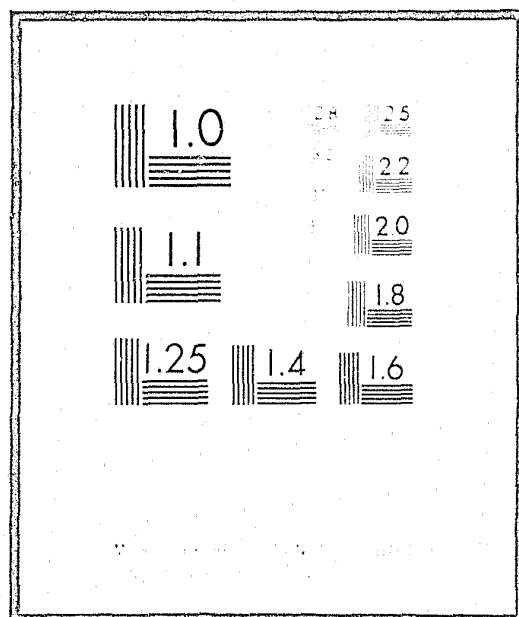


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RESEARCH REPORT NO. 55

28064

DIRECT FINANCIAL ASSISTANCE TO PAROLEES:

A Promising Alternative in Correctional Programming

Craig Reinerman

Donald Miller



RESEARCH UNIT • DEPARTMENT OF CORRECTIONS • STATE OF CALIFORNIA •

May 1975

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RESEARCH REPORT NO. 55

DIRECT FINANCIAL ASSISTANCE TO PAROLEES —
A PROMISING ALTERNATIVE IN CORRECTIONAL PROGRAMMING

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May 1975

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CHAPTER I
INTRODUCTION

The Direct Financial Assistance to Parolees Program (DFA) was designed and initiated to test the utility of financial aid in the first three months on parole—a period generally regarded as important in determining the ultimate success or failure of the parolee. The purpose of the financial aid was to reduce the economic stress caused by a lack of a job upon release or the loss of a job subsequently.

It is generally agreed that the ex-offender in his attempt to re-enter the "free" world and re-establish himself in his home community faces a number of problems, perhaps the most crucial being to acquire and maintain some form of gainful employment (President's Commission on Law Enforcement and Administration of Justice, 1967). Since gainful employment is usually regarded as an intrinsic part of the rehabilitation process, problems in this area may contribute to illegal activities and eventually contribute to recidivism.

Glaser (1964, pp. 328-29) for example reports:

subsequent failures among the releasees whom we contacted were much more often unemployed in their first three months out of prison than were the subsequent successes. Indeed, after the first month out of prison the rates of unemployment were over twice as high for the failures as for those who were successful in avoiding further serious difficulties with the law. While this is not evidence that unemployment alone causes recidivism, it is one more piece of correlational data in our findings which suggest that unemployment may be among the principal causal factors involved in recidivism of adult male offenders.

Pownall (1967) and Irwin (1970) report similar findings.

The fact that unemployment among ex-offenders is a chronic problem is attested to by noting the prevalence of unemployment in one parole region in Southern California where some 18 percent were not gainfully employed according to a reporting system that was in effect in 1971 in the California Department of Corrections. This compared to a local figure of six percent unemployed generally in the area. Typically even in periods of prosperity, parolee unemployment rates are two to three times higher than those of the general population. This is particularly true with respect to ex-offenders just released, where the same reporting system revealed over 26 percent of all unemployed parolees in one of the parole regions were recent releases from prison.

Some idea of the scope of this problem can be deduced from the fact that in a two-month period in 1971 some 223 pre-parole cases lacking a definite job to come to on parole were processed by one regional parole office and an additional 90 cases were referred lacking both a job and a place to live. It is likely that the number of referrals with these disadvantages reported for 1971 is representative of referrals to parole in subsequent years also.

Presumably some of these referrals can be resolved satisfactorily, but in view of the current economic conditions and other demands on the parole agents' time, many probably cannot be resolved by the time the men are released.

Another indication of the magnitude of this employment problem at re-entry was noted in a survey of nearly 400 men recently paroled from one California penal institution (Holt and Miller, 1972). Approximately 25 percent of the men had neither a definite job nor a definite residence arranged prior to their being granted a release, while more than half lacked one or the other and only about 20 percent had both arranged at that time. In fact it is not uncommon for 30 percent or more to be released in a given month with no definite job or residence awaiting them.

Some idea of the nature of the employment difficulties of the released ex-offender is gained when one considers the typical offender's pattern of employment history and job skills. The majority have no skills as such and have minimal job experience. If for no other reason than the way the criminal justice system operates, better risk cases, usually those with stable life styles and jobs, tend to be systematically excluded from prison commitments (Babst and Mannering, 1965). Taggart (1972, pp. 1-2) argues that ". . . whether it is a cause or an effect of their criminality, offenders are generally failures in the world of work [and that] . . . it is widely accepted that increasing employability is an important part of rehabilitating the offender."

However, only about 30 to 40 percent of prison inmates receive vocational training, and even here, a follow up of vocational trainees revealed that only about one in three actually gained employment in their field of training or one that was closely related. Other studies indicate that even with skills, employers are hesitant to hire ex-offenders. The stigma attached to hiring men with records is quite notable. For example, Himelson (1966) asserted that:

When personnel managers for manufacturing firms were questioned on general hiring policies . . . we find that 23 percent of the respondents stated their firms would at least sometimes hire men with records; 49 percent hedged and stated that their firms would under certain conditions; and 28 percent felt that the chances of their firms doing this were slight or none at all.

Most prisons have work programs for inmates designed to fill some of these gaps. However, "inside" work experience does not often reach the majority of the inmates. Glaser (1964) reports that in the Federal Prison System only about one-fourth of the offenders work in non-maintenance jobs. California's prison industries employ about one-third of its inmates, according to Mitford, 1973. For those who do get this work experience it is often the first of their adult lives and even so offenders frequently find no openings in the field of their experience upon release, and of those who do find work the pay is often minimal (Glaser, 1964).

As previously mentioned the ex-offender's general lack of personal resources in the community, plus his usual lack of skills, plus the stigma of being an ex-offender all combine to pose a problem of some magnitude for him. Once his release money, usually amounting to some 30-40 dollars, is gone, the parolee is in a difficult spot indeed.¹ A recent study revealed that parole difficulty and return to prison is most common in the group with less in the way of ties to family and friends. An examination of employment and parole adjustment in Virginia (Bureau of Public Administration, 1955) over a long time period indicated that recidivism was inversely related to the monthly earnings of the parolees.

It is generally agreed that economic hardship is a major contributor to criminality in general and to property crimes especially. In an important study, Glaser and Rice (1959) demonstrated that the rate of property crimes varies directly with unemployment levels. These authors point out that nearly 90 percent of crimes in the FBI statistics are property crimes. This underscores the significance of the relationship between economic hardship and crime. When one adds the special employment difficulties and previous familiarity with crime of ex-offenders to the more general relationship just mentioned, the problem becomes manifest. A further clarification is provided by Fleisher's (1963) secondary statistical analysis of Glaser's data. He shows that loss of income is actually more crucial to successful parole adjustment than loss of employment.

Clearly people need money to survive, and for many it is a most scarce commodity. Jobs are often scarce, especially for ex-offenders. How can ex-offenders be expected to survive until they gain employment? One source might be in-prison earnings, except that the majority of inmates do not work at industrial jobs and are not paid. Of those that were employed in the California system, the wages ranged at the time of this study from five to nineteen cents per hour (Mitford, 1973). Needless to say it is difficult for them to accumulate enough funds in the institution to tide them over until their initial paycheck on the outside.

Taggart (1972) summarizes general adjustment difficulties in four basic points:

1. parolees face severe adjustment problems, e.g. usually more than half have no job at release;
2. they rarely have funds built up to rely on until they do find employment;
3. employment is hard to find for anyone, especially an ex-offender more likely to be unskilled, inexperienced, and viewed as undesirable by employers;
4. with fewer alternatives and legitimate means of survival, an offender is more likely to return to a world he knows, crime.

¹ Since the experimental project described in this report was completed, the release allowance for each inmate released on parole or discharge was increased to \$200. While this is a substantial increase in an absolute sense, it can scarcely be regarded as adequate for providing financial support for any length of time.

Many administrators and practitioners in the criminal justice system have long been aware of this situation and a few experiments with financial aid to parolees have been tried. The Rikers Island Project in New York made loans to individual parolees of up to \$200 but averaging about \$50. It was not evaluated for impact. The administrators of the Draper Project in Alabama, who passed out grants averaging \$90 concluded that releasees tended to "blow" the money (Taggart, 1972). They concluded that the money was necessary, but tighter controls were needed over its allotment.

Perhaps the most extensive experiment was in the Division of Vocational Rehabilitation in the State of Washington where over 200 parolees received up to \$1,000. The early indications were that there had not been a significant drop in recidivism, although their final report was not available at the time this report was being written. However, the opinion of the staff of the Washington project is that financial assistance is a viable concept and a necessary factor in successful parole adjustment.

No conclusions, positive or negative, can be drawn from the experiments to date. The re-entry of ex-offenders is a complex and dynamic phenomena, and the circumstances promoting successful re-entry are difficult to assess. More and more practitioners, however, have been convinced by their experiences with individual parolees that financial aid is a sorely-needed resource. It was in this spirit that the California Department of Corrections sought and received funding for a financial assistance experiment.

It should be borne in mind, however, that while the goals of the DFA project were to lower recidivism and reduce further crime, particularly property crimes, money as such is only one factor in a rather complex situation, albeit an important factor capable of having some impact.

Premises of the Experiment

The basic logic of the Direct Financial Assistance concept, from methods to objectives, can be summarized in the following sequence of steps:

- Provide a group of parolees, at the point of their release and during the crucial early months on parole, with enough funds to realistically lessen the economic hardships of that period . . .
- Thereby reducing the emotional stress of parolees which arises from the financial inability to meet basic economic needs such as food, clothing, and shelter . . .
- Thereby helping to at least partially remove one of the primary motives for re-involvement in criminal activity . . .
- Thereby reducing the extent of criminal involvement, especially property crimes, and the long-term likelihood of recidivism.

These premises were the foundation of the DFA project and what it was intended to test. The project was designed to deliver up to \$960 at the rate of \$80 per week for up to three months to an experimental sample of 120 parolees.

