qquad (16)$$

Estimation of equation (12) on the sample of respondents will not take into account the final term in equation (16). Thus, the bias that arises from use of this "censored" sample exists solely because the conditional mean of ε_i is omitted from the regression. The bias that results from use of respondent-only data may then be interpreted as arising from normal specification error. This interpretation suggests a simple solution: provide an instrument for the missing variable (λ_i) and estimate equation (16). Heckman (1976) proposes just this solution to the general problem of selection bias. His approach (applied to our model) suggests that if data on the variables (Z) determining the likelihood of response are available, an approximation to λ_i can be obtained by estimating a probit model of response, such as that implied by equations (6) and (7), and then using the estimated coefficients to form $\hat{\lambda}$ for each observation. Equation (16) can then be readily estimated by ordinary least squares regression. Although the equation still must be fit only on data from respondents, any bias that this might impart to the coefficients, $\hat{B}$, is corrected for by

inclusion of the λ_i term,[1/] if the assumptions of the model hold and λ is reliably estimated.

For this study, we are interested only in bias in the coefficient measuring experimental impact. Adding λ to the estimating equation will change our estimate of Supported Work's impact only to the extent that λ is correlated with status. Hence, we shall be particularly concerned with those cases in which experimental status affects the probability of response.

In the next section, a model to explain response to Supported Work interviews is developed, and the results from this estimation are used in the final section to implement Heckman's approach to correct for selection bias.

II. A MODEL OF THE PROBABILITY OF RESPONSE TO SCHEDULED INTERVIEWS

The probability that an individual will respond is assumed to depend upon his or her demographic characteristics, past and present behavior, and experience with the enrollment interview. While this includes many of the same variables that are important control variables in the outcome regressions, equations (9)-(11) suggest that <u>all</u> variables affecting outcomes should be included in the response model, even if they are felt to have no direct impact on response responsibilities. In addition, a number of variables that are assumed to have no impact on

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[1/] The estimates of B are unbiased only asymptotically, since an estimate of λ_i must be substituted for the unobserved true value in the regression.

outcomes but that are felt to affect the probability of response are included in the model. Examples of such variables are the number of moves made during the two years prior to enrolling (since those moving are often the hardest to locate); some variables describing personal living arrangements; expected earnings if employed; whether the individual applied to Supported Work because of some agency pressure to find a job; some indicators of the nature of the interviewing process itself, such as the length and location of the baseline interview; and the individual's degree of cooperativeness in completing the enrollment interview (as reported by the interviewer).

Because the data on these determinants of response are collected from the enrollment interview, the parameters of the model of response to the follow-up interviews can be estimated. From equations (6) and (7), assuming η has a standard normal distribution, we have

$$\begin{aligned} P(R_i = 1) &= P(R_i^* \geq 0) \\ &= P(z_i\delta + \eta_i \geq 0) \\ &= P(\eta_i \leq z_i\delta) \\ &= \frac{1}{\sqrt{2\pi}} \int_{-\infty}^{z_i\delta} \exp(-\eta_i^2/2)\, d\eta_i. \end{aligned}$$

Forming the likelihood function for the sample gives

$$L = \Pi_i [P(R_i = 1)]^{R_i} [1 - P(R_i = 1)]^{1-R_i}.$$

Estimates for the parameters of this probit model, δ, are those values that maximize L, and are readily obtained from a probit computer program.

Sample sizes used in the nonresponse model and for the subsequent outcome regressions for each time period are shown in Table C.1. The number of observations for the 36-month analysis is relatively small and, therefore, may be of limited value. However, for other time periods, sample sizes are generally adequate.

Response equations were estimated for each time period. As the results presented in Table C.2 show, race and living arrangement were important determinants of response to each of the follow-up interviews. Among racial groups, there were substantial and significant differences in the probability of response, with Hispanics being the least likely to respond, followed by whites, and then blacks. Individuals who lived with their parents at enrollment were more likely to respond than were those who did not. (While the magnitude of this effect on response to the 36-month interview was large, it was not statistically significant.)

There were also several factors which influenced response to one follow-up interview but not the others. For example, individuals enrolled at sites that had been operating for a year or longer were more likely to respond to the 9- and 18-month interviews than those enrolled at newer sites. Individuals whose baseline interviews took place in a Supported Work office or had not moved in the two years prior to enrolling or who were described as uncooperative during the baseline interview were all more likely than average to respond to the 36-month interview.

