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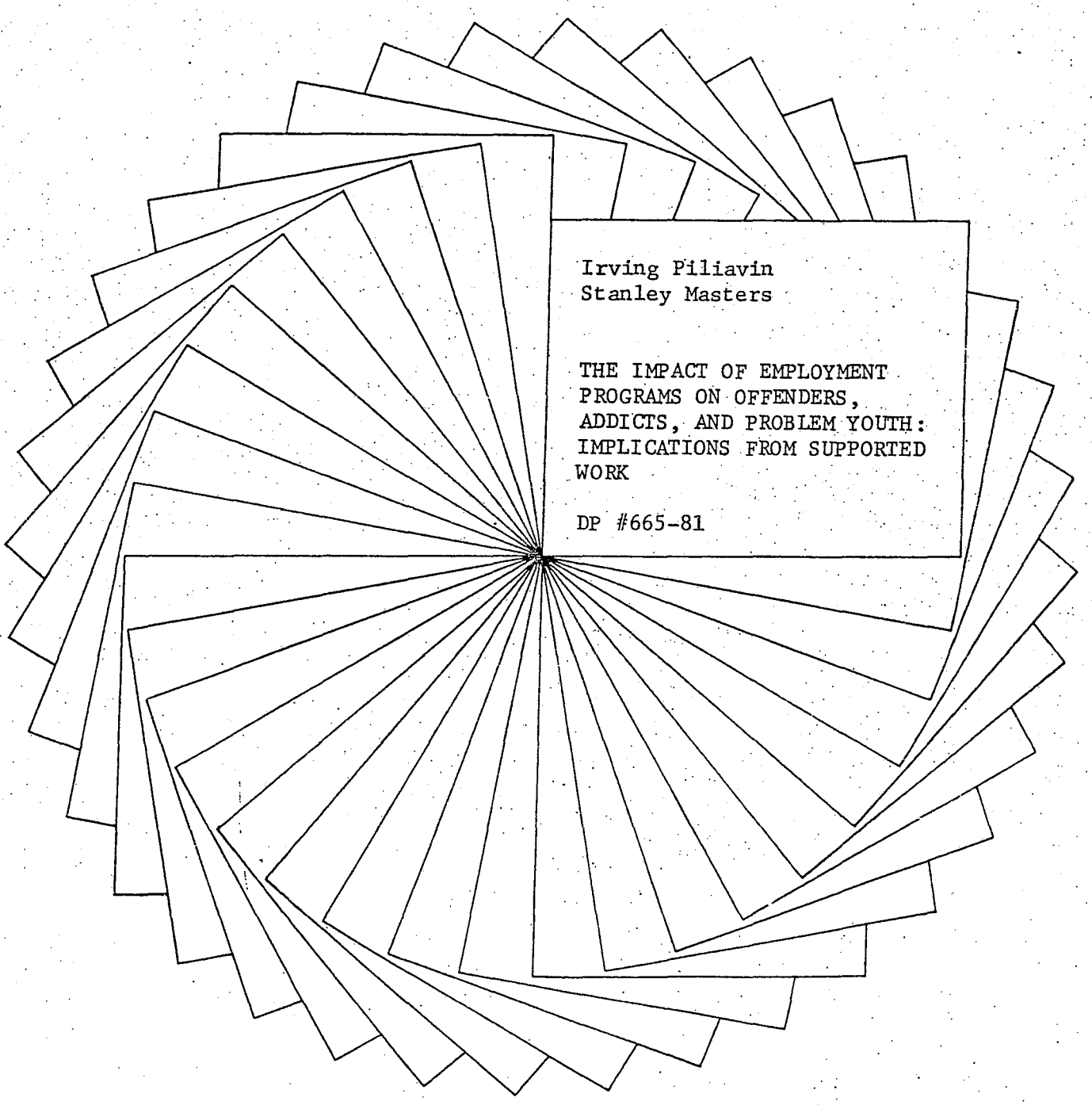

The Impact of Employment Programs on Offenders, Addicts, and Problem Youth: Implications from Supported Work

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ABSTRACT

Over the years, the United States government has initiated numerous programs whose aim has been to improve the employability of individuals who experience problems obtaining and retaining jobs. This paper reviews the results of such programs for individuals previously involved in crime and drug use. It places special emphasis on the impact of Supported Work, the most recent of these programs.

The data suggest that employment-enhancing programs are at best selectively effective. One group of participants who appear to be particularly responsive are those who are past 35 years of age. The possible reasons for this responsiveness and possible policy implications are briefly discussed.

The Impact of Employment Programs on Offenders, Addicts, and Problem Youth: Implications from Supported Work

INTRODUCTION

For almost twenty years, the federal government of the United States has expended large quantities of resources on programs that employ disadvantaged workers, especially disadvantaged youth, and teach them skills. Many of these programs have had as their aim, the putting of slack resources to use. Other programs have had more complex objectives. Their intent has been not simply to use resources but, through training, work experience, and other means, to help individuals become employable. Increased employability is assumed in turn to lead to reductions in the derivative problems these individuals may experience.

