The Effects of Increased Gate Money

Final Report

on the

Parolee Reintegration Project

for the Department of Correction

CDC # 75-01

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Summary

The Parolee Reintegration Project asked the question: "Will increased financial support alone improve the chances of success for newly released parolees?" In this study, an experimental group receiving a stipend of $470 was compared with two control groups, one receiving the standard $20 "gate money" upon release and another $50. Several indications of parolee success were used, parole violation, rearrest, parole officer assessment, and employment. While the experimental group consistently performed better than the control group, the differences were not statistically significant and no causal inference can be drawn. There is, in short, no strong evidence to suggest that increasing the financial support to newly released parolees increases their chances of successful readjustment.

I

INTRODUCTION

The Parolee Reintegration Project is an experimental program sponsored by the Connecticut Department of Correction to determine whether increased financial assistance will affect the employment and rearrest rates of men released on parole. According to the Department's action grant application, "the principle objective of this program is to demonstrate whether financial rewards can make a significant reduction in the recidivism rate for released adult offenders." In this case, the incentives consisted of increasing the financial support from the standard $20 received at release to a total of $470 received over an eight week period.

This report restates the original assumptions and purposes of this experimental project, describes its design and operation, discusses the results after each man had been released for a period of twelve months, and makes an assessment of the overall inpact of the project.

Assumptions and Goals

There are a number of assumptions implicit in this attempt at reducing recidivism among parolees by increasing their financial resources. One is that most parolees have few or no financial resources awaiting them upon release. Another is that the lack of a means of support is causally related to criminal activity. Thus it is expected that increased employment will in turn tend to lead to reduced criminal activity and decreased incarceration. Working with these assumptions and applying them to the parolee population, a number of specific expectations were developed by the Department of Correction. They are:

1. Parole violations will be lower for those receiving increased financial incentives than for those receiving the standard amount.
2. Arrest rates will be lower for those receiving increased financial incentives than for those receiving the standard amount.
3. Of those arrested, criminal charges will be less severe for those receiving increased financial incentives than for those receiving the standard amount.
4. Parole officers will be more likely to report favorable adjustments for those receiving financial incentives than for those receiving the standard amount.
5. Employment rates will be higher for those receiving financial incentives than for those receiving the standard amount.

II

RESEARCH DESIGN

The basis for the evaluation of this experiment and the testing of these specific expectations is the comparison of the post-release behavior of one (experimental) group which received increased financial support with another (control) group which did not. Post-release behavior of the parolees in both types of groups was measured by indicators referring to the variables mentioned in the hypotheses listed above. Those in the experimental group were allotted a total of $470, in the form of an initial payment of $110 upon release and four additional payments of $90 each at two week intervals. Persons who fled the jurisdiction or were rearrested were automatically terminated from continued participation. Those in the control group continued to receive only the standard $20 upon release. The reasons for selecting a second control group are discussed later in this report.

A standard post-test-only control group design was utilized. A major assumption of this type of design is that the experimental and control groups are equivalent prior to the differential (here the introduction of the financial incentives) treatment. The two standard means for attempting to achieve this equivalency -- matched comparison and random selection -- were not feasible in this project. Matched comparisons were rejected because of the large population (and hence long time period) which they would have involved, and because there is no persuasive theory or consensus about crime-related characteristics which would point to any of a large number of possible characteristics as possible bases for matching. The second means, random selection into one or another of the two types of groups, was rejected by the staff of the Department of Correction on grounds that it would be too difficult to administer, raised constitutional and ethical questions, and would be self-defeating anyway since it would probably be frustrated by attempts to alter the "natural" order of releases.

The alternative finally used as the means for selecting the experimental and control groups was to designate as the experimental group forty-five men released during January and February of 1973, and identify as one control group the previous forty-five men released, and as a second control group the next forty-five. While not ideal, this selection procedure was adopted after determining that the release dates of prisoners are not based on any pattern likely to produce systematic biases in any of the three groups. A detailed examination of the profiles of these groups is found in Appendix I of this report. Our faith in the selection procedure adopted is supported by these tables although something less than perfectly equal distributions are seen.

The subjects participating in this experiment were drawn from men released on parole from the state's two major correction facilities at Somers and Enfield. Men released from other institutions or through other means (e.g. local jails or special work release programs) were excluded in order to simplify the administration of the project and because many of the other alternative release routes are experiments themselves or involved a variety of experimental work or training programs, the effects of which could not easily be controlled for in this rather small project. Inmates released from the women's facility at Niantic were excluded due in part to their few numbers and also due to the variety of other types of release programs currently available at that center. Nevertheless it should be kept in mind that if any program for increasing financial support for persons released on parole or concluding their sentence is adopted, it would probably include everyone released from the custody of the Department of Correction and not just those released from the two institutions from which the sample was drawn.

Background and personal information on each of these 135 men was obtained from central parole office records in Hartford and was gathered in February and March 1973. Follow-up of post-release behavior commenced approximately two months after the first parolee in the experimental group was released and continued for fourteen months until twelve month follow-up data on the last man released was obtained. This follow-up information was obtained from the regular periodic reports of individual parole officers. After an initial personal visit to each of the three regional parole offices by a research assistant, data were obtained by either telephone or personal interviews, depending on the preference of the individual parole officer.