Chi-square tests of the hypothesis that all coefficients in the response equation were equal to zero, rejected this hypothesis at the one percent level for each of the three response models. Furthermore, each model contained at least one significant variable that was not

TABLE C.1

SAMPLE SIZES USED IN ESTIMATING RESPONSE MODEL
AND IN REGRESSION ANALYSIS

(Response Rates in Parentheses)

	Months 1-9[a]	Months 10-18	Months 19-27	Months 28-36
Nonresponse Model	1056 (71)	1056 (71)	892 (74)	300 (66)
Respondents	754	754	664	198
Nonrespondents	302	302	228	102
Outcome Regressions[b]	733	712	634	171

NOTES: The numbers of respondents differ from the sample sizes used in the regression model because of observations with missing data on the specific dependent variables examined. Although these observations are also lost to analysis and thus could be considered nonrespondents, it is unlikely that the same model applies to both interview nonresponse and item nonresponse. Because missing data items could result from several causes (including coding errors) and because the number of respondents with missing values for the desired dependent variables is generally small, item nonresponse is ignored here.

The response rates presented here differ slightly from those given in Table A.3 because observations with missing values on necessary baseline explanatory variables were excluded from this analysis.

[a] Individuals were classified as respondents in the 1-9-month and 10-18-month analyses only if they completed both interviews. Hence, the sample sizes given in the column headed "Months 10-18" apply here as well.

[b] These sample sizes differ from those in the main body of this report because observations with missing data on needed baseline variables were excluded from this analysis, but not from the main analysis.

TABLE C.2

THE IMPACT OF PREENROLLMENT CHARACTERISTICS AND PROGRAM VARIABLES
ON THE PROBABILITY OF RESPONSE TO THE FOLLOW-UP INTERVIEWS

(Excluded Category in Parentheses)

Variable	Follow-up Interview: 9- and 18-Month	27-Month	36-Month
VARIABLES ALSO USED IN OUTCOME ANALYSIS REGRESSIONS			
Member of experimental group	2.42	4.03	-7.90
Site			
(Jersey City)			
Chicago	7.90	-7.04	-20.10
Oakland	-6.28	-25.24***	-111.44***
Philadelphia	-12.93*	-10.84	10.29
Education			
(Less than 9 Years)			
9 - 11 Years	-5.23	0.30	3.56
Over 11 Years	-6.76	3.76	1.07
Age			
(Under 21)			
21 - 25	1.08	3.06	12.85
26 - 35	-1.01	0.45	23.94
Over 35	2.16	6.89	0.73
Male	-5.47	-1.56	17.16
Race			
(Black and Other)			
White	-5.99	-6.62	-5.04
Hispanic	-18.54***	-13.05***	-32.32***
Married	-1.92	-1.21	-7.92
• Household Size	1.42	0.17	-3.28
Any Welfare Prior Year	2.14	0.22	0.70
Any Dependents	4.73	2.40	7.51
Technically Eligible for Own Target Group	-2.98	-11.11*	-1.36
Length of Site Operation			
(Under 12 months)			
12 - 18 Months	10.23***	-0.04	n.a.
Over 18 Months	2.89	-8.34	n.a.
Longest Job Ever			
(None)			
1 Year or Less	3.21	7.68	-36.44
Over 1 Year	3.72	6.36	-40.08*
• Weeks Worked Last Year	0.11	-0.05	-0.12
• Area Unemployment Rate	1.91	0.16	0.67
Time Since Last Incarceration			
(Never Incarcerated)			
12 Months Ago or Less	-4.13	-1.51	-15.98
More than 12 Months Ago	-0.12	1.57	3.09
Had Any Arrest Last Year	6.05	-5.34	-10.62
• Number of Arrests Ever	-0.09	0.20	0.53

Table C.2 (continued)