Funding

The California Department of Corrections (CDC) Parole and Community Services Division was awarded grant funds in the total amount of \$183,659 from the California Council on Criminal Justice (CCCJ).² This amount was supplemented with an in-kind, grantee contribution of \$63,944 in the form of personnel services from CDC. Of the grant funds, a maximum of \$120,000 was allocated for direct financial assistance to the parolees.

² The name of this agency has since been changed to the Office of Criminal Justice Planning.

CHAPTER II THE DIRECT FINANCIAL ASSISTANCE TO PAROLEES PROGRAM

Structure

The DFA project was based in Region II of the CDC Parole and Community Services Division, which consists of the San Francisco Bay Area and the North Coast Counties, with conventional and reduced caseloads included in the project. Parole agents became involved in the project when a parolee, soon to be released, was assigned to their caseload. It is important to note at this juncture that no changes were made in the case assignment procedures. Agents simply were assigned cases normally by their supervisors and were advised as to whether or not the parolee in question had been selected for DFA afterward.

All agents and supervisors were sent a memorandum explaining the project and its procedures. Later, a briefing was held at each parole office in Region II for purposes of training agents in the procedures for distribution of DFA, record keeping, and completion of evaluative and financial monitoring forms. Further, a clerk in each unit was trained in the procedures for accounting and securing the individual checks as requested by the agents.

Operations

The initial step in the program was the selection of the experimental and control parolees. The research component (conducted by the Scientific Analysis Corporation) began in July 1972 with the selection of parolees for the project who were due to be released to Region II beginning in August 1972. Parolees whose CDC identification number ended in an odd digit were designated experimentals and thus were eligible for DFA monies; those whose number ended in an even number were designated controls and were not eligible. The file in the Region II records office of parolees scheduled for release was the source from which the selections were made.

The following tables show which CDC institutions (Table 1) the groups were released from and the parole units (Table 2) to which both the experimental and control parolees were released.

Once an experimental parolee had been selected, the parole unit to which he was to go received notification of eligibility for DFA. At this point the agent examined the pre-release information and the parolee's file noting the parolee's financial and employment resources. If necessary, the agent talked over a case with the unit supervisor to decide whether or not a designated experimental parolee needed the financial assistance. The only criterion used by parole agents to decide whether or not a selected parolee would be offered DFA was need. If an experimental parolee did not have adequate employment and/or other financial resources he was offered DFA by his agent.

In the course of the project, 23 eligible persons did not receive financial assistance because they did not need the money, according to agent reports. A comparison of three groups (those persons who were selected as eligible but did not get money, those who did get money, and the controls) indicated

that parole agents were accurate in determining the financial needs of these parolees. Specifically, those excluded by parole agents were reported as working on parole more than both those in the experimental group who got assistance and the control group.

Table 1

INSTITUTION FROM WHICH THE EXPERIMENTAL
AND CONTROL SUBJECTS WERE RELEASED

Facility	Experimental Group		Control Group	
	Total Number	Percent	Total Number	Percent
San Quentin	65	48	54	45
Correctional Training Facility-Soledad	15	11	15	13
Calif. Men's Colony-San Luis Obispo	14	10	13	11
Calif. Medical Facility-Vacaville	13	10	8	7
Calif. Conservation Center-Susanville	8	6	6	5
No. Calif. Conservation Center-Garberville	10	7	7	6
Folsom	4	3	6	5
Calif. Correction Institution-Tehachapi	3	2	3	3
Deuel Vocational Institution-Tracy	3	2	2	2
Calif. Institution for Men-Chino	1	1	6	5
TOTAL	136	100	120	100

Table 2

PAROLE UNITS TO WHICH EXPERIMENTAL
AND CONTROL SUBJECTS WERE INITIALLY RELEASED

City	Experimental Group		Control Group	
	Total Number	Percent	Total Number	Percent
San Francisco	48	35	34	28
Oakland	35	26	41	34
San Jose	30	22	28	23
Santa Rosa	14	10	9	8
Salinas	9	7	8	7
TOTAL	136	100	120	100

Table 3

AMOUNT OF INITIAL DFA PAYMENT, BY PAROLE UNIT

Parole Unit	Full Amount-\$80		\$60-79		\$1-59		No Money		Total	
	Total Number	Percent	Total Number	Percent	Total Number	Percent	Total Number	Percent	Total Number	Percent
San Francisco	32	67	-	-	2	4	14	29	48	100
Oakland	32	91	-	-	1	3	2	6	35	100
San Jose	23	76	-	-	2	8	5	17	30	100
Santa Rosa	7	50	1	7	-	-	6	43	14	100
Salinas	8	89	-	-	-	-	1	11	9	100
All Units	102	78	1	1	5	3	28*	21	136	100

* Five of these parolees were given DFA at later dates.

After determining need, the agent filed a "request for DFA" with the clerk in his unit. The clerk in turn entered the parolee's name on an individual account sheet and sent a "DFA check request form" to the CDC accounting department. Usually within three days the agent received the check and gave it to the parolee. Every attempt was made to make the selection, notification of agent, and determination of need prior to the experimental parolee's release so that the DFA check could be processed and sent to the agent in time to meet initial expenses. During the first meeting between parole agent and parolee, the latter was asked for information about his circumstances and the final decision to accept or not accept the assistance was made.

Each week the experimental parolees met with their respective parole agents to discuss employment prospects or problems, to account for expenses over the past week, and to receive their next weekly DFA checks. All decisions regarding when to stop DFA or changes in the amount of DFA were left to the parole agent to work out with the parolee. There were no guidelines given to agents; the only criterion used was financial need.

The DFA period was to be 12 weeks and the financial assistance up to \$80 per week. No one received more than \$80 in any week, although a few exceptional cases were granted one to three week extensions in time by the project director. This was made financially possible by other parolees who obtained employment before their 12 week DFA period had expired and still others who never required DFA even though eligible. Another variation which occurred several times was a parolee who, when offered DFA, originally did not take it because he (and his agent) felt he had sufficient employment to make do. Later, some of these men were laid off or lost their jobs and then began receiving DFA. Table 3 gives details on the number of parolees by the amounts they received initially.

As indicated earlier, several experimental parolees did not need DFA funds. Several others found suitable employment and were either slowly phased out of or were dropped from the program. The end result was a variability in the amount of funds received by the parolees. The following table (Table 4) shows the distribution of total amounts of DFA funds received by the experimental parolees.

It should be noted that more than the proposed 120 experimental parolees were selected due to the fact that some had adequate employment and/or resources. All parole personnel connected with the project felt an obligation not only to fulfill the experimental requirements but to insure that the greatest number of parolees possible benefited from the DFA. This presented slight difficulties for selection. The number of experimentals selected had to be maximized so as to utilize fully the funds allotted for distribution to parolees, while at the same time caution was used to avoid over-subscribing the funds in order to allow each experimental parolee who needed it his full \$960 (12 weeks at \$80).³

³ In the original proposal the money was to be disbursed formally as a loan with the provision that all normal expenditures (e.g., housing, food, transportation, medical costs, work expenses, etc.) were totally forgivable. Unforgiven expenditures were to have been repaid beginning four months after release; this determination was left up to the individual agent.

Table 4

TOTAL AMOUNTS OF DFA RECEIVED BY EXPERIMENTAL PAROLEES

Total DFA Funds Received	Number of Parolees	Percent
0	23	14.0
\$1-319	17	11.9
\$320-639	21	15.6
\$640-959	16	11.9
\$960-1120	59	43.7
TOTAL	136	100.0

(For those who received DFA funds, the average total amount received by each was \$735.68.)

Toward the end of the disbursement period it became apparent that the selected experimental group would not entirely deplete available DFA funds. The surplus was distributed by Region II agents to new releasees on the basis of the same "need" system, the difference being that these additional men were not needed to complete the research on the experiment and were not included in the group under study, as they were not randomly selected on the basis of their identification numbers. Region-wide disbursement commenced September 1, 1972, and continued through May 1973 when the last parolee received his last DFA check. At this point the \$120,000 allocated for DFA had been expended.

CHAPTER III
THE RESEARCH EVALUATION: SAMPLING AND METHODS

Sampling

In order to clarify the effects of the financial assistance on parole adjustment the research evaluation used an experimental model. Two samples, an experimental group (eligible for DFA) and a comparison or control group (ineligible) were randomly selected from the releasees to Parole Region II from August to November 1972. Random selection was used (instead of selection on the basis of financial need) to insure statistical "sameness" between the experimental and control groups thereby allowing valid comparisons between "aided" and "unaided" parolees.⁴ This design called for both groups to be of the same size, 120 parolees each. However, within the constraints of the program operation mentioned earlier, it became necessary to select slightly more experimental parolees in order to disburse the total funds allocated for financial assistance to parolees. The end result was an experimental group of 136 and a control group of 119.⁵ As it became apparent that some eligible parolees had no need for DFA, 16 extra experimentals were selected to bring the number actually receiving funds closer to the ideal total of 120.

Women were excluded from the DFA program in the original design; this caused considerable criticism from some agents for sound reasons. The exclusion was in part predicated on the sexist assumption that women parolees would not have similar financial and re-entry problems, as they would be more likely to be supported by someone else. Although there is clearly some justification for such an assumption, it resulted in undue discrimination against women ex-offenders. However, in the distribution of the DFA funds which were unused by the 136 experimental parolees, women did receive DFA, although they are not included in the subjects under study.

All parolees who had been committed to the California Rehabilitation Center for treatment for involvement with narcotics were excluded on the recommendation of the contracting agency task force that reviewed the proposal. The rationale was essentially that the narcotic addict had a different order of difficulties in re-entry, in which economic problems could be easily overshadowed by problems with drug abuse. There were not, however, any other efforts to exclude narcotic offenders. There were no other exclusions from the sample; all other types of ex-offenders were included.

Selection was made on the basis of three factors: CDC identification number, parole date, and region. Parolees whose numbers ended in an odd digit were designated as experimentals, and those with even numbers as controls. Selection began September 1, 1972, and continued until late November when the samples had been addumulated. The pool of parole Region II releasees was the universe from which selection was made. The following table shows how many parolees were selected in each month of the sampling process.

⁴ See Appendix A - A Methodological Note on Sampling.

⁵ One control group parolee deceased.

Table 5
EXPERIMENTAL AND CONTROL SUBJECTS SELECTED,
BY MONTH RELEASED TO PAROLE

Month	Experimental Group		Control Group	
	Total Number	Percent	Total Number	Percent
August	5*	4	3*	2
September	46	34	36	30
October	51	37	43	36
November	33	24	38	32
December	-	-	-	-
January	1**	1	-	-
TOTAL	136	100	120	100

* These parolees were originally scheduled for parole during September but their dates were advanced to August.