Controls

It is all too common in government-sponsored pilot programs to find that while the ends desired by the experimental project are achieved, the results are not necessarily "caused" by the specific actions or agents designed to bring about the change, but rather by some other set of unanticipated factors. One common problem is that the very designation of "experimental" or "pilot" can alter substantially the behavior of a group and its treatment by researchers or officials who have a special interest in following the experiment. Ideally this problem should be met by conducting a "blind" experiment, so that the subjects -- control and experimental -- are not even aware of the experiment, or a "double blind" experiment in which even the researcher does not have knowledge of the particular status of the subjects. Neither of these courses of action was open in this case, since participation involved receiving varying amounts of money. However, in order to guard against bias and the possibility of a "self-fulfilling prophecy" in the experiment, several efforts at controlling for "researcher" induced changes were undertaken. A second control group was given $30 upon release in addition to the standard gate allowance of $20. Like those receiving a total of $470, these men and their parole officers were informed that they had also been selected to participate in an experiment. It was thought that the additional thirty dollars would not substantially alter the parolee's financial status, and that any differences between this group and the group receiving $20 would be attributed to a "Hawthorne" effect rather than any increased support. Thus the study proceeded with one true experimental group and two control groups.

Several additional steps have been made to control for other possible influencing factors. Appendix I reports on the degree of comparability of the three groups used in this experiment, and some effort has been made to measure the impact of the increased financial support while controlling for the amount of savings in possession of the parolee upon release. Unfortunately however the small size of the sample prohibited the systematic introduction controls into the study. This problem of small sample size was further frustrated by the fact that on almost all of the indicators, the great bulk of the subjects tended to fall into only one of the several alternative categories (e.g. most subjects in each of the groups were _not_ rearrested). This had the effect of creating many blank cells in those cross-tabulations which introduced a third variable.

III

THE FINDINGS

A. Parole Violations after Twelve Months

The first of the several indicators to be used to judge the effects of increased financial support is the rate of parole violations for those in the experimental and control groups. Violations have been tabulated by the frequency of violations of all types issued for each group. The results are reported in Table A. It shows that for the most part there are no strong variations among the three groups. With but partial exception the single largest group of parolees received _no_ formal warnings, reprimands or misconduct reports of any sort. Group II, the experimental group receiving the $470 payment, looks remarkedly like the $20 control group, although the former has a slightly larger percentage receiving multiple reports. The $50 control group has the smallest percentage receiving no reports and conversely has the largest number receiving multiple reports. These differences, however, are not statistically significant and therefore no inference of a positive impact due to increased payments can be drawn with confidence.

Table A

Number of Formal Warnings or Misconduct Reports filed for Each Parolee after Twelve Months

	I(20)	II(470)	III(50)
1)none	23(51.1%)	23(51.1)	16(36.4)
2) 1-2	15(33.3)	12(26.6)	17 (38.6)
3) 3-4	6(13.3)	4(8.9)	4(9.1)
4) 5-6	1(2.3)	3(6.7)	4(9.1)
5) 7 or more	0(0.0)	3(6.7)	3(6.8)
	45(100.0%)	45(100.0%)	44(100.0%)

X^2 II-I NS *

X^2 II-III NS

*throughout this report all tests of significance are at the .05 level of significance.

B. Parole Rearrest during the Twelve Months

Perhaps the most important hoped for result of the Project was a decrease in recidivism for those subjects in the experimental group. Needless to say "true" measures of recidivism are impossible to come by and this study has had to fall back on rearrest figures as obtained by the parole officers. Table B-1 presents a breakdown of total number of arrests for parolees in each group. Again the pattern observed in the previous section is repeated here. There is no difference between the $20 and $470 groups (75.6% of both groups I and II were never rearrested) while the $50 group fared much worse, with only slightly over half (52.3%) of the men in it remaining free from an arrest during the twelve month period.

Table B-1

Relation Between Payment Groups and Number of Parole Rearrests After Twelve Months

	I(20)	II(470)	III(50)
1) none	34(75.6%)	34(75.6%)	23(52.3%)
2) 1-2	5(11.1)	6(13.3)	13(29.5)
3) 3-4	4(8.9)	2(4.4)	6(13.6)
4) 5-6	1(2.2)	3(6.7)	1(2.3)
5) 7 or more	1(2.2)	0(0.0)	1(2.3)
	45(100.0%)	45(100.0%)	44(100.0%)

X^2 II-I NS

X^2 II-III NS

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X^2 II-I NS

X^2 II-III NS

While the differences in the groups are in the expected directions, the variations among them are either inexplicable (i.e. the differences between group I and III) or are not great enough to be treated as meaningful (the differences between I and II). It is therefore impossible to conclude that the Project had any measurable impact on rearrest rates. Another aspect of rearrest-reincarceration will be treated in a separate section below, so any conclusive assessment of the effect or lack of effect of the Project on recidivism rates should await this discussion.

Table B-2 displays information on the status of the parolees at the end of one year. Here too only slight differences among the three groups were found. While 88% of the parolees in group II were "free" at the end of the period, parolees in the two control groups also tended to be free at about the same rate (85% and 83% for groups I and III respectively).