Variable	Follow-up Interview: 9- and 18-Month	27-Month	36-Month
Regular Use of Heroin (Never)			
Both Heroin and Cocaine	-8.02	-5.91	-10.61
Heroin but Not Cocaine	-4.96	-1.85	14.80
In Drug Treatment During Last Six Months	5.02	12.01	11.75
In Methadone Program During Last Six Months	2.01	3.13	13.22
In Drug-free Program During Last Six Months	6.92*	4.00	11.31
VARIABLES NOT USED IN OUTCOME ANALYSIS REGRESSIONS			
Residence (Institution)			
Own Home	-4.06	-4.85	1.02
Other's Home	-5.30	-4.08	n.a.
Rent	-0.97	-0.85	7.37
Baseline Interview in Supported Work Office	6.57	2.78	26.31**
• Number of Moves in Last 2 Years	-1.81*	0.03	-5.39**
• Expected Wage Per Week Per $100	0.56	3.88*	0.74
Pressured to Find Job	0.66	1.15	-0.29
Live in Public Housing	4.26	-0.35	-6.02
Live with Parents	8.03**	10.89***	15.24
• Length of Interview in Minutes	-0.09	-0.06	0.00
Respondent Cooperative	6.49	0.89	-25.51***

NOTES: The effect of a change in a continuous variable Z_1 on the probability of response is $\delta_1 * f(X\delta)$, where δ is the coefficient on Z_1 in equation (11) (the probit model) and $f(Z\delta)$ is the standard normal density function, evaluated at the point $Z\delta$. This expression also serves, in most cases, as a very good approximation to the effect of a change in a discrete (dummy) variable on the probability of response. Hence, this is the method used to compute the marginal impacts presented here. The density function $f(Z\delta)$ is evaluated using the mean values for all the variables in Z. All of these partial impacts are expressed in terms of percentage points ($100 * \delta_i f(Z\delta)$).

For continuous variables (those marked with • at the left-hand margin), a change of one unit is predicted to lead to a change in the probability of responding equal to the value given, all other factors being equal. For discrete variables (those not marked with •), there may be two or more possible values. Race, for example, has three possible values (black, Hispanic, or white), while "live with parents" has only two possible values (yes or no). For variables with only two possible values, the value given in the table is the difference in the probability of response for those who do and do not exhibit the given trait. For variables with three or more outcomes, the value given is the amount by which the predicted probability of response for individuals with the specified characteristics exceeds the expected response probability for those with the characteristics given in parentheses.

*Estimate of coefficient corresponding to this variable is statistically significant at the 10 percent level (two-tailed test).

**Estimate of coefficient corresponding to this variable is statistically significant at the 5 percent level (two-tailed test).

***Estimate of coefficient corresponding to this variable is statistically significant at the 1 percent level (two-tailed test).

• Indicates variable is continuous. All others are discrete.

n.a. = not applicable.

included in the outcome regressions. Therefore, these models should be able to produce reasonably reliable nonresponse bias-adjustment instruments.

III. THE EFFECT OF NONRESPONSE ON ESTIMATED PROGRAM IMPACTS

Using the above estimates of the parameters of the nonresponse model, we can construct the estimate of that part of the disturbance term in equation (12) that is correlated with the regressors Z. As explained previously, this procedure yields a new variable, $\hat{\lambda}$, which can then be included as an additional regressor in the estimation of equation (12). Under the assumptions of the procedure, this regression produces asymptotically unbiased estimates of the effect of experimental status (and control variables) on the outcome variable (Y) of interest, despite the fact that only data on respondents are used in the regression. Comparison of these results with the estimates obtained with $\hat{\lambda}$ excluded provides evidence of whether or not analysis of data on respondents leads to unbiased inferences about the impact of Supported Work.[1/] Although unadjusted estimates of program effects are presented in the main body of this report, we repeated the calculations on the sample analyzed here in order to ensure that any differences between the adjusted and

[1/] As pointed out previously, the reliability of this evidence depends upon the validity of the assumptions involved in the model. Furthermore, although discrepancies between the alternative estimates suggest that there is likely to be nonresponse bias, a correspondence of the two sets of estimates may indicate only that the model of nonresponse is not good enough to permit detection of bias.

unadjusted estimates of program impact result from the adjustment alone rather than to differences in the samples used.[1/]

Although many different outcome variables are examined in the final reports on the effects of Supported Work, only a subset of the more important outcomes has been selected for examination here. These are hours worked, earnings, whether participants were arrested, and whether drugs were used for each of the four nine-month periods.[2/]