The concern of this paper is with the second class of programs, particularly those that deal with individuals previously involved in crime and drug use. Our contention is that by concentrating on serving youth, such programs have neglected disadvantaged adults, for whom such programs often may be more effective. This assertion is based on an experimental evaluation of a major subsidized work-experience program in the United States called Supported Work. For this program, we have found little effect on delinquents' postprogram employment or on their criminal activity during or after program participation. In contrast, for adult offenders and drug addicts, particularly those over 35, we have found increased employment and reduced crime effects.

We begin this paper with a discussion of the kinds of employment and training programs that have evolved in this country, including the rationale for such programs and the groups at which they have been targeted. Next we discuss the Supported Work program and its evaluation. Then we compare the results for this evaluation with results for evaluations of other programs. Based on this evidence, we conclude that there is a reasonable case for redirecting our present work-experience programs toward adults rather than youth.

Although public efforts to improve citizens' job-related skills have a long history in the United States (e.g., public education), special programs for the disadvantaged and chronically unemployed have been a major item on the national political agenda only twice, first during the depression of the 1930s and second for an extended period beginning with the Kennedy administration in the early 1960s and continuing to the present day. The depression programs, primarily designed to put slack resources to use, were largely focused on adult workers. They were terminated in the early 1940s, when the demand for manpower associated with World War II essentially eliminated involuntary unemployment.

The first factor leading to the development of employment and training programs in the early 1960s was the recession of 1958. The high unemployment at that time was often attributed to automation and the replacement of unskilled labor by machines, a diagnosis that led easily to a prescription of the need for retraining workers. An important effort to implement retraining efforts was the Manpower Development and Training Act (MDTA)

of 1962. The initial objective of this program was to develop new skills among family heads who, although having much prior work experience, had been displaced by technological or economic changes. In most crucial respects these individuals were viewed in the same manner as the unemployed of the thirties--motivated and otherwise ready for work but lacking job offers because of lessened demand for their skills.

A second major impetus for the employment and training programs of the 1960s was the civil rights movement. One major area of concern within the movement related to job opportunities for older black workers, whose unemployment problems were similar to those of whites but worsened by racial discrimination. A second area of concern pertained to youth. For these individuals, unemployment was assumed to be due not only to the lack of marketable skills, but to what was called the poverty subculture--here referring to the lack of discipline necessary for sustained employment and to negative attitudes toward education and work. This thesis led many policymakers to propose that society intervene to provide better opportunities for young people to enter the labor force and society's mainstream. The merit of such opportunities was reinforced by the hypotheses of some economists, who saw education as an investment in human capital with a high rate of return. The work of Denison (1962) in particular suggested that this investment accounted for much of the economic growth of this country in the twentieth century. Similar effects were expected from training programs, especially those aimed at young people, since youth have the longest working period ahead of them in which to reap the gains of better training.

The intellectual underpinnings for an emphasis on employment and training programs for youth were reinforced by events during the 1960s. First, while the overall unemployment rate fell dramatically from 6.8% in 1958 to 3.8% by 1966, the rate for those aged 16 to 19 only declined from 15.9 to 12.9%. This relatively small reduction was probably caused in part by the substantial rise in the teenage population during the mid-1960s. A major consequence of this mix of circumstances is that the absolute number of unemployed youth remained constant during the 1960s while that for other population segments declined.

Another critical phenomenon contributing to the developing stress on employment programs for youth at this time was the growth of urban disorder and crime, especially after the riot in the Watts area of Los Angeles in 1965. During the later years of the sixties political and civil rights leaders argued that providing youths with jobs both to increase income and to give them "something constructive to do" would lower the incidence of crime and violence. These arguments initially provided the rationale for the development of summer job programs for teenagers and by the 1970s became the basis for the development of other programs for youth. Among others, the Neighborhood Youth Corps provided community-based work experience and the Job Corps gave training to young people in institutional settings. Later, the Comprehensive Employment Training Act (CETA), established in 1974 and aimed to a large extent at youth, provided community-based work under local government administration. It has been estimated that, in each year from 1965 through

1972, more than half the participants in employment and training programs throughout the United States were aged 16 to 19 and that, since then, the proportion has been just under 50% (Killingsworth and Killingsworth, 1978).

The development of employment programs for offenders and drug addicts finds justification in a long line of studies beginning as early as 1930 (Glueck and Glueck) that has indicated a strong relationship between unemployment and crime. Although early research failed to unravel the causal linkage implied by this relationship, recent studies have provided some support for the hypothesis that unemployment increases the likelihood that individuals will commit crime (Evans, 1968; Cook, 1975). These studies, and the repeated failure of alternative and less expensive efforts to stem recidivism (Lipton et al., 1975), perhaps provided the major impetus for the manpower programs for offenders that began to appear in the early 1970s.