Table B-2

Relation Between Payment Groups and Reincarceration Status at the End of Twelve Months

Status	I(20)	II(470)	III(50)
Free*	35(85%)	38(88%)	35(83%)
Incarcerated	6(15)	5(12)	7(17)
	41(100%)	43(100%)	42(100%)

*Includes those convicted but free and awaiting sentence at the end of 12 months

X^2 II-I NS

X^2 II-III NS

Turning from the rates of arrest and reincarceration, to the seriousness of offenses committed during the twelve month period of release, Table B-3 again shows no distinctive position for the experimental group. Group III had the highest number or rearrests, while groups I and II were nearly identical in their breakdowns. A closer inspection of the seriousness of the charges is afforded by the adjusted table. While the differences here -- particularly between groups I and II -- are not great enough to warrant any excitement, it is interesting to note that group II has the highest proportion of felony arrests among those who were rearrested. The inescapable conclusion to be drawn from Table B-3, however, is that there is no evidence to suggest that the increased financial support produced the expected results.

Table B-3

Relation Between Payment Groups and Seriousness of Rearrests during the Twelve Months

Arrests	I(20)	II(470)	III(50)
no arrests	34(75.6%)	34(75.6%)	23(52.3%)
misdemeanor	9(20.0)	8(17.8)	18(40.9)
felony	2(4.4)	3(6.6)	3(6.8)
	45(100.0%)	45(100.0%)	44(100.0%)

X^2 = II-I NS

II-III NS

Table B-3 (adjusted)

Arrests	I(20)	II(470)	III(50)
misdemeanor	9(81.8%)	8(72.7%)	18(85.7%)
felony	2(18.2)	3(27.3)	3(14.3)
	11(100.0%)	11(100.0%)	21(100.0%)

X^2 II-I NS

X^2 II-III NS

In conclusion, then there is no strong support for the proposition that increased financial support leads to a decrease in the frequency and/or seriousness of rearrests.

C. Parole Officers' Assessments of Parolees Adjustment after Twelve Months

What may be lost in the more easily quantifiable figures on parole violation reports and rearrest statistics may be partially gained in impressionistic assessments of the parolees' adjustment. The Department of Correction has a standard format which parole officers use in their periodic reports to judge the adjustment of their parolees. This "adjustment scale" ranges from a high of "excellent" to a low of "failure." Tables C-1 and C-2 tabulate these evaluations for each of the men in the three groups after one complete year of release.

Table C-1

Relation Between Payment Group and Parole Officer's Assessments of Adjustment after Twelve Months

Adjustment Rating	I(20)	II(470)	III(50)
1)Excellent	7(15.9%)	2(4.4%)	2(4.5)
2)Good or Average	21(47.8)	30(66.7)	23(52.3)
3)Minimal or Fair	6(13.6)	4(8.9)	3(6.8)
4)Poor or Failure	10(22.7)	9(20.0)	16(36.4)
	44(100.0%)	45(100.0%)	44(100.0%)

X^2 II-I NS

II-III NS

Table C-2

	I(20)	II(470)	III(50)
Excellent, average, good:	28(63.6%)	32(71.1%)	25(56.8%)
Minimal, fair, poor, failure:	16(36.4)	13(28.9)	19(43.2)
	44(100.0%)	45(100.0%)	44(100.0%)

X^2 II-I NS

II-III NS

Table C-1 presents quite a mixed picture, and one that is not easily summarized. Contrary to the expected hypothesis, the $20 group has the largest proportion of men receiving the highest adjustment rating (15.9% compared to 4.4% and 4.5% for the other two groups). Turning to the other extreme, group III, the $50 control group, has the highest proportion of parolees receiving "poor" or "failing" ratings (36.4%) with the other two groups having a much lower number of persons receiving this least favorable assessment. These striking features of Table C-1, however, tend to be weakened then they are considered in light of the two center rankings. For example while group I had the highest portion of "excellents" it had the lowest portion of "good or average" assessments. Thus there is no trend for any one group to consistently fare better than the others. This failure to find any consistent pattern across all rankings is seen more clearly if the table is collapsed as in Table C-2. This reduced presentation shows quite clearly that there are only slight differences among the three groups. Still, however, the differences are in the direction originally expected; that is the experimental group does receive the most favorable overall assessment (71.1% of those in the experimental group received the higher ratings, as compared to only 63.6% and 56.8% in the two control groups). These differences while encouraging are not, however, statistically significant and therefore any inference of causality attributed to the increased financial support seems unwarranted.

D. Employment after Twelve Months

An important aspect of the Project was the expectation that increasing financial support during the early and presumably most crucial post-release period would allow the parolee a greater opportunity to stabilize himself in his new environment and give him additional time to find satisfactory employment. This section examines the post-release employment patterns of the one experimental and two control groups. Several different indicators of employment have been used here: 1) whether the parolee was employed at the end of the twelve month period, 2) the portion of the period he was employed, and 3) his average monthly income during the period. While each of the indicators tells something about the parolee's employment, each by itself is an inadequate basis on which to make an important judgment. Among a group which is likely to be particularly susceptible to seasonal and sporadic employment, employment status at the end of any given period presents a far from complete picture. Consequently the total number of weeks employed is also considered. Likewise, the Project was interested in not only whether increased support could lead to increased opportunities for employment but also whether it could lead to increased income from better positions. Thus we included a measure of average monthly income.