Estimates of the impact of Supported work on each of these outcomes, both with and without correction for possible nonresponse bias, are presented in Table C.3. In general, the alternative sets of estimates are very similar. Estimates that were statistically insignificant prior to adjustment for potential bias remained insignificant, while those that were significant exhibited almost no change. The largest adjustment for a significant estimate was less than 7 percent. Thus, these findings suggest that nonresponse bias does not seem to be a prevailing problem

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[1/] The methodology employed treats as nonrespondents only those who did not submit to an interview. However, observations were also made unavailable for analysis when respondents failed to answer specific key questions. For a number of reasons, including the fact that only a small number of observations was involved, we ignored this type of nonresponse. Also, observations with insufficient data on personal characteristics were excluded from the analysis. These were often respondents who had received early versions of the enrollment interview.

[2/] It should be pointed out that each of these variables is either a binary variable (such as whether arrested) or a limited dependent variable (hours worked). Hence, ordinary least squares regression is not the most appropriate method of analysis. However, for cost reasons it is the primary methodology used throughout the analysis of the effects of Supported Work. Since the purpose of this Appendix is to determine whether the results of these analyses suffer from nonresponse bias, we employ the same estimation techniques. It should also be noted that comparison of the least squares results to those obtained from more appropriate techniques such as probit and tobit showed very little difference.

TABLE C.3

REGRESSION-ESTIMATED EXPERIMENTAL-CONTROL DIFFERENTIALS FOR SELECTED VARIABLES, UNADJUSTED AND ADJUSTED FOR NONRESPONSE BIAS
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 27-36	
Outcome Measure	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted
Hours worked per month	80.86***	81.80***	19.01***	19.43***	4.45	5.36	22.82*	21.06*
Earnings per month	200.08***	200.01***	55.12**	58.91**	16.14	12.21	119.17*	111.28*
Probability of arrest (x100)	-3.11	-2.88	-4.29	-3.34	-1.23	-3.26	0.29	0.10
Probability of drug use (x100)	-0.14	-1.34	-1.76	-1.77	-0.24	-1.89	1.85	1.22

NOTE: These estimates of program impact differ somewhat from those contained in the final reports on the evaluation of Supported Work because sample sizes are smaller here. The sample sizes result from limiting the nonresponse analysis to those individuals for whom all necessary pre-enrollment variables are available.

The significance levels indicated for experimental effects after adjustment for nonresponse may not be strictly accurate because the estimated standard errors used for these significance tests, obtained from the regression program, are biased if the covariance σ_{12} defined in equation (14) is not equal to zero. However, in practice, the true test statistics are usually very close to the ones reported by the regression program. Hence, the significance levels given here are indicative of the actual significance levels.

*Statistically significant at the 10 percent level (two-tailed test).
**Statistically significant at the 5 percent level (two-tailed test).
***Statistically significant at the 1 percent level (two-tailed test).

n.a. = not applicable.

for the analysis, at least when the effects of status a
simple way used here.

However, it is also important to determine whet
Supported Work's impact are biased when such estimates
with characteristics of the program. One finding that
that program impacts differ by site. Hence, we also ex
program impacts obtained from a model that takes this
evidence of nonresponse bias. The results contained in
show almost no evidence of substantial nonresponse bia
more general models.

IV. CONCLUSION

In this Appendix, we have investigated whether nonresponse to follow-up interviews led to biased estimates of the impact of Supported Work. Using demographic and background data obtained from a baseline interview administered to virtually all eligible applicants to the program, we estimated a model to predict the probability of response for each individual. We then used the parameters of this model to construct a new variable that, when included in the regression equation of interest, accounts for the fact that data only on the respondents are available for analysis. Under reasonable assumptions, estimates of Supported Work's impact obtained from the standard regression model with this additional variable included will be free (asymptotically) of any nonresponse bias that may have been present in the unaltered regression model.

TABLE C.4

HOURS WORKED PER MONTH, REGRESSION-ESTIMATED EXPERIMENTAL-CONTROL DIFFERENTIALS, UNADJUSTED AND ADJUSTED FOR NONRESPONSE BIAS
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted
Site								
Chicago	89.76***	90.66***	21.77**	22.09**	13.61	14.14	41.26*	33.90
Jersey City	91.65***	93.60***	20.17***	20.83***	-2.39	-1.72	13.08	11.89
Oakland	61.68***	62.08***	17.30	17.46	-17.24	-16.54	a/	a/
Philadelphia	62.09***	63.38***	15.17	15.58	13.26	13.65	17.47	20.10

NOTE: These estimates of program impact differ somewhat from those contained in the final reports on the evaluation of Supported Work because sample sizes are smaller here. The sample sizes result from limiting the nonresponse analysis to those individuals for whom all necessary pre-enrollment variables are available.