** One parolee was selected on the basis of his original September date but was not released until January.

Data Collection

Four basic sets of data were gathered on each parolee:

1. Background information
2. Financial monitoring information (experimental group only)
3. Information about adjustment on parole
4. Six and twelve-month parole follow-up information

In addition, each parole agent involved in administering DFA to one or more parolees was interviewed. These interview data were obtained in order to determine the attitudes of agents to various aspects of the DFA program as it related to parole.

In this section the types of variables, methods of collection, and coding processes are discussed for each of the four parolee data sets.

1. Background Information on Parolees

To assess the general social, demographic, and criminal career characteristics of the parolees in our samples, the California Department of Corrections records were examined for each parolee. The following information was compiled for each control and experimental parolee:

Age
Ethnicity
Religion
Education (claimed and measured)
Family history
Marital history
Work experience and history
Criminal career history (juvenile, prior offenses, and terms served)
Outside social contacts while incarcerated
Length of sentence
Base-expectancy score
Resources and plans for release
Commitment offense

Four categories of commitment offenses were employed in this study:

Violence, including manslaughter, murder, and assaultive offenses;
Property, including burglary, robbery, and forgery;
Drugs, including all narcotic and dangerous drug offenses;
Other, including those not previously enumerated.

2. Financial Monitoring Information

At the same time parole agents were notified of the eligibility of one of their newly-released parolees, they were given an initial interview form. This interview schedule was administered to each experimental group parolee during his first visit with the agent.⁶ Whether or not the decision was made to give the parolee DFA, he was asked the following questions in the initial interview: if and when he began work, his rate of pay, his type of job, his approximate expenses, and if, how much, and how long he would need DFA. Approximately each month thereafter for three months the agent was sent a monthly financial report form to complete for each of his parolees on DFA.

3. Information about Adjustment on Parole

During the fourth month after each parolee (both experimental and control) had been released, the agent was interviewed on the general parole adjustment of the parolee. The following items of data were gathered on each parolee with respect to his initial three months on parole:

number and type of contacts with agent
nature and permanence of living arrangement
employment patterns since release
number of arrests and charges
drug use (including alcohol)
agent's prognosis for success
effects of DFA on adjustment
parole services rendered
personal and legal problems of parolee.

⁶ Financial monitoring forms were not administered to control group parolees because they received no funds.

The interview schedule was comprised of both open-ended and structured questions. Included in these data were the items in the "Parole Adjustment Scale" as developed by the Research Division of the Department of Corrections (Richardson, 1962, pp. 6-7).

Much of the data gathered through this interview consisted of subjective opinions of agents and therefore were inappropriate for the basic analysis of outcome on parole. Instead they were used for exploratory, descriptive, or contextual purposes. Data concerning employment patterns, associations, personal problems, and parole services rendered were analyzed for their relationship to parole outcome. Other data, for example, the number and type of agent/parolee contacts were used to describe the indirect effects of the DFA program.

4. Six and Twelve-Month Parole Status Reports

To obtain some measure of outcome, i.e., success on parole, the statuses of the parolees were gathered from the agents and central office records at the end of two intervals, six and twelve months. The parole outcome categories used ranged from a successful, arrest free parole adjustment to a return to prison. The following are the categories used:

1. Successfully on parole and employed, in school, or retired
2. Successfully on parole and unemployed
3. On parole but trial pending
4. Parolee-at-large or location unknown to agent
5. Incarcerated awaiting trial
6. In detention - narcotics treatment and control unit
7. In custody, mental hospital or hospital
8. Returned to prison or serving jail sentence.

This number of categories proved to be unworkable because there would have been too few cases in some of them for analytical purposes. Therefore the nine categories were combined into three basic categories as follows:

1. SUCCESSFUL ADJUSTMENT

- including 1. Successfully on parole and employed, in school, or retired
2. Successfully on parole and unemployed

2. POSSIBLE TROUBLE

- including 3. On parole, trial pending
4. Parolee-at-large or location unknown to agent
5. Incarcerated awaiting trial

3. UNSUCCESSFUL ADJUSTMENT

- including 6. In detention in Narcotics Treatment and Control Unit
7. In custody, mental hospital or hospital
8. Returned to prison or serving jail sentence

This outcome categorization was cross-tabulated against selected background and adjustment variables for both the control and experimental groups, forming the basis of the comparisons.

In this report the emphasis of the analysis will be on the description of the size and the direction of the outcome differences noted rather than on any statistical tests as such due to the rather small number of cases in some of the tables. These small numbers make adequate tests of significance or further partialing by introducing a third or fourth variable impossible. Larger scale studies must be undertaken to allow these forms of testing. The numbers of cases vary slightly from table to table as a result of information being unavailable on or inapplicability of the question to some cases.

CHAPTER IV
THE RESEARCH EVALUATION: FINDINGS

Comparison of Experimental and Control Subjects

A comparison of the experimental and control groups with respect to 13 selected characteristics was undertaken to determine the comparability of the two samples. In cases where non-probability samples are used (and even sometimes when they are) it is prudent to check on possible biases or differences which might have an effect on the dependent variable being studied, in this case six and twelve-month parole outcome. This is especially true for small samples drawn over a brief period of time. In such cases atypical samples can be drawn, and they must be guarded against.

Special attention was paid to the presence or absence of differences between the experimental and control samples in the 13 characteristics and the direction of these differences to determine whether or not they would tend to have positive or negative effects on parole outcome rates. Included in this group of variables were ones usually observed to have an association with outcomes.

The first factor that the experimental and control subjects were compared on was the base expectancy score (Gottfredson and Bonds, 1969). This is an actuarial device designed to predict parole outcome. In Section A of Table 6, no appreciable differences appear between the experimentals and the controls in the percentages of cases in the three base expectancy levels. Therefore, any differences in parole outcome appearing between the two groups would have to be explained on the basis of things other than those represented in the base expectancy.

Similarly in Section B of Table 6, another factor which bears some relationship to recidivism, ethnicity, is presented. Worthy of note was the presence of a small difference between the groups in the proportions classed as "Whites". Approximately half of the controls and only 44 percent of the experimentals received this designation. Since Whites tend to perform better on parole than Blacks and other minorities (Kassebaum, Ward, and Wilner, 1971), the observed differences while small would presumably have given the control group slightly better prospects for successful parole outcome.

Similarly in Section C of Table 6, it is shown that the distribution of educational grade achievement levels also favored the control group to some extent. For the experimentals some 37 percent recorded six or fewer grades as their tested achievement level, compared to a figure ten percent lower for the controls. Since most studies reveal slight differences in outcome favoring the better educated, somewhat better performance on parole from the control group than from the experimentals could be expected.

Section D of Table 6 reveals that no significant differences were found between the two samples with respect to the history of narcotic use, nor did they differ appreciably in the history of alcohol use.

With respect to the number of prior prison terms (Section E), first termers in the control group exceeded those in the experimental by about five percent

Table 6
COMPARISON OF EXPERIMENTAL AND CONTROL SAMPLES
ON BACKGROUND FACTORS

Background Factors	Experimentals (N-135)		Controls (N-119)	
	Number	Percent	Number	Percent
A. Base Expectancy Scores				
"High"	46	34.6	41	34.8
"Medium"	52	39.1	48	40.7
"Low"	35	26.3	29	24.6
B. Ethnicity				
White	59	44.0	60	50.4
Black	46	34.3	38	31.9
Mexican-American	25	18.7	14	11.8
Other	4	3.0	7	5.9
C. Measured Grade Level				
0-6 years	48	37.2	30	27.0
7-8 years	39	30.2	31	27.9
9-10 years	27	20.9	29	26.1
11+ years	15	11.6	21	18.9
D. Drug History				
Narcotics	51	38.3	43	36.4
Alcohol	27	20.3	25	21.2
None	55	41.4	50	42.4
E. Termer Status				
First termer	77	57.0	74	62.2
Second termer	34	25.2	23	19.3
Third or more termer	24	17.8	22	18.5
F. Age				
21-25	29	21.5	18	15.1
25-30	37	27.4	24	20.2
31-35	26	19.3	25	21.0
36-40	16	11.9	18	15.1
41+	27	20.0	34	28.6
G. Commitment Offense				
"Violence"	15	11.1	30	25.2
"Property"	75	55.5	55	46.2
"Narcotic"	20	14.8	14	11.7
"Other"	25	17.0	20	16.8

(Frequencies vary slightly when information on a variable was unavailable for some parolees.)

(some 62 percent compared to 57 percent). Here again, based on previous reports and studies which tend to show that first termers do better on parole, it might be expected that the controls would outperform the experimentals.

Age differences between the two samples can be observed in Section F of Table 6, with the experimental group including greater proportions of men aged 21-25 and 26-30 than the controls. Overall, almost half of the experimentals but only 35 percent of the controls were 30 years of age or younger. Again, a younger age group usually has a slightly higher rate of recidivism (Kassebaum, Ward, and Wilner, 1971) so on this count again the controls might be expected to be a little more successful.

Section G of Table 6 presents the distributions of commitment offenses for the two groups which indicate an over-representation of 14 percent of violent offenders and a corresponding under-representation of some ten percent of property offenders in the control group. In general, since violent offenders tend to have lower recidivism rates, while property offenders have higher rates, it would be expected that the control group should have somewhat better outcomes or fewer failures on parole on this basis.

Release Plans and Resources

Not only are background and historical factors associated with success or failure on parole, so also are features of the release situation faced by the parolee (Holt and Miller, 1972). In this section of the report evidence will be presented on the distribution of these situational factors in the experimental and control groups in terms of how it might influence the results of the study.

The first matter to be considered here will be employment arrangements or offers of employment just prior to release. In Section A of Table 7 no real differences of any size are observable between our two samples with respect to job offers. Notably, most had no offers, approximately 75-79 percent, that is, and the balance were either already working or had offers on record.⁷

Residential plans are shown in Section B of Table 7 with the experimentals exceeding the controls by some seven percent in the no arrangement category. Since residence with family or spouse tends to be associated with a higher rate of parole success and residing alone, with other than family, or having no arrangements tend to be associated with a lower rate of parole success, the experimental group might be expected to do somewhat more poorly than the controls.

Similarly in the case of release money, numerous studies indicate those with less money do less well on parole in general. Section C of Table 7 indicates that the control group is definitely over-represented among those with larger amounts of release money (\$50 plus), with 27 percent in that group as compared

⁷ Those already working were on work furlough and so would probably simply continue on their current jobs.