The results however do not support the original expectations. At best there is a mixed picture with no clear indication that the experimental group fared significantly better than the control groups. Table D-1 indicates that those in the $20 control group were most likely to be employed at the end of the Project (63.4% as compared to

62.8% and 54.8% for groups II and III respectively). Again the experimental group II is bracketed by the two control groups clearly indicating that no inference of causality can easily be drawn.

Table D-1

Relation Between Payment Group and Employment at the End of Twelve Months

Employed:	I(20)	II(470)	III(50)
1) yes	26(63.4%)	27(62.8%)	23(54.8%)
2) no	15(36.6)	16(37.2)	19(45.2)
	41(100.0%)	43(100.0%)	42(100.0%)

X^2		
	II-I	NS
	II-III	NS

There are, however, a variety of reasons to explain unemployment and a closer inspection of these data must be undertaken before any conclusions can be drawn. In particular it is important to distinguish between those who are unemployed due to illness or incarceration and those who are employable but without jobs. Table D-2 focuses only on those who are employed and those who are employable but unemployed. Those who are unemployed due to incarceration, hospitalization, retirement, or incapacitation have been dropped from consideration here. These reduced figures parallel the figures of Table D-1, with the \$20 control group still having the highest employment rate (81%) followed by the

Table D-2

Relation Between Payment Group and Employment at the End of Twelve Months

	I(20)	II(470)	III(50)
Employed (or school)	26(81%)	27(79%)	23(70%)
Unemployed, but employable	6(19%)	7(21%)	10(30%)
	32(100%)	34(100%)	33(100%)

X^2 II-I NS

II-III NS

$470 group (79%) and the $50 group (70.6%). It appears therefore that even when controlling for the impact of illness, incapacitation and incarceration, the increased financial support upon the likelihood of being employed at the end of the twelve month period still shows no measurable impact on subsequent employment.

Turning to the second indicator of employment success, the number of weeks employed, Table D-3 presents a slightly more mixed picture, with those in the experimental group (II) faring better than those in the two control groups. 61.9% of those in this group were employed for forty-one weeks or longer, as opposed to only 47.7% of those in group I and 38.1% in group III. This same pattern is seen throughout the table, with those in the experimental group more likely to have been employed from 21 to 52 weeks than those in the other two groups and conversely least likely to be employed twenty weeks or less. Despite the substantial percentage differences in the expected direction, the differences are still not statistically significant at the .05 level.

Table D-3

Relation Between Payment Groups and Employment after Twelve Months

	I(20)	II(470)	III(50)
No. of weeks worked:			
0-10	15(35.7%)	7(16.7%)	6(14.3%)
11-20	1(2.3)	2(4.8)	7(16.7)
21-30	3(7.1)	1(2.3)	7(16.7)
31-40	3(7.1)	6(14.3)	6(14.3)
41-52	20(47.7)	26(61.9)	16(38.1)
	42(100.0%)	42(100.0%)	42(100.0%)

X^2 II-I NS

II-III NS

Table D-4 indicates this same pattern of increased performance for those in the experimental group, although the difference in average incomes is not as dramatic and there are some qualifications that should be noted. Those in the experimental group (II) were least likely to earn under $200 per month and most likely

Table D-4

Relation Between Payment Groups and Average Monthly Income During Twelve Months of Release*

Average Monthly Income	I(20)	II(470)	III(50)
0-$200	19(42.2%)	15(33.3%)	19(43.2%)
$201-$400	9(20.0)	18(40.0)	15(34.1)
$401 +	17(37.8)	12(26.7)	10(22.7)
	45(100.0%)	45(100.0%)	44(100.0%)

X^2 II-I NS
II-III NS

* This includes those with non-employment incomes from social security, pensions, VA support, etc.

to earn from $201-400 per month. On the other hand parolees in this group were not those most likely to be in the highest earning category. 37.8% of those in group I earned an average of $400 or more in contrast to only 26.7% of those in the experimental group (II) and 22.7% of those in the other control group (III). As with most of the findings of the Project, the differences tended to be in the expected direction, but were not strong or distinct enough to warrant a clear inference of positive impact.

E. Total Number of Man-Months of Freedom During the Twelve Months

An incomplete but nevertheless useful minimal notion of parole success might be regarded as the ability to remain free from the custody of the Department of Correction. In a period of increasing costs of incarceration, this consideration is of particular interest and the Department of Correction hoped that the costs of the increased gate money program might be offset by the savings to the Department resulting from a decrease in reincarcerations. Consequently, this section compares the overall "success" (as defined above) of the three groups of parolees. A comparison of these groups is facilitated by an examination of the total number of man-months of freedom experienced by the parolee in each of the three groups.

Comparison of the three groups was facilitated by the generation of an index of Group Success Rate (GSR). According to the Department's original hypotheses, the experimental $20 group (II) was expected to have a substantially higher GSR than the two control groups. The computing formula and the GSR's for each of the three groups are included below.

$$\text{Group Success Rate} = \frac{\text{Actual number of man-months of freedom}}{\text{Total possible number of man-months of freedom } (12 \times 45)^{*}}$$

* Ideally this would be 540 for each of the three groups. In fact however the denominator varied for each group due to a death, absconding, early parole termination, and incomplete information. Thus for several men, data were collected for a period of something less than twelve months, and as a result the denominator is less than 540 and varies for each group.