IMPACTS OF EARLY PROGRAMS

Through the mid-seventies the achievements of employment programs for the various population groups they served could not be stated with much certainty, in part because of data problems. Relatively few studies had been undertaken using control or comparison groups; among comparison-group studies, sample selection biases were generally not well controlled; and follow-up periods were generally short. Perhaps as a result of these problems, or perhaps because some programs were run better than others, findings from various studies were

not consistent. Research overviews, however, suggest (Ginzberg, 1980) that the general conclusion of analysts on the merits of job training programs was one of cautious optimism--optimism because of measured employment gains, cautious because of the aforementioned data problems and inconsistencies. Conclusions concerning employment impacts on youth and known offenders specifically were mixed. For the Neighborhood Youth Corp (NYC), a national evaluation of its summer component by Somers and Stromsdorfer (1972) found that the increased postprogram earnings of participants were less than the program's cost. On the other hand, a study of its out-of-school component for Indians found effects that were approximately equal to costs. Evaluations of the Job Corps and the Concentrated Employment Program (CEP) were also mixed, although somewhat more favorable at least for CEP (Kirschner Associates, 1969). However, these evaluations were based on even weaker data than were the NYC studies. The achievements of employment programs for known offenders and addicts through the mid-seventies were also poorly documented, in part because few employment programs targeted these individuals for services. One study of special interest was the experimental evaluation of Project Wildcat, a New York based work experience program for addicts (Vera Institute of Justice, 1974). The results of this evaluation, contrary to those of other programs for offenders and addicts as well as for disadvantaged youth, indicated that, for about two years after program entry, experimentals increased their employment and reduced their criminal activities compared to controls.

This then was the general picture with regard to employment programs for individuals of the type served by Supported Work. There were mixed results for youth. With one exception, there were generally negative results for known criminals and addicts. The exception was Project Wildcat, whose apparent success became central to the development of Supported Work. Wildcat's success was thought to be due to certain program elements not shared by other employment training programs. These included gradual inculcation of participants to work routines, opportunity to work with peers, increasing wages accompanying increased job demands, and other features associated with precepts of learning theory. Because of Wildcat's apparent achievements and innovative character, officials of the Ford Foundation, the Department of Labor, HEW, and other major governmental agencies decided to put the program to test in a nationwide experiment. Three of the groups targeted for the program were previously incarcerated offenders, known drug addicts, and youths known to be--or considered by school officials as likely to become--delinquent.[1] The first two of these groups clearly paralleled Project Wildcat participants. It is not clear, however, how similar the third group was to participants of other youth employment programs. The Supported Work youth sample members were selected partly because of their potential for crime. This frequently was not the case in other programs.

SUPPORTED WORK: PROGRAM AND DEMONSTRATION DESIGN

As finally implemented, Supported Work operated in 21 sites of which 9 were included in the program evaluation for the target groups

being discussed here. Offender participants were recruited at seven sites, while addict and youth participants were recruited at four and five sites respectively. The jobs provided by Supported Work were similar to the generally unskilled or semi-skilled jobs of Wildcat. Also, the demonstration continued to emphasize the key program features that characterized Wildcat. Depending on the site, participants could remain in the program no longer than 12 or 18 months.

The evaluation of Supported Work utilized an experimental design in which participant status at each of the ten evaluation (demonstration) sites was based on random assignments. Sample selection began in March 1975 and continued through July 1977. The evaluation sample included 2200 ex-offenders, 1400 ex-addicts, and 1200 youth. All sample members were scheduled to receive interviews upon enrollment and after 9 and 18 months. Those enrolled prior to 1977 were scheduled to receive an interview after 27 months, and those enrolled prior to April 1976 were scheduled to receive an interview after 36 months.

The characteristics of sample members at the time of their application to Supported Work are presented in Table 1. Most are male, members of minority groups with limited education and work experience. From one-third to one-half of the sample members, depending on the target group, had not held a regular job during the two years preceding sample enrollment. As might be expected, ex-offenders and ex-addicts had extensive reported arrest histories.

The allocations of the analysis samples by target group, site, and reference period for the outcome measures are presented in Table

TABLE 1

CHARACTERISTICS OF THE SUPPORTED WORK RESEARCH SAMPLE AT ENROLLMENT, BY TARGET GROUP

Characteristics	Target Group		
	Ex-Addicts	Ex-Offenders	Youth
Average age (years)	27.8	25.3	18.3
Percent male	80.1	94.3	86.4
Race and ethnicity			
Percent Black, non-Hispanic	77.7	83.6	78.2
Percent Hispanic	8.2	8.8	15.6
Percent White, non-Hispanic	13.8	7.4	5.9
Percent other	0.3	0.2	0.2
Percent currently married	23.1	11.8	3.7
Average number of dependents in household	0.9	0.4	0.2
Education			
Average years of schooling	10.6	10.4	9.7
Percent with 12 or more years	28.5	26.7	0.7
Welfare receipt month prior to enrollment[a]			
Percent with any	39.2	17.1	12.5
Average amount received ($)	79	29	21
Months since last full-time job			
Now working or less than 2	11.6	7.4	12.1
2-12	31.1	20.4	37.7
13-24	20.0	22.3	19.6
25 or more	32.4	38.9	8.6
Never worked	4.9	11.0	21.9
Average weeks worked during previous 12 months	10.0	5.5	9.3
Average earnings during previous 12 months ($)	1,227	580	827
Percent reporting use of heroin			
Regular use[b]	85.4	31.3	2.6
Any use	94.3	44.5	7.8
Percent reporting regular use of any drug other than marijuana[b]	88.5	36.7	4.4
Percent reporting use of marijuana	90.8	80.6	60.2
Percent in drug treatment last 6 months	88.6	12.2	1.7