Table 7

COMPARISON OF RELEASE PLANS AND RESOURCES OF EXPERIMENTALS AND CONTROLS

	Experimentals (N-135)		Controls (N-118)	
	Number	Percent	Number	Percent
A. Employment Plans				
No offers	105	78.9	87	75.0
Offers	25	18.8	26	22.4
Other	3	2.3	3	2.6
B. Residential Plans				
Spouse	15	11.5	13	11.2
Family	48	36.6	52	44.8
Other	20	15.3	16	13.8
None	48	36.6	35	30.2
C. Financial Resources				
\$10 or less	75	58.1	49	43.8
\$10-50	34	26.4	33	29.5
\$50-100	10	7.8	13	11.6
\$100+	10	7.8	17	15.2
D. Release Skills, Training				
Yes	52	39.4	61	52.1
No	80	60.6	56	47.9
E. Social Visits				
Yes	75	58.6	67	60.4
No	53	41.4	44	39.6
F. Correspondence				
Yes	110	84.0	100	88.5
No	21	16.0	13	11.5

(Frequencies vary slightly as a result of the unavailability of information for some parolees.)

to only 15 percent of the experimentals. On the basis of this, the control group would be expected to do somewhat better than the experimentals on parole.

While vocational skills have only a slight relationship to success on parole, Section D of Table 7 shows that the experimental group differed by more than ten percentage points from the control group in the percent possessing vocational skills, the controls being the more skilled. This under-representation of the skilled should contribute to slightly poorer outcomes for the experimentals.

The final variables considered are the visiting and correspondence patterns of the parolees in their last year of imprisonment. Studies have shown that inmates with more in the way of visits and letters do better on parole than do those with less frequent contacts or correspondence (Holt and Miller, 1972). Sections E and F of Table 7 reveal that the two groups do not differ appreciably from one another with respect to their social contacts. Some 60 percent of both groups received some visits, and 84 percent or more received some correspondence. Therefore, there is no reason to suspect any bias was introduced into the experiment by an inequality in the distribution of social contacts.

In summary, there were either no differences of any significance between the two groups (on five occasions) or (on the other eight occasions) differences that should have contributed to better parole outcomes for the control group. None of the comparisons revealed differences that would lead to the expectation that the experimentals would surpass the controls in their success rates.

Overall Six-Month Parole Outcome

One basic assumption underlying this project is that some forms of crime, usually the property types, stem more or less directly from economic needs or problems. Remove the need or problem and crimes of this sort and the resultant recidivism will diminish. This, essentially, was the hypothesis to be tested. As applied to ex-felons, this point of view argues that particularly during the re-entry phase or the first few weeks of parole, economic problems are likely to be most acute and resources to alleviate these conditions are most necessary. This experiment represented an attempt to assess the impact of alleviating economic problems on parole outcome. The expectation was that there would be less involvement in property crimes and a lower rate of recidivism on the part of the subjects receiving the financial assistance.

This expectation seems to be confirmed by the data on six-month parole outcomes presented in Table 8. It can be seen that nearly 80 percent of the experimental group (those eligible for financial aid) could be classed as successes at the six-month interval on parole, as compared to a figure of only 71 percent for the control group (those not eligible for aid). Apparently, dispensing direct financial assistance during the immediate post-release period does have impact. This difference between the experimental and control groups, while not statistically significant, becomes noteworthy when one considers the larger differences noted below in the comparisons of various subgroups of the samples. As is, the overall difference indicates the general trend toward lower recidivism for the experimental group.

Table 8

PAROLE OUTCOME AT SIX MONTHS FOR EXPERIMENTAL AND CONTROL GROUPS

	Successful Adjustment		Possible Trouble		Unsuccessful Adjustment	
	Number	Percent	Number	Percent	Number	Percent
Experimentals	108	79.9	13	9.7	14	10.4
Controls	85	71.2	15	12.7	19	16.1
Difference		+8.7		-3.0		-5.7
(2 deceased not counted)						

Selected Factors and Six-Month Parole Outcome

In this section the question of differences in parole outcome between various sub-groups in the experimentals and their counterparts in the controls will be investigated. This analysis of differential effects between sub-groups should assist in determining the actual impact of the project on certain target groups and also help in establishing guidelines for further work in this area.

Age. As noted earlier, the experimental group exceeded the control group in the proportions of parolees aged 21-25 and 26-30, age groups which usually have higher recidivism rates. In Table 9, an interesting pattern emerges in the comparison of six-month parole outcomes within age groups. For those aged 21-25, direct financial assistance seemed to be associated with more failure. Only 72 percent of the experimentals in this age group were still on active parole, while among the control subjects some 83 percent remained on parole at six months. In the age group 26-30, no appreciable effect was noted, while for those 31 or older the trend was reversed, and financial help seemed to be associated with more success on parole.

Although the numbers in some cells of the table are rather small (only 18 and 29 cases appear in the age group 21-25, for example) and thus some caution in interpreting the data is required, the overall pattern does seem clear and suggests that the younger men are perhaps not helped to the same degree by financial assistance. The older men may be experiencing what some offenders have termed the "burn out" phenomenon. That is, some may be approaching retirement from their criminal careers and therefore be in a better position to benefit from financial aid. In any case, the DFA seemed to augment positive outcomes for older men.

Table 9

AGE AND PAROLE SUCCESS AT SIX MONTHS
AMONG THE EXPERIMENTAL AND CONTROL SUBJECTS

	Age Group					
	Age 21-25		Age 26-30		Age 31 +	
	Number	Percent Successful	Number	Percent Successful	Number	Percent Successful
Experimentals	29	72.5	37	78.4	67	83.6
Controls	18	83.3	24	79.2	76	65.8
Difference		-10.9		- .8		+17.8

Ethnicity. As shown in Table 10, it seems that the White parolee profited more from financial help than did the Black parolee. (The number of Chicanos here is too small to form any definite conclusions.) Although both Blacks and Whites profited from the funds provided, in the case of the Blacks the difference between experimentals and controls was only some seven percent, whereas the Whites in the sample showed a difference of 13 percent or almost twice as much in their six-month outcomes.

Since Blacks are the more stigmatized of the two, it might be expected that they would profit less than the Whites from any program such as this. In any case some Blacks, even under the added burdens of greater stigmatization and institutional racism, did profit from financial aid, and race per se certainly should not preclude financial assistance in future programs.

Table 10

ETHNICITY AND PAROLE SUCCESS AT SIX MONTHS
AMONG THE EXPERIMENTAL AND CONTROL SUBJECTS

	Ethnic Group							
	Black		Chicano		White		Other	
	Number	Percent Successful	Number	Percent Successful	Number	Percent Successful	Number	Percent Successful
Experimentals	45	77.8	25	84.0	59	84.7	4	0.0
Controls	38	71.1	13	61.5	60	71.7	7	85.7
Difference		+ 6.7		+22.5		+13.0		-

Measured Grade Level. The educational group (defined in terms of measured grade level) receiving the greatest benefit from the financial help, as shown in Table 11, seemed to be that with grade school levels of achievement of six years or less. Here some 81 percent successfully remained on parole after a six-month period; this exceeds the control group figure of only 63 percent, a difference of about 18 percent. No other educational group showed a difference of this degree.

Where financial aid was rendered, all educational groups had nearly equal rates of success. While in the control group, those with more education had the higher probability of success (by 10 percentage points or more). Traditionally, this is the general picture; it would seem that the monetary assistance canceled out the usual relationship in the experimental group.

Table 11

MEASURED GRADE LEVEL AND PAROLE SUCCESS AT SIX MONTHS
AMONG THE EXPERIMENTAL AND CONTROL SUBJECTS

	Measured Grade Level					
	0-6 Years		7-8 Years		9 + Years	
	Number	Percent Successful	Number	Percent Successful	Number	Percent Successful
Experimentals	48	81.3	54	79.6	27	77.8
Controls	32	62.5	48	79.2	32	71.9
Difference		+18.8		+ 0.4		+ 5.9

Drug History. One of the most surprising findings to be presented in this report is contained in Table 12. Here we see that quite unexpectedly, narcotic and barbiturate users and addicts displayed increased rates of parole success at six months when afforded financial assistance, as did those parolees with neither a narcotic nor an alcoholic abuse history. Those with alcoholic histories as might be expected, did not profit from receiving financial aid. Instead, they seemed to do worse; 84 percent of the control group were still on active parole after six months as compared to only 74 percent of those receiving aid.⁸

⁸ This may perhaps indicate the need for a more structured program for alcoholics. Perhaps instead of dispensing the entire \$80 directly to the man with a history of drinking problems, support could be paid differently--small amount for personal use, balance directly to landlord, Halfway House, etc.

Those with neither type of drug abuse problem (i.e., drugs other than alcohol and alcohol) were expected to do better with financial aid, and they did. Some 84 percent of these who received aid had no serious difficulty after six months on parole; while among those not receiving money, only 74 percent had this degree of success.

Any explanation for the improvement in parole performance noted for the drug users in the sample might entail a re-examination of the assumptions regarding the "compulsive" nature of drug use. Perhaps the role that economic factors, money, jobs, etc., play in the incidence of relapse to drug use is greater than is generally supposed, at least in the short run. It is, however, clear that due to the high cost of heroin caused by its illegality, a parolee would find it impossible to support his habit with the \$80 DFA weekly allotment; it is therefore necessary to conclude that the improvement of the drug offenders in parole performance is not attributable to financially supported drug use.

Table 12

DRUG HISTORY AND PAROLE SUCCESS AT SIX MONTHS
AMONG THE EXPERIMENTAL AND CONTROL SUBJECTS

	Drug History					
	Narcotic and Barbiturate Use		Alcoholic		Neither	
	Number	Percent Successful	Number	Percent Successful	Number	Percent Successful
Experimentals	50	80.0	27	74.1	55	83.6
Controls	42	61.9	25	84.0	50	74.0
Difference		+18.1		- 9.9		+ 9.6

Occupational Background. On the basis of the figures presented in Table 13, it would seem that parolees with more skills seemed to profit least from the financial assistance given them. For example, if the skilled workers are combined with the service and sales workers, some 88 percent were successful without financial assistance, and some 84 percent were successful with financial aid.

In contrast, for the unskilled category, those receiving aid displayed a success figure of about 77 percent, as compared to only 67 percent for those not funded. The semi-skilled showed an even greater difference, with the experimentals having a success percentage of about 85 as opposed to the 58 percent of the controls who were still on active parole at six months.