The significance levels indicated for experimental effects after adjustment for nonresponse may not be strictly accurate because the estimated standard errors used for these significance tests, obtained from the regression program, are biased if the covariance σ_{12} defined in equation (14) is not equal to zero. However, in practice the true test statistics are usually very close to the ones reported by the regression program. Hence the significance levels given here are indicative of the actual significance levels.

a/ Only one observation for Oakland was available.

*Statistically significant at the 10 percent level (two-tailed test).
**Statistically significant at the 5 percent level (two-tailed test).
***Statistically significant at the 1 percent level (two-tailed test).

TABLE C.5

DOLLAR EARNINGS PER MONTH, REGRESSION-ESTIMATED EXPERIMENTAL-CONTROL DIFFERENTIALS, UNADJUSTED AND ADJUSTED FOR NONRESPONSE BIAS
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted
Site								
Chicago	227.41***	227.38***	41.79	44.09	189.12***	138.44***	139.40	160.47
Jersey City	210.56***	210.48***	49.74	54.49	-73.20	-80.74	97.04	91.71
Oakland	197.32***	197.30***	106.50	107.72	-23.13	-31.04	a/	a/
Philadelphia	155.51***	155.46***	56.44	59.44	26.22	21.77	78.97	90.74

NOTE: These estimates of program impact differ somewhat from those contained in the final reports on the evaluation of Supported Work because sample sizes are smaller here. The sample sizes result from limiting the nonresponse analysis to those individuals for whom all necessary pre-enrollment variables are available.

The significance levels indicated for experimental effects after adjustment for nonresponse may not be strictly accurate because the estimated standard errors used for these significance tests, obtained from the regression program, are biased if the covariance σ_{12} defined in equation (14) is not equal to zero. However, in practice the true test statistics are usually very close to the ones reported by the regression program. Hence the significance levels given here are indicative of the actual significance levels.

a/Only one observation for Oakland was available.

*Statistically significant at the 10 percent level (two-tailed test).
**Statistically significant at the 5 percent level (two-tailed test).
***Statistically significant at the 1 percent level (two-tailed test).

TABLE C.6

PROBABILITY OF USING DRUGS (x100), REGRESSION-ESTIMATED EXPERIMENTAL-CONTROL DIFFERENTIALS, UNADJUSTED AND ADJUSTED FOR NONRESPONSE BIAS
EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted
Site								
Chicago	9.67	8.85	3.53	3.36	8.34	6.72	13.20	10.71
Jersey City	0.23	-1.34	-6.21	-6.57	-5.27	-7.42	-0.53	-0.93
Oakland	-24.48**	-24.80**	5.10	5.03	-1.40	-3.66	a/	a/
Philadelphia	-1.30	-2.35	-2.69	-2.92	0.88	-0.39	-5.40	-4.51

NOTE: These estimates of program impact differ somewhat from those contained in the final reports on the evaluation of Supported Work because sample sizes are smaller here. The sample sizes result from limiting the nonresponse analysis to those individuals for whom all necessary pre-enrollment variables are available.

The significance levels indicated for experimental effects after adjustment for nonresponse may not be strictly accurate because the estimated standard errors used for these significance tests, obtained from the regression program, are biased if the covariance σ_{12} defined in equation (14) is not equal to zero. However, in practice the true test statistics are usually very close to the ones reported by the regression program. Hence the significance levels given here are indicative of the actual significance levels.

a/Only one observation for Oakland was available.

*Statistically significant at the 10 percent level (two-tailed test).
**Statistically significant at the 5 percent level (two-tailed test).
***Statisticahly significant at the 1 percent level (two-tailed test).