While the denominator is a figure representing the total possible number of man-months of freedom, the numerator excludes all those months that parolees were actually incarcerated in the custody of the Department of Correction. The resulting figures for the three groups are:

I	II	III
413/480 = .36	426/468 = .91	426/492 = .87

91% of the total time of those in the experimental group was spent free of custody in contrast to 86% and 87% for the control groups. Like so many other of the indicators, these figures are in the expected direction but the differences are two small to be very convincing.

F. Summary and Assessment

An analysis of the four separate measures of post-release adjustment -- frequency and nature of parole violations, arrest records, parole officers' assessments, and employment -- and the overall measure of total months free from departmental custody, all lead to a general conclusion that the increased financial incentives have had no appreciable impact. The rate and seriousness of recidivism was more or less evenly distributed among the three groups. Although there were some differences, the parole officers' assessments did not clearly distinguish among the three groups. However there was a mixed picture on employment, with those in the experimental group tending to perform better than those in the two control groups. While these differences seemed to be substantial, they were still not statistically significant. Likewise the composite measure of total number of months free of departmental custody placed the experimental group in the most preferred ranking, although here too the differences were only slight. A conservative assessment of all these findings leads to the conclusion that there is no consistent or measurable impact of the increased financial incentives project. A more liberal position might argue that the data present a mixed picture with perhaps the Project resulting in some slight net benefit. Before making any definitive assessment of the Project's impact, however, there are several other factors that must be considered.

IV

SOME ADDITIONAL CONSIDERATIONS

A. A Comparison of the Twelve Month Findings with the Three and Six Month Findings

It might be argued that the effects of the increased payments would have important short-run effects which would be impossible to identify after a period as long as twelve months. Thus despite a finding of no or only very slight impact after one full year, the Project may have had short-run effects which could still provide a net savings to the Department in that it retarded the rate of the "cycle" of release and return to prison. However even this more modest set of expectations does not seem to have been the case. There is a remarkable degree of consistency in the findings in each of the three reports, at the end of three, six and twelve months. In each case the findings tend to indicate slightly favorable positions for the experimental group, but the differences are not great enough to warrant any convincingly causal inferences. What can be concluded, then is that there appears to be no clear-cut indicators that the Project benefited the parolees either in the short or in the long run.

B. Consideration of Some Possible Confounding Factors

A crucial feature of any experimental social research is the assumption that the initial control and experimental groups are "equal" save for the introduction of the experimental intervention. Ideally this assumption is satisfied by random assignment to the groups and with the use of double blind experimental settings. As indicated earlier, however, neither of these two conditions could be met in this project, and a procedure of serial assignment to the experimental and control groups was relied upon. This section therefore attempts to assess the degree to which this alternative procedure tended to distribute the men evenly as identified by several different characteristics. As a test for internal validity, comparisons of the two pairs of groups (II and I and II and III) have been made along several characteristics. Tables A-1 through A-9 in Appendix I compare the parolees in these three groups by race, age, savings at time of release, marital status, prior employment, drug use history, length of most recent incarceration, and parole release conditions.

For the most part there are only slight differences between the pairs of groups, indicating overall that the three groups are more or less "equal." This conclusion is reinforced by the fact that with but two exceptions none of the differences in the total of eighteen comparisons was statistically significant. Thus the selection procedure eventually adopted, while far from perfect, does seem to have done an adequate job in distributing the characteristics of the parolees throughout the three groups. There are, however, some reservations which must be attached to this conclusion, and they should be spelled out here. First, there was one important and statistically significant difference between the parolees in Group I and Group II (Table A-2). They differed in racial composition. While Group I had a White to Black ratio of almost two to one, just the reverse was found for Group II. (In this case the division in Group III coincided perfectly with Group II.) The other statistically significant difference was found in comparing the prior employment histories of those in

Groups II and III (Table A-6, Appendix I). While 64% of those in Group II had been employed prior to their incarceration only 41% of those in Group III had. (There was little difference between Groups I and II.)

It is difficult to know what to make of these two exceptions. While Groups I and II differed in racial composition, they differ very little on the other characteristics. Unfortunately, the small sample size prohibits a separate control for race, and in the absence of still additional information such a control would not be terribly fruitful. On the other hand the differences on prior employment between Group II and III point to a trend that cuts across several of the characteristics. While the differences were not statistically significant, those in Group III were not only less likely to have been employed prior to their incarceration, they had the highest rate of convictions on drug charges (Table A-5) and the highest rate of histories of drug usage (Table A-7). While it is difficult to assert with any degree of confidence -- as already noted the differences are not statistically significant -- perhaps these cumulative differences between Group III and the other two groups account for the fact that the parolees in Group III tend to have the least impressive record of the three groups on the several post-release indicators.

In conclusion, while the personal characteristics of the three groups are not perfectly distributed, there are no consistent and strong differences to warrant abandonment of the experiment or to render as meaningless the results of this study.

Nevertheless some unanswerable questions persist, and it is recommended that the Department of Correction give serious consideration to a policy that allows random assignment in all future experimental research conducted under its auspices. At a minimum the Department should immediately prepare a policy statement that considers both the important constitutional and administrative problems attached to the use of random selection for the provision of benefits or deprivations to experimental groups in short-term projects.