Table 13

OCCUPATIONAL BACKGROUND AND PAROLE SUCCESS AT SIX MONTHS
AMONG EXPERIMENTAL AND CONTROL SUBJECTS

	Occupational Background					
	Unskilled		Semi-Skilled		Skilled, Service and Other	
	Number	Percent Successful	Number	Percent Successful	Number	Percent Successful
Experimentals	65	76.9	26	84.6	32	84.4
Controls	55	67.3	19	57.9	34	88.2
Difference		+ 9.6		+26.7		- 3.8

Work History. Table 14 presents information on the relationship between a parolee's work record and his parole outcome. Those whose records showed a "steady" work background seemed to profit most with almost 90 percent of those receiving aid succeeding on parole.⁹ This compares to 73 percent succeeding among those with a "steady" history of work who did not receive any aid.

Table 14

WORK HISTORY AND PAROLE SUCCESS AT SIX MONTHS
AMONG EXPERIMENTAL AND CONTROL SUBJECTS

	Work History					
	Steady		Sporadic		None	
	Number	Percent Successful	Number	Percent Successful	Number	Percent Successful
Experimentals	48	89.6	60	75.0	25	72.0
Controls	40	72.5	57	68.4	20	75.0
Difference		+17.1		+ 6.6		- 3.0

⁹ A person with a steady work record was considered to be one who had worked half of his adult time period minus the time in confinement.

For those with "sporadic" work history, or "no work history", much less improvement was noted with a difference of about four percent separating the experimentals and the controls.

Again it seems necessary to state that more research with larger samples of cases should be done. The number of cases studied here is not really enough on which to base conclusions. In the concluding section which summarizes the major findings of this study more will be said regarding this and certain other results.

Commitment Offense. An examination of Table 15 reveals that parolees with drug commitments showed a surprisingly substantial improvement in parole outcome when financial aid was given. Some 84 percent succeeded among those receiving aid as compared to only 69 percent among those not receiving money. This parallels the case noted previously for narcotic users and addicts; this is understandable because the groups of narcotic users and offenders with drug commitments were likely to contain many of the same people.

Parolees with property offense commitments, as might be expected, also showed marked improvement. On the basis of the assumption that economic hardships sometimes lead to thefts and crimes against property, it would appear logical that financial assistance would reduce this type of crime. Some 77 percent had no difficulties in the first six months of parole among those receiving aid, as compared to only 60 percent of those receiving no aid.

For those with violent types of commitment offenses or those grouped together as other, no appreciable improvement occurred with the administration of funds as expected. Substantially the same proportion succeeded whether or not they received funds. Again the numbers are rather small, and further research is needed in order to replicate the findings.

Table 15

TYPE OF COMMITMENT OFFENSE AND PAROLE SUCCESS AT SIX MONTHS
AMONG THE EXPERIMENTAL AND CONTROL SUBJECTS

	Type of Commitment Offense							
	Property		Drugs		Violence		Other	
	Number	Percent Successful	Number	Percent Successful	Number	Percent Successful	Number	Percent Successful
Experimentals	78	76.9	19	84.2	15	86.7	20	80.0
Controls	55	60.0	13	69.2	33	84.8	17	82.4
Difference		+16.9		+15.0		+ 1.9		- 2.4

Prior Prison Terms. In general most parole outcome studies reveal that first termers, people without any prior prison terms, do substantially better on parole (or have less recidivism) than do multiple termers. This study is no exception; but it is noteworthy that when financial aid was present, only minor differences in rates of parole success were observed among those with differing numbers of prison terms.

Stated another way, first termers displayed less improvement in parole performance than did the multiple termers when given financial assistance. As shown in Table 16, some 81 percent of the experimental group's first termers were still on parole at six months, as compared to 76 percent of those first termers not receiving aid, a difference of only five percent or so. In contrast, the multiple termers receiving aid had a parole success rate of 79 percent as compared to only 64 percent for those without financial help, a difference of about 15 percent or roughly three times that noted for the first termers. This is perhaps a function of the diminishing returns possible for the latter group, which was already enjoying a relatively high rate of success; but certainly both groups were helped to some extent by the financial aid rendered.

Table 16

NUMBER OF PRISON TERMS AND PAROLE SUCCESS AT SIX MONTHS
AMONG EXPERIMENTAL AND CONTROL SUBJECTS

	Termer Status			
	First Termer		2 + Termers	
	Number	Percent Successful	Number	Percent Successful
Experimentals	77	80.5	57	78.9
Controls	74	75.7	44	63.6
Difference		+ 4.8		+15.3

Base Expectancy Groups. Base expectancy scores were obtained for the sample, and three groups were established using the 30-40-30 percent cutting points established by the California Department of Corrections. Those in the "high" group have the highest success rates, followed by the "medium" and "low" groups, respectively. Table 17 reveals, much as might be expected, that those already with "high" base expectancy scores did not fare substantially better as a result of this program while some difference was noted in the "medium" category. The greatest degree of improvement seemed to occur with the "low" base expectancy group; those receiving aid had a percentage of success on parole of 74.3 compared to 55.2 for the controls, a difference of 19 percent.

Overall, in fact, the utility of the base expectancy index itself seems somewhat diminished for those receiving financial assistance, with noticeably smaller differences occurring between "high" and "medium" scores and none at all to speak of between "medium" and "low" scores within the experimental group. This perhaps indicates a strong economic bias or factor underlying the index.

Table 17

BASE EXPECTANCY SCORE LEVEL AND PAROLE SUCCESS AT SIX MONTHS
AMONG EXPERIMENTAL AND CONTROL SUBJECTS

	Base Expectancy Score Level					
	High		Medium		Low	
	Number	Percent Successful	Number	Percent Successful	Number	Percent Successful
Experimentals	46	87.0	52	76.9	35	74.3
Controls	41	85.4	48	68.8	29	55.2
Difference		+ 1.6		+ 8.1		+19.1

Vocational Skills. Apparently those parolees with some vocational skills (acquired through either work experience or vocational training inside or outside prison) profited most from the financial aid. In Table 18 it can be seen that about 87 percent succeeded among those with some vocational skills who received aid, compared to only 73 percent who did not receive aid. When no skills were present, only slight improvement was noted, 75 percent success as compared to 70 percent success for those not receiving DFA. Apparently when prospects are extremely limited, as in the case of the Black's situation noted earlier, less in the way of improvement is possible.

Employment Offers. In Table 19 the data indicate that having a job offer at release was not exactly common in either group, with only about one in four subjects having one. Financial aid seemed associated with parole success somewhat more in the case of those with an offer, but parolees with no job offers also did better when financial aid was given. An improvement of some 13 percentage points was noted for those with offers, and about six percent improvement was noted for the larger non-job offer group where success increased from 73 percent for the controls to nearly 79 percent for the experimentals.

Table 18

VOCATIONAL SKILLS AND PAROLE SUCCESS AT SIX MONTHS
AMONG EXPERIMENTAL AND CONTROL SUBJECTS

	Skills		No Skills	
	Number	Percent	Number	Percent
Experimentals	52	86.5	79	74.7
Controls	60	73.3	56	69.6
Difference		+13.2		+ 5.1

Table 19

EMPLOYMENT OFFERS AND PAROLE SUCCESS AT SIX MONTHS
AMONG EXPERIMENTAL AND CONTROL SUBJECTS

	Job Offer		No Offer	
	Number	Percent	Number	Percent
Experimentals	28	82.1	104	78.8
Controls	29	69.0	86	73.3
Difference		+13.1		+ 5.5

Post-Release Residence Plans. In the comparison among those with various types of residence plans, the largest improvement of the experimentals over the controls appeared in the category of arrangement with other (residence with someone other than spouse or family), as shown in Table 20. The DFA may have allowed this group to "hold up their end" financially in a shared apartment or house. Those parolees who had an arrangement for housing with wife or family did somewhat better (nearly nine percent) when financial aid was rendered, perhaps for similar reasons. Those with no arrangement did only slightly better as a result of financial aid.

Table 20

RESIDENCE PLANS AND PAROLE SUCCESS AT SIX MONTHS
AMONG EXPERIMENTAL AND CONTROL SUBJECTS

	Residential Plans					
	Arrangement with Family, Wife		Arrangement with Other		No Arrangements	
	Number	Percent	Number	Percent	Number	Percent
Experimentals	62	80.6	20	80.0	48	77.1
Controls	64	71.9	16	68.8	35	74.3
Difference	+ 8.7		+11.2		+ 2.8	

Financial Resources at Release. From the data portrayed in Table 21 it seems clear that parolees with \$50 or less in their inmate savings accounts at time of release profited most from the financial assistance project. (Gate money is provided at time of release by the Department of Corrections. At the time of the project, it usually amounted to \$40.) The figures indicated here do not include gate money, since data were unavailable on this.

One might expect that those parolees with more financial resources, \$50 or more in this case, would have essentially the same degree of success with or without financial assistance. This proved to be the case; some 80 percent were on active parole at six months among the experimentals as were 79 percent of the controls.

The impact of aid seems much more clear for those with less than \$50 but more than \$10 in their inmate accounts. Among these cases, the experimentals, or those receiving monetary aid, displayed a success rate of 94 percent, while the controls showed only about 79 percent success. Those with \$10 or less also profited in terms of parole success from the financial assistance they received; here some 73 percent remained on active parole for the six-months period, compared to only 63 percent in the control group.

To put it another way, it seems that the six-month outcomes of those with no money or less than \$10 in their accounts who received aid compared favorably with those in the control group who had larger amounts saved.

Further research should probably explore the maximum feasible cutting point beyond which financial help is superfluous in the interest of establishing guidelines for programs of this type.

Table 21

FINANCIAL RESOURCES AT RELEASE AND PAROLE SUCCESS AT SIX MONTHS
AMONG EXPERIMENTAL AND CONTROL SUBJECTS

	Amount of Money in Personal Accounts at Time of Release					
	\$10 or less		\$11-\$50		\$51 or more	
	Number	Percent	Number	Percent	Number	Percent
Experimentals	74	73.0	34	94.1	20	80.0
Controls	49	63.3	33	78.8	29	79.3
Difference	+ 9.7		+15.3		+ 0.7	

Social Contacts while Incarcerated. Another resource important to successful adjustment has always been people. One indication of support from friends and/or relatives is the social contact a parolee maintains while incarcerated. Table 22 shows that both parolees who maintained visiting and corresponding contacts and those who maintained only written contact or none were somewhat positively affected by financial assistance. Those with visitors and correspondents had a somewhat larger improvement when DFA was granted, but both groups seemed to benefit.