TABLE C.7

PROBABILITY OF BEING ARRESTED (x100), REGRESSION-ESTIMATED EXPERIMENTAL-CONTROL DIFFERENTIALS, UNADJUSTED AND ADJUSTED FOR NONRESPONSE BIAS EX-ADDICT SAMPLE

	Months 1-9		Months 10-18		Months 19-27		Months 28-36	
	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted	Unadjusted	Adjusted
Site								
Chicago	-1.80	-1.67	-2.19	-1.39	-1.57	-3.54	-1.15	-2.98
Jersey City	-7.81*	-7.53	-0.35	1.29	-5.74	-8.37*	-2.60	-2.90
Oakland	-13.24	-13.18	-13.24	-12.82	4.84	2.09	a/	a/
Philadelphia	7.02	7.21	-9.14*	-8.10	3.82	2.28	4.71	5.37

NOTE: These estimates of program impact differ somewhat from those contained in the final reports on the evaluation of Supported Work because sample sizes are smaller here. The sample sizes result from limiting the nonresponse analysis to those individuals for whom all necessary pre-enrollment variables are available.

The significance levels indicated for experimental effects after adjustment for nonresponse may not be strictly accurate because the estimated standard errors used for these significance tests, obtained from the regression program, are biased if the covariance σ_{12} defined in equation (14) is not equal to zero. However, in practice the true test statistics are usually very close to the ones reported by the regression program. Hence the significance levels given here are indicative of the actual significance levels.

a/Only one observation for Oakland was available.

*Statistically significant at the 10 percent level (two-tailed test).
**Statistically significant at the 5 percent level (two-tailed test).
***Statistically significant at the 1 percent level (two-tailed test).

To determine Supported Work's impact on a select set of outcomes, both the standard regression equation and the augmented equation were estimated for each outcome. Comparing the alternative sets of estimates, both for simple models and more flexible models that allow estimates of program impacts to vary by site, we found little evidence of nonresponse bias.

Although the conclusions above are clearly indicated from the results obtained, they are valid only if the assumptions on which the methodology is based hold. The key assumptions of this methdology, developed by Heckman (1976), are:

- That the disturbance terms in the regression and response equations are distributed as bivariate normals
- That a reliable model of the response equation is specified and estimated.

A method for testing the normality assumption is not readily available, since estimates of the residuals in the response equation cannot be obtained. However, if we can do a good job of predicting response, then we have more confidence in our conclusions about whether response bias is a problem. Clearly, if we add a variable that is just random noise to the model, we would expect little change in the original coefficients, including the one on status.

Another aspect of doing a "good" job of predicting the probability of a response is to identify and include variables that affect the response decision but do not affect the outcome of interest. The presence of such variables will lessen the likelihood that multicollinearity between the

constructed variable (λ) and the standard regressors (including experimental status) will confound the results.

Judging from χ^2 tests for the sets of coefficients and standard "t tests" for individual coefficients, it appears that the estimated models of nonresponse are reasonably reliable. Furthermore, in every case, at least one of the variables that was included in the response equation but not in the outcome equation was found to be a statistically significant determinant of response. Thus, we have reason to believe that the model does yield reasonable predictions of the probability of response and does not introduce a high degree of collinearity into the regression model. Without actual data for the missing observations, we cannot be certain of the accuracy of our claim that nonresponse bias is minimal. However, our results do not appear to suffer from problems that we know could lead to erroneous conclusions about the presence of nonresponse bias.

APPENDIX D

THE EFFECTS OF LENGTH OF TIME SPENT IN SUPPORTED WORK ON PROGRAM IMPACTS FOR EX-ADDICTS

by

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and

Anne Mozer

A key difference among Supported Work participants that potentially would influence the effectiveness of the program is the length of time spent in Supported Work. Individuals who dropped out of Supported Work shortly after entering might not receive the beneficial effects hypothesized to accrue to participants, while the magnitude of the program impacts for those staying longer might be expected to increase with the length of time spent in the program. This appendix assesses the extent to which results reported in the main body of this report are sensitive to the amount of exposure to the program treatment.

An intuitive approach to allowing the estimate of experimental impact to vary with length of time spent in the program would be to regress outcomes of interest, such as earnings, hours worked, number of arrests and drug usage, on demographic characteristics of sample members and on a measure of their length of stay in Supported Work (LOS). The experimental impact then could be measured as $\hat{\alpha}$LOS, where $\hat{\alpha}$ is the regression estimate of the effect of length of stay on the outcome of interest.

