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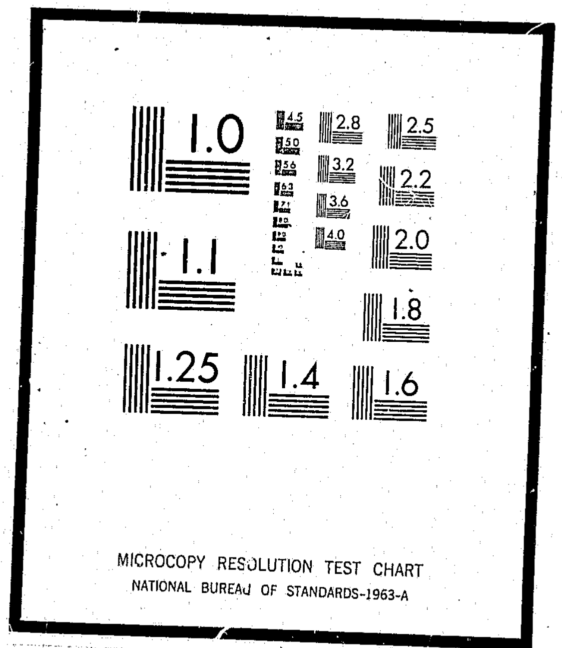

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0899

DIRECT FINANCIAL ASSISTANCE TO PAROLEES PROJECT

RESEARCH EVALUATION

Contract #01.10.17

SCIENTIFIC ANALYSIS CORPORATION

July 1973

A Project of

The California-Department of Corrections

Parole and Community Services Division

Funded by

The California Council on Criminal Justice

A NON-PROFIT ORGANIZATION - 4340 CALIFORNIA STREET, SAN FRANCISCO, CALIFORNIA 94118 / TELEPHONE (415) 752-7160

0899

X

Eval.

Mr. Bob Bales
CCCJ
7171 Bowling Drive
Sacramento, California 95823

Dear Bob;

Enclosed please find a copy of our final report on the Direct Financial Assistance Project. As you will note the results are encouraging if not totally conclusive as we might have liked.

We regret, of course, that the second year was not funded as that would have said a great deal more about the efficacy of the program. Nevertheless we all appreciate your efforts as usual and we look forward to working with you again soon.

I hope to be able to obtain some of the follow up data on the statuses of both groups on my own although time and funds are non-existent. I will be sure to send you a copy of any analysis I manage to do. Please let me know if you here of any opportunities for a program like DFA.

I wish I could have sent you more copies but our funds did not allow it; if possible please let Messers Lawson, Gist, and Galloway have a look at the report if you cannot get them copies made.

Thanks again for all your help.

Sincerely,

Scientific Analysis Corporation

Craig Reinarman

Craig Reinarman
Research Analyst
Center for the Study of the
Administration of Justice

Enclosure

15596

Scientific Analysis Corporation

August 7, 1973

Isaac Rivers, Project Director
Direct Financial Assistance to Parolees Project
Parole and Community Services Division
California Department of Corrections
2908 Fulton Street
San Francisco, CA 94118

Dear Sir:

Enclosed herewith is the twelve-month evaluation report on the Direct Financial Assistance to Parolees Project. We feel strongly that the results of this experimental program have important implications for the future of corrections. The financially-aided parolees manifested lower recidivism, agents felt financial assistance was a useful tool in parole adjustment, and the experimental program showed a high return per dollar invested. We can therefore recommend that the program be replicated and expanded.

We wish to thank all those members of the Department of Corrections connected with the project for their timely assistance.

Sincerely,

Craig Reinarman

Craig Reinarman
Center for the Study of the
Administration of Justice
Scientific Analysis Corporation

CR/amg
encl.

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Acknowledgements

In addition to those persons whose primary function is to administer or complete a research evaluation, there are always many others who make all-important contributions. Within the Department of Corrections, we are indebted to all the para-professionals, parole agents, supervisors, clerks, and clerical personnel in Region II who distributed and administered the financial assistance and who were often subjected to what may have seemed like voluminous questionnaries and monitoring forms. In the Region II records office Ivalee Bolinger and her staff were consistently helpful in gathering background data on the parolees under study. Thirdly, without the initial push and overall perserverance of Ike Rivers, Parole Work Unit II, San Francisco, the program might never have been attempted in California. And, finally, we are indebted to Donald Miller of the Los Angeles Research Unit who contributed his expertise in the form of countless ideas both analytical and reportorial.

Within Scientific Analysis Corporation we are first and foremost indebted to Dr. Dorothy Miller, who not only contributed to the original conceptualization of the program idea, but provided the energy, the ethos, and the expertise to see it through. The research was begun by Dan Waldorf whose work in design, planning, and operationalizing the study was invaluable. John Maybury, Brendon Brown, and Martin Orlick interviewed some sixty parole agents on some 250 parolees under impossible time limitations. Lester Morgan provided both technical and tutorial skill in the

computer analysis of the data. Ann-Marie Garrigues and her co-workers contributed expert editorial and clerical efforts throughout the course of the research. Patrick Biernacki critiqued early drafts of this report, making it more rational and readable. And, finally, Elizabeth Hudgins made major contributions to every phase of the evaluation.

TABLE OF CONTENTS

I. SUMMARY OF FINDINGS

SUMMARY

The Direct Financial Assistance to Parolees Project (DFA) was an experiment to test the utility of financial aid as a tool in improving the parole adjustment of newly released offenders. The major goal of the program was to reduce economic hardship in the crucial early parole period thereby lowering recidivism.

To assess the impact of the financial assistance, two groups of parolees in Parole Region II (Northern California) were randomly selected: an experimental group of parolees was eligible for financial assistance on release, while a comparison or control group was ineligible. An examination of the parole outcomes of the two groups at a six-month time interval revealed that nearly 80 percent of the financially-aided group of parolees remained successfully on parole as compared to only 71% of those ineligible.

An analysis of background variables and social characteristics of the two groups showed no differences capable of creating such a difference in parole success rates. In fact, as discussed later, the control group should have exceeded the experimentals in successful parole outcomes as more controls fell into sub-groups which have traditionally had lower recidivism. This background analysis tended to accentuate the improved outcomes of the financially-aided group.

The examination of variations in parole outcome within various subgroups of the sample revealed that effects ranged from marked positive ones to more moderate ranges, plus a few no differences

and even some negative effects.

The following groups seemed to profit as follows:

A. Markedly (improvement of 10% or more in outcome)

1. Age 31 years or older
2. Grade school education level
3. Property, criminal and narcotic offenders
4. Non-addict and non-alcoholics
5. Unskilled or only semi-skilled
6. Multiple termers (2 or more prior prison terms)
7. Low "base expectancy" score
8. Less than $50 inmate account
9. Some job offer
10. Steady work history

Especially noteworthy in this group are several categories of offenders that are perenially noted for their high recidivism rate or to put it another way, chronic parole failures who traditionally are not affected by correctional programs.

Property offenders are generally noted for their propensity to recidivate as are narcotic addicts and yet for both groups we see substantial gains made when financial aid was rendered--some 17 percent for the former group and 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 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 15 percent for those with prior prison terms behind them. (See Table P. 5)

B. Somewhat (improvement of 5-9%)

1. All ethnic groups (white most)
2. High school education
3. No job offer
4. Sporadic work history
5. First termers
6. Medium B.E. score

C. No Improvement (0-4%)

1. Age group 26-30
2. Violent or miscellaneous offense
3. Vocationally skilled
4. No-work history
5. No living arrangement
6. High B.E. score
7. Over $50.00 in inmate account

D. Negative Results (5% or more increase in unfavorable outcome)

1. Age group 21-25 years old
2. Alcoholic drinking problem

In view of these rather broad ranging effects and improvement noted when financial assistance was provided and the relative lack of negative findings or even those of little improvement, it seems

prudent at this stage of research to suggest that future programs of this sort be enacted, and that they should be applied generally across the board, at least until further research clearly indicates otherwise. Finally, it must be noted that a comparison of costs and benefits related to the program indicates that a substantial portion of program costs were returned in the form of tangible benefits.

II. INTRODUCTION

II Introduction

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.[1] 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, 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.[2]

Pownall (1967) and Irwin (1970) report similar findings.[3]

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

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1. Presidents Commission on Law Enforcement and Administration of Justice, The Challenge of Crime in a Free Society, U.S. Government Printing Office, Washington D.C., February 1967, p. 168.