C. The Possibility of Spurious Correlations

An obvious qualification of the discussion of the findings has to do with the possibility of spuriousness. In social research it always remains a possibility that any differences (or in this instance the lack of any strong differences) may be attributable to a third, undetected or uncontrolled variable. In this study some consideration was given to the systematic introduction of controls for third variables, but was rejected since it would have meant that the size of the resulting subsamples and the distributions would have been too small for meaningful analysis. For example if each group had been divided by race and then distributed into each of the appropriate outcome alternatives, many of the cells would have been blank or had only one or two subjects in them. While the form of the study may have been more rigorous, the small sample size and distribution would have rendered any findings meaningless.

There was however an effort to control for some particularly important factors. In examining post-release employment practices, those who were unemployable were excluded from consideration in an effort to better identify the "active" unemployment rates in the three groups. Also in an experiment designed to focus on the impact of financial support of parolees, the amount of the parolees' savings at their release from the correctional facility was particularly important. A separate analysis which excluded from consideration all those with savings of $100 or more indicated no substantial deviations from the patterns formed by the full forty-five man groups, the results of which serve as the basis for discussion in this report. Appendix II reports on the distribution of rearrests for those with savings of under $100. The pattern it identifies is typical of the other findings on the other indicators as well.

D. Parole Officers' Reactions to the Project

After the termination of the Project, an attempt to contact all the parole officers was made. Views on the Project were solicited from about two-thirds of them. They were asked to express their views on the administration of the Project and more generally the possible benefits of increased financial support to parolees. The responses ranged from mild skepticism to outright opposition. While some parole officers felt that the increased support <u>could be</u> beneficial, they questioned the form that this particular program took. Others were more skeptical as to the likely impact of <u>any</u> program for increasing the financial support to parolees.

Of those who tended in principle to support a policy of increasing financial support, almost all of them favored some type of disbursement program that placed control over disbursement of funds in the hands of the parole officers themselves. Precise reasons for this varied, although for the most part these parole officers felt that they were best equipped to determine who was in need of additional support or who would benefit most from it. One parole officer objected to the experimental program since parolees tended to regard the regular installments of $90 as a matter of right rather than as being conditioned upon successful adjustment to parole. Others argued that a predetermined amount was unfair since parolees with large savings accounts, steady jobs, or some other source of a regular income received the same stipend as did those without any of these advantages. On the whole those who supported in principle the idea of increased financial incentives supported a procedure that would allow the parole officers to exercise discretion in administering them. To this extent they seemed to prefer an expansion of such types of flexible, discretionary programs as the Crisis Fund rather than the experiment which provided for an across-the-board payment of $450.

There were several other parole officers who were much more skeptical of the benefits flowing from this or any other program as well. One parole officer cited the experience of having one of his parolees spend most of his money on an expensive set of new clothes, and thought that any increase in support should be dependent upon the parolees' first learning how to manage money. Another parole officer thought that the program was administered to precisely the wrong people. Rather than increasing the support to the older and "hard-core" offenders being released from Somers and Enfield, he suggested that it would have been more beneficial to increase support to those younger and less experienced parolees being released from Cheshire Reformatory. However, the most frequently cited argument in opposition to the program was that the financial support simply allowed parolees greater opportunities to squander money on drugs and alcohol. Thus there was some feeling that the stipend program may have had, if anything, a negative effect.

E. A Comparison of the Connecticut Parolee Reintegration Project with Other Similar Projects

In something as tenuous and incomplete as most applied policy analysis must be, one way to gain confidence in the conclusions drawn from incomplete data and imperfect research designs is to see how the impact of a single project conforms to the findings of other similar experiments. Hopefully what is lost in the single setting and limited study is compensated for by replication in several settings. Thus it is possible to gain some measure of confidence in the assessment of the impact of a policy even though it has never been assessed according to the strict concerns of experimental research design and execution.

In this case it is possible to draw on the preliminary findings of several other projects that have also sought to measure the impact of increased financial support for recently released offenders. Three projects in particular provide a useful basis for comparison: Project LIFE in Maryland, The California Direct Financial Assistance Project, and a program sponsored by the State of Washington.

The California Direct Financial Assistance to Parolees Project (DFA) was evaluated in a 1973 report prepared by Scientific Analysis Corporation.* The DFA Project was an experiment involving approximately 240 parolees randomly and equally divided into control and experimental groups to determine if increased financial support during the first three months of parole would improve the adjustment of the men. This program called for the experimental group to receive weekly payments of up to $80 for a period ranging from one to twelve weeks. Both the amount and length of support were determined by parole officers' judgments as to their parolees' needs. Based on an adjusted analysis of the results after six months of release, the final report on the California

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* "Direct Financial Assistance to Parolees Project: Research Evaluation," (San Francisco, California: Scientific Research Corporation, July 1973). A Project of the Department of Corrections Parole and Community Service Division, funded by The California Council on Criminal Justice.

DFA Project concluded that "effects ranged from marked positive ones to more moderate ranges, and even some negative effects" (p.4 and 5). Overall the authors of the report concluded the Project had a net positive effect with an average nine percent more men in the experimental group succeeding on parole.