Unfortunately, this intuitive approach to the problem may lead to erroneous conclusions. If, controlling for measured differences in personal characteristics, individuals leaving the program early tend to be those with the poorest post-program performance, then the estimated coefficient of LOS will pick up not only the effects of Supported Work tenure on post-program outcomes, but also the effects of any unmeasured characteristics that affect both length of stay and performance. For example, if the more motivated individuals tend to stay in Supported Work longer than average and if they also tend to have higher than average post-program earnings, regression estimates will indicate a significant,

positive impact ($\hat{\alpha}$) of LOS on earnings. This result will occur even if length of stay, per se, has no effect whatsoever on post-program outcomes.

Statistically, the problem lies in the fact that length of stay represents a behavioral decision by the participant, much like the labor supply decision, and as such may be correlated with the regression error term, which includes the effects of all unmeasured variables (such as motivation) on the dependent variable. Thus, in this case, least squares regression produces biased coefficient estimates, a problem that is referred to in the econometrics literature as "selectivity bias" in the estimates.

Since LOS is an endogenous regressor, an instrumental variable estimator is required to produce estimates that are asymptotically unbiased. A model to explain experimentals' length of stay as a function of their personal characteristics was defined and estimated. For experimentals, the instrumental variable for LOS to be included in the outcome regressions was then constructed as the predicted value obtained from this model. For controls the instrument was zero.

A Tobit model was used to predict length of stay for experimentals, since LOS is a bounded variable. Furthermore, since it was felt that Supported Work would have no impact on outcomes for those remaining in the program only a short time, all participants remaining in the program for less than two months were considered to have an effective length of stay of zero.

The results of the estimation of the LOS model are presented in Table D.1. They suggest that ex-addicts in Jersey City held their Supported Work jobs for significantly longer periods of time than did

TABLE D.1

ESTIMATED TOBIT COEFFICIENTS AND PARTIAL IMPACTS OF VARIABLES USED TO PREDICT MONTHS EXPERIMENTALS SPENT IN SUPPORTED WORK

(Omitted Variables in Parentheses)

Variable	Estimated Coefficient	Estimated Effect on LOS[a]	t-ratio
Chicago	-2.24	-1.97	-3.12
(Jersey City)			
Oakland	-5.77	-5.07	-5.48
Philadelphia	-3.31	-2.91	-4.61
(≤ 8 Years of School)			
9 - 11 Years of School	0.32	0.28	0.41
≥ 12 Years of School	1.06	0.93	1.24
(Age < 21)			
Age 21- 25	0.72	0.63	0.61
Age 26 - 35	1.03	0.90	0.84
Age > 35	0.29	0.26	0.20
Male	-0.96	-0.84	-1.41
(Female)			
White	-2.31	-2.03	-2.92
Hispanic	0.66	0.58	0.70
(Black and Other)			
Married	0.73	0.64	1.12
(Not Married)			
Household Size	-0.02	-0.01	-0.16
Has Dependents	0.50	0.43	1.24
(No Dependents)			
Received Welfare Prior Month	-1.38	-1.21	-2.51
(Did Not Receive Welfare)			
Eligible for Supported Work	0.29	0.26	0.31
(Not Eligible)			
(Program Age at Enrollment < 12 Months)			
Program Age at Enrollment 12 - 18 Months	-1.57	-1.34	-2.57
Program Age at Enrollment > 18 Months	-0.39	-0.34	-0.56
(No Previous Regular Job)			
Longest Regular Job Lasted ≤ 1 Year	-0.42	-0.38	-0.43
Longest Regular Job Lasted > 1 Year	-0.50	-0.44	-0.55
Weeks Worked in Last 12 Months	-0.07	-0.06	-3.33
(Never in Jail)			
Time Since Jail ≤ 1 Year	0.60	0.53	0.77
Time Since Jail > 1 Year	1.38	1.21	1.97
Number of Arrests Ever	-0.69	-0.61	-0.76
Maximum (Number of Arrests - 8, 0)	-0.01	-0.01	-0.39
On Parole or Probation at Enrollment	0.18	0.16	0.30
(Not on Parole or Probation)			
Used Heroin and Cocaine Regularly	0.39	0.34	0.39
(Did Not Use Heroin and Cocaine Regularly)			
Used Heroin but Not Cocaine Regularly	0.65	0.57	0.85
(Never Used Heroin Only Regularly)			
In Drug Treatment in Last Six Months	-0.79	-0.70	-0.63
Methadone Treatment in Last Six Months	-0.51	-0.50	-0.74
Drug-free Drug Treatment Last Six Months	-0.65	-0.57	-0.83
(No Drug Treatment in Last Six Months)			
Constant	10.25	9.01	5.06