TABLE 1 (Cont'd)

Characteristics	Target Group		
	Ex-Addicts	Ex-Offenders	Youth
Type of treatment (for those in treatment)			
Methadone maintenance	54.2	n.a.	n.a.
Drug-free program	21.3	n.a.	n.a.
Other type of treatment	24.5	n.a.	n.a.
Arrests			
Percent with any	89.6	99.6	64.2
Average number	8.3	9.2	2.2
Convictions			
Percent with any	74.7	95.0	34.0
Average number	2.9	3.0	0.6
Average number of weeks incarcerated	129	195	20
Percent ever incarcerated	69.9	96.0	27.9
Number in sample	974	1,497	861

SOURCE: Baseline interviews administered to the research sample of individuals (experimentals and controls) at ten sites who completed the baseline, 9-month, and 18-month interviews.

NOTES: Distributions may not add to 100 percent because of rounding. Data on average number of years receiving welfare are available only for the AFDC group. Except as noted, data apply to the entire sample. Questions pertaining to drug use or criminal histories were not administered to the AFDC sample. Similarly, data on type of drug treatment are not available for other than the ex-addict group.

Eligibility requirements for participation in the demonstration specify a history of drug use for ex-addicts and of incarceration for ex-offenders. However, less than 100 percent of the sample of ex-addicts reported drug use and less than 100 percent of ex-offenders reported incarceration. This could reflect either that the ineligibility of certain respondents was not detected by program operators or that the respondents inaccurately reported their histories in these areas during the research interviews.

n.a. = not applicable.

[a]"Welfare" includes AFDC, GA, SSI, and other unspecified cash welfare income.

[b]"Regular" use refers to those individuals who reported drug use at least once a day for at least two months.

2. As seen from these data, analysis of impacts for the various postprogram periods is based on different subgroups of enrollees, distinguished from one another by distribution across sites and by the date of program enrollment. Thus, to the extent that individual characteristics, local labor market conditions, and programs themselves varied for these sample subgroups, the long-term results based on these particular subsamples may not be representative of those that actually occurred (but were not observed) for the full sample. In subsequent discussions of the evaluation findings this possibility will be taken into account.

FINDINGS

In order to test the effectiveness of Supported Work we estimated OLS models of two general forms. The first regressed employment and crime outcomes against the experimental status variable and a vector of site and participant characteristics assumed to be relevant to employment and criminal behavior. The second model regressed outcomes against the same independent variables as well as experimental status interacted with selected site and participant characteristics. Average hours worked per month were used to measure employment outcomes. Crime outcomes were indexed by a dummy variable in which a score of one, indicative of failure, was given an individual after his first arrest. Interview-reported arrests were used to index crime rather than reported illegal activities, because arrest data could be verified. The use of a dichotomous rather than a continuous variable to index failure was based on the assumption that multiple

TABLE 2

ALLOCATION OF OUTCOME SAMPLES BY TARGET GROUP, SITE, AND MONTHS OF FOLLOW-UP DATA

	Ex-Offenders			Ex-Addicts			Youth		
Sites	18	27	36	18	27	36	18	27	36
Atlanta							41	8	0
Chicago	97	88	34	63	89	51			
Hartford	73	82	13				194	135	16
Jersey City	62	83	48	65	198	90	98	99	67
Newark	120	82	10						
New York							69	10	0
Oakland	149	113	24	42	45	3			
Philadelphia	59	82	54	55	163	98	34	46	38
San Francisco	76	79	36						
Total	636	609	219	225	495	242	436	298	121

arrests might be misleading, since arrest for a serious crime is likely to lead to incarceration and thus to no further arrests during the analysis period. While probit or logit analyses are the appropriate techniques to use in estimating arrest equations, our approach was determined by the fact that the empirical work for this paper was part of a very large evaluation effort for which free use of maximum-likelihood techniques were prohibitively expensive. We have, however, reestimated selection equations reported here and found the findings to be very insensitive to the estimation techniques.

Employment and Arrest Results: Overall

For all target groups being discussed here there were strong positive employment results during the first 9 months after sample entry (Table 3). This was obviously to be expected simply because experimentals in contrast to controls had guaranteed employment. However, the experimental-control differentials were not as great as they might have been had experimentals remained with Supported Work through their guaranteed stay. In fact participants fell far short of this guarantee, with addicts and youths remaining as active participants on average about 7 months, while offenders remained about 6 months. Reflecting this withdrawal, the failure of many participant drop-outs to obtain alternative jobs, and the gradual increase in employment among controls, overall experimental-control employment differences for the three target groups diminished steadily over the next 9 months and by months 16-18 experimental-control employment differentials became statistically nonsignificant. They generally remained nonsignificant