2. Glaser, D., The Effectiveness of a Prison and Parole System, Bobbs-Merrill Co., 1964, pp. 328-9.

3. Pownall, George, Employment Problems of Released Prisoners, U.S. Department of Labor, 1967 and Irwin, John, The Felon, Prentice-Hall, Inc. 1970.

are currently not gainfully employed.[4] This compares 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 a recent study revealed over 26 percent of all unemployed in the parole region were fresh out of prison.

Some idea of the scope of this problem can be deduced from the fact that in a recent two month period 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.[5] Presumably some of these referrals will be resolved satisfactorily, but in view of the current market conditions and other demands on the parole agents' time, many will probably not be resolved by the time the men are released.

Another indication of the magnitude of this re-entry employment problem was noted in a survey of nearly 400 men recently paroled from one California penal institution. 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

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4. Region III (Los Angeles), 1971, and U.S. Labor figures.

5. Region III (Los Angeles), 1968.

had both arranged at that time.[6] 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 unemployment difficulties of the released ex-offender is gained when one considers the typical offender's pattern of employment history and job skills. One cannot help but note that 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.[7] Taggart 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."[8]

However, only about 30 to 40 percent of prison inmates receive some vocational training, and even here, the most recent

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6. Holt, N. and Miller, D. E., Explorations in Inmate-Family Relationships, California Department of Corrections, Report No. 46, January, 1972.

7. San Francisco Project, "A Study of Federal Probation and Parole," NIMH Report, April, 1969. See also Babst, D. and Mannering, J., "Probation versus Imprisonment for Similar Types of Offenders," Journal of Research in Crime and Delinquency, Vol. 2, No. 2, July, 1965.

8. Taggart, R. III, The Prison of Unemployment, Johns-Hopkins University Press, Baltimore, 1972, pp. 1-2.

follow up of vocational trainees reveals only about one in three actually gain employment in their field of training or one that is closely related.[9] Other studies indicate that even with skills, employers are hesitant to hire ex-offenders. The stigma attached to hiring men with records is quite noticeable; for example,

> 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.[10]

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 reports that in the Federal Prison System only about one-fourth of the offenders work in non-maintenance jobs.[11] California's prison industries employ about one-third of its inmates.[12] 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.[13]

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9. Dickover, R. et. al., A Study of Vocational Training in the California Department of Corrections, California Department of Corrections, Research Report No. 40, January, 1971.

10. Himelson, A., Risk and Rehabilitation: A Study of Fidelity Bonding of Former Offenders, Institute for Study of Crime and Delinquency, Sacramento, California, 1966.

11. Glaser, D.; op. cit., p. 158.

12. Mitford, Jessica, Kind and Usual Punishment: The Prison Business, Alfred A. Knopf, New York, forthcoming, October, 1973.

13. Glaser, D.; op. cit., p. 222.

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 this ex-offender. Once his release money, usually amounting to some 50-60 dollars, is gone, the parolee is in a difficult spot indeed. A recent study revealed that parole difficulty and return to prison is most common in this particular group.[14] An examination of employment and parole adjustment in Virginia over a long time period indicated that recidivism was inversely related to the monthly earnings of the parolees.[15]

It follows from this, and is generally agreed, that economic hardship is a major contributor to criminality in general and to property crimes especially. In Crime, Age, and Unemployment, Glaser and Rice demonstrate that property crimes vary directly with unemployment levels.[16] In a later study Glaser shows that nearly 90 percent of crimes in the FBI statistics are property crimes. This too points to 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 secondary statistical

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14. Holt, N. and Miller, D. E., op. cit., p. 48

15. Bureau of Public Administration, The Virginia Parole System -- An Appraisal of its First Twelve Years, University of Virginia, Charlottesville, 1955.

16. Glaser, D. and Rice, K., "Crime, Age, and Unemployment," in American Sociological Review, Vol. 24 (10/59), pp. 679-686.

analysis of Glaser's data.[17] 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 are employed in the California system, the wages range from five to ninteen cents per hour.[18] Needless to say it is difficult to accumulate enough funds to "tie them over" until their initial paycheck on the outside.

Taggart[19] 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.

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17. Fleisher, Belton M; "The Effect of Unemployment on Delinquent Behavior" in Journal of Political Economics; Vol. 61, 1963: pp. 543-55 - quoted in Taggart, R., op. cit., p. 15-16

18. Mitford, Jessica; op. cit.

19. Taggart, Robert III; op. cit., pp. 60-70

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 and are being tried. The Rikers Island Project in New York made loans to individual parolees of up to $200.00 but averaging about $50.00. It was not evaluated for impact. The Draper Project[20] in Alabama, which gave out grants averaging $90.00 concluded that releasees tended to "blow" the money. They concluded that the money was still necessary but tighter controls were needed.

Perhaps the largest experiment was in Division of Vocational Rehabilitation in the state of Washington where over 200 parolees received up to $1,000.00. We understand that there has not yet been a significant drop in recidivism although we have not reviewed their final report and that the project staff still feel strongly that financial assistance is a viable concept and a necessary factor in successful parole adjustment. A similar project is currently operating in Baltimore, Maryland. It is designed to test the effects of employment assistance and income maintenance.

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, difficult at best to assess. More and more practitioners, however, are being convinced by their experiences with individual parolees that financial aid

20. As Mentioned in Taggart, Robert III; op. cit., p. 71

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.

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.[21]

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 making some impact.

21. Berecochea, John; Himelson, A.; and Miller, D.E.; "The Risk of Failure During the Early Parole Period: A Methodological Note," Journal of Criminal Law, Criminology, and Police Science; Vol. 68, No., 1972, p. 93

The Theory of the Experiment

The basic logic of the Direct Financial Assistance concept, from methods to objectives, follows from the problem background material. It 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.

It is this theory which the DFA project is built upon and which this experiment examines. The project is designed to deliver up to $960.00 at the rate of $80 per week for up to 3 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). This amount was added to by a CDC in-kind, grantee contribution of $63,944 in the form of personnel services. Of the grant funds, a maximum of $120,000 was allocated for direct financial assistance to the parolees.

III. THE DIRECT FINANCIAL ASSISTANCE TO PAROLEES PROJECT

III. THE DIRECT FINANCIAL ASSISTANCE TO PAROLEES PROGRAM

Structure

The DFA project was based in the CDC Parole and Community Services Division, Region II. The Eureka sub-office was excluded from the project reluctantly because of the great distance from the San Francisco base of the study and the limited budget allocation for travel. All other parole offices in Region II, both conventional and work units, were included.

Isaac Rivers, PA III of San Francisco Work Unit #2, was the proponent of the project for CDC and was its Project Director. The parole agents became involved 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 on 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 procuring 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 (Scientific Analysis Corporation) began in July 1972 selecting parolees 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 of soon-to-be-released parolees in the Region II records office was the source frc . which the selections were made.

The following tables show which CDC facilities the groups were released from and the parole units of release for both experimental and control parolees.

FACILITY RELEASED FROM

Facility	Experimental Group Total Number	Experimental Group Percent*	Control Group Total Number	Control Group Percent*
San Quentin	64	47%	52	43%
Calif. Training Facility-Soledad	14	10	13	11
Calif. Men's Colony-San Luis Obispo	13	9	11	9
Calif. Medical Facility-Vacaville	12	9	7	6
Calif. Conservation Center-Susanville	7	5	5	4
No. Calif. Conservation Center-Garberville	9	5	6	5
Folsom	4	3	5	4
Calif. Correction Institution-Tehachapi	3	2	3	3
Deuel Vocational Inst.-Tracy	3	2	2	2
Calif. Institute for Men-Chino	1	1	5	4
TOTAL	136	100%	120	100%

PAROLE UNITS OF RELEASE (initial)

City	Experimental Group Total Number	Experimental Group Percent*	Control Group Total Number	Control Group 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%

*Figures may not add up to 100% due to independent rounding

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 given experimental parolee needed the financial assistance. The only criteria

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

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 his input and the final decision to accept or not accept the assistance was made.

Each week the experimental parolees met with their respective

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Each week the experimental parolees met with their respective

parole agents to discuss employment prospects or problems, to account for his expenses over the past week, and to receive his next 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 only criteria used was financial need.