This California project differed from the Connecticut project in several important ways. First was its size; each group contained well over 100 men (in contrast to only 45 per group in Connecticut) thereby facilitating the introduction of controls. Second was the lack of any test of significance or measures of association in the California study. A chi-square test of significance, for instance, would have provided the DFA Project's reporters at least one criteria for making a judgment as to whether differences between the experimental and control groups were great enough to be considered important. My own computations of such tests on the California data indicate that most of the differences between the experimental and control groups that the evaluators regarded as meaningful were not statistically significant at the .05 level. Thus it seems appropriate to revise downward the assessments of the California Project's results as stated in its final report. As with the Connecticut project, the California findings tended to point in the expected directions, but for the most part of the differences were not great enough to allow one to infer causality or positive impact with any confidence.

Another difference involved the amount of discretion and the role of the parole officers in the administration of the two projects. In sharp contrast to the California program, the Connecticut experiment sought to minimize the exercise of discretion and to reduce to a minimum any special role for the parole officers. In Connecticut there were no discretionary judgments as to the amount or duration of the financial assistance. In order to maximize the comparability of the groups and to avoid the undetectable bias and unsystematic intervention such discretion introduces into an analysis, the Connecticut project sought to minimize the role of the parole officer in administering the project. Furthermore, the Connecticut program went to great lengths to avoid any possible Hawthorne effects, effects that the California project seems to have fostered. The Connecticut project was designed to test the effects of increased financial support alone, not support coupled with increased counseling, job placement services, or discretionary disbursement of funds. While the systematic examination of the latter combination of practices is no doubt desirable, with limited funds and the small sample of parolees to work with, such multiple forms of intervention make it impossible to separate out and measure the imporatnce of each of these factors independently. Nevertheless despite these several important differences and problems, it is interesting to note the similarity of results between the two projects: for the most part there were differences between the experimental and control groups in the expected directions, but these differences and despite the conclusions of the California Project, they were never great enough to warrant an unqualified conclusion that increased financial support results in increased parolee adjustment.

The Maryland Project (Project LIFE) is an experimental program designed to test whether 1) direct financial support and 2) specialized job placement programs for parolees will reduce the rearrest rate among ex-offenders.* These two separate intervention possibilities resulted in three experimental groups (support only at $60/week; job placement services only; and $60/week plus job placement services) whose post-release behavior was then compared to one control group. Approximately 125 men released in the Baltimore area were randomly assigned to each of the four groups and their progress as obtained in monthly interviews is being watched for a period of one year following release from prison. At the time of this writing, the project is still underway and no final report is yet available. Nevertheless, the earlier progress reports on Project LIFE have drawn some tentative conclusions from the limited data base and are interesting to view in light of the Connecticut findings.

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* "Quarterly Progress Report: Research-Demonstration Study of Effects on Ex-Prisoners of Financial Aid and Employment Assistance Programs Designed to Facilitate Post-Release Adjustments," (Bureau of Social Science Research, Inc., March, 1973).

Although the March 1973 report cautions on the limited nature and scope of the study, it tentatively concludes that ". . .the data on employment suggest that our job services have had no effect [on rearrest rates]," and that ". . .economic aid discourages the men from taking a job placement services has an impact on arrest. (p. 13)."

While the Maryland findings represent only preliminary and partial analysis, it is nevertheless of interest to compare them with the Connecticut results. In Connecticut, both the short-term findings (after three and six months) and the final twelve-month findings indicated no statistically significant differences. The same bleak analysis is also found in the progress report of the ongoing Maryland study, despite the fact that the financial incentives are larger (a total of $780 per man as compared to $470 in Connecticut) and that the Project also introduces job placement services as well.

There has been still another state-supported project to test the effects of increased financial support to parolees.*

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* "Adult Corrections Release Stipend Program: Evaluation Report No. 2," (Office of Research-I, Department of Social and Health Services, State of Washington, August 20, 1973.)

The Department of Social and Health Services of the State of Washington sought to compare an experimental group of 405 parolees who participated in their Stipend Program with a control group of 330. Those in the former group received an option that amounted to unemployment compensation of $55 per week for up to twenty-six weeks or another option that provided for a one-time payment of up to $200 immediately upon release. Those in the control group continued to receive the standard allotment for parolees at release.

While the Washington experiment has not been designed and reported with the same degree of experimental sophistication as the others, the Project is still being carefully observed and periodic reports assessing its impact have been issues. A recent progress report makes some tentative findings after most of the parolees had been released for a period of six months. Generally they found that those in the experimental categories were reincarcerated at a slightly lower rate than those in the control group. Their differences, however, averaged only two or three percentage points lower, too small for any confidence to be placed in them as indications of a positive impact of the increased stipends. The Washington progress report, however, cautions against undue inferences and withholds any assessment of impact until the project has been underway for a longer period of time and more controls are introduced. Nevertheless here too the tentative and incomplete results seem to conform to the findings in the Connecticut Project; some differences, but too small to be regarded as meaningful.

V

CONCLUSIONS

While the findings in this Connecticut study, as well as those of the other similar projects, are tentative, incomplete and the studies are in need of further replication and more sophisticated designs, there is perhaps some insight to be gained from the consistency of the reports from the several partial studies. In each study and on most of the several indicators, the findings tended to support and reinforce each other. In none of them was any strong evidence found to support the original expectations that increased financial support would make a positive contribution to reducing recidivism and increasing employment among recently released parolees. For the most part the control and experimental groups were indistinguishable from each other, although most of the indicators in the several studies tended to show slight but insignificant differences in the hypothesized directions. Perhaps at a minimum, there is some slight net benefit provided by the increased support programs. It would be impossible, however, to conclude that additional financial support can be justified solely on the basis of improvements found in these studies.