Table 22

SOCIAL CONTACTS AND PAROLE SUCCESS AT SIX MONTHS
AMONG EXPERIMENTAL AND CONTROL SUBJECTS

	Type of Social Contacts			
	Visits and Correspondence		Correspondence Only or Neither	
	Number	Percent	Number	Percent
Experimentals	75	82.7	52	73.1
Controls	66	74.2	43	67.4
Difference	+ 8.5		+ 5.7	

Parole Outcome at One Year

At the end of the first year on parole, some 40 percent of the controls and 47 percent of the experimentals had experienced no known difficulties, such as arrests, convictions, or other parole violations. In other words, an improvement of nearly seven and one-half percentage points in parole performance was associated with receiving direct financial assistance. While this difference is not statistically significant, it would still seem to be some evidence pointing to the promise of financial aid as a correctional tool, especially if the number and variety of parolees included in the project is considered.

The following sub-groups, or samples, were singled out as showing the most marked levels of improvement in the six-month follow-up. Differences of ten percent or more were considered marked differences in the analysis of the six-month follow-up date. Some measure of the durability of these differences can be seen in the same list of factors recorded below, accompanied by the approximate differences in percentages in favor of the experimental subjects noted at the twelve-month point. (A more detailed presentation is made in Tables 23 and 24 on pages 34 and 35.)

<u>Factor</u>	<u>Percentage Differences in Favor of Experimental Subjects</u>
1. Age group, 31 or older	20
2. Grade school education level	9
3. Property criminal	20
4. Narcotic offender	15
5. Non-addict and non-alcoholic	14
6. Unskilled or only semi-skilled	8
7. Multiple termers (two or more prior prisons)	12
8. Low base expectancy score	21
9. Less than \$50 in inmate account	15
10. Some job offer	15
11. Steady work history	17

In all but two cases, those of the unskilled or semi-skilled and the grade school educated, the magnitude and direction of the differences noted at six months continued or even increased at the twelve-months level.

Two of the above mentioned sub-group differences between the experimentals and the controls were statistically significant. These were in the age property crime commitment sub-groups. Differences as large as those noted could happen purely by chance less than five times in a hundred as measured by the chi-square test.

Discussion of Outcome Data

The bulk of this evidence indicates some substantial and relatively long lasting impact of financial aid on further crime and recidivism, most strikingly for the older, property offenders on parole. This is so clear cut as to warrant further study and more extensive study of financial aid as a tool in correctional programming.

Many other sub-groups exhibited some degree of moderately improved parole outcomes. Several of these sub-groups are among those which traditionally show little or no improvement as a result of conventional correctional programming, for example the addict and the low base expectancy score groups.

Two possible interpretations suggest themselves and seem worthy of further study. First, the overall improvements noted may be accounted for by some general social psychological phenomenon which operates during the initial reentry period, in that financial aid somehow cushions the adjustment during this period. Another possibility may be that the stigma of the "ex-con" is such that regardless of resources, skills, advantages nearly everyone released can profit at least slightly from some financial aid upon release regardless of background or skills (of course, some more than others). Again here the money would seem to serve as a cushion until suitable employment can be arranged. Some combination of these factors may account for the slight but persistent and generally across-the-board improvements noted. On the basis of the assumption that this finding will hold up in further research, it seems wise to provide assistance generally rather than try to single out and aid particular groups only.

Clearly some promise exists in this new and relatively untried method of assisting parolees both in terms of human and cost effectiveness. Lest the reader feel these improvements in parole performance were somehow created by the project or are an artifact of the research design as has been charged in connection with some community based correctional projects, he is reminded that the sole criterion of success is the presence or absence of further criminal activities. This is determined almost completely by the local police and law enforcement officials, not by the parole staff.

In view of the size, direction, and stability of these differences, further research would seem to be warranted to replicate and expand on some of the findings noted in this report.

Table 23

ONE YEAR PAROLE OUTCOMES BY SELECTED
BACKGROUND CHARACTERISTICS

Background Characteristics	Experimentals		Controls		Percent Difference
	Number	Percent with No Known Difficulties	Number	Percent with No Known Difficulties	
<u>Total</u>	(131)	47.3	(115)	40.0	+ 7.3
<u>Age</u>					
21-25	(17)	35.3	(8)	37.5	- 2.2
26-30	(43)	41.9	(26)	46.2	- 4.3
31+	(65)	55.4	(77)	35.0	+20.4*
<u>Ethnicity</u>					
White	(59)	55.9	(61)	39.4	+16.5
Black	(48)	41.7	(39)	38.5	+ 3.2
Brown & Other	(25)	40.0	(17)	41.2	- 1.2
<u>Priors</u>					
No Prior Prison	(81)	48.1	(79)	41.8	+ 6.3
One or More Priors	(52)	46.2	(38)	34.2	+12.0
<u>Commitment Offense</u>					
Violence	(10)	60.0	(21)	71.4	-11.4
Property	(84)	47.6	(64)	28.1	+19.5*
Narcotic	(23)	47.8	(16)	31.3	+16.5
Other	(16)	31.3	(15)	53.3	-22.0
<u>Drug History</u>					
Narcotics	(50)	34.0	(42)	23.8	+10.2
Alcohol	(27)	55.6	(25)	64.0	- 8.4
Neither	(54)	55.6	(48)	41.6	+14.0
<u>Work History</u>					
Steady	(47)	61.7	(40)	45.0	+16.7
Sporadic	(60)	38.3	(56)	30.4	+ 7.9
None	(25)	40.0	(19)	47.4	- 7.4
<u>Base Expectancy</u>					
High	(42)	76.1	(38)	60.5	+15.6
Medium	(54)	33.3	(46)	41.3	- 8.0
Low	(38)	34.2	(32)	12.5	+21.7
<u>Education</u>					
0-6	(75)	45.3	(64)	35.9	+ 9.4
7+	(58)	48.3	(53)	43.4	+ 4.9

*Significant at .05 level.

Table 24

ONE YEAR PAROLE OUTCOME BY
RELEASE PLANS AND RESOURCES

Release Plans and Resources	Experimentals		Controls		Percent Difference
	Number	Percent with No Known Difficulties	Number	Percent with No Known Difficulties	
<u>Voc. Trade or Skill</u>					
Yes	(49)	57.1	(59)	42.4	+14.7
No	(80)	41.3	(55)	38.2	+ 3.1
<u>Financial Resources</u> (inmate fund)					
\$50 or less	(107)	46.7	(80)	37.5	+ 9.2
\$51 or more	(20)	45.0	(29)	51.7	- 6.7
<u>Job Offer</u>					
Yes	(24)	50.0	(26)	34.6	+15.4
No or other	(107)	45.8	(87)	42.5	+ 3.3
<u>Residence Plans</u>					
Family	(63)	44.4	(63)	34.9	+ 9.5
Other	(20)	40.0	(16)	50.0	-10.0
None	(46)	52.2	(34)	47.1	+ 5.1
<u>Social Contacts</u>					
Visits and Correspondence	(71)	47.9	(63)	44.4	+ 3.5
Correspondence or Neither	(62)	46.8	(53)	34.0	+12.8
<u>Occupations</u>					
Skilled, Service, Other	(35)	57.1	(40)	50.0	+ 7.1
Semi-skilled	(25)	44.0	(17)	35.3	+ 8.7
Unskilled	(66)	40.9	(57)	33.3	+ 7.6

Parole Agents' Opinions About DFA

In the design of the DFA project, parole agents were the principal decision-makers. When an agent was notified that one of his parolees (soon to be released) was eligible for DFA, he reviewed the case, often with his supervisor's assistance. The agent's initial decision about whether or not to offer the parolee financial assistance was based on the parolee's social and employment resources. The final decision about whether to offer, or continue to offer, financial assistance, was based on discussions with the parolee himself. As mentioned above, quite often parolees either removed themselves from DFA or were removed by their agents as a result of these discussions.

In order to study agents' attitudes toward DFA, 57 of the 58 agents were interviewed who had at least one parolee in the experimental group. The questions in these interviews, for the most part in-depth and open-ended, dealt with four areas: agent decision-making regarding eligibility, impact of DFA on parolees, impact of the program on agents, and means for the improvement of the program.

Opinions: Decision-Making Regarding Eligibility for DFA. More and more scholars and practitioners in the social services, including corrections, have been questioning the justification and the efficacy of further intervention by agencies into the lives of those they serve. Since the agents in DFA were involved in this sort of intervention, they were asked a series of questions designed to probe this issue.

The first question directed at them was, "What are your thoughts about parole agents having the power to give financial assistance to parolees?" Most of the agents (67 percent) said that the power should be in the hands of parole agents. The most commonly mentioned reason for this opinion was that the agent is closest to the parolee's situation and so is in the best position to make this kind of decision. For various other reasons, 14 percent of the agents did not believe that they should be the ones to make the decision, and 16 percent were ambivalent. There was an interesting difference between small-city units (Santa Rosa and Salinas) and large-city units (San Francisco and Oakland). All the small-city agents were in favor of making the decisions, whereas the large-city agents gave responses distributed over all three categories ("Yes", "No", and "ambivalent"). It appears, then, that the small-city agents are consistently more comfortable with the responsibility of making the kind of decision that the DFA project entailed.

The agents were then asked to respond to the questions, "Did you ever decide against giving an eligible parolee the assistance?" Slightly more than half the agents (56 percent) answered "no", and slightly less than half (44 percent) answered "yes". Here, too, there was a difference between small cities and large cities. A considerable majority (74 percent) of the responses from small-city units were in the "yes" group, while a clear minority (29 percent) of the respondents from large-city units had decided against giving DFA. This response pattern might suggest that small-cities have more resources to offer parolees, or it might suggest that small-city agents are less sympathetic (and large-city agents more sensitive) to the difficulties of "making it" on parole. There is some support for both these hypotheses in the fact that most

of the agents who decided against giving an eligible parolee financial assistance (86 percent) did so because they believed the parolee had other sources of support. Either the parolees in the small cities actually had more resources, or the agents in the small cities had lower standards for judgment of the adequacy of resources. The converse might also apply. Either parolees in large cities had fewer (or needed more) resources, or large-city agents had higher standards for what was adequate. Discussions of the issue with agents have yielded some support for all these explanations.