The DFA period was to be 12 weeks at up to $80.00 per week. No one received more than $80.00 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 twelve 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 1.1 give details on the number of parolees and amounts received initially.

AMOUNTS OF MONEY GIVEN INITIALLY BY PAROLE UNITS

	Full Amount-$80		$60-79		$1-59		No Money		Total	
Units	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.

As shown 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 differential distribution of funds as determined by the agent and the individual parolee on the basis of need. The following table roughly shows how many experimental parolees received how much DFA.

TOTAL AMOUNTS RECEIVED BY EXPERIMENTAL PAROLEES

Total DFA Funds Received	Number of Parolees	Percent
0	23	14.0%
$1-319	16	11.9
$320-639	21	15.6
$640-959	16	11.9
960-1120	59	43.7
TOTAL	135	100%

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

It must here 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 benefited from the DFA. This presented slight difficulties for selection; we had to maximize the number of experimentals selected

so as to fully utilize the funds alloted for distribution to parolees while at the same time use caution so as not to over-subscribe the funds and allow each experimental parolee his full $960.00 (12 weeks @$80.00).[1]

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 under 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 these groups under study as they were not randomly selected on the basis of their identification numbers.

The DFA was disbursed to August 1972 releasees in the three San Francisco parole units as a pre-test to iron out all administrative and procedural difficulties in both the parole and research components; this pre-test group was not included in the study as they were drawn from San Francisco releases only. 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.

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1. 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.

IV. THE RESEARCH EVALUATION: SAMPLING AND METHODS

IV. The Research Evaluation: Sampling and Methods

Sampling

In order to clarify the effects of the 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.[1]

This design calls 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 fully disburse the funds allocated for financial assistance to parolees. The end result was an experimental group of 135 and a control group of 119. As it became apparent that some eligible parolees had no need for DFA, fifteen extra experimentals were selected to bring the number actually receiving funds closer to the ideal total of 120.

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1. See appendix, "Methodological Note on Sampling."
2. One control group parolee deceased.

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 supported by a man. Although there is clearly some empirical justification for such an assumption, it resulted in undue discrimination against women ex-offenders. However, in the distribution of DFA funds which remained unused by the 136 experimental parolees, women did receive DFA, although they are not in the samples under study.

All parolees who had been committed to the California Rehabilitation Center for narcotics treatment (i.e., those with "N" identification numbers) 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 re-entry problem, one in which economic problems could be easily over-shadowed by drug abuse problems. There were not, however, any other efforts to exclude narcotics offenders. There were no other exclusions from the sample; all types of ex-offenders were included.

Selection was made within three parameters: CDC identification number, parole date, and region. Parolees whose number 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 filled. 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.

TABLE 4.1 MONTH RELEASED ON PAROLE

Month	Experimental Group Total Number	Percent	Control Group 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 a 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. Parolee background information
2. Parole financial monitoring information (experimental group only).
3. Parole adjustment information
4. 3- and 6-month parole status reports

In addition, we interviewed each parole agent involved in administering DFA to one or more parolees. This parole agent data is not part of the research design, but is analyzed for

agent attitudes on various aspects of the DFA program vis-a-vis parole. This is discussed near the end of the next chapter.

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

1. Parolee Background Information

To assess the general social, demographic, and career characteristics of the parolees in our samples, researchers examined the California Department of Corrections records folder for each parolee. The record files were provided by the CDC, Region II records office. Using an instrument much like a questionnaire, 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 contact while incarcerated
Length of sentence
Base-expectancy score (raw BE-61A and judged)
Resources and plans for release

This information was originally recorded in the broadest logical categories so as to include the full range of information on any given variable and to permit more rational categorization later. For example, raw ages were recorded instead of brackets like "40-50"; actual measured grade levels were recorded rather

than groupings of "high school and above," etc.; the committment offense was recorded as charged instead of under headings of "violence," "property," etc.

The next step in the process was to compute the frequency distributions for each possible piece of data under each variable. Frequency distributions were output by the computer; these data were then re-grouped or collapsed on the basis of logical cutting points manifest in the frequency distributions. Care was taken to include enough cases in each category for proper analysis and to insure reasonable groups of responses. For example, "committment offense" was broken down into "theft," "narcotics," "violence," and "other" for two reasons: first, to insure a small number of groups of offenses so that enough cases would fall in each group for analysis; second, and most important, because studies have shown these to be offender "types" with certain generally consistent characteristics. All background variables underwent a similar coding and collapsing process.

2. Financial Monitoring Information

At the same time parole agents were notified as to 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[3]

3. Financial monitoring forms were not administered to control group parolees because they received no funds. These additional forms perhaps would have been an unfair burden on both the agents and the control parolees.

visit with the agent. Whether or not the decision was made to give the parolee DFA, he was asked these questions on the initial interview form: 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. As the release dates all varied, so did the approximate due dates of these financial monitoring forms. To minimize the confusion the research component mailed these monthly forms a few days before a month had expired. The monthly reports, however, usually lagged one and sometimes even two weeks behind schedule as meetings with parolees were often postponed. As a result, the information speaks only of a time period and not an exact point in time after release. This was reflected in the categories used in analysis of employment data from these forms.

3. Parole Adjustment Data

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 pieces of data were gathered on each parolee with respect to the 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 drugs of the alcohol variety)
Agent prognosis for success
Effects of DFA on adjustment
Parole services rendered
Personal and legal problems of parolee

This data was gathered via an in-depth interview with each parolee's agent. The interview schedule was comprised of both open-ended and structured questions. Included in these data were the components of the "Parole Adjustment Scale" as developed by the Research Division of the Department of Corrections.[4]

Much of the data gathered with this instrument were the 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. Variables concerning employment pattern, 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/parole contacts, were used to describe the indirect effects of the DFA program.

Other factors will be examined in future research in a variety of ways. Data concerning the effects of DFA on initial parole adjustment will be compared with similar data from interviews with the parolees. Arrest data will be verified by official CII arrest reports and then analyzed for patterns in property crimes vis-a-vis DFA. Open-ended responses on such

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4. Richardson, R.B.; A Pilot Investigation of Parole Follow-up Criteria (Research Report #9); Research Division, California Dept. of Corrections; p. 6-7

items as "reasons the parolee will be returned to prison" and "effects of DFA on parole adjustment" will be content-analyzed.

4. Three-Month and Six-Month Parole Status Reports

To grasp some measure of outcome, i.e., success on parole, the statuses of the parolees were gathered from the agents at two intervals. Ideally, this parole status information might be gathered at one specific point after each parolee's release, e.g., 90 days; several facts, however, made this procedure inappropriate. First and foremost the releases were staggered over a three month period with many parolees' release dates being moved forward or back anywhere from one day to three months without notice. This made any attempt to obtain parole status information, at one point after release, impractical. Second, the use of CII arrest reports to gain this data was impossible as the disposition of any arrest may have taken months, and even then reporting procedures seemed to take varying lengths of time. Lastly, the CDC 12-month Follow-up Reports (which contain all the necessary information) would not be available for at least 13 months after the first experimental parolee was released, and then their issurance would likewise be staggared over the three month release period. Because we are examining DFA effects during the initial parole period, a measure of parole success was necessary earlier than at one year after release.

In the absence of a suitable method for gathering this early outcome data at a specific time for each parolee, we derived a median point in time at which half the parolees under

study would have been released for more than three months and half for less than three months. We then derived a later point in time when six-months was the median time since release for both groups of parolees. On this basis, agents were asked to complete a "status-report" for each of his parolees in either group showing their status at that 3-month point in time. Three months after that, agents were asked again for this information; thus the two basic measures of outcome so far in the research are the three and six month parole status reports. These provided the basic outcome measures (dependent variables) upon which the time-series comparisons of the two groups were made.

The various parole statuses used formed a list (not unlike a recidivism scale) of statuses ranging from a most successful, arrest free parole adjustment to a new incarceration in prison. The following are the raw statuses used:

1. Successful 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.
9. Deceased[5]

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5. Two parolees, one experimental and one control, passed away while on parole. Due to the small number and the fact that the actual parole adjustment of these men could not be properly determined, these two cases and that status were dropped from the analysis.