[a] The effect of the i^{th} variable on the expected LOS is computed as $\hat{\beta}_i F(X\hat{\beta}/\hat{\sigma})$, where $\hat{\beta}_i$ is the estimated Tobit coefficient on the i^{th} variable, $\hat{\beta}$ and $\hat{\sigma}$ are the estimated coefficient vector and standard error, respectively, and $F(X\hat{\beta}/\hat{\sigma})$ is the normal distribution function, evaluated at the mean value of the variables.

ex-addicts in the other sites.[1] Whites stayed significantly shorter periods of time than did ex-addicts from other ethnic/racial groups, and those who were receiving welfare at the time they applied to Supported Work, those who enrolled when programs had been operating for one to one and a half years, those with some recent work experience, and those with no arrest history were relatively short-term stayers. The common attribute of these ex-addicts is that their employment alternatives might be expected to be better than the average for ex-addicts.[2]

These estimated relationships between personal characteristics and length of stay were used to construct an instrument for length of stay, and the outcome equations were then estimated using instrumental variable techniques. Table D.2 contains the instrumental variable estimates of the effect of length of stay in Supported Work on hours worked and earnings during months 16 to 18 and 19 to 27 and whether arrested and whether used drugs for the 19- to 27-month period. Also presented for comparison are ordinary least squares regression estimates of the effect of length of stay, which as noted above, are likely to suffer from selectivity bias.

The results for the employment outcomes tend to confirm this suspicion. The least squares estimates suggest that staying in Supported Work longer led to a statistically significant increase in the hours gains of participants, but not to significant changes in earnings, drug use, or arrest effects. The coefficients imply that for each additional month

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[1] This result is also seen in the unadjusted data presented in Table II.5.

[2] However, judging by the results in Tables IV.6 and IV.7, it is not evident that better opportunities elsewhere were the motivation for leaving the program.

TABLE D.2

ESTIMATES OF THE EFFECTS OF AN ADDITIONAL MONTH IN SUPPORTED WORK ON EXPERIMENTAL-CONTROL DIFFERENTIALS

(t-statistics in parentheses)

	Months 16-18		Months 19-27	
Outcome Measures	Ordinary Least Squares Estimate	Instrumental Variable Estimate	Ordinary Least Squares Estimate	Instrumental Variable Estimate
Average hours worked/month	1.09**	-0.20	0.72	0.21
	(2.18)	(-0.30)	(1.29)	(0.27)
Average earnings/month ($)	3.56	-1.95	1.58	-0.48
	(1.46)	(-0.59)	(0.53)	(-0.12)
Percentage Arrested	n.a.	n.a.	-0.01	-0.03
			(0.37)	(-0.74)
Percentage Used Drugs	n.a.	n.a.	-3.40	-0.01
			(-0.95)	(-0.29)
Number in Sample	968		718	

NOTE: These sample sizes and, thus, experimental-control differences differ slightly from those in the text since only observations with data on all of the dependent variables are used.

**Statistically significant at the .05 level, two-tailed test.

n.a. = not applicable.

spent in Supported Work, ex-addicts would work (on average) about one additional hour per month during the 16 to 18 month period. Thus, ex-addicts who remained in Supported Work for seven months (roughly, the average length of stay for ex-addicts) worked seven more hours per month in the 16- to 18-month period than comparable control group members. However, the instrumental variable estimates suggest that there is virtually no effect of length of stay on employment during this or any other period. Implicitly, this finding suggests that the effect of length of stay on outcomes estimated through ordinary least squares techniques was due solely to differences in unobserved characteristics, such as motivation.

For earnings, the probability of being arrested, and the likelihood of using drugs, both sets of estimates indicate that there is no effect of Supported Work regardless of the length of time one stayed in the program. Both the least squares and instrumental variables estimates are small and not statistically significant.

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