TABLE 3

HOURS WORKED PER MONTH, BY TARGET GROUP

Month	Ex-Offenders			Ex-Addicts			Youth		
	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
1-3	144.4	37.1	107.7**	138.4	32.4	106.0**	143.3	31.2	112.1**
4-6	113.8	51.0	62.8**	116.7	46.7	70.0**	120.1	43.9	76.2**
7-9	90.9	47.5	43.4**	97.3	42.9	54.4**	97.1	44.8	52.3**
10-12	73.6	52.7	20.9**	80.2	46.7	33.5**	79.4	50.2	29.2**
13-15	63.7	59.4	4.3	64.9	51.4	13.5**	67.2	62.2	5.0
16-18	60.1	59.5	0.6	50.4	52.3	-1.9	60.4	61.3	-0.9
19-21	59.1	57.9	1.2	55.1	55.4	-0.3	64.4	63.6	0.8
22-24	60.6	60.8	-0.2	61.6	60.2	1.4	69.6	70.0	-0.4
25-27	59.8	59.8	0.0	63.7	58.9	4.8	69.1	70.4	-1.3
28-30	76.1	63.9	12.2	66.6	56.3	10.3	87.2	83.0	4.2
31-33	77.5	69.9	7.6	73.1	51.9	21.2**	92.8	82.2	10.6
34-36	71.8	64.6	7.2	70.4	50.0	20.4**	83.3	75.8	7.5

**Statistically significant at the 5% level.

for the remainder of the follow-up analyses. These overall and not very optimistic findings do not, however, apply equally to all members of our target group samples. Some types of participants fared substantially better than others. We will return to this point shortly.

Turning now to results concerning the percentage of sample members arrested over time, the data reveal trends that depart from those reflected in regard to employment (Table 4). Among offender sample members no reliable experimental-control differences are observed over the follow-up period. By the end of the observation period, 36 months after intake, experimentals reported 8% more members remaining arrest-free than did controls, but the difference was not statistically significant. Among addicts, experimentals reported more arrest-free members than controls throughout the follow-up observation period, with differences statistically significant at the 27-month and 36-month interviews. Finally, among youths no strong experimental-control differences appear until the 27-month observation, when experimentals report almost 9% more members remaining arrest-free than controls. The above findings suggest no simple consequence of Supported Work participation. Seemingly, ex-addicts benefit most consistently from the program, but even for this group employment effects become essentially nonexistent between months 16 and 27. In contrast to the decay effects that are normally anticipated following program exposure, there appear to be delayed enhancement of effects for all three target groups. Finally, no clear relationship appears between employment and its impact on crime. Elaboration of

TABLE 4

CUMULATIVE PERCENTAGE ARRESTED, BY TARGET GROUP
AND MONTHS OF FOLLOW-UP DATA

Ex-Offenders						Ex-Addicts						Youth			
Months 1-18		1-27		1-36		1-18		1-27		1-36		1-18		1-27	
Exp.-Control Differ'al	Control Group Mean	Exp.-Control Differ'al	Control Group Mean	Exp.-Control Differ'al	Control Group Mean	Exp.-Control Differ'al	Control Group Mean	Exp.-Control Differ'al	Control Group Mean	Exp.-Control Differ'al	Control Group Mean	Exp.-Control Differ'al	Control Group Mean	Exp.-Control Differ'al	Control Group Mean
1.0	46.2	0.4	53.3	-8.0	64.8	-8.2	33.5	-10.9**	43.3	-18.1**	53.1	-0.3	27.0	-8.8*	39.3

*Statistically significant at the 10% level.

**Statistically significant at the 5% level.

these results by examination of possible mediators of program impact provides some classification and several interesting hypotheses regarding the possible long-term effects of programs such as Supported Work.

Employment and Arrest Results: Conditional Influences

The possibility that different groups of participants might respond differently to Supported Work is suggested by various considerations. Thus, participants' time of program entry may have influenced their responses to Supported Work as a result of halo effects (early positive effects stemming from enthusiasm and commitment), organization problems encountered at program start-up, quality decay problems encountered at program termination, or changes in the condition of labor markets. In addition, various individual attributes of participants may influence amenability to program intervention. Age, education, prior criminality, and previous work history have been linked elsewhere to criminality and employment. Thus it is possible these characteristics may mediate the impact of Supported Work on future criminality and employment. We now turn to these possible mediating effects.

Time of sample entry. Table 5 contains data on employment responses of addicts, offenders, and youth Supported Work sample members who were early, middle, and late entrants to the Supported Work sample. These are referred to respectively as the 36-month, 27-month and 18-month follow-up cohorts. There is a general tendency, with a short exception among youths, for experimentals in the 36-month cohort to work more