This variety of parole statuses was an unworkable number if research was to distinquish between outcomes or objective meausres of parole adjustment. Further, if all nine statuses were used there would have been too few cases under each status for analytic purposes. Therefore the raw statuses were collapsed into three basic categories as follows:

1. SUCCESSFUL ADJUSTMENT

including 1. Successfully on parole and employed in school, 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, Narcotics Treatment and Control Unit
7. In custody, mental hospital or hospital
8. Returned to prison or serving jail sentence

This three-part outcome variable was then crosstabulated with selected background and adjustment variables for both the control and experimental groups, forming the basis of the comparisons.

Summary: A Sketch of the Basic Analytical Framework

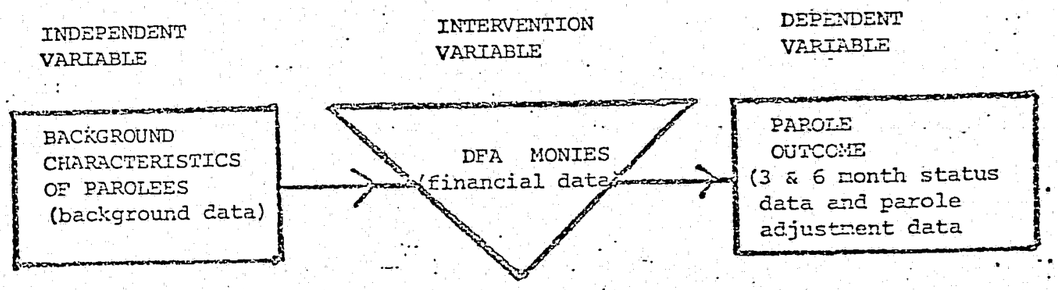
In the previous sections we have described the instruments, data collection procedures, coding steps, and resultant variables. All that is necessary in the way of a summary is a diagram showing how these data or variables relate to one another and to assessing

the impact of DFA in parole adjustment.

If one views the DFA program as an act performed on parolees, the analysis becomes quite simple. We begin the experiment with a group of parolees who obviously bring to the program a set of social, psychological, demographic, and career characteristics already developed. This "input" may be collectively described as the "independent variable" because these characteristics have been developed without the influence of the program.

When these parolees are released to parole we are interested in how well they adjust as measured by the presence or absence and extent of further trouble. This is the "outcome" or the dependent variable.

When DFA is introduced to this relationship we have "intervened" into the normal parole process; something has been added which might influence the dependent or outcome variable. Graphically the design looks this way:



Simply stated, our task was to assess the impact of the DFA intervention on parole outcome while controlling for the effects of the background characteristics. Because the only systematic difference between the experimental and control groups is DFA money, any impact on outcome should be a consequence of the application of DFA.

Data Analysis

The actual data analysis procedures used in this study to data involve a simple form of multivariate analysis. The principal technique is a comparison of experimental (those eligible for DFA monies) versus controls (not eligible) with respect to the 6-month parole outcomes,[6] while partial crosstabulations are used to control for the effects of various background or other variables on this outcome.

In this particular report the emphasis of the analysis will be on 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 partial tables. Any further comprehensive tests of significance or further partialing by introducing a third or fourth variable is impossible; larger scale studies must be undertaken to allow these forms of testing.

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6. Usually the 6-month parole outcome only is used as the major dependent variable as it is the most current measure.

Unless otherwise specified, the respective tables in the section on findings will present the percent still successfully on parole at six months; each table will present the difference and its direction.[7]

7. In some tables the reader will note that the total number of cases (N) will vary. This is the result of either information being unavailable for some cases on that particular variable, or question being not applicable to a number of cases as will be obvious from the table.

V. THE RESEARCH EVALUATION: FINDINGS

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Sample Characteristics

A comparison of the experimental and control groups with respect to thirteen selected characteristics was undertaken to make the composition of the research sample clear to the reader, and to help evaluate 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 make for an effect on the dependent variable being studied: in this case six month parole outcome. This is especially true for small samples drawn over a brief concentrated period of time. In such cases atypical samples can occur and of course, regardless of sampling design, atypical results can occur purely by chance; hence the constant need for replication studies.

Special attention will be paid to the presence or absence of any sample differences and especially noted will be the direction of these differences (if any)--that is, whether or not they would tend to have positive or negative effects on parole outcome rates observed. Included in this array of variables are ones usually noted for their association with outcomes and most will be used later in the finding section.

Background Factors

Beginning with Base Expectancy Scores[1] let us examine the composition of our two samples. In Table 1A (all tables follow) we see that no significant differences appear between the "experimentals" and the "controls" in this study--hence no probability of bias in the results exists as measured by the Index.

Similarly in Table 1B, another factor which bears some relationship to recidivism, namely ethnicity, is presented. Worthy of note is the presence of a small difference between groups with respect to the proportion of each group which is classed as "white." Approximately half of the "controls" and only 44 percent of the "experimentals" receive this designation. Since Whites quite often do somewhat better or enjoy less recidivism than Blacks and other minorities, the observed differences while small would presumably give the control group a somewhat better picture on parole outcome, but only slightly.[2]

Similarly we see in Table 1C that the samples' make-up on educational grade levels also favors the control group to some extent. For the "experimentals" some 37 percent record 6 or fewer grades as their tested level, as compared to a figure 10 percent

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1. For a discussion of the base expectancy and its predictive power, see Don Gottfredson and Jack Bonds, "A Manual for Intake Base Expectancy Scoring," California Department of Corrections, Research Division, March 1969 (mimeo).

2. See Kassebaum, G.; Ward, D.; and Wilner, D.M.; "Prison Treatment and Parole Survival: An Empirical Assessment," Wiley and Sons, 1971, especially Ch. VIII, p. 254.

TABLE 1 : SAMPLE CHARACTERISTICS

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

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

lower than this for the controls. Since most studies reveal slight differences in outcome favoring the better educated on this score, we might expect somewhat better performance from the "control" group than from the "experimentals."

Table 1D reveals that no significant differences were found between the two samples drawn with respect to their history of narcotic use--with 36 or 38 percent having a history of some drug use, and around 20 percent credited with drinking problems and the remaining 42 percent with neither alcohol nor drug problems noted in their histories.

When the two samples were compared with respect to the number of prior prison terms--first termers in the control groups exceed those in the experimentals by about 5 percent (some 62 percent as compared to only 57 percent). Here again based on previous reports and studies which tend to show that first termers do better on parole--we might expect the controls to do somewhat better than the experimentals simply because of their greater number of first termers (see Table 1E).

Age differences between the two samples can be observed in Table 1F--with the experimental group comprised of more men aged 21-25 and 26-30 than is true for the controls. Overall, almost half of the experimentals but only 35 percent of the controls are 30 years of age or younger. Again, being younger usually means slightly more recidivism, so on this count again the controls might be expected to be a little more successful due to their older age

distribution.[3]

Table 1G presents the respective distribution of commitment offense[4] for the two groups and seems to indicate an over-representation--some 14 percent--of "violent" offenders and a corresponding under-representation of some 10 percent with respect to "property" crimes in the control group. In general, since "violent" offenders tend to have low recidivism rates, while "property" offenders have rather high rates, it would be expected that the control group should have somewhat better outcomes or less parole failure on this score.[5]

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.[6] In this section of the sample description we shall present evidence on how these situational factors

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3. See Kassebaum, Ward, Wilner (op. cit.)

4. Four types of commitment offenses were derived: "violence" category contained cases of manslaughter, murder and ADW offenses; while the "property" category included burglary, robbery and forgery. "Drugs" included all narcotics and dangerous drug offenses with the "others" being a catch-all category of those offenders not fitting in the previous three groups.

5. Kassebaum, Ward and Wilner (op. cit.), p. 261.

6. See Holt, N. and Miller, D.E., "Explorations in Inmate-Family Relationships," Research Division, California Department of Corrections, January 1972 (especially Chapter VI, p. 42-49).

are distributed in the study population and speculate as to how this might influence the results of the study.

The first aspect of release plans to be considered here is that concerning employment plans or offers of employment just prior to release. In Table 2A we note no real differences of any size between our two samples with respect to job offers. Notably, most have no offers, approximately 75-79 percent, that is, and the balance are either already working or have offers on record.[7]

Residential plans are shown in Table 2B for the respective groups 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 more parole success and residing alone, with other than family, or having no arrangements tend to have higher failure rates, the experimental group might be expected to do somewhat poorer than the controls.