The last question in this series was, "How did you feel about making that decision?" As one would expect, given the reasons mentioned for the decisions, all the agents said they felt comfortable. It appears, then, that whether or not the agents approved of their power to make the decisions, most of those who decided against giving an eligible parolee the assistance were satisfied with their way of handling the situation.

Opinions: Impact of DFA on Parolees. Each agent was asked, "Do you see the clients in the DFA program more often than you see your other clients?" A majority of the agents (71 percent) said "yes". These agents were next asked, "What effect has this increased contact had on your relationship with them?" A large majority (83 percent) mentioned positive effects, including most often the facilitation of a better agent-client relationship.

Those agents who reported increased contact were asked, "Do the clients in the DFA program tend to get increased services because you see them more often?" Most of the responses to this question (62 percent) were "yes". (There is some question about whether the DFA parolees actually received increased services. See "Performance of Agent Function" below.) When asked what types of services these were, most of the agents mentioned counseling and help in finding jobs. They often suggested that the increased occasions for contact made the parolees more available for the kind of informal counseling that occurs as the agent-client relationship develops and more available for job leads.

The agents who reported increased services were asked, "What do you think has had the most impact - the money, the services, or what?" Most of the agents (67 percent) felt that the money had had the greatest positive impact on parole adjustment, and 21 percent felt that each was equally important (Table 25).

In order to find out the agents' opinions on the general effect of financial assistance, they were asked about each parolee individually, "Do you believe the financial assistance he received helped him?" For a large majority of the parolees (81 percent) the response was "yes". For some of the parolees (13 percent) the response was "no", while for a small group (four percent) the response was "yes, in promoting his illegal activities".

The question was also raised about each parolee, "What other help could be given the parolee that would be just as important as money?" For 27 percent of the parolees the response was that nothing was as important as money or that no help was needed other than money; for 26 percent, the agent was concerned about personal problems; for 24 percent, the most important consideration was a good job.

Table 25

AGENT OPINIONS ON ELEMENT IN DFA PROGRAM
WITH GREATEST IMPACT ON PAROLEE ADJUSTMENT

Element of Greatest Impact	Number and Percent of Agents Responding	
	Number	Percent
"Money"	16	67
"More contacts"	2	8
"Both"	5	21
"Neither, no impact"	1	4
Other	-	-
Total	24	100

Agent Opinions: Impact of DFA on Agents. All agents were asked, "Does the DFA program make your job any easier?" Approximately one-half (53 percent) said "yes", and approximately one-fifth (21 percent) said "no", often mentioning that the program took up more of their time with additional paperwork. The remaining agents (26 percent) had mixed responses.

The agents were also asked, "What does the DFA program offer you as a parole agent?" A large majority (81 percent) said that the program offered them something positive, such as a "good tool", a "very necessary resource", more time to concentrate on the non-essential needs of the parolees, and a "way of keeping contact" with parolees who might otherwise be less accessible.

Agent Opinions: Improvement of DFA. In the DFA experiment, information from the practitioners on the line was crucial, especially if results were to be interpreted correctly. In an effort to uncover any criticisms or comments missed by the specific questions, a general question was asked, "Do you have any suggestions on improving the DFA program?" The total of 75 responses included 23 suggestions for changing the initial random determination of eligibility to one of agent assessment of need, 17 for wider application, 17 for refinement of the mechanics of the program, five for handling the assistance outside the realm of the parole agent, and ten for miscellaneous improvements. Three respondents indicated that no improvements were needed.

Parole Agents' Relationships with Parolees

While the responses to the above questions directly indicated agents' attitudes toward DFA as a program, there were other questions that were designed to compare agents' relationships with the experimental parolees as a group and their relationships with the control parolees as a group. This set of questions covered two areas that were considered particularly important: performance of the agent function with respect to contacts and services

delivered and assessment of parole adjustment with respect to criminal associations, personal problems, and probable outcome of parole.

Agents' Relationships with Parolees: Performance of Agent Function. As suggested by the agents' responses to the general question about increased contact in the previous section, a comparison of the experimental and control groups on the number of office contacts with agents did show a greater frequency for the experimental group. The average for the experimental group was 6.04 contacts, while the average for the control group was 3.08 contacts. The average number of field contacts, however, was nearly equal (approximately four) for the two groups. This difference between field contacts and office contacts is to be expected, since most of the experimental parolees came into the office weekly for their DFA checks. Further, the overall difference in the amount of total contacts between the two groups is explained by this increased office contact. Evidence of the overall increased contact with the experimental parolees is further provided by the agents' opinions that 34 percent of the control parolees but only 18 percent of the experimental parolees, were care-less or negligent in maintaining contact.

There was, however, only very slight support of the agents' belief that increased contact led to increased services. According to agent reports, the mean number of services delivered was only slightly higher for the experimental group (2.26 for each experimental, 2.05 for each control). To discover if there was any relationship between the number of services delivered and outcome, a correlation coefficient was computed. No strong relationship was found ($r = 0.07$, $p > .10$).

Although the proportion of parolees successfully on parole at six months had a tendency to increase with the total number of contacts, any causal hypothesis may be unwarranted for at least two reasons. First, a parolee who had fewer contacts than most could be either a parolee-at-large (unsuccessful), or he could be working full time (successful) and unable to visit his agent as frequently as a parolee who was "on the streets" during the day. Secondly, a higher frequency of contacts says nothing of the substantive nature of those contacts. Indeed, as we have seen, the total difference in frequency of contact between experimentals and controls rests solely in the office category, a fact easily understood when one considers that experimentals had to visit the agent each week to pick up his DFA check. In short, then, no evidence was found to support the expectation that the increase in office contact would be related to an increase in success on parole by the financially aided group.

Agents' Relationships with Parolees: Assessment of Parole Adjustment. When asked about the parolees' criminal associations, the agents expressed more confidence in the experimental group. They expressed a lack of concern about the associations of 74 percent of the experimental parolees but about only 60 percent of the control parolees.

On another variable that is related to parole adjustment, personal problems, the agents saw neither group as having more than the other. They did, however, see the experimental group as less likely to be dangerously involved in drugs or alcohol. Eight percent of the experimental parolees, as opposed to 19 percent of the control parolees, were suspected of having these problems on parole.

In predicting success on parole for the two groups, the agents were more optimistic about the experimental parolees, 63 percent of whom they believed would stay out of prison, as opposed to 50 percent of the control parolees.

There are differences between the experimental group and the control group, then, in the agents' assessment of criminal associations, existence of alcohol and drug problems, and likelihood of return to prison. These differences could reflect the agents' inclination to more positively view the experimental parolees because of closer relationships resulting from the increased contact. But it is also likely that the differences reflect the actual facts of parole adjustment--that the experimental group as a whole was somewhat less involved in criminal associations, that it had fewer problems with drugs and alcohol, and that it was likely to show a lower rate of recidivism.

Considering the responses of agents to all questions, it may be said in summary that they perceived DFA as: (1) having a positive impact on parole adjustment; (2) having a positive effect on the agent/client relationship; and (3) being a useful and needed tool or resource.

Costs and Returns: A Promising Note

As specified in the original project proposal, one type of evaluation to be furnished was one focusing on the cost-effectiveness of the project. By this it was meant an examination of the costs of the project and the projected savings to be realized by the project in reaching its objective of reducing recidivism.

Using the overall nine percentage points difference in parole outcomes (at six months) favoring the experimental group as our best estimate of the improvement engendered by financial aid, a projection of savings in prison costs is possible. On the basis of the estimated annual prison cost of \$4,400 that was current at the time of the project and subtracting the estimated annual parole cost of \$600, it is concluded that for each man kept on parole for one year at that time some \$3,800 was saved over what it would cost per year if he were returned to prison. Since each man returned to prison at the time of the study typically spent about 19 months on the average before being re-paroled, the total cost per man returned to prison was approximately \$6,000. The computation of savings is as follows:

	<u>Experimentals</u>	<u>Controls</u>
Total Number of Parolees	134	118
Number of Recidivists at Six Months	27	34
Percent Recidivism	20.1	28.8
Cost of Recidivism (Per Man)	\$6,000	\$6,000
Total Cost of Recidivism	\$120,600	\$172,800

Subtracting, we find that approximately \$52,200 in the cost of imprisonment was saved with the financially-aided group.

It must be noted further that this figure does not include other system or processing costs such as damages or losses to private citizens; costs of jails, police, courts, etc.; and often the costs for maintaining an offender's family on welfare. Conversely, benefits such as increased payment of taxes and/or increased productivity on the part of a successful parolee are important bonuses to consider which have not been included in the computation of savings.

To compute the dollar return, these savings are divided by the total money expended to achieve these savings--in this case, \$82,396 in financial aid which was given to experimental parolees. This yields an estimated return of \$.63 for every \$1 invested. (The balance of the \$120,000 was provided to pre-test and supplemental parolees, not included in the study sample.) It must be remembered, however, that the overhead or administrative expense involved in running the program is not incorporated into these computations. The Department of Corrections donated this expense as their in-kind contribution to the project. As total return on investment varies inversely with overhead costs (i.e., the higher the overhead cost, the lower the return per dollar invested), a primary fiscal objective in a program of this sort must be to minimize overhead or administrative expense.

The relative rank of the Direct Financial Assistance project with other programs, of course, is not known, but it is probably safe to assume it would fall among the top money-returners in the field. Although further research is needed to gather more information on this point, it seems clear that the concept of financial aid should be of great interest to prison administrators, legislators, and taxpayers who are tired of the spiraling costs of building and maintaining prisons, jails, and parole agencies.

CHAPTER V RECOMMENDATIONS

The findings indicate that economic factors are paramount in the personal, social, and psychological adjustment of many parolees in the early months of re-entry into conventional society. The importance of financial assistance in this process seems highly significant. It would seem that even this small move toward financial independence has the potential for improving not only the number of legitimate day-to-day opportunities a parolee has but also his or her feelings of self-worth and the positive aspects of his or her role in various social groups.

Few who are connected with the field of corrections would dispute the fact that financial aid to newly-released parolees is sorely needed. The DFA experiment has indicated that such a program does have positive impact on parole adjustment in terms of reducing recidivism and is cost-effective.

The rather broad effect of direct financial aid across most of the sub-groups in the sample as well as the scope of the resulting decrease in recidivism suggests that the project should be replicated and the results more extensively verified in larger future studies. In the meantime, the results of this study strongly suggest the utility of making some financial assistance available across the board.