TABLE 5

AVERAGE HOURS EMPLOYED PER MONTH BY TARGET GROUP AND COHORT

Cohort	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
Youth								
18-month cohort	75.0	40.9	11.2	64.3	n.a.	n.a.	n.a.	n.a.
27-month cohort	87.0	36.6	6.7	56.4	3.4	65.1	n.a.	n.a.
36-month cohort	82.8	43.0	34.4	44.8	-8.6	73.3	8.8	85.9
Ex-offenders								
18-month cohort	70.3	50.6	7.6	63.7	n.a.	n.a.	n.a.	n.a.
27-month cohort	73.9	39.8	9.5	53.5	-1.4	59.8	n.a.	n.a.
36-month cohort	67.2	48.6	11.1	51.7	3.0	60.7	8.3	66.8
Ex-addicts								
18-month cohort	74.8	40.5	-1.3	60.1	n.a.	n.a.	n.a.	n.a.
27-month cohort	77.8	37.6	17.6	46.0	2.0	61.3	n.a.	n.a.
36-month cohort	81.6	43.0	31.6	44.8	6.0	52.7	13.6	53.3

NOTE: The 18-month cohort is made up of those who were enrolled in the program from 1977 on; the 27-month cohort is made up of those who were enrolled prior to 1977; and the 36-month cohort consists of those who were enrolled prior to April 1976.

n.a. = not applicable.

relative to controls than is true for experimentals in other cohorts. Better performance among the 36-month cohort experimentals is also reflected in arrest results shown in Table 6. The sources of this often weak but persistent phenomenon are not known. The possibility was explored that early program applicants differed from later ones in terms of demographic characteristics relevant to program response. While this exploration revealed some participant attributes which were marginally related to time of program application, these failed to account for the cohort effect.[2] A plausible residual hypothesis that must be considered is that the relatively more positive response of early program entrants to Supported Work reflects a halo effect, which often accompanies new endeavors. Unfortunately the data here provide no opportunity to test this possibility.

Participant attributes. More interesting for present purposes is the degree to which certain characteristics of sample members appear to mediate the employment and arrest effects of Supported Work. The results are presented in Tables 7 through 12. First, among youth, experimentals' probabilities of arrest relative to controls were found to be associated with arrests at time of sample entry. Among those who reported no previous arrest histories, Supported Work experience was found to substantially reduce the probability of subsequent arrest. Among those with extensive prior arrest histories, program experience was estimated to have no arrest-reduction effects.[3] Second, among offenders, those who were eligible to be addict target-group members,

TABLE 6

CUMULATIVE PERCENTAGE ARRESTED BY TARGET GROUP AND COHORT

	Ex-Offenders						Ex-Addicts						Youth			
Months:	1-18		1-27		1-36		1-18		1-27		1-36		1-18		1-27	
Cohort	Exp.-Control Diff.	Control Group Mean	Exp.-Control Diff.	Control Group Mean	Exp.-Control Diff.	Control Group Mean	Exp.-Control Diff.	Control Group Mean	Exp.-Control Diff.	Control Group Mean	Exp.-Control Diff.	Control Group Mean	Exp.-Control Diff.	Control Group Mean	Exp.-Control Diff.	Control Group Mean
Total Sample	1.0	46.2	0.4	53.3	-8.0	64.8	-8.2	33.5	-10.9**	43.3	-18.1**	53.1	-0.3	27.0	-8.8*	39.3
18-month cohort	2.3	45.0	n.a.	n.a.	n.a.	n.a.	3.1	31.4	n.a.	n.a.	n.a.	n.a.	3.8	22.2	n.a.	n.a.
27-month cohort	1.1	45.5	2.8	50.6	n.a.	n.a.	-9.5**	32.2	-9.4	42.1	n.a.	n.a.	-1.9	32.4	-8.1	41.4
36-month cohort	-5.2	53.5	-10.3	63.5	-8.0	64.8	-17.3**	38.4	-14.2**	46.1	-18.1**	53.1	-13.4	32.4	-10.9	32.4

NOTE: The 18-month cohort is made up of those who were enrolled in the program from 1977 on; the 27-month cohort is made up of those who were enrolled prior to 1977; and the 36-month cohort consists of those who were enrolled prior to April 1976.

*Statistically significant at the 10% level.

**Statistically significant at the 5% level.

n.a. = not applicable.

TABLE 7

AVERAGE HOURS EMPLOYED PER MONTH BY SELECTED BACKGROUND CHARACTERISTICS, YOUTH 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	80.7**	39.7	11.7**	58.2	0.6	68.2	7.2	81.4
Prior drug use								
Used drugs	73.3**	47.4	15.5	47.6	16.3	57.6	10.8	84.0
No drug use other than marijuana	82.5**	37.4	10.2*	61.1	-5.8	72.6	0.4	85.8
Prior arrests								
0	79.0**	42.9	17.8*	61.0	-5.0	81.8	12.8	88.5
4	85.0**	38.6	8.1	57.6	10.8	57.2	6.7	87.0
9	79.2**	38.9	3.9	38.0	9.6	58.0	4.0	91.8

*Statistically significant at the 10% level.

**Statistically significant at the 5% level.

TABLE 8

AVERAGE HOURS EMPLOYED PER MONTH BY SELECTED 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
Total sample	78.2**	40.5	16.4**	50.0	1.5	58.6	18.3**	52.6
Age								
Under 21	69.8**	49.9	-5.7	68.4	8.6	69.3	a	a
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

[a]Data not presented because fewer than 20 sample members appear in this category.

*Statistically significant at the 10% level.