Similarly in the case of release money, numerous studies indicate those with less money do less well on parole in general. We see in Table 2C that the control group is definitely over-represented among those with larger amounts ($50 plus), with 27 percent in that group as compared to only 15 percent of the experimentals. Based on this large a difference then we might expect the control group to do somewhat better than the "experimentals" as far as 6-month parole outcomes are concerned.

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

It is noteworthy that while vocational skills have only a slight relationship to success on parole, Table 2D shows that the experimental group differs by more than 10 percentage points from the control group in the percent possessing some vocational skills, the controls being the more skilled. This under-representation should make for slightly poorer outcomes for the experimentals, assuming all other things equal.

The final variables considered relate to 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.[8] Table 2E and 2F reveal that the study samples do not differ appreciably from one another with respect to their contacts-- in all some 60 percent of both groups received some visits and 84 percent or more received some correspondence. In short there is no reason to suspect any bias was introduced into the experiment by this distribution of social contacts.

In summary then, this section on the study population's characteristics, its description and the comparison between experimental and control groups using thirteen variables, has indicated in each instance either no difference of any significance between the two groups (on five occasions), or (on the other eight occasions) the differences observed should contribute to <u>better</u> parole outcomes

8. See Holt and Miller, (op. cit.).

for the control group. None of the comparison would lead us to think the experimentals should surpass the controls in their success rates.

TABLE 2: RELEASE PLANS AND RESOURCES

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

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

Overall Six-Month Parole Outcomes

One basic assumption in the orientation underlying this project posits that some forms of crime--usually the property type of offense--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 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, needs are apt to be most acute and resources to alleviate these conditions are most necessary; hence this experiment. Given some financial assistance, the hypothesis of this pilot study would expect lower recidivism and fewer property crimes (future research will analyze arrest reports and CDC follow-up data to determine the fluctuations in the rates of property crimes).

The validity of this orientation seems to be borne out by the data revealed in Table O. 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, this method of dispensing direct financial assistance over the immediate post-release period does show promise. Such a finding, while not statistically significant overall, becomes noteworthy when one considers the sharper differences found in various subgroups of the sample where numerous significant and near-significant differences occurred. As is, the overall difference indicates the general trend toward lower recidivism for the experimental group. Certainly the project, to be successful,

would have to show its effect in the early parole period, as it has, but it will be interesting to see just how stable and enduring this apparent success is over a longer parole period--say one year. In the following sections the success rates and impact of the project within various sub-groups of the parolee population will be examined.

TABLE O: DIRECT FINANCIAL ASSISTANCE AND 6 MONTHS OUTCOMES

	Successful Adjustment		Possible Trouble		Unsuccessful Adjustment	
	%	N	%	N	%	N
Experimentals	79.9%	107	9.7%	13	10.4%	14
Controls	71.2	84	12.7	15	16.1	19
Difference	+8.7		-3.0		-5.7	

(2 deceased not counted)

Selected Factors and Six-Month Parole Outcome

Having examined the overall association of financial aid and outcome, in this section we shall take up questions of which subgroups, if any, display either more or less parole success when receiving this aid. This analysis of differential effects 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 and Direct Financial Assistance

As noted earlier, the experimental group does exceed the control group in the proportion of parolees aged 21-25, and 26-30 years old--an age group incidentally, which is usually associated with higher recidivism rates. In Table 5A we see that when we compare six-month outcomes within age groups, an interesting pattern emerges. For those aged 21-25, direct financial assistance seems to make for more failure with only 72 percent still on active parole, while in the control group aged 21-25, some 83 percent remain on parole. In the next age group 26-30, no appreciable effect is noted, while for those age 31 or older the trend is reversed and financial help seems 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 indicates some caution in interpreting this data, the overall pattern does seem clear, and suggests that the younger men

perhaps are not as motivated as the older men and hence are not helped to the same degree by financial assistance. Another possibility of course is that the younger men, being less mature, have less patience, less tolerance of the frustration endemic to parole and more quickly resort to crime. 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 seems to augment positive outcomes for older men.

TABLE 5A: AGE AND PAROLE SUCCESS

	Percent Successful at 6 Months'					
	Age 21-25		Age 26-30		Age 31 +	
	%	N	%	N	%	N
Experimentals	72.5%	29	78.4%	37	83.6%	67
Controls	83.3	18	79.2	24	65.8	76
Difference	-10.9		- .8		+17.8	

Ethnicity and Financial Assistance

As shown in Table 5B it seems that the White parolee distinctively profits more from financial help than does the Black parolee. (The number of Chicanos here is too small to form any definite conclusions about their behavior.) Although both Blacks and Whites profit from the funds provided, in the case of the Blacks the improvement is only some 7 percent whereas the Whites in the sample exhibit some 13 percent improvement or almost twice as much in their six-month outcomes.

One cannot help but conclude that the Blacks are after all the most stigmatized of the two, and perhaps because of this "extra" difficulty should profit less than the Whites from any program such as this. In any case some Blacks, even under the added constraints of double stigmatization and institutional racism, do profit from financial aid and race per se certainly should not preclude financial assistance for any ethnic group in future programs.

TABLE 5B: ETHNICITY AND PAROLE SUCCESS

	Percent Successful at 6 Months							
	Black		Chicano		White		Other	
	%	N	%	N	%	N	%	N
Experimentals	77.8%	45	84.0%	25	84.7%	59	0.0%	4
Controls	71.1	38	61.5	13	71.7	60	85.7	7
Difference	+6.7		+22.5		+13.0		--	

Measured Grade Level and Financial Assistance

The educational group receiving most benefits from the financial help, as shown in Table 5C, seems to be those with grade school levels--six years or less. Here some 81 percent successfully remain on parole after a six month period, which exceeds the control group figure of only 63 percent, a difference of about 18 percent. No other educational group showed this degree of improvement.

In effect, where financial aid was rendered and held constant, all educational groups had nearly equal rates of success, while in the control group with no financial aid those with more education had

the higher probability of success (by 10 percentage points or more). Traditionally this is the general picture overall, so it would seem that monetary assistance cancels out the normal assocation.[9]

TABLE 5C: MEASURED GRADE LEVEL AND PAROLE SUCCESS

	Percent at 6 Months					
	0-6 Years		7-8 Years		9 + Years	
	%	N	%	N	%	N
Experimentals	81.3%	48	79.6%	54	77.8%	27
Controls	62.5	32	79.2	48	71.9	32
Difference	+18.8		+0.4		+5.9	

Drug History and Financial Assistance

One of the most surprising findings to be presented in this report is contained in Table 5D. Here we see that quite unexpectedly, narcotic and barbiturate users and addicts display increased six months rates of parole success when afforded financial assistance, as do 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.[10]

9. It should be noted that Whites make up between 80 and 90 percent of those with a high school education and, as already noted, Whites often enjoy more parole success. In all probability then, if ethnicity could be controlled, these educational differences would disappear or at least diminish.

10. This may perhaps indicate the need for a more structured program for alcoholics. Perhaps instead of dispensing the entire $80

Those with neither type of drug abuse problem 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 of 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 addicts in the sample might entail a re-examination of the assumptions regarding the "compulsive" nature of drug use. Perhaps the role that economic factors, e.g., 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 would be interesting to see if these initial improvements persist beyond the six-month period and apply to one-year follow ups as well. 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; we must reason that the improvement of drug offenders is not attributable to financially supported drug use.

TABLE 5D: DRUG HISTORY AND PAROLE SUCCESS

	Percent Successful at 6 Months					
	Narcotic Use		Alcoholic		Neither	
	%	N	%	N	%	N
Experimentals	80.0%	50	74.1%	27	83.6%	55
Controls	61.9	42	84.0	25	74.0	50
Difference	+18.1		-9.9		+9.6	

10. (continued)
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.

Occupational Backround and Financial Assistance

Judging from the figures in Table 5E presented below, it would seem that parolees with more skills seem to profit least from the financial assistance given them. For example, if we combine the skilled workers with the service and sales workers, some 88 percent are successful without financial assistance, and some 84 percent are successful with financial aid.

In contrast, for the unskilled category, those receiving aid display a success figure of about 77 percent, as compared to only 67 percent for those not funded. The semi-skilled show even more improvement, with experimentals having a success percentage of about 85 and among the controls some 58 percent are still on active parole as of six months.