At this point it would be a mistake to create definitive guidelines for financial assistance. This is especially true in view of the wide-ranging benefits experienced by parolees in most of the sub-groups studied. With the possible exception of high B.E.'s, alcoholics, and the youngest age groups, most groups profited at least to some extent.

Due to the relatively small size of the sample, it was impossible to control for all factors that might have influenced the outcome of the parolees. Further research on a larger scale could employ a more sophisticated form of analysis and could develop more accurate profiles or combinations of factors where optimum effects might be achieved. From what was learned in this experiment, two major recommendations emerge.

1. The DFA program should be extended to a larger sample. The findings to date indicate that the possibilities for positively affecting parole adjustment are large. The program should be enlarged to include a greater number of parolees, perhaps a state-wide program or at the least another parole region.
2. This enlargement of the DFA program should include a research component to determine its effects and to aid in the administration of the program. Particular attention should be paid to the impact of financial assistance on the parole outcome of sub-groups who traditionally do less well on parole such as narcotic addicts and those with low base expectancy scores.

CHAPTER VI SUMMARY

There has been a great deal of speculation, and theorizing about economic factors underlying crime in general and prisoner rehabilitation specifically. In keeping with this, a research and demonstration project was initiated by the California Department of Corrections with funding from the California Council on Criminal Justice (now known as the Office of Criminal Justice Planning) to test the impact of financial aid during the first three months of parole on recidivism and further crime, particularly property crime, on the part of parolees being released from the California prison system. The first three months on parole is viewed by some experts as a particularly crucial period.

Operations

The initial step in the program was the selection of the experimental and control parolees. Parolees who were due to be released to the San Francisco Bay Area beginning in August 1972 formed the pool of subjects for the project. Those whose CDC identification number ended in an odd digit were designated experimentals and thus were eligible for direct financial assistance (DFA) monies; while those whose number ended in an even number were designated controls and were declared ineligible.

Once an experimental parolee had been selected, the parole unit to which he was to go received notification of eligibility for DFA. At this point the agent examined the pre-release information and the parolee's file noting the parolee's financial and employment resources to determine if there was a need for DFA. If an experimental parolee did not have adequate employment and/or other financial resources, he was offered DFA by his agent.

Each week the experimental parolees met with their respective parole agents to discuss employment prospects or problems, to account for expenses over the past week, and to receive the weekly DFA check. All decisions regarding when to stop DFA or changes in the amount of DFA were left to the parole agent and the parolee. There were no guidelines given to agents.

The period when DFA was available was up to 12 weeks at a maximum of \$80.00 per week.

As mentioned earlier, several experimental parolees did not need DFA funds. Several others found suitable employment and were either slowly phased off or were dropped from the program. The end result was a differential distribution of funds as determined by the agent and the individual parolee on the basis of need.

Region-wide disbursement commenced September 1, 1972, and continued through May 1973 when the last parolee received his last DFA check. At this point the entire \$120,000 allocated for DFA had been expended.

The experimental group numbered 135 men, and the group not receiving aid, 119. Six and 12-month parole outcomes for the two groups were compared to test the effectiveness of the aid program. A comparison of the two samples revealed

only slight differences in composition in terms of background characteristics; most of these differences would contribute to increasing the success of the control group.

The criterion used for measuring "success" consisted of simply whether the man remained on active parole. "Failures" included all those who were missing, or incarcerated, or awaiting trial as reported by the parole agents at six and 12 months after release to parole even though subsequently some of these might be reinstated or continued on active parole status.

Findings

Some 80 percent of the experimental sample and about 71 percent of the control group were still on active parole status at the end of six months of parole. This difference of approximately nine percent is particularly significant when one considers the wide variety of offenders represented in the experiment, ranging from violent offenders to sex criminals and addicts as well as the primary target of the program, the property offender.

The examination of variations in parole outcome within the various subgroups of the sample revealed that effects ranged from marked positive ones to more moderate, plus a few instances where no differences and even some negative effects were noted.

The following groups seemed to profit as follows:

A. Markedly (10 percent or greater difference in favor of DFA recipients)

1. Age 31 years or older
2. Grade school education level
3. Property offenders and narcotic offenders
4. Those classed as unskilled or only semi-skilled occupationally
5. Multiple termers (one or more prior prison terms)
6. Low "base expectancy" score (Scores 0-32)
7. Those having less than \$50 in the inmates savings account
8. Those having some job offer
9. Those displaying a steady work history

Especially noteworthy in this group are several categories of offenders that are perennially noted for their high recidivism rate. Property offenders are generally noted for their propensity to recidivate as are narcotic addicts, and yet for both groups substantial gains were made when financial aid was rendered--a gain of some 17 percent for the former group and an 18 percent gain in parole success for the latter offender group. Similarly in two other groups noted for high failure rates, the low base expectancy score group and the multiple termers, marked increases in success were apparent when money was provided upon release. A 19 percent increase in success was noted for the low B.E. group, as was a figure of over 16 percent improvement for those with prior prison terms behind them.

Somewhat lesser degrees of success were noted in the following subgroups.

B. Somewhat (difference of 5-9 percent in favor of DFA recipients)

1. All ethnic groups (white somewhat more)
2. High school education or more
3. Those with no job offer
4. Those with a sporadic work history
5. First termers (no prior prison terms)
6. Medium B.E. scores (Scores 33-45)

C. No Improvement (difference of 0-4 percent in favor of non-recipients)

1. Age group 26-30
2. Violent or miscellaneous commitment offense
3. Those classed as vocationally skilled
4. Those with no-work history
5. Those with no living arrangement at time of release
6. High B. E. score (Scores 46-76)
7. Those with over \$50.00 in inmate savings account

D. Negative Results (difference of 5 percent or more in favor of non-recipients)

1. The age group 21-25 years old
2. Those with an alcoholic or drinking problem

Some measure of the durability of these differences between the performance on parole of the experimental and control subjects can be seen in the same list of factors recorded below, accompanied by the approximate differences in percentages in favor of the experimental subjects noted at the 12-month point.

<u>Factor</u>	<u>Percentage Difference in Favor of Experimental Subjects</u>
1. Age group, thirty-one or older	20
2. Grade school education level	9
3. Property criminal	20
4. Narcotic offender	15
5. Non-addict and non-alcoholic	14
6. Unskilled or only semi-skilled	8
7. Multiple termers (two or more prior prisons)	12
8. Low base expectancy score	21
9. Less than \$50.00 in inmate account	15
10. Some job offer	15
11. Steady work history	17

Discussion

In view of the broad range of the levels of improvement noted when financial assistance was provided and the relative lack of negative findings or even evidence of little improvement, it seems prudent at this stage of research to suggest that future programs of this sort be initiated excluding subjects only on the cases of need, at least until there are clear indications that certain other exclusions are warranted.

A comparison of costs and benefits related to the program indicates that a substantial portion of program costs were returned (an estimated 50 to 60 percent of the sum distributed to parolees) in the form of fewer dollars spent on incarceration, courts, and other costs.

Several recommendations were made as a result of the project findings, including some suggestions as to the need for further research and replication of these findings. Other recommendations were made concerning possible ways of financing such programs within the existing system. For example the moderate costs of the program, especially in view of the reduction in reincarceration costs could be easily defrayed by releasing offenders three to six months earlier, with no perceptible increase in the danger to community. Unfortunately it seems clear that special types of programs such as these are necessary, since regular unemployment compensation programs tend to exclude in effect most, if not all, ex-offenders. The results of this study make it clear that the concept of direct financial aid should be of great interest to prison administrators, legislators, and taxpayers who are concerned with the spiraling costs of building and maintaining prisons and jails.

APPENDICES

Appendix A - A Methodological Note on Sampling

In the course of sample selection a set of anomalies in terms of this research project were discovered in the pre-release record system. Well into the project it was learned that many parolees have their records delayed or lost, and/or have their parole dates moved forward or back, without changes being noted in the pre-release file for some time. As a consequence, some parolees were passed by in the selection process. Upon making this discovery we polled all units in the experimental region on their releasees since September 1, 1972; 42 experimentals and 45 controls had been missed in the selection process.

Inquiries into the workings of the records system showed no reason to believe that the errors were in any way systematic. However, to insure the representativeness of the samples, the background data on those missed were accumulated and compared to those for the selected groups. Seventeen basic variables were examined including the following:

- Age
- Race
- Education
- Family arrest history
- Drug use
- Marital history
- Occupation and work history
- Juvenile arrests and jail terms
- History of weapons use
- Prison terms served
- Base expectancy score
- Employment and financial resources for release

There were no differences on these variables between those selected and those not selected. Thus although the samples were not consecutive releasees, their randomness and representativeness were confirmed.

Appendix B - Employment among Experimental Parolees

As discussed in the introduction, gaining employment on release is crucial. More often than not it is a serious problem for parolees; the experimental group was no exception. Two out of three (66 percent) of the 136 experimental parolees did not report working during the first week of parole. This is not surprising when one considers that only about one-fourth of the parolees had a job offer at release.

Appendix Table A

RESPONSES OF THE EXPERIMENTAL SUBJECTS TO THE QUESTION,
"DID YOU WORK LAST WEEK?", BY PAROLE UNIT

Parole Units	Response			
	Yes		No	
	Number	Percent	Number	Percent
San Francisco	18	36	30	64
Oakland	11	31	24	69
San Jose	6	20	24	80
Santa Rosa	8	57	6	43
Salinas	3	33	6	67
ALL UNITS	46	34	90	66

The jobs that were held did not by any means, produce a lot of money for parolees. Nearly half could have received as much from DFA as they did from their jobs. More specifically, 48 percent of all the experimental group reported making \$100 or less base pay each week. At the other end of the distribution, only a little more than a quarter (28 percent) received \$126 or more.

The data in Appendix Table B show the length of time elapsing between release and the acquisition of a new job for the experimental subjects. The time period covered is the first 90 days after release.

Appendix Table B

TIME FROM RELEASE TO FIRST JOB FOR EXPERIMENTAL PAROLEES*

Time Period	Number	Percent
3 weeks or less	58	43.6
3-6 weeks	4	3.0
6-9 weeks	13	9.8
9 weeks or more	9	6.8
No job reported	49	36.8

*Refers to period from release through 90 days.

Fully 36.8 percent of the experimental subjects acquired no employment within the 90-day period after release. Obviously, the DFA would have been of considerable assistance to them. The rest of the subjects obtained a job after varying periods of time on the streets. For these latter subjects, the DFA might be viewed as assisting in the transition from prison to participation in the labor market and evaluated accordingly.

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