**Statistically significant at the 5% level.

TABLE 9

AVERAGE HOURS EMPLOYED PER MONTH BY SELECTED BACKGROUND CHARACTERISTICS, EX-OFFENDER 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	71.1**	46.0	8.5**	57.8	-0.2	60.0	8.2	66.8
Years of age								
Under 21	70.7**	43.6	3.2	58.4	-4.7	53.0	33.2	62.5
21-25	73.2**	46.1	9.2*	56.2	0.8	60.2	-2.4	78.0
26-35	69.7**	44.2	6.5	60.2	0.3	61.7	10.7	50.5
Over 35	63.5**	59.7	28.0*	51.7	1.2	65.4	a	a
Prior drug use								
Used heroin regularly	72.3**	45.3	12.8*	52.2	1.1	45.4	18.5	59.4
No regular heroin use	70.1**	46.3	6.5	60.2	-1.0	66.7	0.5	70.5

[a]Data not presented because fewer than 20 sample members appear in this category.

*Statistically significant at the 10% level.

**Statistically significant at the 5% level.

TABLE 10

CUMULATIVE PERCENTAGE ARRESTED BY SELECTED BACKGROUND CHARACTERISTICS, YOUTH SAMPLE

	Months 1-18		Months 1-27	
	Experimental-Control Differential	Control Group Mean	Experimental-Control Differential	Control Group Mean
Total sample	-0.3	27.0	-8.8*	39.3
Prior drug use				
Used drugs	-7.7	35.3	-10.4	46.0
No drug use other than marijuana	2.0	24.6	-7.3	34.6
Prior arrests				
0	-1.8	25.6	-13.6**	37.9
4	1.6	28.3	-4.6	37.9
9	5.9	31.6	6.8	37.8

NOTE: Results for the 1-36 month period are not presented because of the limited sample size (79).

*Statistically significant at the 10% level.

**Statistically significant at the 5% level.

TABLE 11

CUMULATIVE PERCENTAGE ARRESTED BY SELECTED 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
Age						
Under 21	-3.5	36.6	14.5	34.7	39.1	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

*Statistically significant at the 10% level.

**Statistically significant at the 5% level.

TABLE 12

CUMULATIVE PERCENTAGE ARRESTED BY SELECTED BACKGROUND CHARACTERISTICS, EX-OFFENDER 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	1.0	46.2	0.4	53.3	-8.0	64.8
Years of age						
Under 21	-10.4	55.7	-8.9	68.8	-10.5	54.3
21-25	8.5**	43.2	11.7**	48.6	-0.3	59.6
26-35	-0.9	46.1	-8.0	55.0	-5.7	72.2
Over 35	-7.6	38.5	-14.7	39.4	a	a
Prior drug use						
Used heroin regularly	-1.0	47.6	-2.7	56.3	-13.3	65.2
No regular use of heroin	1.6	45.6	1.4	51.9	2.2	60.2

[a]Data not presented because fewer than 20 sample members appear in this category.

**Statistically significant at the 5% level.

that is who reported they were regular heroin users at time of sample entry, were more likely to report lower arrest probabilities and more hours of employment relative to controls. Third, among addicts and offenders, there were important age effects. For experimentals in both groups, those who were older generally reported the greatest program benefits. That is, they worked more hours and had more arrest-free members relative to their controls than did younger experimentals. It is of some interest to note as well that older controls (over 35 years of age) generally reported more arrest-free members than did controls who were younger. This trend was particularly strong among offenders.

CONCLUSIONS

The results of the Supported Work experiment for the groups discussed here do not lend themselves to a simple summary. Clearly not all participants benefited. Furthermore, crime reduction effects were not, as hypothesized, uniformly dependent on increased employment effects. An important case in point pertains to the group of offenders over 35 years of age. These individuals consistently reported a larger arrest-free rate than controls, but their employment record after 18 months of observation was no better than the record of controls. Conceivably the absence of an employment effect after 18 months could be due to employers' refusal to hire these individuals, but the arrest-rate reduction in the absence of an employment effect is not explained by the theoretical models which

guided Supported Work. A similar problem in interpretation is found in the failure of experimentals to uniformly report fewer arrests than controls at a time when they uniformly worked more than controls, that is, during the first year of their sample membership. At the very least, these results imply a complex and perhaps attenuated link between employment and crime.[4]

These imponderable results notwithstanding, the general pattern of the Supported Work findings suggests a few crucial attributes of participants that may mediate the impacts of employment programs for offenders, addicts, and problem youth. First, youth without previous arrests at time of intake are more likely to respond positively than are youths who are known to be delinquent. Second, as noted previously by Project Wildcat, drug addicts are likely to respond favorably, especially in terms of arrests. Third, the arrest effect for addicts appears to be mediated by age, with older experimentals more likely to remain arrest-free relative to controls than those who are younger. The same interaction pattern is observed among offenders. In brief, older individuals with criminal records appear to be responsive to the Supported Work program; those who are younger do not.