There is perhaps one additional factor to consider. At the outset of this project, a number of parole officers in Connecticut expressed a belief that not only would the increased financial support program fail to achieve its desired goals, but that it would have counterproductive effects. In essence they hypothesized that increased financial support would act as an incentive not to obtain employment and would also lead to increased rearrests in that some of the parolees -- particularly those with histories of drug related offenses -- would be given the resources to purchase drugs and set themselves up as dealers. Neither of these two sets of counter-hypotheses seemed to be borne out. If it is difficult to determine if the Project had any positive impact, there is no evidence whatsoever to conclude that it had these types of negative effects. Virtually none of the indicators even begins to suggest this interpretation. To this extent then a decision to increase aid to newly released parolees can be justified on the grounds that it may help and certainly will not hurt them.

Table A-2

Internal Validity, Comparison of the Two Pairs of Groups by Ethnicity

	I(20)	II(470)	III(50)
White	28(62.3%)	17(37.7%)	17(37.7%)
Non-white	17(37.7)	28(62.3)	28(62.3)
	45(100.0%)	45(100.0%)	45(100.0%)

X^2 II-1 sig at .05

II-III NS

Table A-3

Internal Validity, Comparison of the Two Pairs of Groups by Savings Upon Release

Savings	I(20)	II(470)	III(50)
$100 or under	36(80%)	32(71%)	34(76%)
over $100	9(20)	13(29)	11(24)
	45(100%)	45(100%)	45(100%)

X^2	II-I	NS
	II-III	NS

Table A-4

Internal Validity, Comparison of the Two Pairs of Groups by Marital Status

Marital Status:	I(20)	II(470)	III(50)
Single	23 (52.3%)	22(50.0%)	17(38.2%)
Married	8 (18.2)	13 (29.5)	13(29.5)
Separated, Divorced, Widowed	13(29.7)	9(20.5)	14(31.8)
	44 (100.0%)	44 (100.0%)	44(100.0%)

X^2	II-I	NS
	II-III	NS

Table A-5

Internal Validity, Comparison of the Two Pairs of Groups by Types of Most Serious(sentenced)Offence

	I(20)	II(470)	III(50)
Property	22(48.8%)	17(37.8%)	15(33.3%)
Drugs	11(24.4)	13(28.9)	19(42.2)
Violent	3(6.6)	7(15.6)	7(15.6)
Sexual	9(20.0)	8(17.7)	4(8.9)
	45(100.0%)	45(100.0%)	45(100.0%)

X^2 II-I NS

II-III NS

* Offender types are determined on the basis of the most recent crime committed by the s. If two or more charges appear, the s's record was scanned, and he was assigned to the category with the largest number of priors in his record.

Table A-6

Internal Validity, Comparison of the Two Pairs of Groups by Employment at the Time of Arrest for Which Later Sentenced

	I(20)	II(470)	III(50)
Employed at Time of Arrest:			
Yes	27(60%)	29(64%)	18(41%)
No	18(40)	16(36)	26(59)
	45(100%)	45(100%)	44(100%)

X^2	II-I	NS
	II-III	sig at .05

Table A-7

Internal Validity, Comparison of Two Pairs of Groups by Drug Histories of Parolees

	I(20)	II(470)	III(50)
None	15(33.3%)	22(48.9%)	13(28.9%)
Alcohol	10(22.2)	3(6.7)	4(8.9)
Hard Drugs	20(44.5)	20(44.4)	28(62.2)
	45(100.0%)	45(100.0%)	45(100.0%)

X^2	II-I	NS
	II-III	NS

Table A-8

Internal Validity Comparison of the Two Pairs of Groups by Length of Time Incarcerated on Most Recent Offense

	I(20)	II(470)	III(50)
one year or less	8(17.8%)	11(24.4%)	11(24.4%)
1-1 1/2 years	10(22.2)	7(15.6)	15(33.3)
1 1/2 - 2 years	8(17.8)	6(13.3)	5(11.1)
2-2 1/2 years	5(11.1)	2(4.4)	5(11.1)
2 1/2-3 years	4(8.9)	5(11.1)	3(6.8)
3-3 1/2 years	5(11.1)	3(6.8)	1(2.2)
over 3 1/2 years	5(11.1)	11(24.4)	5(11.1)
	45(100.0%)	45(100.0%)	45(100.0%)

X^2 I-II NS

II-III NS

Table A-9

Internal Validity, Comparisons of the Two Pairs of Groups by Parole Provision

	I(20)	II(470)	III(50)
Special	35(77.8%)	28(62.2%)	34(75.6%)
Standard	10(22.2)	17(37.8)	11(24.4)
	45(100.0%)	45(100.0%)	45(100.0%)

X^2	II-I	NS
	II-III	NS

Table A- I

Relation Between Payment Groups and Rearrest Rates Controlling for Amount of Savings Upon Release (those with savings of $100 or more exc.)

Arrests	I(20)	II(470)	III(50)
None	26(74.4%)	25(80.6%)	16(50.0%)
Misdemeanor	8(22.9)	4(12.9)	14(43.8)
Felony	1(2.7)	2(6.5)	2(6.2)
	35(100.0%)	31(100.0%)	32(100.0%)

X^2	II-I	NS
	II-III	NS

END