TABLE 5E: OCCUPATIONAL BACKGROUND AND PAROLE SUCCESS

	Percent Successful at 6 Months					
	Unskilled		Semi-Skilled		Skilled, Service and Other	
	%	N	%	N	%	N
Experimentals	76.9%	65	84.6%	26	84.4%	32
Controls	67.3	55	57.9	19	88.2	34
Difference	+9.6		+26.7		-3.8	

Work History and Financial Aid

Table 5F presents information on the relationship between a parolee's work record and his parole outcome. Those whose records

showed a "steady" work background[11] seem to profit most with almost 90 percent of those receiving aid succeeding on parole. This compares to 73 percent succeeding among those with a "steady" history of work who do not receive any aid.

For those with "sporadic" work history, or "no work history," much less improvement was noted with a difference of about 4 percent separating the experimentals and the controls.

Again we think it necessary to state that more research with a larger sample of cases should be treated differentially in such programs since the number of cases studied here is rather small. In the concluding section which summarizes the major findings of this study more will be said regarding this and certain other findings.

TABLE 5F: WORK HISTORY AND PAROLE SUCCESS

	Percent Successful at 6 Months					
	Steady		Sporadic		None	
	%	N	%	N	%	N
Experimentals	89.6%	48	75.0%	60	72.0%	25
Controls	72.5	40	68.4	57	75.0	20
Difference	+17.1		+6.6		-3.0	

11. Typically a "steady" work record consisted of working at least half of the adult time period prior to commitment.

Prior Prison Terms and Financial Aid

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 is present, little difference in parole success rates are observed with respect to prison terms.

Looked at another way, first termers display less improvement in parole performance than do the multiple termers when given financial assistance. As shown in Table 5G, some 81 percent of the experimental groups' first termers are still on parole at six months, as compared to 76 percent of those first-termers not receiving aid--a difference of only 5 percent or so. In contrast, the multiple termers receiving aid have a success parole 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 first termers. This is perhaps a function of the diminishing returns possible for that group which is already enjoying a relatively high rate of success, but certainly both groups are helped to some extent by the financial aid rendered.

TABLE 5G: PRISON TERMS AND PAROLE SUCCESS

	Percent Successful at 6 Months			
	First Termer		2 + Termers	
	%	N	%	N
Experimentals	80.5%	77	78.9%	57
Controls	75.7	74	63.6	44
Difference	+4.8		+15.3	

Commitment Offense and Financial Aid

As noted earlier, in the case of the narcotic users and addicts an examination of Table 5H reveals that parolees with drug commitments show a surprisingly substantial improvement in parole outcome when financial aid is given. Some 84 percent succeed among those receiving aid as compared to only 69 percent among those not receiving money.

Parolees with "property" offense commitments, as might be expected, also show marked improvement. Assuming that economic hardships sometimes lead to thefts and property crime, at least in some cases, financial assistance should logically reduce this type of crime. Some 77 percent have no difficulties in the first six months of parole among those receiving some aid, as compared to only 60 percent of those receiving no aid.

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

TABLE 5H: TYPE OF COMMITMENT OFFENSE AND PAROLE SUCCESS

	Percent Successful at 6 Months							
	Property		Drugs		Violence		Other	
	%	N	%	N	%	N	%	N
Experimentals	76.9%	78	84.2%	19	86.7%	15	80.0%	20
Controls	60.0	55	69.2	13	84.8	33	82.4	17
Difference	+16.9		+15.0		+1.9		-2.4	

Base Expectancy Groups and Financial Assistance

Base expectancy scores were obtained for the sample and three groups were established using the 30%-40%-30% 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 5I reveals, much as might be expected, those already with "high" expectancy scores do not fare substantially better as a result of this program while some difference is noted in the "medium" category. The greatest degree of improvement seemed to rest with the "low" expectancy group, with a difference of 19 percent between the success figures for those receiving money and those who did not (in favor of those who did).

Overall, in fact, the utility of the BE 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, particularly the lower

BE ranges. Before excluding higher BE groups from further experiments, however, more data should probably be examined.

TABLE 5I: BASE EXPECTANCY SCORES AND PAROLE SUCCESS

	Percent Successful at 6 Months					
	High		Medium		Low	
	%	N	%	N	%	N
Experimentals	87.0%	40	76.9%	40	74.3%	26
Controls	85.4	35	68.8	33	55.2	16
Difference	+1.6		+8.1		+19.1	

Vocational Trades or Skills and Financial Aid

Apparently those parolees with some skills and trade experience profitted most from the financial aid. In Table 5J it can be seen that about 87 percent succeed among those with some vocational assets when given some aid, as compared to only 73 percent succeeding on parole for those not receiving aid. When no skills are 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 as noted earlier, less in the way of improvement is possible. Again, it would seem more research is needed to clarify this finding by holding constant other factors such as race.

TABLE 5J: VOCATIONAL TRADE OR SKILLS AND PAROLE SUCCESS

	Percent Successful at 6 Months			
	Skills		No Skills	
	%	N	%	N
Experimentals	86.5%	52	74.7%	79
Controls	73.3	60	69.6	56
Difference	+13.2		+5.1	

Employment Offers and Financial Aid

In Table 5K we see that having a job offer was not exactly common among either group, with only about one in four having a job offer at release. 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 12 percentage points was noted for those with offers and about 6 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 5K: EMPLOYMENT OFFERS AND PAROLE SUCCESS

	Percent Successful at 6 Months			
	Some Offer		No Offer	
	%	N	%	N
Experimentals	82.1%	28	78.8%	104
Controls	69.0	29	73.3	86
Difference	+13.1		+5.5	

Residence Plans and Financial Aid

As shown in Table 5L the largest improvement between experimentals and controls appears in the category of "arrangement with other," meaning those who had a residence arranged upon release but with someone other than a spouse or family. We hypothesize that the DFA 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 9%) when financial aid was rendered, perhaps for similar reasons. Those with no arrangement did only slightly better as a result of financial aid.

TABLE 5L: RESIDENCE PLANS AND PAROLE SUCCESS

	Percent Successful at 6 Months					
	Arrangement with Family, Wife		Arrangement with Other		No Arrangements	
	%	N	%	N	%	N
Experimentals	80.6%	62	80.0%	20	77.1%	48
Controls	71.9	64	68.8	16	74.3	35
Difference	+8.7		+11.2		+2.8	

Financial Resources at Release and Financial Aid

From the data portrayed in Table 5M it seems clear that parolees with $50.00 or less in their inmate savings accounts at time of release profit most from the financial assistance project. (Gate money for those with less than $200.00 is provided at time of release by the Department of Corrections. It usually amounts to

$40.00. Note: our figures do not include gate money; data was unavailable.)

And one might expect those parolees with more cash resources--$50 or more in this case--to have essentially the same degree of success with or without financial assistance. Some 80 percent are on active parole among the experimentals as are 79 percent of the controls.

The case of need seems much more clear for those with less than $50 but more than $10 in their inmate accounts. Here the experimentals, or those receiving monetary aid, display a success rate of 94 percent while the controls show only a figure of about 79 percent success. Those with $10 or less saved also profit in terms of parole success from the financial assistance they recieved; here some 73 percent remain on active parole for the six months period as compared to the control group where only 63 percent succeed in remaining in good standing over a six-month period.

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

Further research should probably explore the maximum feasible cutting point beyond which financial help is superfluous and help further establish guidelines for further programs of this type, and/or explore possible ways of earning or acquiring larger sums of savings while in prison to achieve the same effect.

TABLE 5M: FINANCIAL RESOURCES AT RELEASE AND PAROLE SUCCESS

	Percent Successful at 6 Months					
	Release Money $10 or less		Release Money $11-$50		Release Money $51 or more	
	%	N	%	N	%	N
Experimentals	73.0%	74	94.1%	34	80.0%	20
Controls	63.3	49	78.8	33	79.3	29
Difference	+9.7		+15.3		+0.7	

Social Contacts while Incarcerated and Financial Aid

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 maintained while incarcerated. We see in Table 5N that both parolees who maintained visiting and corresponding contacts, and those who maintained only written contact or none, were somewhat positively effected by financial assistance. Those with visitors and correspondents had a somewhat larger imporvement when DFA was rendered, but both groups seemed to benefit.