There is some reason to regard the age effects noted in the study as having both policy and theoretical relevance. It has long been recognized by criminologists that offenders appear to "burn out" in the sense that recidivism rates decrease with age after young adulthood. A common interpretation of this decrease is that older offenders "tire" of crime, become less willing to take risks, and turn to more conventional

lives. In its current general form, the burn-out thesis has left unanswered a variety of questions, including those addressing the identity of those who burn out, the conditions that increase the probability of burn-out, and whether burn-out (reduced recidivism) may in fact reflect a turn to less detectable criminal activity. Despite these weaknesses, the burn-out thesis supplies an explanation for the finding that older Supported Work controls were less likely to incur arrests than those who were younger. This thesis also provides an explanation for the interactions between age and experimental status observed among the offender and addict samples. That is, it suggests that employment programs like Supported Work provide older offenders an opportunity and added incentive to move more rapidly toward an already contemplated career change. If this hypothesis is true, then the age by experimental status interactions should be found among investigations of other employment-enhancing programs. Unfortunately, published findings from these studies do not, in general, lend themselves to such examination. An exception of sorts was found in a report of the Baltimore Life Experiment (Lenihan, 1976). This program provided financial assistance and employment counseling to offenders rather than jobs. However, as with subsidized employment, financial assistance can be seen as providing individuals the opportunity to make career changes, and indeed, the results of the Baltimore Life Experiment are consistent with those of Supported Work. Among experimentals, those over 26 were found after one year to have an arrest rate almost 11% less than controls; for experimentals between

21 and 25 years of age the difference was 8%; for participants less than 21, it was 2.3%. Again, older controls were less likely to be rearrested than those who were younger. Obviously the Supported Work and Life results are not definitive. They do, however, lead to interesting implications. Historically older offenders have not been targeted by programs aimed at lowering recidivism. Such programs have typically focused on juvenile and young adult criminals. While the Supported Work and Life results do not flatly reject the possibility that younger offenders can benefit from these programs, the results indicate that older, more mature, offenders may be better candidates for assistance. The number of such individuals in U.S. prisons is not small. In 1977, prison population data indicated that 40% of those in penal institutions were over 30 years of age. Thus the potential benefits of targeting employment programs for older offenders are substantial.

Finally, we note that the amenability of older participants to employment programs may not be limited to "burned-out" criminals. We cite the following examples. Cooley et al. (1975) note that increased earnings due to training are enhanced with participants' age; a similar observation was reported by Sewell (1971). Furthermore, Supported Work itself had a fourth target group rather different from the three which are the primary concern of this paper. This group was composed of women who had been in AFDC at least three years and who did not have pre-school age children at the time the women enrolled in Supported Work. Crime data were not collected for this target group. In marked contrast to

the results for the other target groups, the postprogram employment results were consistently positive, statistically significant, and sizeable in magnitude. Moreover, the average age (34 years) is greater for this AFDC group than for either the ex-addicts or ex-offenders. Thus the AFDC results are consistent with the contention that the effects of work experience programs like Supported Work may be greater for older adults with limited employment than for seriously disadvantaged youth.

As Ginzberg indicates (1980, p. 16), employment and training programs in the United States have not given older workers much opportunity to participate. Presumably this is due to the assumptions that employment and training programs would have their largest payoffs for youth because youth are not yet committed to careers, legal or otherwise. Supported Work results provide some evidence that this assumption may be true for poor youth who have yet to be involved in crime, at least officially. But, more important, we believe, the evidence in this experiment and elsewhere suggests older disadvantaged workers, including those who are known offenders, may be much more responsive to the opportunity to participate in employment programs. It may well be worth the costs to provide this opportunity more fully.

NOTES

[1]The fourth target group was composed of women who were long-term AFDC recipients. For this group, no crime data were collected, since their criminal activity was assumed to be negligible. Consequently in this paper we focus on the other three target groups. We shall comment further on the AFDC sample in our concluding section.

[2]These include participants' age, prior receipt of public assistance, and previous employment.

[3]Fewer arrests at entry had a mixed effect on the employment results for youth.

[4]This conclusion is in sharp contrast to that of Berk, Linihan, and Rossi, who state that their analysis of the data from the Transitional Aid Research Project (TARP) indicates that " . . . for offenders, at least, unemployment and poverty do cause crime at the micro-level." (p. 784). TARP provided unemployment insurance or job counseling to its participants, all recent releases from prison. Although no statistically significant results were initially found across experimental treatments, Berk et al. did observe beneficial effects when the TARP data were analyzed using a complex structural model, in which predicted TARP payment and weeks worked were both found to have a statistically negative effect on the number of arrests in the 12-month experimental period. The authors conclude that there was no simple experimental effect of the TARP payments because such payments were reduced (often dollar for dollar)

as earnings increased, thus leading to fewer weeks worked. While this explanation appears plausible, we emphasize that the structural results on which Berk et al. base their conclusion depend heavily on the instrumental variables developed to predict weeks worked and TARP payments. If the predicting equations include any terms that affect arrests directly, rather than just through their effects on weeks worked and payments, then the interpretation of Berk and his colleagues of the effects of weeks worked and payment on arrests could be based on spurious relationships. The direct experimental methodology underlying our results eliminates this possibility when interpreting our results.

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