TABLE 5N: SOCIAL CONTACTS AND PAROLE SUCCESS

	Percent Successful at 6 Months			
	Visits and Correspondence		Correspondence Only or Neither	
	%	N	%	N
Experimentals	82.7%	75	73.1%	52
Controls	74.2	66	67.4	43
Difference	+8.5		+5.7	

Parole Agents' Attitudes toward DFA

According to the design of the DFA project, parole agents were the principal units of decision-making. 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 experimental parolee himself. As mentioned above, quite often parolees who were eligible for financial assistance 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, we interviewed 57 of the 58 agents 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 elgibility, impact on parolees, impact on agents, and improvement of the program.

Agent Attitudes: 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, we asked them a series of questions designed to probe this issue.

First, we asked, "What are your thoughts about parole agents having the power to give financial assistance to parolees?" Most of the agents (67%) 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% of the agents did not believe that they should be the ones to make the decisions, and 16% were ambivalent. There was an interesting difference between small-city units (Santa Rosa, Salinas, and San Jose), 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.

We next asked the agents, "Did you ever decide against giving an eligibleparolee the assistance?" Slightly more than half the agents (56%) answered "no," and slightly less than half (44%) answered "yes." Here, too, there was a dichotomy between small cities and large cities: a considerable majority (74%) of the responses from small-city units were in the "yes" group, while a clear minority (29%) of the responses 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 of both these hypotheses in the fact that most of the agents who decided against giving an eligible parolee financial assistance (86%) 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 judgement 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. Our discussions 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 <u>against</u> giving an eligible parolee the assistance were satisfied with their way of handling the situation.

<u>Agent Attitudes: Impact of DFA on Parolees</u>

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%) said "yes."

These agents were next asked, "What effects has this increased contact had on your relationship with them?" A large majority (83%) 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%) 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 these agents (67%) felt that the money had had the greatest positive impact on parole adjustment, and 21% felt that each was equally important.

AGENT ATTITUDES ON IMPACT OF DFA PROGRAM ON PAROLEE ADJUSTMENT ("What has had the most impact -- the money, the increased contact, or what?")

Response	Total number and percent of agents responding: Total number	Percent
"Money"	16	67%
"More contacts"	2	8
"Both"	5	21
"Neither, no impact"	1	4
Other	--	--
TOTAL	24	100%

In order to find out the agents' opinions on the general effect of financial assistance, we asked them about each parolee individually: "Do you believe the financial assistance he received helped him?" For a large majority of the parolees (81%) the response was "yes." For some of the parolees (13%) the response was "no," while for a samll group (4%) the response was "yes, in illegal activities."

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

Agent Attitudes: Impact of DFA on Agents

All agents were asked, "Does the DFA program make your job any easier?" Approximately one-half (53%) said "yes," and approximately one-fifth (21%) said "no," often mentioning that the program took up more of their time with additional paperwork.

The remaining agents (26%) had mixed responses.

The agents were also asked, "What does the DFA program offer you as a parole agent?" A large majority (81%) 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 Attitudes: Improvement of DFA

In the DFA experiment, input from the practitioners at the line-level was crucial, especially if results were to be interpreted correctly. And so in an effort to cover any criticisms or comments missed by our specific questions, we asked a general question: "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, 5 for handling the assistance outside the parole agent realm, 10 for miscellaneous improvements, and 3 opinions that no improvements are 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 we 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 4) 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. The fact of the overall increased contact

with the experimental parolees is further supported by the agents' opinion that 34% of the control parolees, but only 18% of the experimental parolees, were careless or negligent in maintaining contact.

There was, however, only very slight support of the agents' belief that increased contact lead 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 any nexus between the number of services delivered and outcome, a correlation coefficient was computed. No strong relationship was found (r = 0.073; significance = .126).

Although the proportion of parolees successfully on parole at six months had a tendency to increase with the total number of contacts, any causal hypotheses may be spurious 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, we found no evidence to support a hypothesis that the increase in office contact bore any relationship to the increased success in outcome of 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% of the experimental parolees, but about only 60% of the control parolees.

On another variable that is related to parole adjustment--personal problems--the agents saw neither group as having more of these problems than the other. They did, however, see the experimental group as less likely to be dangerously involved in drugs or alcohol: 8% of the experimental parolees, as opposed to 19% of the control parolees, were suspected of having these problems thus far on parole.

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

There are differences between the experimental group and the control group, then, in agent assessment of criminal association, 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 an actual fact of parole adjustment--that the experimental group as a whole is somewhat less involved in criminal association, that it has fewer problems with drugs and

alcohol, and that it is less likely to show recidivism.

Considering all agent responses on all questions, it may be said in summary that 1) DFA had a positive impact on parole adjustment; 2) it had a positive effect on the agent/client relationship; and 3) most agents felt DFA was 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 is one focusing on the cost-effectiveness of the project. By this it is 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 (reductions in property crime could not be measured at this stage of the research, but will be assessed in future studies).

Using the overall nine percentage points difference in parole outcomes favoring the experimental group as our best estimate of the improvement engendered by financial aid, a projection of savings in prison costs is possible. Based on an estimated annual prison cost of $4400 and subtracting the estimated annual parole cost of $600, we deduce that for each man kept on parole for one year we save some $3800 over what it would cost per year if he were returned to prison. Since each man returned to prison typically spends about 19 months on the average before he is re-paroled, the total cost per man returned to prison is approximately $6000.*

To compute an estimate of savings or monetary benefits, the differential outcome of 9 percent--roughly 9 men--is multiplied by this $6000 cost-per man-returned for a total savings of about $54,000. It must be noted further that this figure does not include other system or processing costs such as public damages or losses; costs of jails, police, courts, etc.; and often the welfare costs of an offender's

*Based on CDC, Division of Research Estimations.

family. 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. The $54,000, then, may be considered a minimum estimate. The computation may be made as follows:

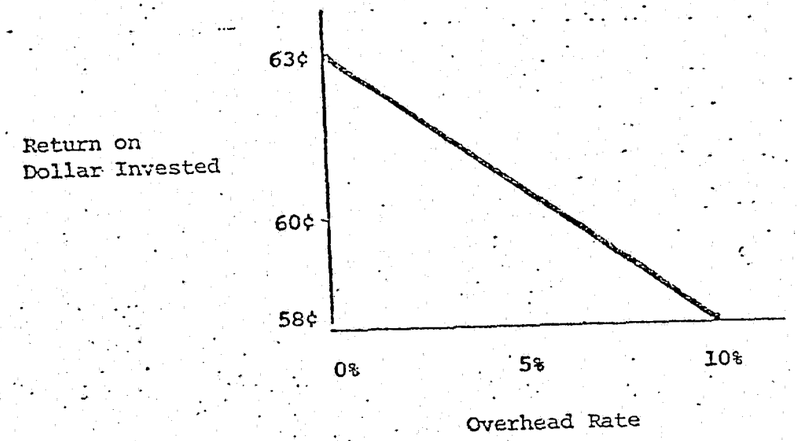
	Experimentals	Controls
Total Number of Parolees	134	118
Number of Recidivists at 6 Months	27	34
Percent Recidivism	20.1%	28.8%
Cost of Recidivism (Per Man)	X$6,000	X$6,000
Total Cost of Recidivism	$120,600	$172,800

Subtracting, we find that approximately $52,200 was saved with the financially-aided group.

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 rendered to experimental parolees.* This yields an estimated return of $.63 for every $1.00 invested. It must be remembered, however, that there was no overhead or administrative expense involved in running the program; the Department of Corrections donated this expense as their in-kind contribution to the grant funds. As total return on investment varies inversely

*The balance of the $120,000 was rendered to pre-test and supplemental parolees, not included in the study sample.

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, perhaps through mass administration. A simple graph illustrates this linear relationship between Return and Overhead.



The return under a given overhead rate is computed by the following equation:

$$\text{Return} = \frac{\text{Savings due to Program}}{\text{Program Costs X (1 + overhead rate)}}$$

at 0% Overhead: $\text{Return} = \frac{\$52,200}{\$82,396 \text{ X } (1 + 0)} = \$.63$

at 10% Overhead: $\text{Return} = \frac{\$52,200}{\$82,396 \text{ X } (1 + .10)} = \$.58$

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 spiralling costs of building and maintaining prisons, jails, and parole agencies.

Further Research

Although the data and findings here presented show great promise, further evaluation is crucial if the Department of Corrections and funding agencies are to maximize the understanding and the effects of the experimental program. If the second year research is completed as proposed, the experimental and control groups will be followed up at twelve months to determine arrest, employment, and recidivism patterns over the longer time period. This will indicate the extent and nature of the more permanent effects of the financial assistance. A model on the maximal amount of funds and the optimum length of disbursement will be developed as a result.

During the second year of research the CDC "12-month follow-up" data and CII arrest reports for both groups will be integrated with the background, initial parole, parole agent, and 3 and 6-month parole outcome data to more fully determine under what conditions and for what types of parolees financial assistance had a concrete positive impact. Further, all parolees in both groups would be interviewed to isolate the social, economic, and psychological factors effecting parole adjustment and the impact of financial assistance, data which is tantamount to understanding the effectiveness of the program. This data would provide the social context of the statistical analyses, the frame through which the portrait must be viewed.

In addition, the CII arrest data will be content-coded and analyzed to assess the fluctuations in the incidence of specific crimes, most particularly property offenses. The arrests and dispositions will be coded at 4, 9, and 12 months after release for each parolee.

VI. CONCLUDING REMARKS AND RECOMENDATIONS

DISCUSSION AND 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 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 vis-a-vis recidivism, and does seem quite cost-effective.

The rather broad effect of direct financial aid across most parameters of the sample as well as the scope of the resulting decrease in further recidivism suggests that the project should be replicated and the results more extensively verified in larger future studies. In general it seems that some financial assistance, across the board, should be made available.

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 we studied. With the possible exception of high B.E.'s, alcoholics, and the youngest age groups, most other groups profitted at least to some extent.

Furthermore, due to the relatively small size of our 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 this more sophisticated form of analysis and this could develop more accurate profiles or combinations of factors where optimum effects might be approached. From what was learned in this experiment, a number of suggestions flow.

1. Without hesitation we urge that the DFA program be replicated. The findings to date indicate that the possibilities for positively effecting parole adjustment loom large, and they far outweigh any chance to the contrary as experienced in this project. The program should be enlarged to include a greater number of parolees; perhaps a state-wide program or at the least another entire parole region.

2. This enlargement of the DFA program should include a research component to test the findings herein and elsewhere, and to aid in the effective administration of the program.

As the size of the samples in this experiment limited the precision of the research, a larger experiment would yield greater validity and reliability of results and greater clarity of the relationships between financial aid and successful adjustment on parole, so that optimum effectiveness could be achieved.

3. Until such time as it has been conclusively proven that certain specific and socially isolatable groups of parolees do not in any way benefit from financial aid, we urge that this assistance be made available to all parolees as they need it.

There exist several reasonable methods of funding projects of this type. The first that springs to mind is submitting a proposal to a funding agency such as the California Council on Criminal Justice, as was the case with this program. Other sources might include Federal funding agencies such as the Law Enforcement Assistance Administration of the United States Department of Justice as well as the United States Department of Labor. Both agencies have been active in funding innovative programs in corrections.

In addition, other approaches are possible which could generate their own funds. For example,

1. Given the rising costs of incarceration and the lack of evidence showing any relationship between longer sentences in either deterrence or improved parole performance, parolees might be released three to six months

ahead of schedule. The savings from this alone could finance nearly a full year of financial aid while on parole, and coupled with the resultant decrease in recidivism might prove to be a prudent investment for the taxpayer.

2. A minimum hourly wage for all work done in prison offers substantial rewards to a number of aspects of the correctional milieu. First and foremost it would allow each offender who wished to work some financial independence. Offenders could pay nominal room-and-board--much the same as work furlough programs--thereby letting the offender bear part of the financial burden for his/her incarcertaion. Secondly, a minimum wage would give offenders some dignity in their work; the founding fathers of Georgia and Australia found offenders willing workers whose rehabilitation paralleled their productivity. Given a real stake in their work, productivity and self-rehabilitation would probably mushroom.

On another front, offender-family relations would benefit. Offenders who worked could help support their families, and in so doing, preserve the close social ties so crucial to successful adjustment in and out of prison. All of these potential fruits make correctional tasks easier as well as allowing inmates to accumulate needed "tie-over" funds for their eventual release. These funds, the parolees' own, would go a long way toward bridging the chasm between release and employment--a chasm frought with potential crime and recidivism.

3. In the same vein, some manner of unemployment compensation program might be instituted which would perform the same functions as a financial assistance program. Programs like the "52-20" provisions for released veterans have been shown viable. The regular unemployment compensation program of the California Department of Human Resources Development would be adequate except that in order to quality an offender would have had to have been working for an employer who paid into the unemployment trust fund. Further, his or her wages would have to have been $750 during the specified "base period." If a mimimum wage had been paid, this would be financed through the earnings of each offender. Otherwise a special form of the unemployment program might be instituted for newly released parolees. In this latter instance, the requirements of the HRD program could be used as a model, e.g., claimant must be unemployed, able to work, actively seeking work, etc.[1]

In conclusion, given the promise of lower recidivism shown in this experiment and the resultant benefits to offenders, correctional departments, and the larger society, it seems judicious to urge that the Direct Financial Assistance to Parolees Program be replicated to include more parolees and that the various avenues of funding be fully explored.

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1. See California Department of Human Resources Development; Job Placement and Unemployment Insurance Programs (pamphlet, no date) and also Unemployment Insurance: Handbook for Claimants (pamphlet, no date).

APPENDICES

Appendix: A Methodological Note on Sampling

In the course of sample selection a set of anomalies (for our purposes only) were discovered in the pre-release record system. Well into the project we 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.

Inquires as to the workings of the records system showed no reason to believe that the errors were in any way systematic (i.e., excluded for any reason). However, to insure the representativeness of our samples we gathered all the background data on those missed and then followed the same analytical steps as with 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 between those selected and those not selected, thus although the samples were not consecutive releasees, their randomness and representativeness were intact.

Appendix: 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%) 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.

"DID YOU WORK LAST WEEK?: (asked of experimental group only on financial report forms)

	Percent Reporting					
	Yes		No			
Units located in:	Total Number	Percent	Total Number	Percent	Total	Total Percent
San Francisco	18	36%	30	64%	48	100%
Oakland	11	31	24	69	35	100
San Jose	6	20	24	80	30	100
Santa Rosa	8	57	6	43	14	100
Salinas	3	33	6	67	9	100
ALL UNITS	46	34%	90	66%	136	100%

These jobs do 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% of all the experimental group reported making $100 or less base pay each week. At the other end, only a little more than a quarter (28%) received $126 or more.

FINDINGS: PAROLEE EXPERIENCES OF EXPERIMENTALS
WEEKLY BASE PAY IF "WORKED LAST WEEK" (asked of experimental group only)

Units	$80 or less Total No.	Per-cent	$81-100 Total No.	Per-cent	$101-125 Total No.	Per-cent	$125orMore Total No.	Per-cent	Rm&Board Total No.	Per-cent	Total Total No.	Total Percent
S.F.	1	6%	5	28%	8	44%	4	22%	-	-	18	100%
Oakland	5	45	2	18	1	10	3	27	-	-	11	100
San Jose	1	17	2	33	-	-	3	50	-	-	6	100
S. Rosa	2	25	1	13	1	13	3	36	1	2%	8	100
Salinas	2	67	1	33	-	-	-	-	-	-	3	100
ALL UNITS	11	24%	11	24%	10	22%	13	28%	1	2%	26	100%

Employment and Parole Outcome

An examination of the employment records of the experimental sample over a longer period revealed that 58 found jobs within three weeks of their parole date, another 26 took more than three weeks and 49 had no jobs reported at the end of the initial 12-week period. Time to first employment was not found to be associated with outcome--with all three sub-groups listed above showing a figure of about 80 percent success over the six-month parole period. (See table on following page)

It seems apparent then that while the money was provided to serve as a bridge from prison to a job in the free world, crossing that bridge can take a varying length of time. Some men can apparently move quickly to a job and go on to a relatively successful parole while others, perhaps not as ready for work, take considerably longer, but have just as much parole success. One is reminded here of the gradual re-entry programs for service-

men after World War II and the need for adjustment or an "acclimatization" process before rejoining civilian society. Further research might do well to explore this hypothesis.

TIME FROM RELEASE TO FIRST JOB* (experimental parolees)

Time Period	# of Parolees	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.

END